Taking It to the Limit: Effects of Increased Student Loan Availability on Attainment, Earnings, and Financial Well-Being*

Sandra E. Black
Columbia University, NBER, IZA, NHH

Jeffrey T. Denning
Brigham Young University, NBER, IZA

Lisa J. Dettling
Federal Reserve Board of Governors

Sarena Goodman
Federal Reserve Board of Governors

Lesley J. Turner
Vanderbilt University, NBER

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Abstract

Growing reliance on student loans and repayment difficulties have raised concerns of a student debt crisis in the United States, but little is known about the effects of student borrowing on human capital and long-run financial well-being. We use variation induced by recent expansions in federal loan limits combined with administrative datasets to identify the effects of increased access to student loans on credit-constrained students' educational attainment, earnings, debt, and loan repayment. Increased student loan availability raises student debt and improves degree completion, later-life earnings, and student loan repayment while having no effect on homeownership or other types of debt.

* Email: sblack@columbia.edu, jeffdenning@byu.edu, lisa.j.dettling@frb.gov, sarena.f.goodman@frb.gov, and lesley.j.turner@vanderbilt.edu. Any views or interpretations expressed in this paper are those of the authors and do not necessarily reflect the views, opinion, or official position of the Federal Reserve Board of Governors. The conclusions of this research do not necessarily reflect the opinion or official position of the Texas Education Research Center, the Texas Education Agency, the Texas Higher Education Coordinating Board, the Texas Workforce Commission, or the State of Texas. We thank Andrew Barr, Judy Scott-Clayton, Adam Isen, Jasmine Santana and seminar participants at Brigham Young University, Duke University, Federal Reserve Bank of New York, University of Maryland, NBER Labor Studies, NBER Education, University of Chicago Harris School, University of Pennsylvania Graduate School of Education, University of Pittsburgh, University of Southern California Center for Economic and Social Research, San Diego State University, University of Virginia, Vanderbilt University, West Point, Wharton School, Brazilian Econometric Society 2022 meeting, SOLE/EALE/AASLE 2020 World Conference, E-con of Education virtual seminar, the Association for Education Finance and Policy 2020 annual meeting, the National Tax Association 2019 annual meeting, and the Western Economic Association International 2019 conference for helpful comments and suggestions.

1. Introduction

Outstanding student loan debt in the United States now exceeds $1.5 trillion (Federal Reserve Bank of New York, 2021), and more than half of undergraduates rely on federal student loans to finance college.[1] Classic models of human capital investment predict that increasing access to credit should also increase human capital (Friedman, 1955) and provide an economic rationale for public provision of student loans. Furthermore, a large body of research shows that, on average, college graduates receive substantial labor market returns, and that these higher lifetime earnings should more than compensate for reliance on student loans to finance college (Card, 1999; Avery and Turner, 2012; Barrow and Malamud, 2015). That said, access to additional debt could harm post-college financial well-being or generate spillovers to other credit markets if students overborrow. Yet surprisingly little is known about the effects of increased borrowing limits—holding college prices and students' other resources constant—on human capital accumulation, earnings, and overall financial well-being.

This study helps to resolve the tension between the potential costs and benefits of student borrowing by leveraging policy-driven expansions in federal student loan limits to provide a comprehensive picture of the short- and longer-run effects of student loan debt. A key challenge to identifying the effects of student loans in the United States is that eligibility is essentially universal.[2] We overcome this limitation by studying the only recent policy changes to federal undergraduate loan limits, which occurred in the fall of 2007 and 2008. These changes produced variation in the maximum amount a student could borrow by entry cohort and class standing. Our difference-in-differences identification strategy compares the outcomes of students who were likely constrained in their borrowing by statutory loan limits to those of students who were likely unconstrained by these limits—based on their first year borrowing decisions.[3]

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[1] Authors' calculations from College Scorecard data (available at: https://collegescorecard.ed.gov/data/).

[2] For undergraduate students, loan limits vary only by three broad categories of class standing (first year, second year, and upper level) and two broad categories of dependency status.

[3] The large number of students borrowing at the statutory federal limit suggests that many would borrow more if subject to higher limits and guides our classification of students as constrained or unconstrained. We refer to all such students as "constrained" for expositional convenience.

An additional challenge to identifying the effects of student loans is data. We leverage two large administrative panel datasets that provide complementary information on education, income, and credit outcomes. The first data set links student-level records from the universe of public colleges and universities in Texas to administrative earnings records. The second is a panel of credit records drawn from the universe of U.S. consumers with credit histories.

The large increases in loan limits, combined with these detailed data sets, provide a unique opportunity to estimate the causal effect of student loan borrowing resulting from increased credit access for a sizeable share of college students. Our analysis follows the outcomes of first-year student borrowers that are financially dependent on a parent at college entry—a group that comprises the majority of borrowers and holds the majority of undergraduate student debt.[4] We examine the key ways liquidity might affect these students' experiences during college—namely, educational attainment, in-school labor supply, and use of alternative forms of credit—as well as financial well-being after college—namely, earnings, loan repayment, homeownership, and engagement with other credit markets. The policy variation we use for identification directly affected students' liquidity without changing any other factors that might also affect higher education investment decisions (e.g., the price of college or access to additional grant aid), allowing us to document the comprehensive consequences of relaxing credit constraints—the first evidence of this sort for a broad population in the United States.

Constrained students who gained access to higher loan limits significantly increased their cumulative borrowing. In the four years following college entry, constrained students who entered four-year colleges in Texas borrowed an additional $1,800 and community college entrants increased borrowing by $1,200. Increases in federal Stafford Loans drive these effects, and our estimates rule out all but small changes in other types of student loans. Higher loan limits significantly reduced the likelihood that four-year entrants worked in the initial years after college entry, suggesting that increased liquidity from federal loans allowed them to reduce in-school labor supply. Nationally, constrained students saw similar increases in

[4] We define dependent and independent students in Section 2. Most undergraduates and 80 percent of first-year borrowers are dependent students. Few independent students appear to be constrained by loan limits and independent students borrowing at the federal limit do not appear to borrow more when loan limits increase.

cumulative student loans and reduced their use of credit cards over the same time horizon, consistent with substitution away from another (typically higher cost) form of credit.

Higher loan limits significantly increased constrained students' year-to-year college persistence and, among four-year entrants, significantly increased bachelor's degree receipt. Importantly, these educational gains translated into improved labor market outcomes and, ten years later, four-year entrants had significantly higher annual earnings.[5] These gains are consistent with models of educational credit constraints and suggest that providing constrained students with additional liquidity enables them to make costly human capital investments that yield positive returns.

The rich credit report data enable insight into whether other indicators of financial well-being improved alongside borrowers' attainment and labor market outcomes. Over the ten years after we first observe them borrowing, constrained borrowers subject to higher limits were less likely to struggle with loan repayment. Consistent with the returns to additional human capital more than offsetting increased debt payment burdens, these students were significantly *less* likely to make late payments or default on their student loans. Further, despite accumulating more student debt, their post-college mortgage and auto-loan take-up rates were statistically indistinguishable from those of their peers, and they were no more likely to be delinquent or in collections on other types of debt, suggesting that increased student borrowing did not have negative spillovers to other credit markets.

Our empirical strategy relies on the key identifying assumption that, in the absence of policy changes to federal loan limits, differences in the outcomes of constrained and unconstrained students would be similar for cohorts affected and unaffected by the two expansions in loan limits. Balance tests across a range of baseline demographic and socioeconomic characteristics, as well as parallel trends in outcomes for constrained and unconstrained borrowers belonging to cohorts that were unlikely to be affected by loan limit increases, support this assumption. Additionally, the timing of loan limit increases fully coincides with increases in borrowing and

[5] Although we find some evidence of increased attainment among constrained community college entrants who gained access to higher loan limits, these students did not experience significant earnings gains. However, estimates are sufficiently imprecise that we cannot rule out increases similar in magnitude to four-year entrants' long-run earnings gains.

attainment. Year-to-year increases in borrowing and reenrollment are only evident in the years in which cohorts had gained access to additional loans. Furthermore, our estimates are consistent across a variety of specifications and robustness checks. Finally, we show that our results cannot be explained by differential exposure to the Great Recession.

Student loans may have heterogeneous effects, helping some students complete college and earn more while leading to worse outcomes for others. Existing evidence suggests that Black and Hispanic student borrowers are more likely to default on their student loans (Haughwout et al., 2019). Hence, we study how the effects of increased credit access varies by race/ethnicity and socioeconomic status. Effects on borrowing, educational attainment, and labor market outcomes do not appear to differ by race/ethnicity or socioeconomic status. Furthermore, constrained borrowers from majority-Black neighborhoods experienced significantly *larger* improvements in student loan repayment compared to those from majority-White or majority-Hispanic areas.[6] This constellation of findings suggests that the human capital gains afforded by higher limits are broadly shared among the traditional-aged students we study rather than being driven by improvements for specific subgroups.

Economists have long been interested in understanding the role of credit constraints for individuals' educational investment decisions. Young adults who expect a positive return would prefer to attend college but lack resources to do so. Because human capital cannot be offered as collateral for a loan, the private market will underprovide credit (Friedman, 1955). Thus, classic models of human capital investment predict that increasing access to credit for education should increase both educational attainment and earnings.[7] A substantial share of

[6] The Equal Credit Opportunity Act bars credit bureaus from collecting race/ethnicity. Thus, when examining heterogeneous effects on outcomes that are only available in credit bureau data, we follow the common approach of using a borrower's neighborhood demographics to proxy for their individual characteristics (e.g., Haughwout et al., 2019; Chakrabarti, Nober, and van der Klaauw, 2020; Dettling and Hsu, 2021).

[7] Evidence on the presence and magnitude of credit constraints is mixed (e.g., Carneiro and Heckman, 2002; Cameron and Taber, 2004; Stinebrickner and Stinebrickner, 2008; Lochner and Monge-Naranjo, 2011; Brown, Scholz, and Seshadri, 2012; Belzil, Maurel, and Sidibe, 2021). Bulman et al. (2021) find little evidence of credit constraints when a parent wins a lottery, potentially due to liquidity being extended to parents rather than children. A related literature looks at the effect of parental housing wealth shocks on college enrollment and finds increases in housing wealth leads to higher enrollment, consistent with either the effect of increased access to credit (via home equity extraction) or a general wealth effect (Lovenheim, 2011; Lovenheim and Reynolds, 2013).

undergraduate students face binding credit constraints, and we provide direct evidence that, that when such constraints are eased, predictions from models of educational investment under credit constraints are confirmed.

This study builds upon a growing body of research examining the effects of access to additional borrowing. Our finding that access to additional student loans increases reenrollment is consistent with experimental and quasi-experimental studies examining the short-run effects of student loans for specific populations (e.g., community college students).[8] We expand upon this literature by showing that student loans lead to persistent gains in *long-run* attainment for a sizeable group of students who have been largely unstudied.

Our findings contribute to a related literature on the effect of student debt on labor market and other lifecycle outcomes. These existing studies use variation in debt driven by sources other than changes in loan access, including changes in college prices, grant aid, other sources of financing for college, or bankruptcy protections for student loans. In contrast to our findings, this literature generally finds that variation in factors such as tuition that results in additional loan debt negatively affects outcomes such as graduate school enrollment (Chakrabarti et al., 2020) and homeownership (Mezza et al., 2020).[9] Changes in debt arising from tuition increases are likely to have a different effect than changes in debt arising from access to additional credit Given our identification strategy and setting, factors such as the price of college and grant aid are held constant and, as a result, our estimates isolate the effect of access to additional liquidity

[8] Dunlop (2013), Wiederspan (2016), Marx and Turner (2019), and Barr, Bird, and Castleman (2021) show that increases in community college students' borrowing due to federal loan availability, the framing of loan offers, or text messages increases reenrollment and course-taking. Four-year college students who gain access to additional grants and loans at the end of their college careers graduate sooner (Denning, 2019). Denning and Jones (2021) show that small increases in the amount students can borrow leads to small increases in student debt but no appreciable gains in credits or GPA. Several papers consider the effects of student loans in other countries including Chile (Solis, 2017; Card and Solis, 2022), South Africa (Gurgand, Loresnceau, and Melonio, 2022), and New Zealand (Chu and Cuffe, 2020). However, the higher education systems in these countries are meaningfully different from the United States in terms of cost, financing options, and college choice.

[9] Increases in student loans also have been linked to post-college job choice (Rothstein and Rouse, 2011; Luo and Mongey, 2019), co-residence with parents (Dettling and Hsu, 2018), and entrepreneurship (Krishnan and Wang, 2019). Goodman, Isen, and Yannelis (2021) show that changes in grant aid and loan limits that occur when a student is classified as financially independent increase homeownership. Gervais and Ziebarth (2019) provide suggestive evidence that borrowing leads to higher wages, but this finding is sensitive to the specification and time frame considered, and the underlying analysis sample is selected based on a potentially endogenous outcome (college graduation).

from the federal student loan program on the outcomes of constrained students and directly inform student loan policy.

Our findings suggest that millions of U.S. students currently borrowing at statutory federal loan limits—about half of all dependent undergraduates who borrow each year—face binding credit constraints that result in suboptimal human capital investment. Further, our results help provide insight into which groups of students are most likely to be affected by expansions to loan access. Expansions are most likely to increase dependent students' borrowing, attainment, and earnings with little effect on independent students' outcomes. Relative to those starting college in a four-year institution, students who first enroll in a community college may see smaller gains. Finally, under relatively weak assumptions, we estimate that the tax receipts generated by students' earnings gains from access to higher loan limits fully compensate the public cost of the additional borrowing and increase social welfare.

2. Overview of U.S. federal student loan programs

Student debt is an increasingly important component of household balance sheets, reflecting rising postsecondary enrollment and college costs. The vast majority of these loans were originated under one of two federal lending programs established under Title IV of the Higher Education Act—the Federal Direct Loan Program and the (now defunct) Federal Family Education Loan (FFEL) Program.[10]

Undergraduate Stafford Loans, the main type of federal loan, feature standardized terms and interest rates.[11] Unlike most other forms of credit, any student who meets the basic eligibility criteria for federal financial aid is eligible for a Stafford Loan, even students with thin or adverse credit histories. Stafford Loans come in two varieties: (1) subsidized loans, which are

[10] Nonfederal student loans represent a relatively small share of student debt—less than 10 percent in recent years (Baum et al., 2019) and no more than 25 percent at the peak of usage in the mid-2000s. Private loans are the largest component of the nonfederal student loan market. Almost all students who finance their education with private student loans also use federal loans (Consumer Financial Protection Bureau, 2012). In contrast to federal student loans, private student loan eligibility and pricing usually depend upon expected ability to pay.
[11] Federal student loan interest rates are set by federal law. See Appendix A and Tables A.1 and A.2 for additional details. Private student loan interest rates are generally higher.

need based and do not accrue interest while the borrower is attending college on at least a half-time basis, and (2) unsubsidized loans, which are not need based.[12]

To qualify for federal grants and loans, students must complete the Free Application for Federal Student Aid (FAFSA), which collects demographic, asset, and income information for students and their households. FAFSA inputs are used to generate a student's Expected Family Contribution (EFC). In combination with the cost of college, the EFC determines eligibility for the federal Pell Grant and the amount of subsidized loan aid students may receive but does not affect the overall amount students can borrow or their unsubsidized loan eligibility. Continuing students must resubmit a FASFA each year.

Importantly for our research design, statutory annual Stafford Loan limits only can be changed by federal legislation and only vary with academic level and whether a student is classified as financially independent by U.S. Department of Education guidelines.[13] The difference in the amount that dependent and independent undergraduate students can borrow in a given year remained unchanged during the period we examine.[14]

Only two changes to undergraduate Stafford Loan limits have occurred in recent history. The Higher Education Reconciliation Act of 2005 raised annual Stafford Loan limits for first- and second-year students beginning in the 2007–08 (hereafter, 2008) academic year, and the Ensuring Continued Access to Student Loans Act of 2008 increased the unsubsidized Stafford Loan limit for all levels of undergraduate students beginning in 2009.[15] These policy changes,

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[12] From 2008-09 to 2012-13, subsidized loans had slightly lower interest rates than unsubsidized loans (Appendix Table A.1). The value of the in-school interest subsidy depends on interest rates and the length of time between borrowing and repayment entry (Appendix Table A.3).

[13] Independent students are 24 or older, have children or other dependents, are married, or are veterans; students who do not meet these criteria are considered dependent students. Whether or not a student is claimed as a dependent for the purposes of income tax filing does not affect their classification for the purpose of student loan limits.

[14] Freshman and sophomore independent students can borrow an additional $4,000 in unsubsidized loans above the dependent student limit, and upper-level independent students can borrow an additional $5,000.

[15] Most students likely learned about the higher loan limits through their financial aid award letters. Nearly all four-year public institutions and half of all community colleges nationwide "package" the maximum amount of loan aid that a student is eligible to borrow in their award letter (Marx and Turner, 2018) and, in a field experiment, Marx and Turner (2019) show that students are significantly more likely to borrow the listed amount. Most students will receive their award letter before the start of the academic year, but the specific timing depends on when they submit their FAFSA. There is no standardized form for financial aid award letters, but the Department of Education requires that schools inform students of their loan eligibility and loan

coupled with the staggered way they affected the amount students in different entry cohorts could borrow based on academic level, generate our identifying variation.

Table 1 summarizes annual borrowing limits—the maximum amount students can borrow in subsidized and unsubsidized loans—for dependent students by college entry cohort and level. The maximum amount students could borrow in their first year of college increased from $2,625 in the 2007 and previous academic years to $3,500 in 2008 and again to $5,500 in 2009. Second-year students could borrow up to $3,500 before 2008, $4,500 in 2008, and $6,500 in later academic years. The maximum amount upper-level students could borrow did not change until 2009, when the $5,500 limit increased to $7,500. Appendix Table A.4 displays the corresponding increase in subsidized loan limits (which were affected only by the 2008 legislation). We focus on the increase in aggregate borrowing limits because of the small price difference between subsidized and unsubsidized loans (see Appendix Table A.3).

Table 2 shows the cumulative effect of these policy changes on aggregate borrowing limits by entry cohort. Even though loan limits did not increase until 2008, students in the 2006 entry cohort who persisted into their fourth year would have experienced a $2,000 increase in their aggregate limit relative to those who entered in 2005 or earlier. Students in the 2007 entry cohort who returned for a second year saw a $1,000 increase in their aggregate loan limit and, by their fourth year, would have been able to borrow $5,000 more than fourth-year students who entered college in 2005 or earlier. Students who entered college in 2008 saw a cumulative increase in loan limits of $7,875, and 2009 and later entrants experienced a $9,875 increase.[16]

Theoretically, students in the 2005 entry cohort could have been "treated" with access to higher loan limits if they remained in school through their fifth year. As we discuss in Section 4, in practice, we find no evidence that students in the 2005 and earlier cohorts borrowed more in response to limit increases that occurred five or more years after they entered college.

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limits if this information is not contained in the award letter. The amount of loan aid listed in a student's financial aid award letter does not affect their eligibility to borrow. Within a given school, student financial aid award letters generally only differ in the amount of grant aid offered but not in the other types of information provided or loan packaging. Appendix A.3 provides additional details around this process.

[16] As we discuss in Section 3, our main analyses focus on students who entered college before the start of the Great Recession (i.e., those who entered in fall 2007 or earlier).

3. Data and sample

Our analysis uses two separate sources of administrative data: Texas education data and credit report data. These two data sets act as complements, allowing us to explore an array of outcomes and document important mechanisms linking increased liquidity to longer-run outcomes. Unfortunately, we cannot link the two data sets.

A. Texas education and earnings data

Our first data set comes from the Texas Higher Education Coordinating Board (THECB) and includes the population of students who entered a public higher education institution in Texas between 2001 and the most recent completed academic year.[17] Texas provides a near-ideal setting to study the effects of student loans because of the large size of the higher-education sector, diversity of institutions, and similarities in student and school characteristics to national averages. Specifically, Texas is the second-largest state in terms of college enrollment, containing 8 percent of all postsecondary students in the United States in recent years (U.S. Department of Education, 2019). The state's large public college system has 60 public community colleges and 37 public universities which, combined, served more than 1.2 million students in fall 2018 (Texas Higher Education Coordinating Board, 2019). Additionally, Texas is similar to the country as a whole in terms of undergraduate tuition, household income, educational appropriations per student, and educational attainment but has slightly lower graduation rates and a higher fraction of Hispanic enrollees (Appendix Table B.1).

The Texas data contains student-level information on college enrollment, credits attempted, degree receipt, and financial aid (grants; student loans, including private and other nonfederal loans; and work-study aid).[18] Importantly, we observe whether and how much each student borrows on an annual basis and can distinguish between loans from different sources. Student-

[17] We obtained access to the Texas administrative data through the University of Houston Education Research Center. Appendix Section B.1 contains additional details.

[18] Students enrolled in private, non-profit universities are included in the data starting in 2004. Our main specification excludes nonprofit students, but we show that our estimates are robust to their inclusion.

level records are linked to Texas Workforce Commission (TWC) data, which include quarterly earnings for individuals employed in jobs covered by unemployment insurance (UI) in Texas.[19]

Our main analysis sample includes first-time college students who entered college in the 2001 through 2008 academic years.[20] This period spans the first year in which the Texas higher education data are available through the last cohort who entered before the start of the Great Recession. We observe students' education and labor market outcomes for at least 10 years after the year they enter college. We focus on dependent students, who make up the majority of undergraduate students and 80 percent of first-year borrowers. Independent students are subject to higher loan maxima, few independent students appear to be constrained by loan limits, and those who are constrained do not borrow more when loan limits increase.

B. National credit report data

Our second data set is the Federal Reserve Bank of New York Consumer Credit Panel (CCP/Equifax).[21] The CCP/Equifax is an individual-level panel data set of consumer credit reports obtained from Equifax—one of the three main credit bureaus in the United States. The data are reported quarterly and consist of a 5 percent random sample of all U.S. consumers with credit histories. The data include detailed information drawn from credit reports, including loan balances and payment status on mortgages, credit cards, student loans, auto loans and other miscellaneous debt, geographic identifiers for current residence, and year of birth, but exclude information on race/ethnicity. Appendix Section B.2 contains additional details.

Because we do not observe school enrollment directly in the credit data, we use the date of loan origination and the amount borrowed at origination for each student loan to create a borrower-by-academic-year data set.[22] We assume that the first year of observed borrowing is

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[19] UI records cover employers who pay at least $1,500 in gross wages to employees or who have at least one employee during 20 different weeks in a calendar year. We winsorize earnings at the 99th percentile.

[20] Although the Texas administrative data contain an indicator for first time in college (FTIC) status, this information is incomplete. Our sample includes students listed as FTIC when they first appear in the data set and a subset of students who are never flagged as FTIC: those classified as first-year students and not listed as transfer students when first observed and who first enroll after the first year in our data set (2001). Our estimates are robust to limiting our sample only to students listed as FTIC at entry.

[21] For additional information, including sampling and methodology, see Lee and van der Klaauw (2010).

[22] We define academic-year borrowing as including loans that originated in July through June of the academic year in question (e.g., July 2003 through June 2004 for the 2004 academic year) and winsorize academic-year

the first year the individual attended school and restrict the sample to those borrowing at or below the first-year Stafford Loan limit for that year. We also exclude borrowers older than 20 at entry from the CCP/Equifax sample. These restrictions help us omit upper-level, graduate, financially independent, and private market student borrowers and ensure we primarily capture new students in our analysis.[23] We focus on cohorts that began borrowing (and presumably entered college) in the 2004 through 2008 academic years because 2004 is the first cohort for whom we can reliably observe first-year borrowing in the CCP/Equifax data set. We use the panel structure of the CCP/Equifax data to assemble information on student loan and other credit outcomes up to 10 years after a borrower likely entered college.[24]

C. Identifying constrained borrowers

Our identification strategy compares changes in outcomes for students who would have been constrained before loan limit increases with the changes in outcomes for unconstrained students. To implement this approach, we must identify those students who would be constrained by the pre-policy change in loan limits.

Our empirical approach to identifying these students is motivated by a high degree of bunching at the federal loan limit. Figure 1 shows the distribution of first year borrowing by cohort in the Texas data (Panels A and B) and CCP/Equifax data (Panels C and D). Vertical lines indicate statutory limits. Panels A and C show the distribution for 2001 through 2007 entry cohorts in the Texas data and 2004 through 2007 cohorts in CCP/Equifax—years in which the

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borrowing at the 99th percentile. We cannot distinguish between federal and private loans in the CCP/Equifax data. Because of concerns regarding delays and infrequency of servicers' reporting of student loans to credit bureaus, we utilize retrospective information on student borrowing amounts by academic year. The data do not include retrospective information on loan payment status, and thus, we do not reliably observe student loan delinquency and default until four years after a student first borrows (at which point all borrowers in our sample have had at least one loan with payment status information on their record for an entire year). All other outcomes are available in all periods.

[23] Most college students who borrow did so in their first year. For example, in the 2016, 73 percent of all dependent undergraduates who ever borrowed and graduated in 2016 first borrowed in their entry year (authors' calculations using the 2016 National Postsecondary Student Aid Study or NPSAS).

[24] To ensure comparability over time, our analyses focus on a balanced panel of borrowers who remain in the credit report data through the tenth year after we first observe them borrowing. Since the CCP/Equifax sampling is based on Social Security numbers, this step removes individuals who pass away over this period. This process also removes any incorrectly duplicated records, which can appear (typically for a limited period) when new accounts are opened and have not yet been linked to an existing credit record.

first-year dependent limit was $2,625. Panels B and D show distributions for the 2008 entry cohort in each data set, when the limit was $3,500.[25] In both samples, we find clear evidence of bunching at the dependent student limit. Conceptually, constrained students are those who would optimally borrow more if federal loan limits were higher.[26] Thus, an assumption is that many of the students who borrowed exactly the loan limit in their first year of college would have borrowed more if they had faced higher loan limits after their first year.

Constrained students represented a large fraction of student borrowers in the years we study. Nationwide, 51 percent of undergraduate federal loan borrowers (17 percent of all undergraduate students) were at the limit in 2004 and 44 percent of borrowers (16 percent of students) borrowed at the limit in 2008 (Delisle and Blagg 2022). These shares are highest among dependent undergraduate borrowers, with 70 percent and 59 percent borrowing at the limit in 2004 and 2008, respectively.[27]

For cohorts that entered prior to the loan limit increases, we classify constrained students as those borrowing an amount exactly equal to the federal limit ($2,625 for dependent students) in

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[25] The panels also highlight the statutory limit for financially independent student borrowers, equal to $6,625 for students entering before 2008 and $7,500 for 2008 entrants. A non-negligible minority of dependent students appear to borrow exactly at the independent student limit. In the Texas data, this could result from misclassification of independent students as dependents in the administrative data or from the fact that dependent students whose parents are denied a Parent PLUS loan due to poor credit are allowed to borrow up to the independent limit (see Appendix A for details). In the CCP/Equifax data, this group could also include students younger than age 20 who are married and/or have dependents. Irrespective of the reason, our main analysis samples exclude students who borrow more than the federal dependent limit in their first year.

[26] While we classify all such students as "constrained," some may not be credit constrained in the classical sense. For instance, Marx and Turner (2018, 2019) show that students may also be inclined to borrow the "packaged" amount of loan aid (i.e., the amount listed on their financial aid award letter) because of information and salience effects. Thus, the bunching at the federal limit shown in Figure 1 could partially result from almost all four-year institutions and half of all community colleges choosing to package the maximum available Stafford Loan. To the extent that students who borrow the maximum available loan because it is packaged continue to borrow at the limit in future years, higher federal loan limits will lead to additional borrowing, and our identification strategy will still uncover a causal estimate of the effects of borrowing on student outcomes. Furthermore, as we show later, estimated effects on human capital accumulation support the interpretation that this group is, on average, credit constrained.

[27] Appendix Tables C.1 (C.2) shows estimates of the share of all (dependent) undergraduate students and share of all (dependent) undergraduate borrowers who are constrained within subgroups defined by higher education sector, race/ethnicity, Pell Grant receipt, and family income, based on the NPSAS (accessed via PowerStats). Overall, constrained borrowers are less advantaged than the typical *student*. However, this is mainly because less advantaged students are more likely to borrow, and once we condition on borrowing status, we see that constrained borrowers are similar to, or perhaps slightly more advantaged than the typical *borrower*. Appendix B has more discussion of the constrained population in our analysis samples.

their first year of college. For 2008 entrants, who faced a higher first-year limit, we consider students to be constrained if they borrowed an amount between the prior-year limit (adjusted for inflation) and the new limit ($3,500 for dependent students).[28] Unconstrained students are those who borrowed an amount less than the pre-policy limit in their first year of college. Students with first-year loan amounts above the federal statutory limit are excluded.

D. Characteristics of constrained and unconstrained borrowers

As our estimates will represent impacts of the policy change on constrained students' outcomes, understanding who these students are is important for interpreting our results. Table 3 displays baseline characteristics of constrained and unconstrained students by entry cohort, separately for students who initially enrolled in a four-year public institution in Texas (Panel A), those who entered a community college in Texas (Panel B), and the CCP/Equifax sample (Panel C). We group entry cohorts that were potentially affected by loan limit increases (those who entered from 2006 through 2008) and cohorts that entered college early enough that students' statutory borrowing limits would have remained unchanged over their first four years of college (those who entered before 2006).[29]

Constrained four-year entrants in the Texas sample are more likely to be White and less likely to be classified as an underrepresented minority (Black, Hispanic, American Indian, or Native Alaskan) than unconstrained four-year entrants, while these groups are equally represented among constrained and unconstrained community college entrants. Among students with available high school records, constrained students entering both two- and four-year institutions have higher math and reading test scores, failed fewer high school courses, and are less likely to be eligible for free or reduced-price lunch than their unconstrained peers.

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[28] Results are quite similar if we do not adjust the prior-year limit for inflation. In the Texas data, higher education institutions sometimes report loan amounts that are net of origination fees to THECB. To allow for this potential measurement error, we classify students borrowing within $50 of the statutory limit as constrained. The CCP/Equifax data are not subject to this issue, and thus, only students borrowing at the limit are considered constrained.

[29] Appendix Table C.3 shows average entry year borrowing, age, and credit outcomes for borrowers in Texas using the CCP/Equifax data, which were similar to those of borrowers nationwide. We provide additional comparisons of the characteristics of constrained and unconstrained students in Appendix Table C.4. Appendix Table C.5 presents summary statistics for two additional groups in Texas: students who do not borrow in their entry year and students who borrow more than the federal limit.

However, constrained and unconstrained students take similar numbers of Advanced Placement courses and advanced math courses. By definition, constrained borrowers have more student debt, but they receive similar levels of federal and state grants compared with unconstrained students. Although students may receive loans from other federal, state, and private sources, the vast majority of their loans came from the Stafford Loan Program.[30]

In the CCP/Equifax data, we see similar evidence that constrained borrowers are more advantaged. Constrained students come from neighborhoods with somewhat higher average income and that have local primary and secondary schools that are slightly higher quality in terms of peer scores and resources per student.[31] Constrained borrowers are less likely to have a credit report or other debt before their first student loan origination.

To summarize, constrained students look more advantaged than unconstrained borrowers, although these differences are relatively small in magnitude. Differences in observable characteristics yield insight into who the constrained borrowers are, but the key difference is that they have signaled through their borrowing decisions that they are likely to accept more debt if offered. Our identification strategy accommodates differences in baseline characteristics between constrained and unconstrained students, as long as their outcomes would have trended similarly in the absence of the policy change.

4. Empirical Strategy

Our identification strategy relies on a comparison between the outcomes of constrained and unconstrained borrowers who entered college in cohorts affected and unaffected by the expansion of federal loan borrowing limits. We focus on student outcomes that occur after the first year of enrollment, as we define our treatment and control groups based on first-year

[30] When we use regression analysis to determine which characteristics are more predictive of constrained status. (Appendix Table C.6), we find that entry college fixed effects are quite predictive, while many demographic and high school characteristics are not once entry college fixed effects have been included.
[31] We obtained the measures of school quality and racial and income segregation constructed by Chetty and Hendren (2017) from Opportunity Insights. The segregation measures are Theil indices, and the test score percentile measure is income adjusted. More information on this data is included in Appendix Section B.2.

borrowing. Students who entered after 2005 experienced substantial increases in cumulative loan limits. We use this variation in a difference-in-differences framework.[32]

The second increase in loan limits in the 2008 academic year coincided with the Great Recession.[33] We address concerns about differential selection into college enrollment and borrowing brought on by the sizable change in economic conditions by focusing on cohorts that entered college before the Great Recession (i.e., those who first enrolled in fall 2007 or earlier).[34] Furthermore, we show that there are no differences in attainment and financial outcomes for borrowers who first attended schools in counties with high versus low peak unemployment during the recession and, speaking to conditions borrowers faced after leaving college, that our results are robust to controlling for contemporaneous local unemployment rates.

It is important to note that our identification strategy limits us to examining the effect of increased access to student loans *among students already enrolled in and borrowing to attend college*. This limitation inherently excludes students who were sufficiently credit constrained that they did not enter college at all.[35] It also prevents us from examining effects of loan limit increases on choice of college at entry (i.e., whether higher loan limits cause students to attend more selective or more expensive colleges).[36] Additionally, students who are unconstrained at college entry

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[32] Our definition of affected and unaffected cohorts is based on a comparison of cumulative borrowing over time for constrained and unconstrained students. While 2005 entrants theoretically could have been affected by loan limit increases that would have occurred in their fifth year of college, the following section shows no differences in cumulative borrowing for any cohorts that entered from 2001 to 2005, and, thus, we consider 2005 entrants to be the final "control group" cohort. Lucca, Naduald, and Shen (2019) use variation in institutional exposure to loan limit increases (based on institution-wide borrowing before the policy change) to examine effects on net tuition. They conclude that increases in loan limits were partially passed through to institutions via increased prices. This behavior will not affect the internal validity of our results as long as tuition increases are equally applied to constrained and unconstrained borrowers. If price increases are larger for constrained students, it should bias us against finding effects of student loans on long-term outcomes.

[33] The legislation that led to the second increase in federal loan limits primarily sought to address concerns that the large number of private lenders exiting from the FFEL guaranteed loan program during the financial crisis would reduce overall access to federal loans by allowing the U.S. Department of Education to purchase FFEL loans. Increases in loan limits were secondary to this purpose. See Smole (2009) for details.

[34] Barr and Turner (2013) estimate that the Great Recession increased college enrollment by 3 percentage points.

[35] The literature on this is inconclusive; Marx and Turner (2019) find small, insignificant effects of student loan offers on enrollment in an anonymous U.S. community college while Solis (2017) shows that access to loans in Chile leads to large enrollment increases.

[36] However, we can examine whether increases in loan limits affect the probability that community college entrants transfer to a four-year institution (or vice versa), which we discuss in Section 5.A.

may become constrained over time if their income or expenses change, which will lead us to underestimate the effects of increased loan access.

We start by estimating event-study models of the following form:

$$Y_{isc} = \beta_1 Cons_i + \sum_{c \neq 2005} \gamma^c \left(\mathbf{1}[Cohort = c] \times Cons_i\right) + \mathbf{X_i}\boldsymbol{\beta_x} + \delta_c + \delta_s + \epsilon_{isc}, \quad (1)$$

where Y_{isc} is an outcome for student i in entry cohort c who first enrolled in school or state s, $Cons_i$ is an indicator for being constrained at entry (i.e., whether a student borrows at the first-year federal Stafford Loan limit in her first year), $\mathbf{X_i}$ is a vector of baseline characteristics (which vary across data sets), δ_c are entry cohort fixed effects, and δ_s are entry school fixed effects (when examining outcomes in the Texas data) or state fixed effects (when examining outcomes in the CCP/Equifax data).

Because of individual selection into institutions, as well as the fact that institutional choice may directly affect the probability a student is constrained through institution-specific characteristics such as tuition, our preferred specification using the Texas data includes entry institution fixed effects. The inclusion of institution fixed effects mimics the thought experiment we have in mind: students select a school and borrowing amount in their first year, and after this point, treatment is determined by exposure to the loan limit expansion. In the CCP/Equifax data analysis, we do not observe entry institution and instead include state fixed effects to capture differences in institutional characteristics across space.[37]

The "treatment" of interest is the interaction between belonging to a specific entry cohort and being constrained at entry: $\mathbf{1}[Cohort = c] \times Cons_i$. Estimates of γ^c will represent the difference in the outcome between constrained and unconstrained students by cohort relative to the 2005 cohort.

[37] As discussed in Section 5.F, when we estimate models that exclude institution fixed effects, we find effects that are similar in magnitude but less precise. Baseline characteristics for models using the Texas data include indicators for whether the student is an underrepresented minority, in-state student, fall (versus spring) entrant, and female, as well as age and EFC at entry. Baseline characteristics for models using the CCP/Equifax data include age at entry fixed effects (aged 17 and 18, 19, and 20), fixed effects for the number of quarters from initial borrowing before creation of a credit report, and, in the year before entry, indicators for having a credit card, auto loan, or mortgage, number of credit accounts (secured or unsecured), and credit score.

The event-study framework is critical for assessing the key identifying assumption of parallel trends in outcomes between constrained and unconstrained borrowers in the absence of loan limit increases. While this assumption is inherently untestable, it generates testable implications, specifically that trends in outcomes for constrained and unconstrained students in cohorts unaffected by the increase in loan maxima should be similar. This approach also allows us to remain agnostic about the relationship between the size of increases in loan limits and increases in borrowing by constrained students.

Our second specification pools the three treated cohorts and all untreated cohorts:

$$Y_{isc} = \beta_1 Cons_i + \psi(\mathbf{1}[Cohort \in \{2006,2007,2008\}] \times Cons_i) + \mathbf{X_i}\boldsymbol{\beta_x} + \delta_c + \delta_s + \epsilon_{isc} \quad (2)$$

In all specifications, we estimate cluster-robust standard errors that are clustered by entry institution for the Texas sample and entry state for the CCP/Equifax sample, to account for within school/geography correlations in borrower outcomes and characteristics. We also present *p*-values from the wild cluster bootstrap-t, where clusters are defined based on entry cohort by constrained status.[38]

Where possible, we separately analyze borrowers attending two- and four-year schools. This is due to large institutional differences across sectors in price, student background, institutional resources, and the fraction of students borrowing – all factors that may affect responsiveness to changes in statutory loan limits.[39]

A. Identifying variation and implications for local average treatment effects

Most existing studies of the effect of student loan debt on attainment and other outcomes use variation in tuition prices or grants for identification, while our empirical approach focuses

[38] There are only a small number of entry cohort by constrained clusters (16 for the Texas sample, 10 for the CCP). Inference based on clustered standard errors can over-reject the null when the number of clusters is small (Bertrand, Duflo, and Mullainathan, 2004). Thus, we do not report standard errors clustered at this level and instead report *p*-values from a test of the null from the wild cluster bootstrap-*t* procedure (Cameron, Gelbach, and Miller, 2008).

[39] Several studies find different responses to financial aid for students in universities versus community colleges (e.g., Denning, Marx, and Turner, 2019; Denning, 2017; Anderson and Goldrick-Rab, 2018; Carlson et al., 2020). We find no evidence that constrained students in cohorts exposed to loan limit increases were more likely to enter a two- versus four-year schools relative to unconstrained students when compared to earlier entry cohorts (Appendix Table C.7). Focusing specifically on the probability of enrolling in a Texas flagship institution (University of Texas – Austin or Texas A&M), we find no significant effects (Appendix Table C.7).

on changes in the amount of available loan dollars. This treatment is likely to affect a different group of students and generate a different local average treatment effect.

Consider current college students who are deciding whether to invest in an additional year of college. In a basic model of educational investment with credit constraints, there are two groups of students at this margin – 1) those for whom the cost of an additional year of school is too high relative to their expected returns and 2) those who would optimally continue to invest in college at the current price given their expected returns but cannot because they are credit constrained. Reducing the price of college through changes in tuition or grants may increase attainment for the first group of students who do not invest when the price is too high, but greater access to student loans would not increase these students' investment in higher education.[40] For the second group – credit constrained students – both reducing the cost of college and easing credit constraints will result in increased human capital investment.

Thus, compliers in our experiment – those who "comply" with the treatment of higher loan limits by borrowing more – should be limited to the latter group – students who are credit constrained, and our estimates can be interpreted as the local average treatment effect of higher loan limits for such students. In contrast, when the variation used to identify effects of student loans comes from increases in prices (either directly in the form of higher tuition changes or indirectly in the form of lower grant aid), compliers will exclude students who are credit constrained as such students have no scope to increase their borrowing in response to higher prices. In addition to increasing borrowing, a long literature suggests that higher prices may have direct effects on these students' outcomes.

To summarize, both increases in higher education prices and increases in loan limits should increase borrowing, but only in the case of higher loan limits will compliers include students who face binding credit constraints. Thus, our setting and identifying variation provide an opportunity to isolate the role of credit constraints on students' higher education investment decisions.

[40] To the extent that federal loan terms provide an implicit subsidy through below-market interest rates, students in this first group may still respond to loan limit increases. However, the value of such a subsidy will likely be small for students who are not credit constrained.

B. Evaluating the key identifying assumptions

We rely on observed borrowing in a student's first year to proxy for constrained status. If the composition of students who borrow the maximum is changing relative to the composition of those who borrow less than the maximum in ways that would lead these students to have systematically better or worse outcomes at the same time as loan limits increase, our approach would yield biased estimates of the effects of changes in borrowing limits.

To address this issue, we show that observable characteristics of constrained students relative to unconstrained students did not change differentially after the expansion of loan limits in the Texas and CCP/Equifax samples. To avoid concerns related to multiple hypothesis testing, we generate a linear prediction of the probability of graduation within 10 years of entry based on the full set of observed baseline characteristics in the Texas sample. We then estimate "treatment effects" on this outcome. Point estimates from equation (1) and corresponding 95 percent confidence intervals are displayed separately for students who initially enrolled in a four-year public institution (indicated by dark circles) and community colleges (indicated by the light gray Xs) in Figure 2.[41] We find no evidence of differential changes in characteristics for the constrained students relative to the unconstrained students.[42]

Table 4 presents the estimated effects for individual baseline characteristics in the Texas sample and the predicted probability of graduation, as well as baseline credit outcomes in the CCP sample, all estimated using equation (2). Panel A displays estimates for students who initially enrolled in a four-year public institution in Texas, Panel B shows estimates for community college entrants in Texas, and Panel C displays the CCP estimates. For four-year college entrants, we find no significant differences on our summary measure of predicted graduation and no differences in race, gender, EFC, and a marginally significant ($p < 0.10$) but economically small difference in age (0.04 years). For community college students, we find significantly lower EFCs for constrained students in treated cohorts but otherwise insignificant differences.[43] This finding suggests that results pertaining to community college entrants may

[41] We exclude baseline characteristics ($\mathbf{X_i}$) from these models; estimates are robust to their inclusion.
[42] Appendix Figure C.1, Panels A through L, include similar figures for each individual baseline characteristic.
[43] The difference in EFC represents a relatively small difference in family income. In the 2008 NPSAS, the $947 difference between constrained and unconstrained community college entrants corresponds to a $3000 (6

represent an underestimate of the effect of limit increases (if constrained community college students were more likely to be negatively selected after the policy change).[44]

In the CCP/Equifax sample, we find no significant differences in most baseline characteristics, including having a credit report before entry, having a credit score, the number of accounts, and having an auto or mortgage loan. We do, however, estimate a statistically significant, albeit small in magnitude, difference in age and a statistically significant difference in the probability of having a credit card at entry. We control for these baseline characteristics in our regressions to account for any differences. Event study estimates, discussed in the next section, provide additional support for our key identifying assumption and we find similar trends in outcomes for constrained and unconstrained students in cohorts that were unaffected by loan limit expansions.

5. Results

We first document how student borrowing to finance college changes for constrained students when loan limits increase. Figure 3 presents point estimates of the constrained versus unconstrained differential cumulative borrowing by cohort (relative to the 2005 cohort) for up to five years after college entry in the Texas sample for all entry cohorts. Borrowing among constrained students in 2005 and earlier entry cohorts is slightly downward trending relative to unconstrained students. This downward trend is mechanical – because we adjust amounts borrowed for inflation and because nominal loan limits are constant over these years,

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percent) difference in family adjusted gross income (AGI). The (insignificant) $1800 EFC difference for four-year entrants corresponds to an approximately $2000 (3 percent) difference in AGI.

[44] We also document changes in other sources of entry-year financial aid in Appendix Table C.8. We find no differences in Pell Grant aid, TEXAS (Towards EXcellence, Access, and Success) Grant aid (the largest state aid program), other grant aid, or work study that are significant at the 5 percent level for four-year college entrants. Overall, our results suggest that for this population, loans increased while other sources of financial aid did not. Constrained community college students in treated cohorts receive significantly greater Pell Grants and face a significantly lower cost of attendance, but both effects are small in magnitude (6 percent and -8 percent, respectively). In 2004, Texas gave institutions more latitude to set tuition. Andrews and Stange (2019) show that programs with higher predicted earnings increased both tuition and need-based financial aid. This policy change does not affect the internal validity of our results, as it was not concurrent with the increase in loan limits. However, to the extent that differential tuition across programs interacts with loan limits, it may affect the extent to which our findings can be generalized to settings where tuition is fixed across programs.

constrained students' borrowing is declining in real dollars.[45] In contrast, the difference in cumulative borrowing between constrained and unconstrained students in post-2005 entry cohorts – those who had access to higher loan limits – is positive and statistically significant. Cohorts exposed to larger limit increases see larger increases in borrowing. Importantly, we see no effects on cumulative borrowing for 2006 and 2007 entrants until the years in which the loan limit increases occurred (one year after entry for 2007 entrants and three years after entry for 2006 entrants). We find similar patterns in the CCP/Equifax sample (Appendix Figure C.2) and when splitting the Texas sample by type of entry institution (Appendix Figure C.3). In contrast, loan limit increases do not result in significant increases in cumulative borrowing among students classified as independent at college entry (Appendix Figure C.4), which motivates our focus on dependent student entrants.

We next turn to our main specification—equation (2)—and focus on pre-Great Recession entry cohorts. Table 5 presents estimated effects of loan limit increases on constrained students' cumulative borrowing at entry and over the six following years. Four years after entry, constrained students exposed to loan limit increases who initially entered a four-year public institution in Texas had approximately $1,800 more in cumulative student debt (Panel A), a 10 percent increase relative to the sample mean. Similar to the patterns shown in Figure 3, increased borrowing largely occurs in the first four years after entry. Impacts on constrained community college entrants follow a similar pattern (Panel B), with smaller increases in borrowing, amounting to around $1,100 in additional debt (a 12 percent increase), four years after entry. Effects on cumulative borrowing in the CCP/Equifax sample are slightly larger in magnitude than the Texas sample (Panel C), with impacts approaching $2,100 (a 7 percent increase) in the four years after initial loan take-up.[46] The increase in borrowing potentially

[45] Estimated effects on nominal amounts are not statistically significant for pre-2005 cohorts.

[46] Effects on loan balances continue to grow in the fifth and sixth years in the CCP/Equifax sample. This potentially reflects borrowing for graduate education (which is observed only for graduate school enrollment within Texas for the Texas sample), debt consolidation loans (in which interest is capitalized and a new loan is originated to replace several previous loans), and differences in the types of students represented in the CCP/Equifax versus Texas data. We cannot distinguish between graduate and undergraduate borrowing in the CCP/Equifax, nor can we identify consolidation loans and, thus, view the initial years (during which increases in loan balances in the CCP/Equifax sample are most likely to represent increased undergraduate borrowing) to be most comparable.

reflects two forces at work: (1) borrowing more in each year of education because of higher loan limits, and (2) increased borrowing in later years if higher limits increase persistence.[47]

These results are notable: Access to additional student loans substantially increases constrained students' borrowing, all other factors, including tuition costs and grants, held constant. This result is *prima facie* evidence of binding credit constraints for students who start college as a dependent student, a group that includes the majority of undergraduate student borrowers.

Recent work suggests that students may increase their borrowing in response to higher loan limits not because they are credit constrained, but rather because they use heuristics or default into borrowing whatever amount offered (Marx and Turner, 2019). However, Marx and Turner (2018) show this behavior can be rationalized as an "internal" credit constraint. While the underlying source of these credit constraints is different (financial markets versus behavioral biases), the end result is the same—when credit constraints are relaxed, students borrow more.

A. Effects of loan limit increases on educational attainment

Figure 4 previews our findings on the effects of raising loan limits on cumulative years of enrollment. We continue to estimate separate models by type of college at entry and examine effects on enrollment in four-year institutions (Panel A) versus community colleges (Panel B). Ten years after entry, constrained students who initially entered a four-year institution in 2007 and 2008 spent significantly more years enrolled in four-year institutions compared with students in earlier entry cohorts. Loan limit increases did not affect the number of years community college entrants spent in four-year institutions. Panel B shows that the increase in years of enrollment for constrained four-year entrants did not come at the expense of a

[47] Increases in cumulative loans is driven by changes in federal loan limits rather than by (potentially endogenous) changes in the availability of other types of loans. Appendix Tables C.9 and C.10 show that increases in cumulative total loans for Texas borrowers come from increases in federal Stafford Loans. We find no statistically significant changes in state loans, Perkins loans, or private loans. Appendix Table C.11 shows estimated effects of loan limit increases on the probability of any student loan take-up by years since entry in the Texas sample. The probability of borrowing increases 4 to 5 percentage points in the first three years after entry among four-year Texas entrants and 2 to 3 percentage points among community college entrants. This is likely due to the increase in persistence that results from increased student loan access—students are more likely to enroll and are hence more likely to borrow.

reduction in community college attendance. However, the significant pre-trends in this outcome for community college entrants' enrollment suggest that we should view estimates for this group with caution.[48]

Corresponding figures with year-by-year estimates of effects on enrollment can be found in Appendix Figures C.5 (any public higher education institution), C.6 (four-year public institution), and C.7 (community college). The timing of enrollment effects aligns with the timing of loan limit increases. Specifically, constrained four-year entrants in the 2007 and 2008 cohorts—who saw their loan limits increase within one year of enrollment—were significantly more likely to reenroll one and two years after entry (Panels A and B of Appendix Figures C.5 and C.6). In contrast, 2006 entrants—who did not gain access to higher loan limits until three years after entry—had only (marginally) significant enrollment gains three years after entry and no changes in the likelihood of enrollment in earlier years (Panel C of Appendix Figures C.5 and C.6). The timing of these effects suggests that increased access to loans, rather than some omitted variable affecting constrained students in the later entry cohorts, led to their attainment gains.

Effects on cumulative credits attempted by entry cohort follow similar patterns (Figure 5). Ten years after entry, constrained four-year entrants in cohorts affected by loan limit increases had attempted significantly more credits within four-year institutions than those in earlier entry cohorts (Panel A). Effects on credits earned in community colleges for four-year entrants are negative but not significant at the 5 percent level for 2007 and 2008 entrants (Panel B). We again see evidence of significant pre-trends among constrained community college entrants for cumulative enrollment.

Finally, Figure 6 shows the effects of loan limit increases on constrained students' degree receipt 10 years after entry. Four-year entrants were significantly more likely to complete a bachelor's degree (Panel A) and slightly less likely to complete an associate degree (Panel B), suggesting that access to higher loan limits prevented some "reverse transfers" from four-year

[48] As discussed in Section 5.F, when we exclude the 2001 and 2002 cohorts—the cohorts for which we see significant pre-trends—we continue to find significant increases in years of enrollment, cumulative credits attempted, and associate degree receipt among community college entrants.

to community colleges. For community college entrants, estimates are relatively noisy, and we find no evidence of significant increases in degree receipt.[49]

The effects on borrowing and educational attainment provide evidence of binding constraints affecting four-year entrants' human capital investment. Higher loan limits did not increase the likelihood that community college entrants "upgraded" to a four-year program, as we find no effect of loan limit increases on these students' probability of enrolling in a four-year institution, number of credits attempted at four-year institutions, or bachelor's degree receipt.

The lack of significant effects on community college entrants' attainment could be explained by the smaller borrowing response to loan limit increases in this population (around 60 percent the size of the increase among four-year entrants). Additionally, estimated effects on community college entrants' enrollment and cumulative credits attempted are sufficiently imprecise that we cannot reject the hypothesis of equivalent effects for community college and four-year entrants (Appendix Table C.13). However, we can reject a test of the hypothesis of equal effects of loan limit increases on the probability of receiving any degree (e.g., $p = 0.076$ for effects 10 years after entry). When we pool community college and four-year entrants, we find results very similar to those for four-year students (Appendix Table C.14 versus Table 6). For brevity, our discussion of effects for the Texas sample will focus on four-year entrants, who represent the majority (71 percent) of constrained borrowers. Appendix C contains corresponding estimates for community college entrants.

The estimates from equation (2) in Table 6 summarize the effect of increased loan limits on constrained four-year entrants' educational attainment. Constrained students were significantly more likely to (re)enroll in college every year, up to the third year after entry, with effects ranging from a 4- to 5-percentage point increase (Panel A). At this point, four-year entrants had attempted 5.5 additional credits (Panel B) and spent an additional 0.15 year enrolled in college (Appendix Table C.15).

In addition to improving persistence in college, access to higher loan limits increased degree receipt within 10 years of entry. The 4.3 percentage point gain shown in Panel C of Table 6 is

[49] We find no evidence of loan limit increases resulting in significant effects on undergraduate certificate receipt for either four-year or community college entrants (Appendix Table C.12).

driven by an approximately 5 percentage point increase in the probability of bachelor's degree receipt combined with small, negative effects on associate degree receipt (Panels D and E). While 19 percent of all four-year graduates earned a bachelor's degree in a science, technology, engineering, or math (STEM) field, almost 30 percent of the increase in bachelor's degree receipt due to higher loan limits comes from STEM graduates (Panel F). In Texas, STEM degrees tend to take longer (Cullinane and Lincove, 2014) and, beginning in fall 2004, students in STEM degree programs faced higher tuition (Stange, 2015; Kim and Stange, 2016). Our results suggest that additional availability of credit allowed students to make investments in potentially costlier degrees that also tend to produce higher average earnings gains (Andrews and Stange, 2019).

Higher loan limits led to small increases in four-year entrants' graduate program enrollment four and six years after entry, and a statistically significant 1.2 percentage point increase in graduate degree receipt within 10 years of entry (Appendix Table C.15). The effects on cumulative years of enrollment, cumulative credits attempted, and degrees are all statistically significant and persist until the end of our panel, a point at which very few students were still enrolled, suggesting that loan limit increases led to a lasting increase in graduation rates rather than simply a retiming of degree receipt.[50]

B. Effects of loan limit increases on other sources of college financing

We next examine whether expanded loan limits affected other potential sources of financing for college—earnings while in college and credit card use. As shown in Table 7, when constrained students gained access to higher federal loan limits, they were also significantly less

[50] Estimates for community college entrants suggest this group also experienced increases in educational attainment. Appendix Table C.16 shows cumulative increases of approximately 0.2 years of enrollment and 5.5 credits attempted in the 10 years after entry. Loan limit increases did not affect constrained community college entrants' likelihood of earning a bachelor's degree but had significant positive effects on associate degree receipt. Appendix Table C.17 shows that four-year entrants' increases in enrollment and cumulative credits attempted were concentrated in the four-year sector, with small offsetting effects on cumulative community college credits attempted. Appendix Table C.18 shows that among community college entrants, we find significant increases in enrollment and credits attempted in community colleges and positive but largely insignificant effects on four-year college enrollment (our proxy for the probability of transferring to a four-year institution). Finally, we find some evidence that four-year entrants who did not initially enroll in a flagship public institution were 0.7 to 0.9 percentage points more likely to transfer to a flagship in their second through fourth years after entry (Appendix Table C.19).

likely to have a credit card in the year after entry.[51] Credit card holders also carried significantly lower balances one and two years after entry. Specifically, credit card use fell 2 percentage points (4 percent) in the year after entry, while cardholders' monthly balances fell by around $85 (5 to 7 percent) one and two years after entry.[52]

Constrained four-year entrants were significantly less likely to have any earnings in the two years following initial enrollment (Panel C of Table 7), suggesting that additional student loans may allow students to spend less time working while in college. Among employed students, earnings were 3 to 5 percent lower, although these effects are statistically significant only one and three years after entry.[53] Taken together, the reduction in credit card utilization and earnings from employment indicate that greater student loan availability reduced constrained students' reliance on other sources of liquidity during school.

C. Effects of loan limit increases on post-college earnings, employment, and credit outcomes

Given that loan limit increases result in constrained students accumulating both more debt and more human capital, we next consider impacts on longer-run credit and labor market outcomes. We focus on these outcomes beginning four years after entry—a point when many students have left college.

We first consider how access to additional loans affects student loan repayment. While the increases in federal loan limits resulted in higher average balances—which could lead to increased difficulty in making minimum payments—constrained students also acquired more

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[51] Appendix Figure C.8 shows that these effects are driven by the 2007 and 2008 cohorts – the two of three "treatment" cohorts that had been exposed to loan limit increases one year after entry. Table 4 indicates a small negative effect on credit card holding in the year before entry by constrained borrowers who faced higher limits; estimates in Table 7 and Figure C.7 are net of this effect, as we control for baseline credit card holding. The Credit Card Accountability Responsibility and Disclosure Act of 2009 (effective 2010) restricted credit card availability for those under age 21. This restriction could have affected the youngest members of the 2008 cohort, who would have still been under age 21 for some of their second year from entry in 2010.

[52] Balances are reported at an arbitrary point in the monthly billing cycle and thus reflect a combination of new spending and revolving debt. In the years we study, approximately 60 percent of households with student loan debt and credit cards revolved debt on a credit card (authors' calculation from 2004-2010 Survey of Consumer Finances).

[53] We also find significant decrease in four-year entrants' unconditional earnings (winsorized at the 99th percentile) in the two years after entry (Appendix Table C.20). Estimates for community college entrants suggest that they experience statistically significant increases in the probability of having nonzero earnings two and three years after entry but no significant changes in unconditional earnings (Appendix Table C.21).

human capital. Despite having more debt, constrained students who entered in 2008 were significantly less likely to be delinquent with their loan payments (more than 180 days late) at any point four through ten years after entry (Figure 7, Panel A).[54] Panel A of Table 8 shows that the reductions in annual delinquency rates occur between four and seven years after entry, with estimates ranging from a 1.4 percentage point (15 percent) reduction four years after entry to a 0.9 percentage point (8 percent) reduction seven years later. Over this entire period, the likelihood of ever being delinquent on payments falls by 1.3 percentage points (5 percent).

We define student loan default as at least 360 days of missed payments, corresponding to the time at which a student loan borrower loses the ability to bring delinquent loans current or obtain forbearance. Constrained students facing higher limits are significantly less likely to default on their loans at any point over the fourth through tenth years after college entry (Panel B of Figure 7). Significant year-by-year effects range from a 1.6 percentage point (18 percent) reduction in default rates four years after entry to a 0.6 percentage point (7 percent) reduction seven years after entry. Over this entire period, the likelihood that a borrower ever defaults falls by 1.8 percentage points (9 percent). Taken together, the significant reductions in delinquency and defaults suggest that even with higher student debt, the increase in human capital induced by higher loan limits outweighed any increases in student loan payment obligations.[55]

Higher loan limits did not appear to affect the probability of having nonzero earnings—our proxy for labor force participation—in years when most students have left college (Panel A of Table 9).[56] We find significant effects of higher loan limits on constrained students' earnings beginning in the sixth year after entry and these effects persist through the tenth year after

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[54] With only two untreated cohorts, we are somewhat limited in our ability to check for pre-trends in the CCP/Equifax data. In Figure 7, we cannot reject a test of the hypothesis that differences between constrained and unconstrained students are statistically the same for the 2005 and 2004 entry cohorts. In contrast, we can reject this hypothesis that this difference is the same for the 2005 cohort compared to the 2008 cohort—which experienced the largest increase in loan limits.

[55] Barr, Bird, and Castleman (2021) also find a negative relationship between borrowing and student loan default over the short-run in an experiment in which community college borrowers were randomly assigned to receive information and guidance about student loan decisions through a text messaging campaign. We show that the relationship persists over a longer period for a broader population.

[56] Since we proxy for labor supply with whether a student has any earnings reported to the Texas UI system, a differential probability of observing earnings could reflect differences in employment or differences in the probability of leaving the state or being self-employed. We find small, statistically insignificant effects on the likelihood of having earnings in later years, suggesting that these concerns are unlikely to affect our results.

entry, with estimates implying annual earnings increases between 2.7 and 5.3 percent (Panel B of Table 9 and Figure 8).[57] Consistent with this increase in earnings, the CCP/Equifax data show that constrained students who gain access to higher loans limits live in more affluent zip codes after college (Appendix Table C.22).[58] These results are again consistent with binding credit constraints preventing students from making beneficial investments in human capital when loan limits are lower.

The potential spillovers from student loan debt to other credit products is also a key concern with respect to financial stability. We next examine effects of loan limit increases on other types of debt, which can address the question of whether borrowers might remit on-time student loan payments at the expense of other debt payments. Because the federal government provides most student loans, direct effects of student loan debt on the financial system are limited. However, if increased student borrowing results in higher default rates on other types of debt, federal loans could pose an indirect threat to the U.S. financial system more broadly. We find no evidence of effects on the likelihood of having any delinquent debt (60 or more days past due) or debt that has been placed in collections (Panel A of Table 10 and Figure 9). Thus, the larger student loan balances induced by the limit increases did not appear to have had negative spillovers to payment behavior on other types of loans.

All else equal, higher levels of student debt could also reduce borrowers' ability to finance other investments, such as home and vehicle purchases.[59] Thus, we next examine the effect of

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[57] We observe earnings in the 10 years after entry for our entire sample. Appendix Figures C.9 and C.10 show year by year estimated effects on the probability of having any earnings and ln(earnings) with corresponding 95 percent confidence intervals. Estimated effects on (unconditional) earnings levels show similar patterns (Appendix Table C.20 and Appendix Figure C.11). We find no significant effects on community college entrants' earnings or employment in later years (Appendix Table C.21), although estimates are sufficiently imprecise that we cannot rule out effects that are equal in magnitude to those experienced by four-year entrants in the eighth, ninth, and tenth years after entry (Appendix Table C.13).

[58] We merge time-varying zip code house prices from Zillow, zip code incomes from Internal Revenue Service Statistics of Income records, measures of segregation and school district quality from Opportunity Insights, and county-level educational attainment and poverty rate from Census to the CCP/Equifax. We find significant increases in zip code income and house prices and significant reductions in county poverty rates but no significant effect on neighborhood segregation or school quality.

[59] Based on our findings thus far, the predicted effects on mortgage and auto ownership are ambiguous. Since debt-to-income ratios are used in underwriting, higher levels of student debt could reduce borrowers' ability to qualify for such purchases, and we do find small, marginally significant, negative effects on credit cardholding up to eight years after entry (Appendix Table C.23). On the other hand, improved student loan repayment,

loan limit increases on other aspects of constrained borrowers' balance sheets. We examine effects on mortgage debt (Panel B of Table 10 and Figure 9) and auto loan debt (Panel C of Table 10 and Figure 9), which, among young adults who borrowed to attend college, provide reasonable proxies for ownership of those assets. We find no effect on the probability of having a mortgage.[60] Higher limits generate small increases in the probability of having an auto loan in the fourth and fifth year after entry.[61] Altogether, we find no evidence that additional borrowing adversely affects constrained students' financial well-being after college.

D. Instrumental variables estimates of the effect of student debt

We estimate IV models in which the interaction between being constrained and being in a cohort exposed to loan limit increases serves as the excluded instrument for student debt. Under the assumption that loan limit increases only affect constrained students' outcomes vis-à-vis effects on the amount borrowed, the coefficient on amount borrowed will represent the causal effect of student loan debt on student outcomes. Even if this assumption is not met, this scaling allows for comparison with estimated effects of other sources of financial aid or student loans in other settings.

We find that, in addition to significantly increasing years of enrollment and cumulative credits attempted, an additional $1,000 of student debt increases four-year entrants' bachelor's degree receipt by 2.8 percentage points and their annual earnings by 2.8 percent 10 years after college entry (Appendix Table C.24). These effects are similar in magnitude to the estimated impact of similar-sized increases in grant aid on degree receipt and earnings (Bettinger et al., 2019; Denning, Marx, and Turner, 2019), further supporting the interpretation that increased access to student loans facilitate human capital investment by easing liquidity constraints.

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coupled with the earnings effects, suggest payment-to-income ratios likely fell. Home and auto loans also often require access to upfront collateral (e.g., down payments), which should be easier to accrue for higher earners, unless higher debt payment burdens reduce savings rates.

[60] Appendix Table C.23 shows estimated impacts on additional mortgage-related outcomes. Higher loan limits have small, statistically insignificant effects on mortgage size at origination (conditional on having a mortgage). We do find a significant decrease in a "pseudo" loan-to-value ratio (mortgage size scaled by Zip code median house prices) four years after entry, but the magnitude of this effect is small and applies only to 4 percent of borrowers who have mortgages at this point.

[61] We find no evidence that higher loan limits affected auto loan amounts (Appendix Table C.23).

We also find that a $1,000 increase in student debt significantly reduces student loan delinquencies in post-college years by 0.8 percentage point and defaults by 1.1 percentage points (Appendix Table C.25). We again find no evidence that student loan debt affects homeownership. The estimated 95 percent confidence interval excludes effects larger than a 0.4 percentage point reduction in homeownership per $1,000 increase in debt. This is substantially smaller than the 1.8 percentage point reduction per $1,000 in student debt found by Mezza et al. (2020), which looks at the effects of student debt that result from tuition increases. The stark difference with our estimates suggests that when student loan debt is used to finance human capital investments (rather than used to buffer against increasing higher education prices), it need not affect young adults' likelihood of homeownership.

E. Heterogeneity by Race/Ethnicity and Socioeconomic Status

Given the large disparities in student loan default by family background (Haughwout et al., 2019; Scott-Clayton, 2018), one might expect heterogeneous effects of access to larger loan limits by student race/ethnicity and socioeconomic status. We test for this in the Texas data by estimating models that are fully interacted with indicators for the three largest race/ethnicity groups (Black, Hispanic, and White).[62] Panel A of Table 11 shows that the estimated effects of higher loan limits on four-year entrants' borrowing, credits attempted, and degree receipt are positive for students in all three race/ethnic groups, with somewhat smaller but statistically indistinguishable point estimates for Black students. Similarly, estimated effects on earnings 10 years after entry are positive with slightly smaller estimates for Hispanic students – and we cannot reject a test of equal effects across the three groups, although it appears that it takes longer for Black students to realize earnings gains (Appendix Table C.27). Overall, our educational attainment and earnings results are broadly similar by race and ethnicity.

The CCP/Equifax data do not contain individual characteristics such as race/ethnicity or income (because the Equal Credit Opportunity Act forbids doing so); we use demographics of a

[62] Unfortunately, the number of students in other race/ethnicity categories is relatively small and estimates too imprecise to draw meaningful conclusions (see Appendix Table C.26).

student's "home" zip code to proxy for these characteristics.[63] Table 11, Panel B shows that higher loan limits lead to significantly larger reductions in student loan delinquency and default for borrowers from Black majority zip codes compared to those from White- and Hispanic-majority zip codes. Constrained borrowers from majority Black communities experience statistically significant 7.5 and 7.2 percentage point reductions in student loan delinquency and default, respectively. Borrowers from majority-White areas experience a significant 1.2 percentage point reduction in default. Higher loan limits do not appear to increase student loan debt among borrowers from majority Hispanic areas and thus, it is perhaps not surprising to find null effects on loan repayment outcomes. Given the large gaps in default by race/ethnicity in the more general population, the sizable positive effects on likely-Black borrowers are notable.

To examine heterogeneity by family income, we use Pell Grant eligibility as a proxy for family socioeconomic status in the Texas data. We find similar effects of loan limits on years of enrollment, credits attempted, and earnings for Pell Grant eligible versus ineligible four-year entrants (Appendix Table C.28, Panel A). However, effects on borrowing and degree completion are almost twice as large for Pell-ineligible students. In contrast, borrowers from lower-income neighborhoods—our proxy for family socioeconomic status in the CCP/Equifax data—appear to benefit the most from higher loan limits in terms of their loan repayment, although differences in point estimates are not statistically significant (Panel B). Taken together, these results suggest that both low- and higher-income students benefit from increased access to credit.

Unfortunately, we cannot isolate the effects of higher loan limits for students who attended for-profit schools (although these students are included in the CCP/Equifax data). These students and schools may well experience different outcomes with increased borrowing. That

[63] We define a "home" zip code as the zip code in which we first observe a student borrower. We then merge 2000 Census data on the racial/ethnic make-up of that zip code and IRS Statistics of Income data on the mean AGI. Models using averages across zip codes that a student borrower ever resided in yield similar results as do models that divide zip codes based on having a racial/ethnic supermajority (more than 75 percent of residents belonging to the racial/ethnic group). The Texas data does not include home zip codes.

said, our heterogeneity results confirm that the effects are broadly similar (and positive) among the traditional students we analyze across income, race/ethnicity, and neighborhood.

F. Robustness

Our estimates hold up to a variety of specification and robustness checks (Appendix Tables C.29 through C.31). We obtain similar results for the Texas sample when we exclude all baseline covariates except fixed effects, add students who initially enrolled in a nonprofit institution to our analysis sample, limit the sample to full-year entrants, and use the 2001 sample restrictions in all years.[64] We show that our results for the Texas sample are also robust to excluding the 2001 and 2002 cohorts to address concerns of possible differences induced by the recession affecting those years, and estimates for both the Texas and CCP/Equifax samples are robust to controlling for contemporaneous unemployment. Finally, our estimates from both samples are similar when using a narrower definition of unconstrained borrowers by excluding those who borrow less than half of the Stafford Loan limit (approximately $1,300) in their entry year.

Although we are able to capture the effects of loan limit increases on a wide variety of outcomes by using two distinct data sets, one challenge is that the available control variables and entry cohorts differ across the two datasets. As such, we have opted to use our preferred specification in each dataset and not limited ourselves to only including variables or cohorts common to both datasets. However, when we do constrain ourselves to common control variables (and hence specifications) or cohorts across samples, our results are largely unchanged. Estimates for the Texas sample from models that exclude school fixed effects are similar, albeit less precisely estimated (Appendix Tables C.29 and C.30, Panel H).[65] Results are largely unchanged when we impose the same sample restrictions on the Texas sample as the CCP/Equifax sample.[66] Estimates from specifications that include core-based statistical area

[64] As discussed in Section 3, we use a slightly different sample definition for the 2001 entry cohort, as it is the first year that data were reported to THECB in Texas. Enrollment and outcomes for students in nonprofit institutions are observed starting only in 2004.

[65] Appendix Table C.32 shows that we obtain similar results from a model that pools two- and four-year entrants and excludes school fixed effects, albeit with less precision.

[66] The CCP sample is restricted to students who were under age 20 at entry and excludes cohorts that entered before 2004.

(CBSA) fixed effects to control for variation across schools at a finer (sub-state) level of geography in the CCP/Equifax sample are similar to our main results (Appendix Table C.31).

We also estimate a dose-response specification that takes advantage of the differently sized statutory limit increases faced by different cohorts

$$Y_{isc} = \beta_1 Cons_i + \theta(AggLimExp_c \times Cons_i) + \mathbf{X_i}\boldsymbol{\beta_x} + \delta_c + \delta_s + \epsilon_{isc} \qquad (3)$$

In equation (3), $AggLimExp_c$ is the aggregate amount available to borrow if a student borrowed the maximum for four consecutive years (Table 2) and $\mathbf{X_i}$ includes the main effect of $AggLimExp_c$. This approach allows the effect of the loan limit increase to vary with the size of the increase but imposes the additional assumption that loan limits have a linear effect on student outcomes. This assumption may not be accurate if the first increase in $1,000 of loan access has a different effect on student outcomes than subsequent dollars, or if money received in an earlier year has a different effect than money received in a later year.

Appendix Table C.33 presents estimates from this specification. The interpretation of the coefficient is the effect of a $1,000 increase in aggregate statutory loan limits within the first four years of college for constrained students relative to unconstrained students. Consistent with our main results, estimates suggest that higher loan limits lead to significant increases in years of enrollment, credits attempted, degree receipt, and earnings, significant reductions in student loan delinquency and defaults, and null effects on repayment of other debt, having a mortgage, or having an auto loan.

G. Great Recession

The loan limit increases we study occurred shortly before or at the start of the Great Recession. Thus, a key threat to our identification is the possibility that the Great Recession differentially affected constrained students after the borrowing limit expansions relative to unconstrained students. This could be the case for a few reasons: the differential effects of the recession could have affected if/where students went to college, the financial situations/decisions of students *during* college, or the outcomes of students *after* college. We address each of these threats in turn.

First, students' decisions of whether or where to enroll in college may have responded to the change in economic conditions during the Great Recession. One way we address this concern is

by restricting our sample to students who first enrolled prior to the start of the Great Recession, thereby mitigating the likelihood of an effect of the economic downturn on enrollment. As another check, we test for heterogeneity in the estimated effects of loan limit increases by the economic conditions in a student's home county in the year before and year of college entry. We estimate models in which our main specification is fully interacted with indicators for above and below median annual county unemployment. We find very little in the way of heterogeneous effects for four-year entrants (Appendix Tables C.34 and C.35), suggesting that student responses to the policy change do not appear to vary based on the intensity of exposure to the recession prior to enrollment.

Second, we test for heterogeneous effects for students who were (potentially) differentially impacted by the Great Recession while in college. If constrained students were more or less affected by the recession than unconstrained students, we would expect our estimates to vary with the severity of the recession. To measure recession severity, we focus on the economic conditions in the county in which a student's college is located (or is first observed in the credit data) and adapt the approach developed in Yagan (2019) to our setting. Specifically, we calculate the change in the average unemployment rate between 2007 and 2009 in the county in which a student first enrolled in college (or is first observed in the credit data) and then estimate models that include an interaction between this change (normalized to have a within-sample mean of zero) and all control variables including our main independent variable. As shown in Table 12, while we find some evidence of increased borrowing among constrained students in areas more affected by the Great Recession in the Texas sample, the main effect of exposure to higher loan limits is quite similar to our previous results for attainment outcomes and, for attainment and financial outcomes, the coefficient on the interaction with recession severity is small and not statistically different from zero. This suggests the Great Recession did not affect the educational investment decisions choices of constrained students or their subsequent financial outcomes.

We do find some evidence that the returns to the additional education obtained by constrained borrowers exposed to higher loan limits may have been lower for students who graduated into weaker labor markets, consistent with literature on the scarring effects of

entering the labor market during a recession (e.g., Kahn 2010; Oreopoulos, von Wachter, and Heis 2012; Borgschulte and Martorell 2018; Schwandt and von Wachter 2019; Rothstein 2021). In particular, we find more muted earnings gains from higher loan limits among constrained students in counties that were more severely affected by the Great Recession while they were in college (relative to those in less severely affected counties) (Table 12 and Appendix Table C.36). Because we do not find any strong evidence that the Great Recession altered students' experiences or choices during school, following Yagan (2019), we interpret these results as consistent with those labor markets remaining relatively weak after college, leading constrained students to experience relatively smaller returns to the additional education they obtained (owing the expansion in loan limits) than their counterparts in better labor markets.

Finally, outcomes that occur largely after school, such as employment and loan repayment, may be affected by economic conditions (e.g., Kahn, 2010)—possibly related to the Great Recession for the early cohorts. Speaking to this concern, we show that our estimates are robust to controlling for contemporaneous unemployment rates in the county of the students' entry institution (in the Texas data) or residence (in the CCP/Equifax data) in the appendix (Panel F of Appendix Table C.29, Panel E of Appendix Table C.30, and Panel A of Appendix Table C.31), suggesting that differential longer-run effects of the recession do not drive our results.

6. Conclusion

Much of the recent public discussion surrounding the rise in student borrowing has focused on the potential *negative* effects of resulting debt burdens, particularly on household balance sheets. Our study is able to provide insight not only on the effects of increased borrowing on human capital but also on borrowers' future earnings and overall financial well-being.

We find that higher student loan limits increase borrowing among constrained students, with evidence that during college, such students are able to reduce time spent on paid work as well as their reliance on credit cards. Among traditional aged students, those who borrow more due to higher loan limits also accumulate more human capital. They are, on average, more likely to (re)enroll in college, more likely to graduate from college, and have higher earnings in

subsequent years as much as 10 years after they initially began borrowing. Additional student loans also lead to, if anything, better financial positions along other key indicators over the same horizon. Increased borrowing *reduces* student loan delinquency and default, a finding that would appear to be counterintuitive without also being able to estimate effects on human capital accumulation and earnings. Our estimates rule out all but negligible effects of increased borrowing on future debt-financed purchases of homes and automobiles, as well as repayment of other types of debt.

We evaluate the welfare consequences of additional borrowing due to loan limit increases using the framework of Denning, Marx, and Turner (2020). Increased borrowing could lower public revenue through four potential channels: (1) foregone tax revenues due to a reduction in earnings while in school, (2) increased spending on public higher education as a result of additional years of schooling, (3) additional grant aid due to increased years of enrollment, and (4) the cost of additional student loan debt that is not repaid due to the increase in borrowing.[67] On the positive side, government revenue increases after students leave college, as they earn more and, as a result, pay more in federal income taxes. Furthermore, the reduced likelihood of student loan default also provides some public savings. Under the assumption that the increase in earnings we observe at ten years after entry persists, we estimate that the government will fully recoup expenditures on additional student loans within 11 years after entry.[68] We also consider the costs and benefits of loan limit expansions from the student's perspective. For a four-year entrant with a 5 percent discount rate, expected benefits exceed costs within 15 years of leaving college and over a 30-year working career, expected benefits will exceed costs for any discount rate below 85 percent.[69]

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[67] As we explain further in Appendix D, additional borrowing has two offsetting effects on defaults. First, increased borrowing results in a reduction in the probability that a borrower defaults on the student debt they would have taken on in the absence of loan limit increases, but the risk of default does not fall to zero. Thus, increased borrowing also results in a larger per-borrower cost of default. In other words, marginal defaults fall, but more student debt is subject to inframarginal defaults, which still result in a loss of public revenue.

[68] Using the welfare-analysis framework of Hendren and Sprung-Keyser (2020), under the assumption that earnings gains persist, federal undergraduate loan limit increases have an infinite marginal value of public funds.

[69] This exercise requires a number of assumptions (see Appendix E for details). Important assumptions include: (1) estimated earnings gains, measured 10 years after entry, persist throughout a borrower's working career, (2) earnings grow 5 percent per year for the first 22 years (when a traditional aged student would be

Our study documents the existence and effects of a classic market failure—binding credit constraints for human capital investment. Further, we quantify the loss to students as a result of these constraints, including foregone wages, lower educational attainment, and worse credit market outcomes. Despite concerns that students are "overborrowing," our findings are most consistent with the students in our sample *underborrowing* for college, on average. Our results also directly inform federal policymakers when considering changes to current loan limits and suggest that raising borrowing limits for dependent students would likely increase human capital accumulation and improve credit outcomes. In addition to the policy insight gained from this paper, our results offer direct evidence of the consequences of binding credit constraints for higher education in the United States. The predictions of a simple credit constraints model are borne out empirically: Increasing access to student loans increases both borrowing and human capital.

One objective of the student loan program is to enable constrained students to invest in college but concerns that these investments have not paid off was one of the main motivations for the recent decision to forgive a substantial portion of federal student loan debt in the United States. Our findings make clear that some mechanisms that could be used to reduce borrowing in order to avoid another "student debt crisis" – such as reducing loan limits – may actually serve to lower educational attainment and negatively affect later-life outcomes for many undergraduate borrowers.

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approximately 45) and remain constant for the remaining 8 years, (3) loan repayment follows the 10-year standard repayment plan, and (4) the cost of defaulted loans is equal to 17.92 percent of the outstanding balance (per https://studentaid.gov/manage-loans/default/collections). If we assume that the (insignificant) earnings gains 10 years after entry in the community college sample (0.022 log points) persists, with a 5 percent discount rate, the benefits of borrowing will exceed costs 22 years after a student leaves college and any borrower with a discount rate below 79 percent will receive a net benefit over a 30-year working career. In contrast, if community college entrants experience no earnings gains, the costs associated with the additional student loan debt will always exceed the benefits.

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Tables and Figures

Table 1: Borrowing limits by class standing and entry cohort

Academic year	Freshmen	Sophomores	Upper level
2006–07 and earlier	$2,625	$3,500	$5,500
2007–08	$3,500	$4,500	$5,500
2008–09 and later	$5,500	$6,500	$7,500

Note: Combined subsidized and unsubsidized Stafford Loan limits. Independent undergraduate students can borrow an additional $4,000 ($5,000 if upper level). Community college students are limited to sophomore status regardless of credit accumulation.

Table 2: Borrowing limits and cumulative increase in limits by entry cohort

	Loan limit	Increase in total loan limit relative to 2005 cohort by years since entry		
Entry cohort	at entry	1	2	3
2005 and earlier	$2,625	$0	$0	$0
2006	$2,625	$0	$0	$2,000
2007	$2,625	$1,000	$3,000	$5,000
2008	$3,500	$3,875	$5,875	$7,875
2009 and later	$5,500	$5,875	$7,875	$9,875

Note: 2005 cohort = 2004–05 entry cohort. The second through fourth columns show the difference in the total borrowing limit (relative to 2005 entrants) by years since college entry for a student who reenrolls and borrows the maximum available loan each year assuming students enrolled for four consecutive years.

Table 3: Baseline characteristics by cohort and constrained status

	Constrained borrowers		Unconstrained borrowers	
Entry cohort =	*2001-2005*	*2006-2008*	*2001-2005*	*2006-2008*
A. Texas Sample, four-year college entrants				
Demographics (measured at college entry)				
Gender = male	0.44	0.45	0.47	0.46
Race = White	0.52	0.43	0.33	0.35
Race = URM	0.41	0.49	0.61	0.60
Texas resident	0.95	0.95	0.96	0.96
Age	18.1	18.0	18.1	18.1
Free/reduced price lunch in HS*	0.19	0.13	0.29	0.24
High school performance*				
Math z-score	0.54	0.54	0.39	0.39
Reading z-score	0.52	0.49	0.39	0.39
Number of AP courses	3.0	3.1	2.8	2.9
Number of advanced math courses	1.0	1.2	0.9	1.0
Number of courses failed	1.1	1.1	1.9	1.6
Financial aid received in entry year (2018$)				
Federal Stafford loans	$3,576	$3,570	$2,481	$2,034
Total loans	$3,582	$3,595	$2,579	$2,186
Federal Pell Grant	$1,724	$1,761	$2,124	$2,080
TEXAS Grant	$1,943	$2,592	$1,983	$2,481
Other grants	$1,193	$1,394	$1,371	$1,671
Work study	$182	$149	$210	$185
EFC (2018$)	$10,422	$12,504	$6,525	$7,029
COA (2018$)	$12,927	$17,440	$12,241	$15,270
Number of students	22,771	20,377	24,532	10,220
B. Texas sample, community college enrants				
Demographics (measured at college entry)				
Gender = male	0.52	0.50	0.55	0.53
Race = White	0.43	0.44	0.44	0.43
Race = URM	0.54	0.53	0.53	0.54
Texas resident	0.96	0.96	0.97	0.96
Age	18.6	18.4	18.7	18.5
Free/reduced price lunch in HS*	0.26	0.18	0.28	0.23
High school performance*				
Math z-score	-0.09	-0.19	-0.12	-0.23
Reading z-score	-0.05	-0.03	-0.07	-0.09
Number of AP courses	0.7	0.7	0.5	0.6
Number of advanced math courses	0.3	0.4	0.2	0.3
Number of courses failed	4.0	3.0	3.9	3.7
Financial aid received in entry year (2018$)				
Federal Stafford loans	$3,536	$3,562	$2,126	$1,950
Total loans	$3,538	$3,572	$2,168	$1,996
Federal Pell Grant	$1,978	$1,876	$1,868	$1,524
TEXAS Grant	$528	$472	$445	$398
Other grants	$212	$285	$201	$206
Work study	$77	$61	$54	$39
EFC (2018$)	$7,223	$7,828	$5,275	$6,731
COA (2018$)	$9,344	$9,859	$7,952	$9,096
Number of students	7,389	10,329	15,138	9,987

Table 3, cont.

	Constrained borrowers		Unconstrained borrowers	
Entry cohort =	*2001-2005*	*2006-2008*	*2001-2005*	*2006-2008*
C. CCP/Equifax Sample				
Total Loans	$3,588	$4,012	$2,154	$2,220
Age	18.5	18.5	18.8	18.7
Credit outcomes before first student loan origination				
Has a credit report	0.18	0.17	0.28	0.28
Has a credit score	0.15	0.15	0.26	0.26
Number of accounts	0.23	0.22	0.44	0.42
Has a credit card	0.09	0.10	0.16	0.17
Has an auto loan	0.02	0.02	0.04	0.04
Home zipcode** characteristics:				
Mean AGI (SOI)	$70,034	$76,816	$61,960	$67,599
Fraction with AGI over $50,000 (SOI)	0.32	0.34	0.28	0.30
Median house price (Zillow)	$283,099	$340,995	$251,546	$301,235
Fraction under poverty line (Census)	0.10	0.10	0.12	0.12
Fraction with college degree (Census)	0.26	0.25	0.23	0.22
Fraction White, Non-Hispanic (Census)	0.76	0.75	0.71	0.71
Fraction Black, Non-Hispanic (Census)	0.10	0.10	0.12	0.12
Fraction Hispanic (Census)	0.09	0.10	0.12	0.12
Racial segregation (Opp. Insights)	0.17	0.17	0.17	0.17
Income segregation (Opp. Insights)	0.07	0.07	0.07	0.07
Test score percentile (Opp. Insights)	-3.41	-3.45	-3.83	-3.92
School exp. per student (Opp. Insights)	6.50	6.49	6.25	6.32
Student teacher ratio (Opp. Insights)	18.24	18.32	18.3	18.42
Number of borrowers	39,716	63,753	17,365	23,016

Note: The sample in Panels A and B includes student borrowers who first enrolled in a public higher education institution in Texas, were classified as dependent students, and borrowed at or below the federal Stafford Loan maximum for first-year students; characteristics with * are only available for students who had nonmissing high school records (see Appendix Table C.4). The sample in Panel C includes borrowers who were younger than 20, borrowed at or below the federal Stafford Loan maximum for first-year students at entry, and maintained a credit report through the 10th year after entry. Total loans in the CCP/Equifax sample are winsorized at the 99th percentile. URM = underrepresented minority (Black, Hispanic, and Native students). EFC = expected family contribution. COA = cost of attendance (tuition and fees plus living expenses). AP = advanced placement. Math and reading z-scores are standardized to have mean = 0, standard deviation = 1 across all high school test takers for the subject-academic year. Racial and income segregation are Theil indices, and the test score percentile is an income adjusted residual (see Appendix B for full definitions). **Home refers to first observed zip code, Opp. Insights data matched at county level.

Table 4: Loan limit increases and baseline student characteristics

	(1) Predicted grad rate	(2) Male	(3) White	(4) URM	(5) EFC	(6) Age
A. Texas sample, four-year entrants (N = 77,900)						
Constrained x cohort ϵ {2006,2007,2008}	0.005	0.009	0.008	-0.018	1,800	0.04
	(0.004)	(0.009)	(0.013)	(0.014)	(1169)	(0.02)
Dependent variable mean	0.602	0.455	0.415	0.518	$9,294	18.1
B. Texas sample, community college entrants (N = 42,843)						
Constrained x cohort ϵ {2006,2007,2008}	-0.007	-0.003	-0.007	0.008	-947	0.16
	(0.005)	(0.010)	(0.010)	(0.010)	(353)	(0.16)
Dependent variable mean	0.330	0.526	0.439	0.537	$6,566	18.6
	(7) Credit record	(8) Credit score	(9) Accounts on record	(10) Any credit card	(11) Any auto loan	(12) Age
C. CCP/Equifax sample (N = 143,850)						
Constrained x cohort ϵ {2006,2007,2008}	-0.002	-0.002	0.009	-0.008	0.001	0.03
	(0.005)	(0.005)	(0.012)	(0.004)	(0.002)	(0.01)
Dependent variable mean	0.204	0.181	0.280	0.116	0.027	18.5

Note: The sample in Panels A and B includes student borrowers who first enrolled in a public four-year higher education institution (A) or community college (B) in Texas, were classified as dependent students, and borrowed at or below the federal Stafford Loan maximum for first-year students. The sample in Panel C includes borrowers who were younger than 20, borrowed at or below the federal Stafford Loan maximum for first-year students at entry, and maintained a credit report through the 10th year after entry. Each cell within a panel contains estimates from separate regressions of the baseline characteristic indicated in the column heading on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts. All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, and entry school fixed effects (Panels A and B) or entry state fixed effects (Panel C). Predicted graduation rate is a linear prediction of the probability of receiving any degree within 10 years of college entry on the other baseline characteristics and school of entry fixed effects. URM = underrepresented minority. EFC = expected family contribution. CCP/Equifax outcomes all measured in the June before initial student loan origination. Robust standard errors, clustered by entry institution (Panels A and B) or entry state (Panel C), in parentheses.

Table 5: Higher loan limits increase constrained students' cumulative borrowing

Years since entry =	0	1	2	3	4	5	6
A. Texas sample, four-year college entrants (N = 77,900)							
Constrained x cohort ϵ {2006,2007,2008}	192	741	1252	1705	1801	1852	1874
	(56)	(222)	(417)	(560)	(630)	(668)	(710)
	{0.641}	{0.048}	{0.02}	{0.028}	{0.019}	{0.031}	{0.058}
Dependent variable mean	$3,086	$6,288	$10,080	$14,160	$17,560	$20,020	$22,030
B. Texas sample, community college entrants (N = 42,843)							
Constrained x cohort ϵ {2006,2007,2008}	79	248	551	804	1113	1224	1215
	(25)	(87)	(130)	(194)	(244)	(282)	(316)
	{0.831}	{0.564}	{0.249}	{0.083}	{0.004}	{0.009}	{0.013}
Dependent variable mean	$2,703	$4,272	$5,874	$7,589	$9,237	$10,570	$11,690
C. CCP/Equifax sample (N = 143,850)							
Constrained x cohort ϵ {2006,2007,2008}	312	586	1163	1646	2095	2842	3126
	(12)	(89)	(188)	(271)	(373)	(489)	(515)
	{0.379}	{0.465}	{0.253}	{0.168}	{0.120}	{0.082}	{0.072}
Dependent variable mean	$3,384	$9,897	$17,108	$23,938	$30,571	$36,077	$40,143

Note: The sample in Panels A and B includes student borrowers who first enrolled in a public four-year higher education institution (A) or community college (B) in Texas, were classified as dependent students, and borrowed at or below the federal Stafford Loan maximum for first-year students. The sample in Panel C includes borrowers who were younger than 20, borrowed at or below the federal Stafford Loan maximum for first-year students at entry, and maintained a credit report through the 10th year after entry. Each cell within a panel contains estimates from separate regressions of cumulative borrowing, measured X years after entry, where X is indicated in the column heading, on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts. All specifications also include an indicator for being constrained at entry and cohort entry year fixed effects. Specifications in Panels A and B also include entry school fixed effects, and controls for URM, age at entry, EFC at entry, gender, fall-entrant, and in-state student. Specifications in Panel C also includes state and age at entry fixed effects, quarters from entry before a credit report was created fixed effects, indicators for having a credit card, auto loan, mortgage, number of credit accounts, and credit score, measured before entry. Robust standard errors, clustered by entry institution (Panels A and B) or by entry state (Panel C), in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Table 6: The effect of loan limit increases on constrained four-year entrants' educational attainment

Years since entry =	1	2	3	4	5	6	8	10
A. Enrollment								
Constrained x cohort ϵ {2006,2007,2008}	0.044	0.046	0.048	0.012	-0.005	-0.012	-0.005	-0.006
	(0.007)	(0.007)	(0.01)	(0.010)	(0.009)	(0.007)	(0.004)	(0.005)
	{0.006}	{0.018}	{0.002}	{0.340}	{0.368}	{0.068}	{0.084}	{0.237}
Dependent variable mean	0.879	0.794	0.728	0.527	0.310	0.204	0.117	0.078
B. Cumulative credits attempted								
Constrained x cohort ϵ {2006,2007,2008}	2.9	4.4	5.5	5.8	5.7	5.6	5.4	5.2
	(0.5)	(0.8)	(1.0)	(1.1)	(1.1)	(1.2)	(1.1)	(1.1)
	{<0.001}	{0.004}	{0.001}	{<0.001}	{0.004}	{<0.001}	{0.007}	{0.010}
Dependent variable mean	51.0	71.9	90.4	101.7	107.5	111.1	115.5	118.1
C. Any degree or credential								
Constrained x cohort ϵ {2006,2007,2008}	-0.001	0.001	0.034	0.040	0.047	0.049	0.044	0.043
	(0.001)	(0.002)	(0.008)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)
	{0.496}	{0.668}	{0.033}	{0.035}	{0.012}	{0.018}	{0.011}	{0.011}
Dependent variable mean	<0.010	0.018	0.188	0.387	0.483	0.531	0.581	0.607
D. Associate degree								
Constrained x cohort ϵ {2006,2007,2008}	--	-0.001	-0.001	-0.003	-0.007	-0.006	-0.008	-0.007
	--	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
		{0.524}	{0.670}	{0.458}	{0.128}	{0.142}	{0.073}	{0.107}
Dependent variable mean		<0.010	0.016	0.025	0.034	0.041	0.055	0.066
E. Bachelor's degree								
Constrained x cohort ϵ {2006,2007,2008}	--	--	0.036	0.044	0.053	0.056	0.051	0.051
	--	--	(0.008)	(0.010)	(0.011)	(0.012)	(0.011)	(0.011)
			{0.035}	{0.013}	{0.012}	{0.008}	{<0.001}	{0.006}
Dependent variable mean			0.168	0.360	0.450	0.494	0.537	0.559
F. STEM bachleor's degree								
Constrained x cohort ϵ {2006,2007,2008}	--	--	0.006	0.014	0.014	0.014	0.015	0.014
	--	--	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
			{0.054}	{0.004}	{0.020}	{0.013}	{0.020}	{0.035}
Dependent variable mean			0.032	0.071	0.089	0.097	0.104	0.108

Notes: The sample includes student borrowers who first enrolled in a public four-year higher education institution in Texas, were classified as dependent students, and borrowed at or below the federal Stafford Loan maximum for first-year students, N = 77,900. Each cell within a panel contains estimates from separate regressions of the outcome on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts. All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for URM, age at entry, EFC at entry, gender, fall-entrant, and in-state student. Dependent variable is indicated in the subpanel heading, measured the number years after entry indicated in the column heading. STEM = science, technology, engineering, and math majors (based on CIP codes included in the National Science Foundation STEM program definition). Robust standard errors, clustered by entry institution, in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Table 7: Effects of loan limit increases on alternative sources of college financing

Years since entry =	1	2	3
A. Any credit card (N = 143,850)			
Constrained x cohort ϵ {2006,2007,2008}	-0.023	-0.007	-0.010
	(0.006)	(0.007)	(0.007)
	{0.060}	{0.596}	{0.390}
Dependent variable mean	0.519	0.587	0.619
B. Credit card balance (conditional on having 1+ cards)			
Constrained x cohort ϵ {2006,2007,2008}	-88	-85	-32
	(28)	(42)	(55)
	{0.019}	{0.037}	{0.621}
Dependent variable mean	$1,358	$1,815	$2,152
Observations	74,599	84,413	89,013
C. Any earnings X years after entry (N = 77,900)			
Constrained x cohort ϵ {2006,2007,2008}	-0.020	-0.012	0.003
	(0.006)	(0.006)	(0.006)
	{0.075}	{0.116}	{0.742}
Dependent variable mean	0.803	0.792	0.796
D. Ln(earnings) X years after entry			
Constrained x cohort ϵ {2006,2007,2008}	-0.054	-0.034	-0.046
	(0.026)	(0.025)	(0.024)
	{0.052}	{0.321}	{0.067}
Dependent variable mean	8.6	8.8	9.0
Observations	62,557	61,722	62,005

Note: Panels A and B: CCP/Equifax samples, Panels C and D: Texas four-year entrant sample. Each cell within a panel contains estimates from separate regressions of the outcome on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts. All specifications also include an indicator for being constrained at entry and cohort entry year fixed effects. Specification in Panels A and B also includes state and age at entry fixed effects, quarters from entry before a credit report was created fixed effects, indicators for having a credit card, auto loan, mortgage, number of credit accounts, and credit score, measured before entry. Specifications in Panels C and D also include entry school fixed effects, and controls for URM, age at entry, EFC at entry, gender, fall-entrant, and in-state student. Dependent variable is indicated in the subpanel heading, measured the number years after entry indicated in the column heading. Robust standard errors, clustered by entry state (Panels A and B) or entry institution (Panels C and D), in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Table 8: Effects of loan limit increases on student loan repayment

Years since entry =	4	5	6	7	8	9	10	Ever
A. Student loan delinquency in year								
Constrained x cohort ϵ {2006,2007,2008}	-0.014	-0.015	-0.008	-0.009	-0.008	0	-0.002	-0.013
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.005)
	{0.118}	{0.103}	{0.230}	{0.037}	{0.037}	{0.957}	{0.664}	{0.106}
Dependent variable mean	0.089	0.105	0.114	0.115	0.11	0.104	0.095	0.241
B. Student loan default in year								
Constrained x cohort ϵ {2006,2007,2008}	-0.011	-0.016	-0.008	-0.006	-0.009	-0.003	-0.002	-0.018
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)
	{0.085}	{0.102}	{0.389}	{0.125}	{0.019}	{0.062}	{0.346}	{0.054}
Dependent variable mean	0.062	0.076	0.087	0.091	0.091	0.086	0.078	0.194

Note: CCP/Equifax sample (N = 143,850). Each cell within a panel contains estimates from separate regressions of the outcome on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts. Specifications also include an indicator for being constrained, state and entry cohort fixed effects, age at entry fixed effects, quarters from entry before a credit report was created fixed effects, indicators for having a credit card, auto loan, mortgage, number of credit accounts, and credit score, measured before entry. Dependent variable is indicated in the subpanel heading, measured in the number of years after entry indicated in the column heading. Student loan borrowers are classified as delinquent if they have a positive past due balance for at least two consecutive quarters (180 days) and are in default if they have a positive past due balance for at least 4 consecutive quarters (360 days). Robust standard errors, clustered by entry state, in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Table 9: Effects of loan limit increases on labor market outcomes

Years since entry =	4	5	6	7	8	9	10
A. Any earnings (N = 77,900)							
Constrained x cohort ϵ {2006,2007,2008}	-0.006	-0.007	-0.003	-0.004	-0.010	-0.004	-0.001
	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
	{0.454}	{0.591}	{0.704}	{0.748}	{0.282}	{0.623}	{0.885}
Dependent variable mean	0.802	0.795	0.784	0.777	0.770	0.761	0.753
B. Ln(earnings)							
Constrained x cohort ϵ {2006,2007,2008}	-0.009	0.016	0.046	0.053	0.044	0.027	0.050
	(0.018)	(0.020)	(0.019)	(0.015)	(0.016)	(0.016)	(0.017)
	{0.608}	{0.290}	{0.011}	{0.065}	{0.099}	{0.218}	{0.018}
Dependent variable mean	9.4	9.8	10.0	10.1	10.3	10.3	10.4
Observations	62,464	61,913	61,092	60,558	59,956	59,291	58,661

Note: Texas four-year entrant sample. Each cell within a panel contains estimates from separate regressions of the outcome on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts. Specifications also include an indicator for being constrained at entry and cohort entry year fixed effects, entry school fixed effects, and controls for URM, age at entry, EFC at entry, gender, fall-entrant, and in-state student. Dependent variable is indicated in the subpanel heading, measured in the number of years after entry indicated in the column heading. Robust standard errors, clustered by entry institution, in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Table 10: The effect of loan limit increases on constrained students' other financial outcomes

Years since entry =	4	5	6	7	8	9	10
A. Delinquent (60+ days past due) on any loans							
Constrained x cohort ϵ {2006,2007,2008}	0	0.003	0.001	-0.002	-0.005	0.006	-0.003
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)
	{0.996}	{0.015}	{0.861}	{0.669}	{0.418}	{0.046}	{0.572}
Dependent variable mean	0.106	0.113	0.119	0.123	0.125	0.129	0.129
B. Has a mortgage							
Constrained x cohort ϵ {2006,2007,2008}	0.005	0.004	0.001	0.002	0.002	0.003	0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.006)
	{0.088}	{0.411}	{0.889}	{0.000}	{0.760}	{0.664}	{0.709}
Dependent variable mean	0.038	0.062	0.091	0.122	0.157	0.193	0.234
C. Has an auto loan							
Constrained x cohort ϵ {2006,2007,2008}	0.018	0.013	0.005	0.007	0.003	-0.008	-0.008
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
	{0.075}	{0.036}	{0.630}	{0.404}	{0.736}	{0.394}	{0.368}
Dependent variable mean	0.261	0.313	0.360	0.402	0.438	0.464	0.484

Note: CCP/Equifax sample, N = 143,850. Each cell within a panel contains estimates from separate regressions of the outcome on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts. Specifications also include an indicator for being constrained, state and entry cohort fixed effects, age at entry fixed effects, quarters from entry before a credit report was created fixed effects, indicators for having a credit card, auto loan, mortgage, number of credit accounts, and credit score, measured before entry. Dependent variable is indicated in the subpanel heading, measured in the number of years after entry indicated in the column heading. Robust standard errors, clustered by entry state, in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Table 11: Heterogeneity in the effect on borrowing, attainment, and financial outcomes by race/ethnicity

A. Borrowing, attainment and earnings outcomes

Dependent variable =	Cumulative loans		Attainment & earnings, 10 years after entry				
	(1) 4 years after entry	(2) 6 years after entry	(3) Years enrolled	(4) Credits attempted	(5) Any degree	(6) BA degree	(7) Ln(earnings)
Constrained x cohort in {2006,2007,2008}							
x Black (N = 16,584)	1349	1674	0.13	4.3	0.032	0.026	0.055
	(748)	(862)	(0.07)	(2.2)	(0.023)	(0.023)	(0.042)
	{0.035}	{0.093}	{0.342}	{0.307}	{0.045}	{0.054}	{0.444}
x Hispanic (N = 23,361)	1896	1616	0.14	5.3	0.029	0.045	0.018
	(390)	(505)	(0.07)	(1.6)	(0.013)	(0.015)	(0.022)
	{0.144}	{0.388}	{0.005}	{0.002}	{0.051}	{0.018}	{0.278}
x White (N = 32,343)	1759	1869	-0.005	3.4	0.043	0.050	0.081
	(997)	(1172)	(0.04)	(1.2)	(0.014)	(0.012)	(0.027)
	{0.062}	{0.148}	{0.915}	{0.023}	{0.025}	{0.038}	{0.020}
Test of equality: *p* -value	0.753	0.980	0.112	0.610	0.662	0.660	0.147

B. Borrowing and financial outcomes

Dependent variable =	Cumulative loans		Financial outcomes, 10 years after entry				
	(8) 4 years after entry	(9) 6 years after entry	(10) Ever delinquent (stud. loans)	(11) Ever default (stud. loans)	(12) Any delinquent debt	(13) Any mortgage	(14) Any auto loan
Constrained x cohort in {2006,2007,2008}							
x Black majority home zip	1834	3453	-0.075	-0.072	-0.033	0.033	0.017
(N = 10,690)	(986)	(1469)	(0.015)	(0.014)	(0.011)	(0.011)	(0.016)
	{0.041}	{0.014}	{0.004}	{0.001}	{0.042}	{0.013}	{0.311}
x Hispanic majority home zip	1402	697	0.005	0	-0.007	0.004	-0.069
(N = 8,907)	(989)	(2073)	(0.017)	(0.019)	(0.009)	(0.011)	(0.018)
	{0.195}	{0.694}	{0.681}	{0.984}	{0.647}	{0.880}	{0.034}
x White majority home zip	2083	3088	-0.007	-0.012	0	-0.001	-0.005
(N = 121,386)	(426)	(593)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)
	{0.083}	{0.055}	{0.291}	{0.056}	{0.833}	{0.893}	{0.602}
Test of equality: *p* -value	0.766	0.512	0.002	0.003	0.017	0.027	0.003

Notes: Texas four-year entrant sample, excluding students with a race/ethnicity other than Black, Hispanic, or White (Panel A) or CCP/Equifax sample, excluding students with home zip codes that do not have a Black, Hispanic, or White majority (Panel B). Each column within a panel contains results from separate regression of the outcome on an indicator for being constrained at entry interacted with an indicator for being in the 2006, 2007, or 2008 entry cohorts interacted with race/ethnicity indicators (Panel A), or indicators for home zip code having a Black, Hispanic, or White majority population (Panel B). Regressions also include standard controls fully interacted with race/ethnicity. Robust standard errors, clustered by entry institution (Texas sample) or entry state (CCP sample), in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Table 12: Heterogeneity in the effect on borrowing, attainment, and financial outcomes by Great Recession severity

A. Borrowing, attainment and earnings outcomes

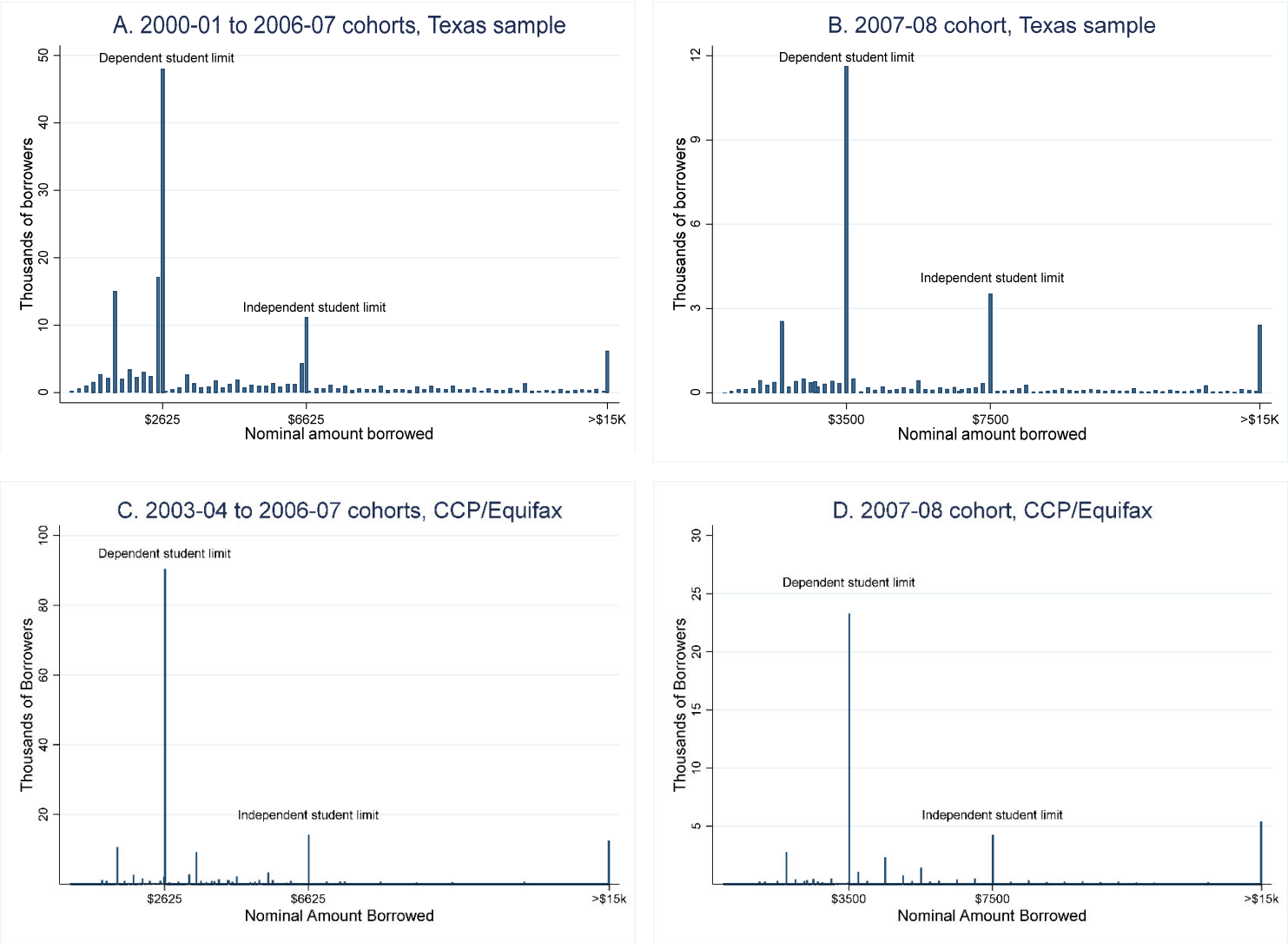
	Cumulative loans		Attainment & earnings, 10 years after entry				
Dependent variable =	(1) 4 years after entry	(2) 6 years after entry	(3) Years enrolled	(4) Credits attempted	(5) Any degree	(6) BA degree	(7) Ln(earnings)
Constrained x cohort in {2006,2007,2008}	1967	2074	0.10	5.3	0.044	0.052	0.047
	(546)	(585)	(0.03)	(1.1)	(0.011)	(0.011)	(0.017)
	{0.016}	{0.032}	{0.008}	{0.005}	{0.009}	{0.008}	{0.016}
x Δ UR 2007-2009	1278	1499	-0.01	0.1	-0.010	-0.009	-0.033
	(737)	(852)	(0.05)	(1.7)	(0.015)	(0.016)	(0.031)
	{0.265}	{0.182}	{0.732}	{0.890}	{0.167}	{0.407}	{0.073}

B. Borrowing and financial outcomes

	Cumulative loans		Financial outcomes, 10 years after entry				
Dependent variable =	(8) 4 years after entry	(9) 6 years after entry	(10) Ever delinquent (stud. loans)	(11) Ever default (stud. loans)	(12) Any delinquent debt	(13) Any mortgage	(14) Any auto loan
Constrained x cohort in {2006,2007,2008}	1982	3088	-0.013	-0.018	-0.001	0	-0.002
	(383)	(559)	(0.005)	(0.005)	(0.004)	(0.006)	(0.003)
	{0.844}	{0.601}	{0.457}	{0.817}	{0.289}	{0.499}	{0.448}
x Δ UR 2007-2009	-45	-215	-0.001	0	-0.004	-0.002	-0.009
	(206)	(332)	(0.003)	(0.003)	(0.002)	(0.003)	(0.006)
	{0.820}	{0.614}	{0.467}	{0.836}	{0.311}	{0.530}	{0.427}

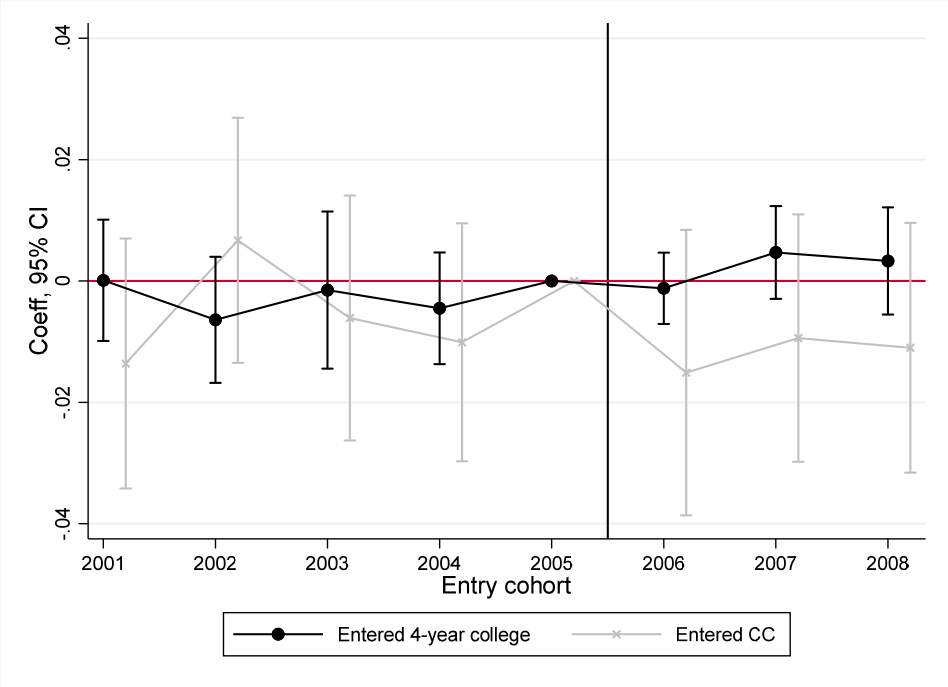
Notes: Texas four-year entrant sample (Panel A) or CCP/Equifax sample (Panel B). Dependent variable is listed in the column headings. Each panel contains results from separate regressions of the outcome on an indicator for being constrained at entry interacted with the percentage point change in the county unemployment rate where a student first enrolled in college (Panel A) or is first observed in the data (Panel B). Regressions also include standard controls and interactions between control variables and the change in the unemployment rate. The change in unemployment rate variable is normalized to have a within-sample mean of zero. Robust standard errors, clustered by entry institution (Texas sample) or entry state (CCP sample), in parentheses; *p*-values from wild cluster bootstrap-*t* in brackets.

Figure 1: The distribution of entry year borrowing



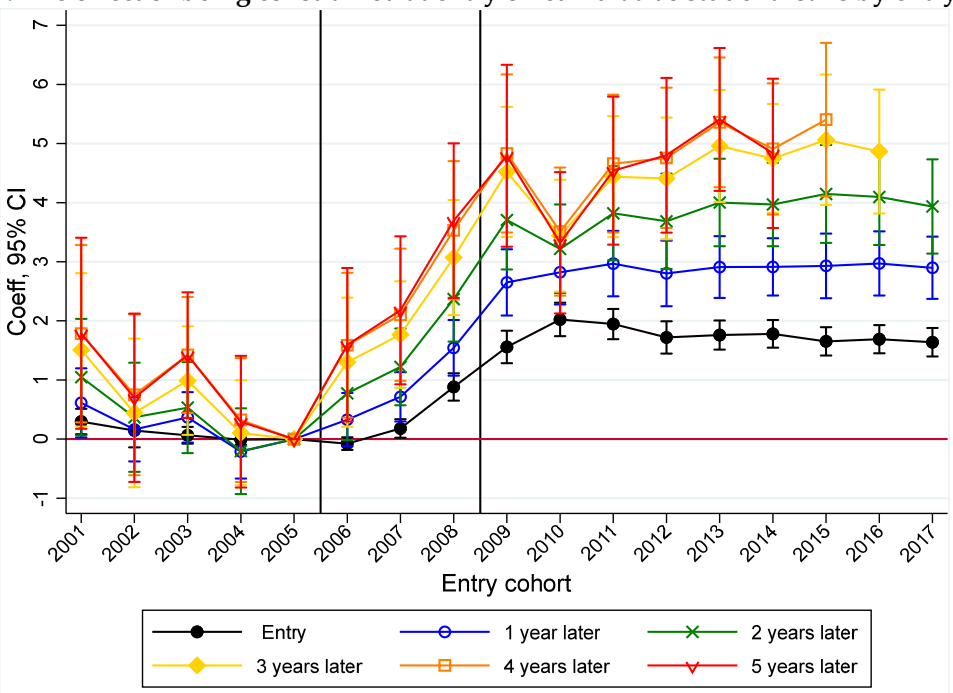
Note: The sample in Panels A and B includes student borrowers who first enrolled in a public higher education institution in Texas and were classified as dependent students. The sample in Panel C includes borrowers who were younger than 20 and maintained a credit report through the 10th year after entry.

Figure 2: The relationship between being constrained and predicted graduation rate



Note: The sample includes student borrowers who first enrolled in a public higher education institution in Texas as dependent and borrowed at or below the federal Stafford Loan maximum for first-year students. The figure displays coefficients and 95% confidence intervals from regressions of predicted graduation rate on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, and entry school fixed effects. The predicted graduation rate is a linear prediction of the probability of receiving any degree within 10 years of college entry on the characteristics displayed in Table 4 (Panels A and B) and entry institution fixed effects. Confidence intervals based on robust standard errors, clustered by entry institution.

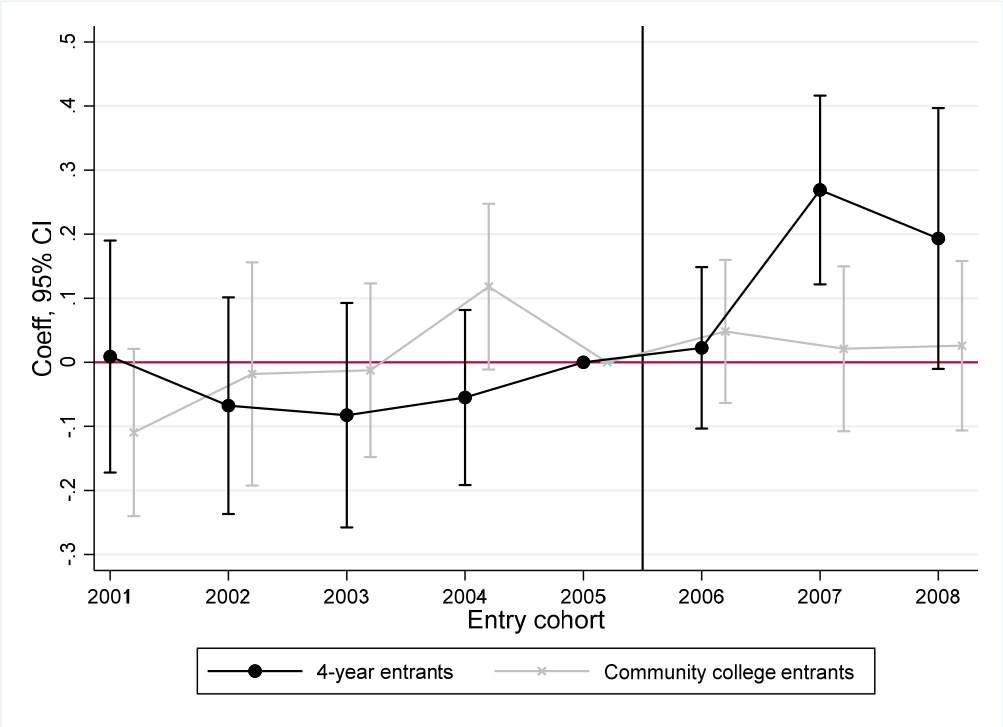
Figure 3: The effect of being constrained at entry on cumulative student loans by entry cohort



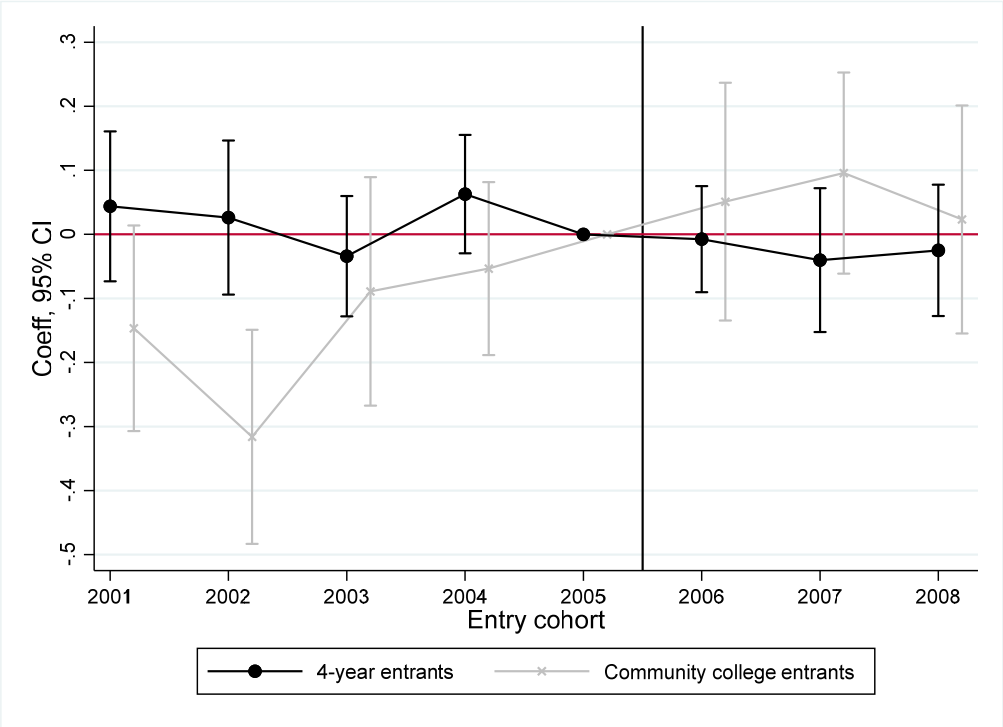
Note: The sample includes student borrowers who first enrolled in a public higher education institution in Texas as dependent students and borrowed at or below the federal Stafford Loan maximum for first-year students. The figure displays coefficients and 95% confidence intervals from regressions of cumulative borrowing on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for URM, age at entry, EFC at entry, in-state student, fall entrant, and gender. Confidence intervals based on robust standard errors, clustered by entry institution.

Figure 4: Effects of loan limit increases on cumulative years of enrollment 10 years after entry

A. Four-year college enrollment



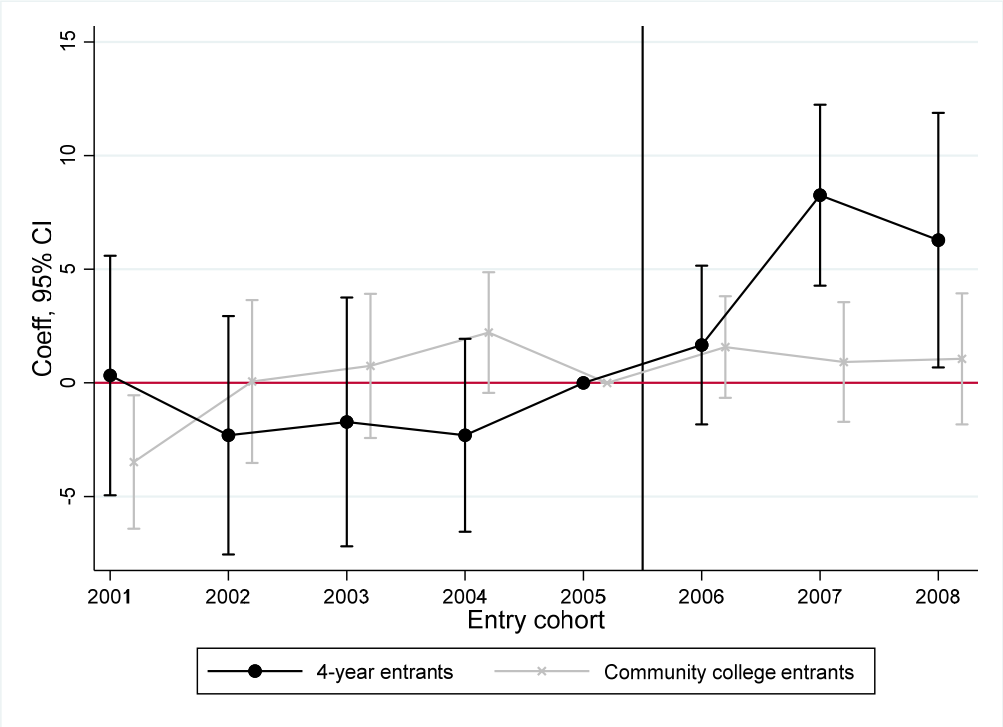
B. Community college enrollment



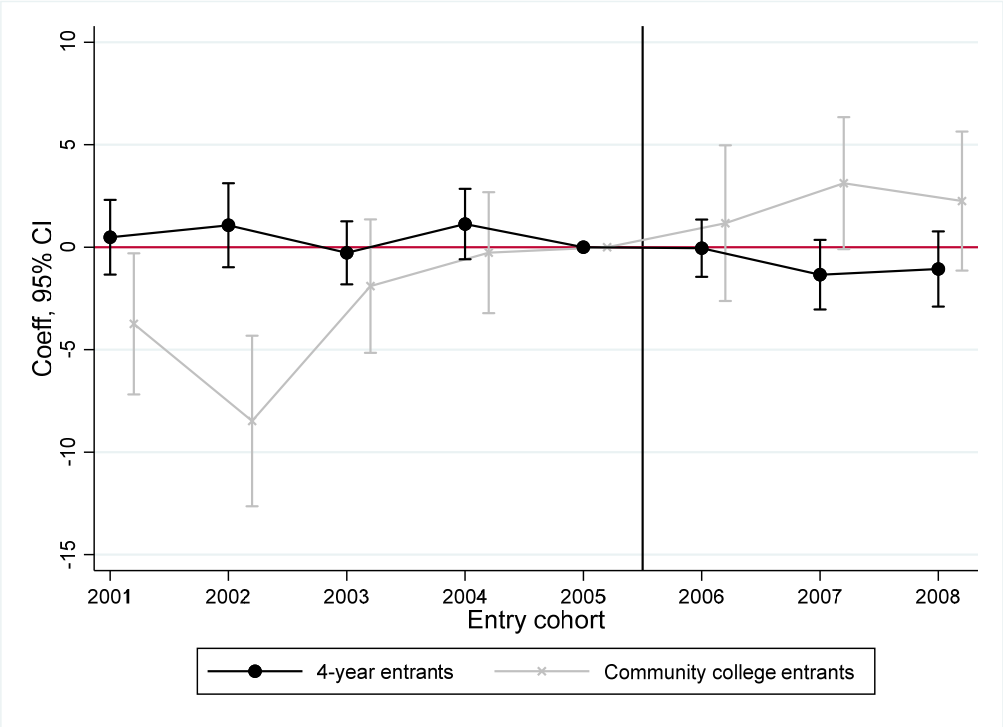
Note: The sample includes student borrowers who first enrolled in a public higher education institution in Texas as dependent students and borrowed at or below the federal Stafford Loan maximum for first-year students. The figures show coefficients and 95% confidence intervals from regressions of cumulative years of enrollment in the 10 years after college entry on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category), estimated separately for community college entrants (light gray lines and markers) and four-year public college entrants (black lines and markers). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for URM, age at entry, EFC at entry, in-state student, fall entrant, and gender. Confidence intervals based on robust standard errors, clustered by entry institution.

Figure 5: Effects of loan limit increases on cumulative credits attempted 10 years after entry

A. Four-year college credits



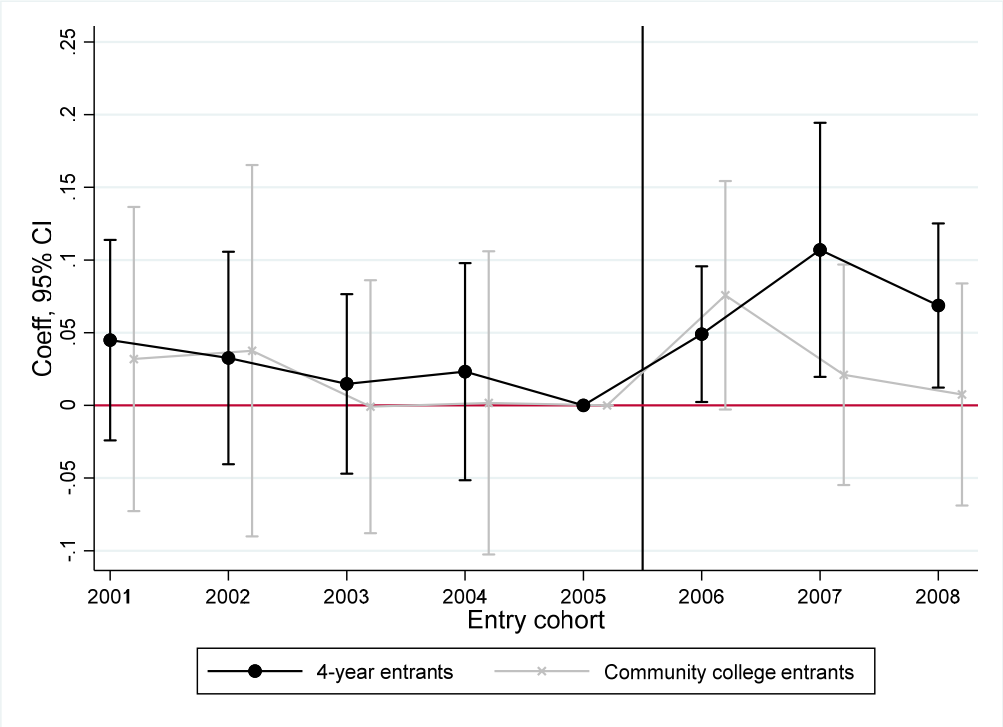
B. Community college credits



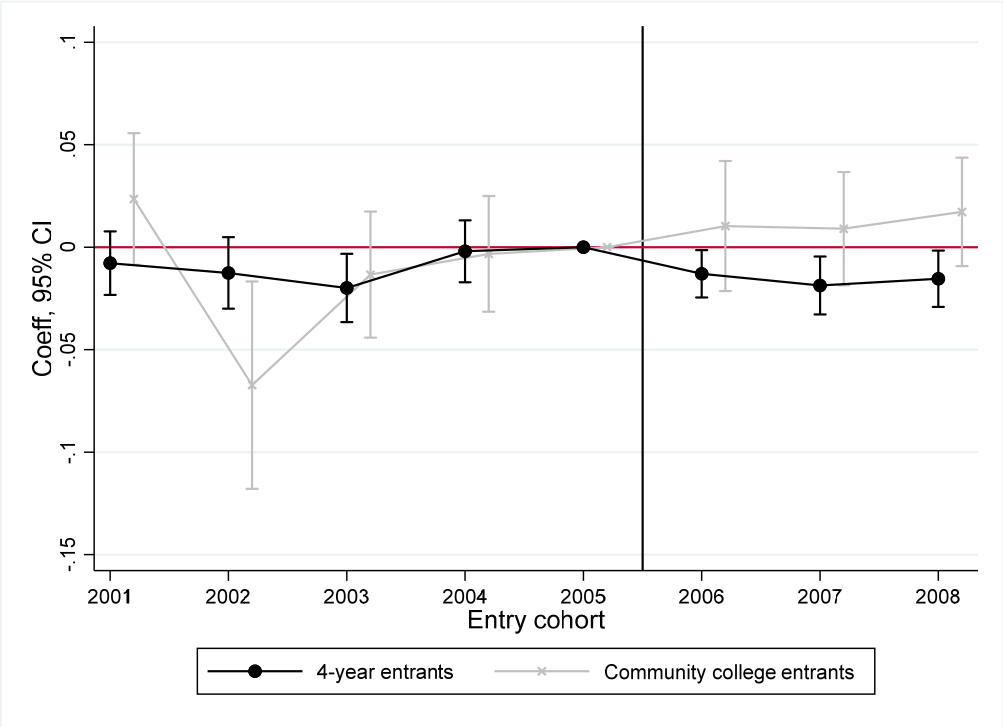
Note: The sample includes student borrowers who first enrolled in a public higher education institution in Texas as dependent students and borrowed at or below the federal Stafford Loan maximum for first-year students. The figures show coefficients and 95% confidence intervals from regressions of cumulative credits attempted in the 10 years after college entry on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category), estimated separately for community college entrants (light gray lines and markers) and four-year public college entrants (black lines and markers). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls URM, age at entry, EFC at entry, in-state student, fall entrant, and gender. Confidence intervals based on robust standard errors, clustered by entry institution.

Figure 6: Effects of loan limit increases on degree receipt 10 years after entry

A. Bachelor's degree



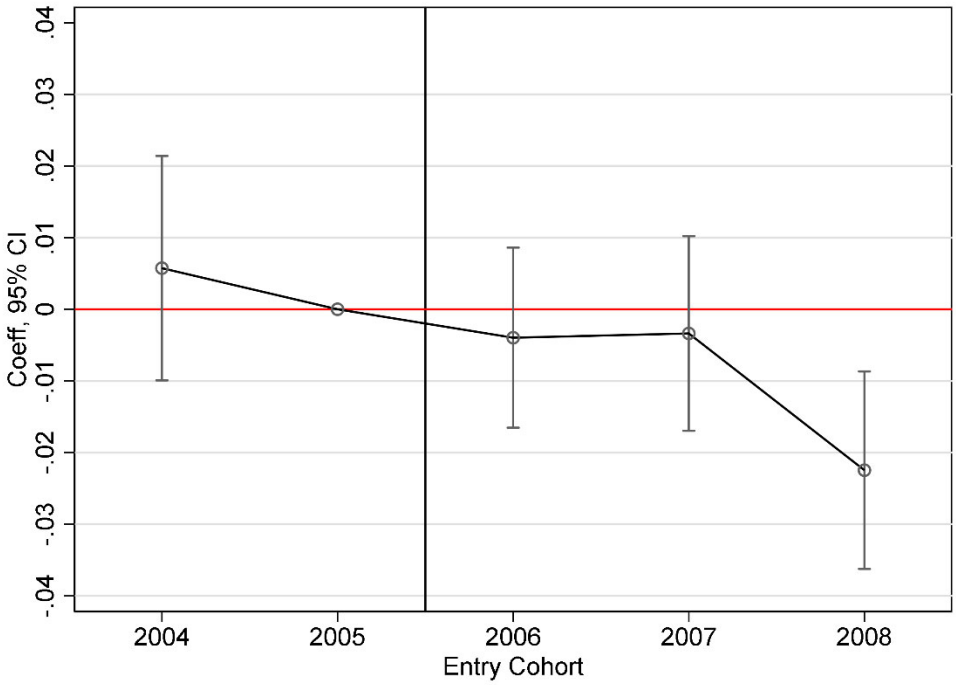
B. Associate degree



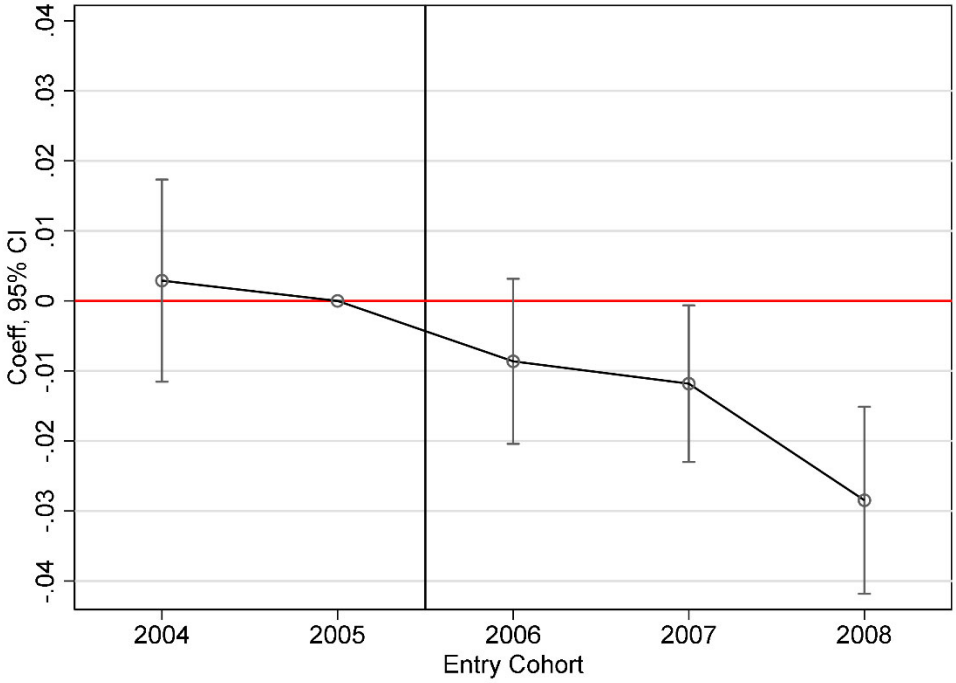
Note: The sample includes student borrowers who first enrolled in a public higher education institution in Texas as dependent students and borrowed at or below the federal Stafford Loan maximum for first-year students. The figures show coefficients and 95% confidence intervals from regressions of the probability of bachelor's degree receipt (Panel A) or associate degree receipt (Panel B) in the 10 years after college entry on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category), estimated separately for community college entrants (light gray lines and markers) and four-year public college entrants (black lines and markers). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for URM, age at entry, EFC at entry, in-state student, fall entrant, and gender. Confidence intervals based on robust standard errors, clustered by entry institution.

Figure 7: Effects of loan limit increases on student loan repayment, 10 years after entry

A. Ever delinquent

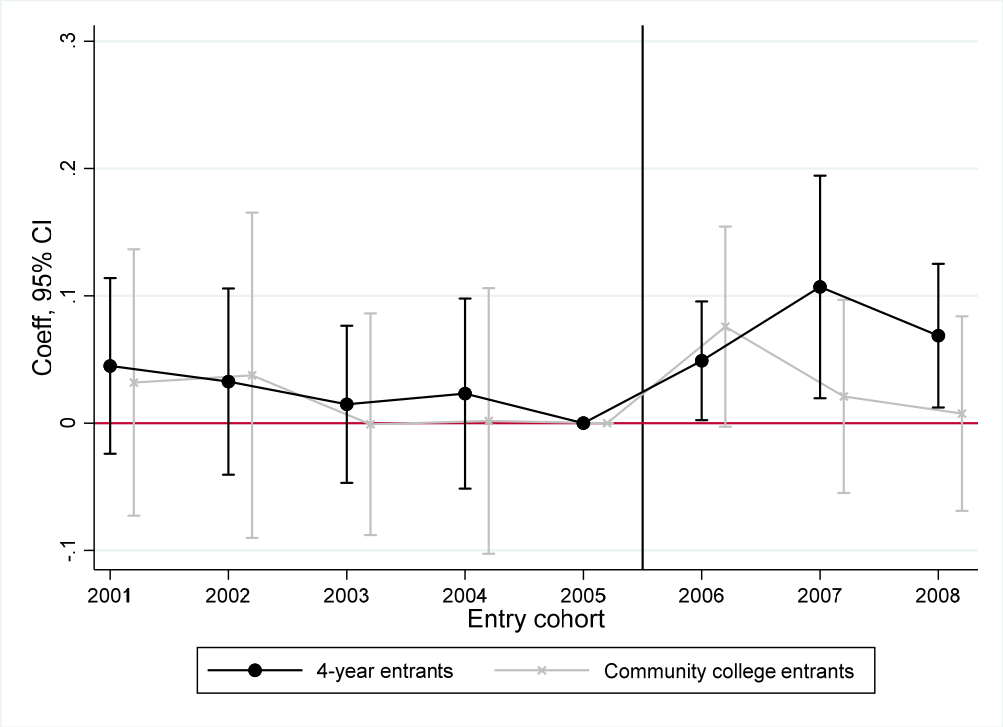


B. Ever default



Note: The sample includes borrowers who were younger than 20, borrowed at or below the federal Stafford Loan maximum for first-year students at entry, and maintained a credit report through the 10th year after entry. Dependent variable is the probability of any student loan delinquency (180+ days late) (Panel A) or any student loan default (360+ days late) (Panel B) in the fourth through tenth years after entry. The figures show coefficients and 95% confidence intervals from regressions of the indicated outcome on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, state and age at entry fixed effects, quarters from entry before a credits report was created fixed effects, indicators for having a credit card, auto loan, mortgage, number of credit accounts, and credit score, measured before entry. Confidence intervals based on robust standard errors, clustered by entry state.

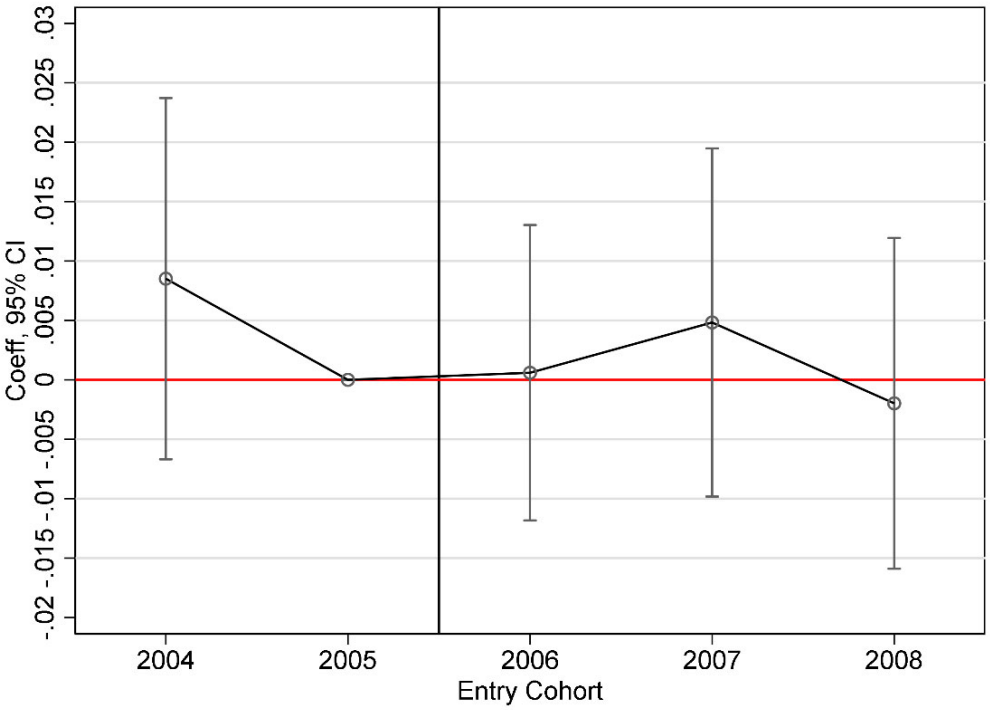
Figure 8: Effects of loan limit increases on earnings 10 years after entry



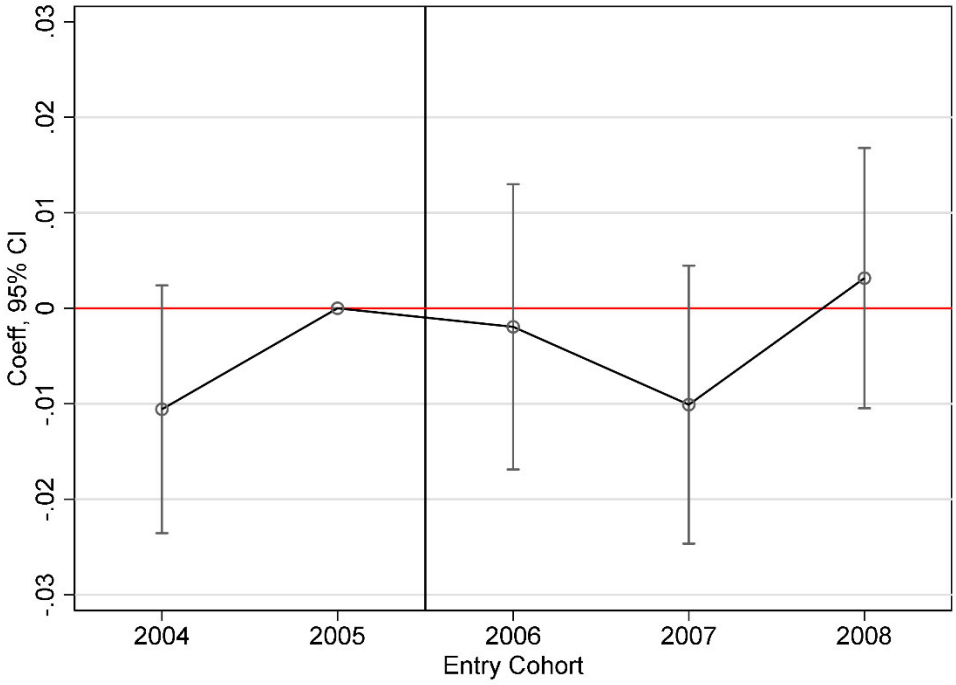
Note: The sample includes student borrowers who first enrolled in a public higher education institution in Texas as dependent students and borrowed at or below the federal Stafford Loan maximum for first-year students. The figures show coefficients and 95% confidence intervals from regressions of ln(annual earnings) in the 10th year after college entry on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category), estimated separately for community college entrants (light gray lines and markers) and four-year public college entrants (black lines and markers). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for URM, age at entry, EFC at entry, in-state student, fall entrant, and gender. Confidence intervals based on robust standard errors, clustered by entry institution.

Figure 9: Effects of loan limit increases on other financial outcomes, 10 years after entry

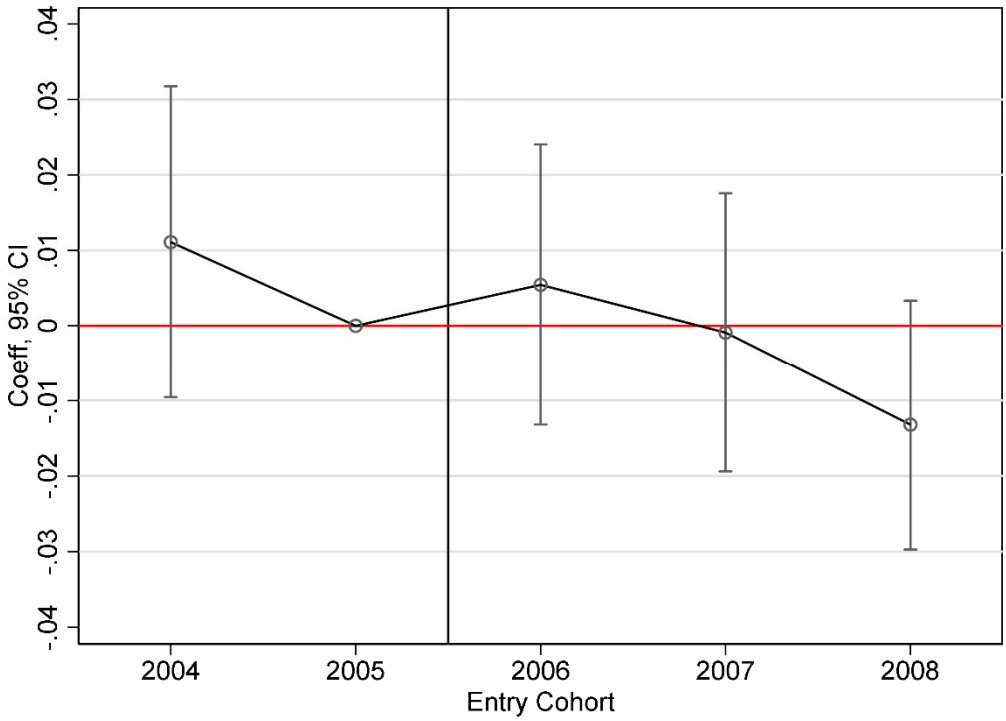
A. Delinquent on any debt



B. Has a mortgage



C. Has an auto loan



Note: The sample includes borrowers who were younger than 20, borrowed at or below the federal Stafford Loan maximum for first-year students at entry, and maintained a credit report through the 10th year after entry. The figures show coefficients and 95% confidence intervals from regressions of the indicated outcome in the tenth year after entry on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, state and age at entry fixed effects, quarters from entry before a credit report was created fixed effects, indicators for having a credit card, auto loan, mortgage, number of credit accounts, and credit score, measured before entry. Confidence intervals based on robust standard errors, clustered by entry state.