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TAKING IT TO THE LIMIT: EFFECTS OF INCREASED STUDENT LOAN AVAILABILITY ON ATTAINMENT, EARNINGS, AND FINANCIAL WELL-BEING

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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. However, 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, together with administrative schooling, earnings, and credit records, to identify the effects of increased student borrowing 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.

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1. Introduction

Concern of a student debt crisis in the United States is growing. Outstanding student loan debt now exceeds $1.5 trillion (Federal Reserve Bank of New York, 2019), and more than half of U.S. undergraduates rely on federal student loans to finance college.[2] Research suggests that attending college is generally a good investment (Card, 1999; Barrow and Malamud, 2015) and that, on average, borrowing to finance college should be more than offset by higher lifetime earnings (Avery and Turner, 2012). Nevertheless, the resulting debt may influence borrowers' post-college behavior (e.g., by affecting future earnings) or harm borrowers' post-college financial well-being (e.g., by reducing homeownership or by leading to unmanageable debt payment burdens). Despite the importance of the question, surprisingly little is known about the effects of increased borrowing—holding college prices and students' other resources constant—on human capital accumulation, earnings, and financial well-being.

Our study helps to resolve this tension between the potential costs and benefits of student borrowing by leveraging policy-driven expansions in federal student loan limits that led to staggered increases in loan availability across entry cohorts. We use this variation, together with administrative panel data on borrowers' educational attainment, earnings, and credit outcomes, to provide a comprehensive picture of the short- and longer-run effects of increased student borrowing. Among individuals whose borrowing is likely constrained by federal student loan limits, increases to loan limits boost borrowing and lead to improvements in degree completion, post-college earnings, and student loan repayment. Importantly, these effects all occur without negative effects on homeownership or repayment of other types of debt. Our findings challenge concerns that student debt harms long-run outcomes, underscoring that the correct counterfactual for evaluating these concerns asks where those with student debt would be without access to the underlying loans.

The economics literature has a long-standing interest in the importance of credit constraints for individuals' educational investment decisions. Young adults who expect a positive return would like to invest in their human capital but lack resources to do so. Because human capital

[2] Authors' calculations obtained using College Scorecard data (available at: https://collegescorecard.ed.gov/data/).

cannot be offered as collateral for a loan, the private market will underprovide credit (Friedman, 1955). Hence, classic models of human capital investment predict that increasing access to credit should also increase human capital. Evidence on the presence and magnitude of credit constraints is mixed.[3] Our paper advances this line of research by providing a test of the existence and importance of borrowing constraints for investments in higher education. The policy variation we use for identification directly affects students' liquidity without changing any other factors that might also affect higher education investment decisions (e.g., the price of college or access to grant aid), allowing us to document the comprehensive consequences of relaxing borrowing constraints—the first of this sort for a broad population.[4]

A key challenge in identifying the effect of increased student loan access in the United States is that eligibility is essentially universal.[5] 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 increases resulted in variation in the maximum amount a student can borrow by entry cohort and years since college entry. Our difference-in-differences identification strategy compares differences in the outcomes of students who entered college before and after the loan limit increases between two groups: (1) students who were likely constrained in their borrowing by statutory limits, and (2) those who were likely unconstrained by loan limits based on their first-year borrowing decisions.[6] A complementary set of analyses using a dose-response specification to look at effects of the *size* of loan limit increases on differences in outcomes between constrained and unconstrained students provides similar results. Finally, we provide

[3] See Carneiro and Heckman (2002); Cameron and Taber (2004); Stinebrickner and Stinebrickner (2008); Lochner and Monge-Naranjo (2011); Brown, Scholz, and Seshadri (2012); and Belzil, Maurel, and Sidibe (2018).
[4] Past research examines attainment effects of increased credit access in Chile (Solis, 2017; Card and Solis, 2020), South Africa (Gurgand, Loresnceau, and Melonio, 2011), and for U.S. community college students (Dunlop, 2013; Weiderspan, 2016), or examines effects on a broader set of outcomes using smaller policy discontinuities (Denning and Jones, *forthcoming*) or a more narrow population (Goodman, Isen, and Yannelis, 2018; Chu and Cuffe 2020).
[5] 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.
[6] A large number of students borrow at the statutory federal limit each year, suggesting many would borrow more if subject to higher limits, which guides classification of students as constrained or unconstrained within our analysis. Students considered constrained in our framework may be credit constrained, or they may perceive the limit as a guideline or recommendation (e.g., Marx and Turner, 2019). We refer to all such students as "constrained" for expositional convenience.

instrumental variables estimates of the effects of an additional dollar of student debt on longer-run attainment, earnings, and financial well-being.

An additional challenge to examining the full effects of student loan borrowing is access to a sufficiently long and detailed panel of borrowers, which we overcome by leveraging two large administrative data sets that together provide information on education, income, and credit outcomes. The first data set includes student-level records from the universe of public colleges and universities in Texas linked to administrative earnings records. The second is a panel of credit records drawn from the universe of U.S. consumers with credit histories.[7]

We take advantage of the large changes in loan limits, combined with these detailed data sets, to provide a comprehensive analysis of the effects of increased loan limits (holding other factors fixed) on both short- and long-run outcomes for a broad population of students. Importantly, in addition to identifying the effect on educational attainment and on in-school labor supply, we provide estimates of long-run effects on earnings and financial well-being. As a result, we can address the question of whether student debt has negative effects on post-college loan repayment, homeownership, and other sources of debt.

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 have been similar for cohorts affected and unaffected by the two loan limit increases.[8] 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 limit increases matches the increase in borrowing

[7] Where possible, we validate the results from the Texas data using the national credit record data and vice versa. This approach enables us to overcome concerns about the representativeness of the Texas sample and omitted variables excluded from credit records (e.g., income, demographics, education).

[8] In our main analyses, we classify students who entered college before fall 2005 as unaffected by the loan limit increases and those who entered from fall 2005 to fall 2007 as exposed to higher borrowing limits. In theory, students in cohorts we classify as unaffected could have experienced higher loan limits if they remained in college for five or more years. While a non-negligible share of students in the Texas data are still enrolled at this point, we find no evidence that students belonging to the 2004-05 or earlier entry cohorts took advantage of the loan limit increases by borrowing more. We expand upon this point in Section 4.

and attainment. Specifically, increases in year-to-year borrowing and reenrollment only occur for in the years in which affected cohorts that gained access to additional loans.

Focusing on dependent students, who represent the majority of undergraduate students in recent years (Fountain, 2019), we first show that loan limit increases significantly raised the amount borrowed by constrained students who first entered public institutions in Texas. In the four years following college entry, four-year entrants' cumulative borrowing rose more than $1,800, while community college entrants accumulated approximately $1,000 more in student debt.[9] Increases in federal Stafford Loans drive these effects, and we can rule out all but small changes in other types of student debt. At the same time, four-year entrants were significantly less likely to work in the two years after college entry, suggesting that this increased liquidity allowed students to reduce their in-school labor supply. Loan limit increases led to significant reductions in credit card use among constrained students early in their college careers, indicating that access to additional student loans reduced reliance on (typically higher cost) credit card debt.

The increase in loan availability also significantly raised four-year entrants' educational attainment and post-college earnings. Constrained borrowers in cohorts exposed to limit increases were significantly more likely to reenroll in college after their first year and, eight years after entry, had spent significantly more time in college and had attempted significantly more credits. Importantly, up to eight years after entry, these students were significantly more likely to have earned a bachelor's degree and had significantly higher annual earnings.[10]

The rich credit report data enable further examination of the effect of increased loan availability on borrowers' financial well-being. We find that borrowers who gained access to higher loan limits had significantly *lower* student loan default rates eight years after we first observe them borrowing—roughly four years after a four-year student graduating "on time"

[9] The effect of loan limit increases on borrowing overwhelmingly occurs in the first four years after college entry in the Texas data. In contrast, loan balances continue to increase in the Federal Reserve Bank of New York Consumer Credit Panel/Equifax sample for the duration of our panel. The latter is likely due to graduate school borrowing and loan consolidations (including any outstanding interest capitalized into the loan principal), which cannot be distinguished from undergraduate loan originations. In general, the Texas data measure federal borrowing more precisely, especially beyond the four years following college entry.

[10] Although we find some evidence of increased attainment among community college entrants, we do not find evidence that these students have higher earnings.

would complete college. Thus, despite having higher student debt, these borrowers appear to be better able to keep up with their payments, suggesting that the estimated positive effects on earnings outweighed increased debt payment burdens.

All else being equal, higher levels of student debt will increase one's debt-to-income ratio and, thus, could reduce one's ability to finance other investments, such as home and vehicle purchases. Indeed, in recent years, policymakers and the popular press have voiced concerns that rising student debt has reduced homeownership among young adults. However, despite accumulating more student debt, constrained borrowers exposed to higher loan limits have post-college mortgage and auto-loan take-up rates that are statistically indistinguishable from those of their unconstrained peers.[11] Additionally, these borrowers are no more likely to be delinquent or in collections on other types of debt. Overall, these findings suggest increased student borrowing did not have spillover effects on debt-financed asset purchases or repayment rates on other types of debt, which are key concerns in debates over the potential threats to financial stability and economic growth stemming from student loans.[12]

Our research builds on several literatures in economics. First, a small but growing body of empirical research examines the short-run effects of student loans using credible experimental or quasi-experimental variation. Findings suggest when borrowing increases because of changes in loan offers, messages, and eligibility, borrowers experience short-run attainment gains.[13] Our study looks beyond short-run educational outcomes, linking increases in short-run attainment generated by student loans to improvements in longer-term outcomes, including degree receipt, earnings, and post-college financial well-being.[14]

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[11] Two studies use credible quasi-experimental variation to examine the effect of student borrowing on homeownership (Goodman et al., 2018; Mezza et al., 2020). In both cases, the research designs entail minimal changes in human capital investment, and results, therefore, reflect whether the identifying variation affected student debt through an increase or decrease in liquidity. Specifically, Mezza et al. (2020) show that changes in tuition prices increased student debt and reduced homeownership, whereas Goodman et al. (2018) show that changes in grant aid and loan limits that occur when a student is classified as financially independent increase homeownership, with evidence effects driven by increased borrowing. In our setting, student loans are used to finance constrained students' human capital investment and have very little effect on homeownership, at least over the medium run.

[12] For example, Feiveson et al. (2018) provide an overview of student lending and possible threats to the macroeconomy and financial stability.

[13] See, for instance, Dunlop (2013); Wiederspan (2016); Solis (2017); Barr, Bird, and Castleman (2019); Denning (2019); and Marx and Turner (2019).

[14] While many papers examine one or two of these outcomes, we are unique in our ability to examine all of them.

Another branch of literature examines the effects of student borrowing on labor market and other lifecycle outcomes after college. In all of these studies, however, variation in student debt is not driven solely by changes in student loan availability but by changes in college prices, grant aid, other sources of financing for college, bankruptcy protections for student loans, or a combination of these factors.[15] In contrast, we examine the effects of loan limit-driven increases in student borrowing, holding constant these other factors. As a result, we provide the first evidence on the comprehensive short- and long-run effects of increased liquidity from student debt for a broad population.[16]

Our findings suggest that many U.S. students face important credit constraints. Among students at four-year institutions, increased access to credit while in college leads to increased college completion and higher earnings, with little if any negative effects on financial well-being later in life. These findings directly inform policy debates concerning future changes in federal loan limits. More than half of all students receiving Stafford Loans in recent years borrowed at the statutory limit, and, thus, policy changes affecting loan limits would affect millions of U.S. undergraduates.[17] Our estimates, obtained from administrative records covering more than 100 colleges and universities and from a nationally representative sample of undergraduate borrowers, are the first to characterize comprehensive effects of loan limit increases on student outcomes. Altogether, our findings suggest that an additional dollar of student loan debt improves borrowers' education, earnings, and financial well-being, thus implying increases in U.S. human capital accumulation and negligible negative spillovers to other financial markets.

[15] Increases in student loans have been linked to post-college job choice (Rothstein and Rouse, 2011), co-residence with parents (Dettling and Hsu, 2018), graduate education (Chakrabarti et al., 2020), entrepreneurship (Krishnan and Wang, 2019), homeownership (Goodman et al., 2018; Mezza et al., 2020) and family formation (Goodman et al., 2018). 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).

[16] Goodman et al. (2018) and Denning (2019) analyze the effects of a change in federal loan limits (and, potentially, Pell Grant awards) that occurs when a student becomes old enough to be considered financially independent (age 24) and, thus, focus on a relatively specialized population (i.e., traditional students that take longer than four years to complete college or nontraditional students). Further, the increased liquidity generally occurs when students are close to completing their undergraduate studies.

[17] Approximately 4.5 million students borrowed at the statutory limit in 2011–12 (Woo and Horn, 2016).

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.[18]

Undergraduate Stafford Loans, the main type of federal loan, feature standardized terms and interest rates.[19] 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 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.[20]

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. The FAFSA inputs are used to generate a student's Expected Family Contribution (EFC). In combination with the cost of college, the EFC determines the amount of subsidized loan aid students may receive, as well as eligibility for the federal Pell

[18] 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 their usage in the mid-2000s. Private loans make up the largest component of the nonfederal student loan market. The vast majority of 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 are generally determined by anticipated ability to pay.

[19] Federal student loan interest rates are set by federal law. Private student loan interest rates are generally higher. Starting in 2013, federal student loan interest rates were pegged to the rate for the 10-year Treasury note plus 2.05 percent, with a cap of 8.25 percent. From 2007 to 2012, interest rates were set by Congress and ranged from 3.4 to 6.8 percent. Before 2007, in-school interest rates were set based on the 91-day Treasury note plus 1.7 percent and interest rates in repayment equaled the 91-day Treasury note plus 2.3 percent (in both cases, capped at 8.25 percent). Federal loan interest rates have been fixed at origination starting in 2007 and were variable rate before 2007. See Appendix A and Tables A.1 and A.2 for additional details.

[20] From 2008 to 2013, 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 (Appendix Table A.3). For example, a student in the 2004 entry cohort who borrows in her first year and enters repayment one year later would pay $34 less in interest for a $1,000 subsidized loan compared with a $1,000 unsubsidized loan. In contrast, a student who entered college in 2001 would pay $82 less in interest for a $1,000 subsidized loan compared with a $1,000 unsubsidized loan.

Grant, 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 can be changed only by federal legislation and vary only with academic level and whether a student is classified as financially independent by U.S. Department of Education guidelines.[21] The difference in the amount that dependent and independent undergraduate students can borrow in a given year remained unchanged during the period we examine.[22]

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. These policy changes, 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 earlier 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).

[21] 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.
[22] 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 more than the dependent limit.

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. Students in the 2007 entry cohort who returned for a second year saw a $1,000 increase in their loan limits and by their fourth year, would have been able to borrow $5,000 more than fourth-year students who entered college in 2005. Students who entered college in 2008 saw a cumulative increase in loan limits of $7,875, and those who entered in 2009 and later years saw a $9,875 increase.[23]

Theoretically, students in the 2005 entry cohort could have been "treated" with 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.

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, because of the data agreements, we cannot link these 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.[24] 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 (U.S. Department of

[23] 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).

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

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 (see Appendix Table B.1).

In the Texas data, we observe 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).[25] Importantly, we observe whether and how much each student borrows on an annual basis and can distinguish among loans from different sources. Student-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.[26]

Our main analysis sample includes first-time college students who entered college in the 2001 through 2008 academic years.[27] We focus on dependent students. Independent students are subject to higher maxima, and, as we discuss in Section 5, few independent students appear to be constrained by loan limits. Furthermore, independent students constrained at college entry do not appear to borrow more in response to higher loan limits.

B. National credit report data

Our second data set is the Federal Reserve Bank of New York Consumer Credit Panel (CCP/Equifax).[28] 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

[25] Beginning in 2004, a subset of information is available for students enrolled in private, non-profit universities. Our main specification excludes nonprofit students, but we show that our estimates are robust to their inclusion.
[26] 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.
[27] Although the 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. Among students in the latter group, we include 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.
[28] For additional information, including sampling and methodology, see Lee and van der Klaauw (2010).

data are reported quarterly and consist of a 5 percent random sample of all U.S. consumers with credit histories.[29] 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. Appendix B.2 contains additional information on the CCP/Equifax data.

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.[30] We assume that the first year of observed borrowing is 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.[31] 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.[32] We use the panel structure of the CCP/Equifax data to assemble information on student loan and other credit outcomes up to eight years after a borrower likely entered college.[33]

[29] The sampling frame is based on Social Security numbers so that once a consumer establishes a credit history and enters the sample, the consumer remains in the sample continuously until death (even in the absence of credit activity). The sample is refreshed each quarter as new individuals establish credit records for the first time.

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

[31] Most college students who borrow began borrowing in their first year. Using the 2016 National Postsecondary Student Aid Study, we estimate that 73 percent of all dependent undergraduates who ever borrowed and graduated in 2016 borrowed in their first year of college (authors' calculations via National Center for Education Statistics PowerStats, available at https://nces.ed.gov/datalab/powerstats).

[32] Before 2004, there was incomplete coverage of student loans in credit reports.

[33] To ensure comparability over time, all of our analyses use a balanced panel of borrowers who remain in the credit report data through the eighth year after we first observe them borrowing. Because 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.

C. Identifying constrained borrowers

Our identification strategy compares changes in outcomes for those 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 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 dependent limit equaled $2,625. Panels B and D show distributions for the 2008 entry cohort in each data set, when the limit was $3,500.[34] 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 they faced higher federal loan limits (regardless of whether such behavior results from liquidity constraints or other factors).[35] 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.

For years before the loan limit increases, we consider students borrowing an amount exactly equal to the federal limit ($2,625 for dependent students) in their first year of college to be

[34] All four panels 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 be borrowing amounts exactly at the independent student limit. In the Texas data, this pattern 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 at the independent limit (see Appendix A for additional details). In the CCP/Equifax data, this group could also include students younger than age 20 who do not meet the age criteria for being classified as independent but either are married or have dependents. Irrespective of the reason, our main analysis samples exclude students who borrow more than the federal limit in their first year.

[35] 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.

constrained. 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).[36] Unconstrained students are those who borrowed an amount less than the pre-policy limit in their first year of college.

D. Summary statistics

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).[37] 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).[38]

Constrained and unconstrained students are quite similar in many respects, although we find small differences in characteristics along a few dimensions. Interestingly, 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. Constrained borrowers also have higher average EFCs, suggesting that they come from families with slightly higher incomes.[39] By definition, constrained

[36] Results are quite similar if we do not adjust the prior-year limit for inflation. In the Texas data, the loan amount reported by higher education institutions to THECB is sometimes net of origination fees (see Appendix Table A.5). To allow for this measurement error in the Texas data, students within $50 of the statutory limit are classified as constrained. The CCP/Equifax data are not subject to this issue, and, thus, only students borrowing at the limit are considered constrained in this sample.

[37] Federal law prohibits lenders from discriminating against applications on the basis of race, ethnicity, sex, marital status, national origin, religion, or receipt of public assistance, so the CCP/Equifax data contain no demographic characteristics except age.

[38] Appendix Table B.2 replicates Panel C for borrowers in Texas using the CCP/Equifax data. Ages and amounts borrowed are quite similar. Texas borrowers are slightly more likely to have a credit report before origination of their first loan but otherwise have comparable credit market outcomes to borrowers nationwide.

[39] The relatively small differences in EFCs between constrained and unconstrained students could be due to differences in grant aid or differences in the cost of attendance, as unconstrained students receive slightly higher amounts of grants and face slightly lower costs of attendance than their constrained counterparts. However, we find no evidence of differential changes in Pell Grant aid for constrained versus unconstrained in treated versus untreated cohorts. Furthermore, the models we estimate using the Texas data include school fixed effects and, thus, make comparisons between students that faced similar costs. Nonetheless, point estimates are similar when school effects are excluded.

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, a comparison of average total loans and average federal Stafford Loans shows that the vast majority of student debt is issued through the Stafford Loan Program.[40] In the CCP/Equifax sample, constrained borrowers are less likely to have a credit report or other debt before their first student loan origination. Entry-year borrowing is similar for students in the Texas and CCP/Equifax samples.[41]

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 (Table 1). 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 borrowing. Students who entered after 2005 experienced substantial increases in cumulative loan limits (Table 2). We use this variation in a difference-in-differences framework.[42] 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

[40] We restrict our sample to students who borrowed at or below the first-year limit in their year of entry. Thus, the very low use of non-Stafford student loans is partially mechanical because most students exhaust their federal Stafford Loans before taking up other student loans. Appendix Table C.1 shows characteristics of non-borrowers and students who borrowed above first-year Stafford Loan limits in their year of entry. Appendix Table C.2 shows the breakdown of first-year borrowing by loan type for those who are constrained, those who are unconstrained, and those who borrow more than the federal limit.

[41] The fraction of the Texas sample classified as constrained is relatively constant before 2007 and increases in both 2007 and 2008. In the CCP/Equifax sample, the fraction of borrowers classified as constrained is constant over all entry cohorts.

[42] 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.

borrowing for any cohorts that entered from 2001 to 2005 in the Texas sample, and, thus, we consider 2005 entrants to be the final "control group" cohort.[43]

The second increase in loan limits coincided with the Great Recession.[44] 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).[45]

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*.[46] This limitation inherently excludes students who were sufficiently credit constrained that they did not enter college at all. 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).[47] Additionally, students unconstrained at college entry may become constrained over time if their income or expenses change. As a result, our results likely do not capture the full effect of access to additional loans.

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

[43] In the CCP/Equifax sample, we find evidence that 2005 entrants borrow significantly *less* than 2004 entrants in their fifth year, the opposite of what would be predicted if constrained students in the 2005 cohort were affected by the loan limit increase.

[44] 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.

[45] The Great Recession led to an approximately 3 percentage point increase in college enrollment (Barr and Turner, 2013). Appendix D contains estimates from alternative specifications that include all post-Great Recession entry cohorts or include all but the cohorts directly affected by the recession (2009, 2010, and 2011).

[46] Marx and Turner (2019) can rule out all but very small effects of student loan offers on enrollment in an anonymous U.S. community college, but Solis (2017) shows large increases in enrollment when students gain access to loan aid in Chile.

[47] 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). We discuss these results in Section 5.

necessarily 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).[48] 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.

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 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 increases in loan limits and increases in borrowing by constrained students. To motivate our focus on students who entered college before the Great Recession and to provide as complete a picture as possible, we include students who entered after 2008 when we estimate event-study models.

Our second specification, shown below, pools the three treated and all untreated cohorts.[49]

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

We present cluster-robust standard errors (clustered by entry institution for the Texas sample and state of entry for the CCP/Equifax sample) as well as *p*-values from the wild cluster bootstrap, where clusters are defined based on entry cohort by constrained status.[50]

[48] Models estimated using the CCP/Equifax data do not include school fixed effects as identifying what school a student was enrolled in when she borrowed is not possible in these data. When estimating models using the Texas data, baseline characteristics include race (white versus underrepresented minority versus other), age at entry, EFC at entry, and gender. Baseline characteristics in 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; indicators for having a credit card, auto loan, or mortgage before entry; number of credit accounts (secured or unsecured) before initial borrowing; and credit score before initial borrowing.

[49] For most outcomes, we cannot reject the hypothesis that the estimated (null) effects of being constrained in the pre-period are equal. This approach can be thought of as estimating the treatment of being constrained in one of three treated cohorts. Estimated effects on loans (and other outcomes) will represent a weighted average of the effects for each treated cohort. We obtain similar estimates from a less parsimonious specification that pools the untreated cohorts but allows loan limit expansions to have different effects for the three treated cohorts. These results are available in Appendix E.

[50] 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,

Next, we present results from a dose-response specification that takes advantage of the differently sized limit increases faced by different cohorts:

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

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 $AggLimExp_c$.[51] 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.

Finally, we present estimates from instrumental variables (IV) models that relate student borrowing to later outcomes. The endogenous variable is the cumulative amount borrowed, D_{isc}, and the interaction between entry cohort and being constrained for the affected cohorts (i.e., $\mathbf{1}[Cohort \in \{2006,2007,2008\}] \times Cons_i$) serves as the excluded instrument in a first-stage equation that takes the form of equation (2). The second-stage equation is

$$Y_{isc} = \beta_1 Cons_i + \lambda \widehat{D}_{isc} + \mathbf{X_i}\boldsymbol{\beta_x} + \delta_c + \delta_s + \epsilon_{isc} \quad (4)$$

Under the assumption that loan limit increases only affect constrained students' outcomes vis-à-vis effects on the amount borrowed, estimates of λ will represent the causal effect of student loan debt on student outcomes. Estimates from equation (4) also allow us to compare our results with existing estimates of the effect of student loans and other sources of financial aid on educational attainment, labor market, and financial outcomes.

A. 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.

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 procedure (Cameron, Gelbach, and Miller, 2008).

[51] Estimates that look at effects of limit increases over a student's first five years produce similar results.

To address this concern, we show that observable characteristics of constrained students relative to unconstrained students do not change differentially after the expansion of loan limits in the Texas and CCP/Equifax samples. First, to avoid concerns related to multiple hypothesis testing, we generate a linear prediction of the probability of graduation within eight 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.[52] We find no evidence of differential changes in characteristics for the constrained students relative to the unconstrained students.[53]

Table 4 presents the estimated effects for individual baseline characteristics in the Texas sample and the predicted probability of graduation, as well as credit outcomes in the CCP sample, all estimated using equation (2).[54] 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.[55] For four-year college entrants, we find no significant differences in baseline characteristics, including race, gender, age, EFC, or the predicted graduation rate. For community college students, we find a small, marginally significant decrease in the predicted graduation rate and significantly lower EFCs for constrained students in treated cohorts. This finding suggests that results pertaining to community college students may represent an underestimate of the effect of limit increases (if constrained students were more likely to be negatively selected after the policy change) and that estimates for this population should be viewed with caution.[56] In the CCP/Equifax sample, we

[52] We exclude the vector of baseline characteristics ($\mathbf{X_i}$) from these models; estimates are robust to their inclusion.
[53] Appendix Figure C.1 displays similar figures for individual baseline characteristics.
[54] The credit report outcomes in the CCP are measured in June before a student's likely college entry (e.g., before the first student loan origination), whereas the Texas sample outcomes are measured at entry. We measure credit report outcomes before student loan origination because those characteristics could be endogenous to treatment if they are measured contemporaneously (e.g., originating a larger student loan could affect the students' credit score or their decisions about other types of borrowing).
[55] Appendix Table C.3 contains pooled estimates for the Texas sample.
[56] We document changes in other financial aid in Appendix Tables C.3 (pooled sample) and C.4 (separately for four-year and community college entrants). Focusing on financial aid in a student's first year, we find no changes 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

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, find evidence of statistically significant, albeit small, differences in age and the probability of having a credit card at entry. We control for these baseline characteristics in our regressions to account for any differences.

We also assess the assumption of parallel trends for constrained versus unconstrained students by presenting event studies of the outcomes (Figures 4 through 10). Four-year entrants (in black) and community college students (in grey) are shown separately for the Texas sample. We find that outcomes for constrained and unconstrained students trend similarly in entry cohorts assumed to be unaffected by the loan limit expansions, whereas outcomes diverge for cohorts exposed to the limit increases.

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 six years after college entry in the Texas sample. Borrowing among constrained students in 2005 and earlier entry cohorts is, if anything, trending downward relative to unconstrained students.[57] Borrowing among constrained students in cohorts that entered after 2005 who had access to increased loan limits significantly increased, and increases are larger for cohorts exposed to larger limit increases.[58] Importantly, we see no effects on cumulative borrowing for

suggest that for this population, loans increased while other sources of financial aid did not. In 2004, Texas gave institutions more latitude to set tuition. Andrews and Stange (2019) show this increased tuition and need-based financial aid for programs with higher predicted earnings. 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.

[57] This downward trend is likely mechanical. We adjust amounts borrowed for inflation, so constrained students' borrowing is declining in real dollars because the maximum is fixed over these years. If unconstrained students borrow the same amount in real dollars, we should find a downward trend in constrained students' inflation-adjusted borrowing relative to that of unconstrained students.

[58] Appendix Figure C.2 displays similar estimates for independent students, a group for whom loan limit increases did not appear to have any significant effect on cumulative borrowing. Appendix Figure C.3 shows separate estimates for four-year and community college entrants in the dependent student sample.

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.4).

We next turn to our main specification—equation (2), estimated on the cohorts that entered before the start of the Great Recession. Table 5 presents the effects of loan limit increases on constrained students' cumulative borrowing at entry and 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). Similar to the patterns shown in Figure 3, increased borrowing largely occurs in the first four years after entry.[59] Impacts on constrained community college student entrants follow a similar pattern (Panel B), with smaller increases in borrowing, amounting to around $1,000 in additional debt. Effects on cumulative borrowing in the CCP/Equifax sample are similar in magnitude to the Texas sample (Panel C), with impacts approaching $1,600, although loan balances continue to grow in the fifth and sixth years after college entry.[60] The increase in borrowing potentially 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.[61]

[59] It may be surprising that we observe continued increases in cumulative borrowing four years after entry (i.e., in a student's fifth year) for the 2006 through 2008 entrants in the CCP/Equifax sample but see no differences in cumulative borrowing between constrained and unconstrained students in the 2005 entry cohort (who would have been exposed to a $2,000 increase in borrowing limits in their fifth year). However, these two results are consistent with access to additional liquidity in earlier years increasing persistence into later years, which we show to be the case for constrained students exposed to loan limit increases.

[60] These differences potentially reflect three factors: (1) borrowing for graduate education (which is observed only for graduate school enrollment within public Texas institutions in the Texas sample), (2) debt consolidation loans in which interest is capitalized and a new loan is originated to replace several previous loans, and (3) 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 sample 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 between the samples.

[61] Appendix Table C.5 shows estimated effects of loan limit increases on the probability of any student loan take-up by years since entry. 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 Texas community college entrants. Appendix Table C.6 shows that increases in cumulative total loans for Texas borrowers are due to increases in federal Stafford Loans. We find no statistically significant changes in state loans (Appendix Table C.7). These results highlight that variation in borrowing is driven by changes in federal policy rather than by (potentially endogenous) changes in the availability of other types of loan aid.

Our results for student borrowing 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. Having established that increasing borrowing limits leads to increases in borrowing, we next investigate effects on constrained students' human capital accumulation.

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 break out effects by type of college at entry and separately examine effects on enrollment in four-year institutions (Panel A) versus community colleges (Panel B). Eight 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 do 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 reduction in community college attendance. The significant pre-trends in community college entrants' enrollment suggest that we should view estimates for this population with caution.

Effects on cumulative credits attempted follow similar patterns (Figure 5), with constrained four-year entrants in cohorts affected by loan limit increases attempting 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.

Finally, Figure 6 shows the effects of loan limit increases on constrained students' degree receipt within eight years of 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). For community college entrants, estimates are relatively noisy, and we find no evidence of significant increases in degree receipt.[62]

[62] We find no evidence of loan limit increases resulting in significant effects on certificate receipt for either four-year or community college entrants (Appendix Figure C.5).

The effects on borrowing and educational attainment provide evidence of binding constraints for human capital investment for four-year college entrants. 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). Higher loan limits did not increase the likelihood that community college entrants "upgrade" 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. Given evidence of significant pre-trends in this group's attainment outcomes, we focus on estimated effects for four-year entrants for the remainder of the paper. Appendix C contains estimates for the community college sample.

Table 6 shows the effect of increased loan limits on constrained four-year entrants' educational attainment, estimated using equation (2). Constrained students are significantly more likely to (re)enroll in college every year, up to three years after entry, with effects that range from a 4 to 5 percentage point increase. As a result, these students spend an additional 0.15 year enrolled in college (Panel B) and attempt 5.5 additional credits (Panel C). The timing of enrollment effects aligns with the timing of loan limit increases (Appendix Figures C.6 and C.7). Specifically, constrained students in the 2007 and 2008 cohorts—who saw their loan limits increase within one year of enrollment—are significantly more likely to reenroll one and two years after entry (Panels A and B of Appendix Figures C.6 and C.7). In contrast, 2006 entrants—who did not experience access to higher loan limits until three years after entry—have 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.6 and C.7). 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.

Higher federal loan limits also increased students' probability of earning a degree within six years of entry by around 5 percentage points. These gains are driven by an approximately 6 percentage point increase in the probability of bachelor's degree receipt combined with small, negative effects on associate degree receipt. The effects on cumulative years of enrollment,

cumulative credits attempted, and degrees are all statistically significant and persist until the end of our panel when only 11 percent of students are still enrolled. This finding suggests that loan limit increases led to a lasting increase in graduation rates rather than simply a retiming of degree receipt.[63]

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 and credit card usage. As shown in Table 7, when constrained students gain access to higher federal loan limits, they are also significantly less likely to have a credit card in the year following entry.[64] Credit card holders also carry significantly lower balances over the same period. Specifically, credit card use falls 2 percentage points (4 percent) in the year after entry, while cardholders' monthly balances fall $82 (5 percent).[65]

Constrained four-year entrants are significantly less likely to receive any earnings in each of the first two years after initial enrollment (Panel B of Table 7), suggesting that additional student loans may allow students to spend less time working while in college. Among employed students, earnings are 3 to 5 percent lower, although these effects are statistically significant only one and three years after entry.[66] Taken together, the reduction in credit card

[63] Estimates for community college entrants suggest this group also experienced increases in educational attainment, although evidence of pre-trends in Figures 4 through 6 lead us to view these estimates with caution. Appendix Table C.8 shows cumulative increases of approximately 0.2 years of enrollment and 5.5 credits attempted eight years after entry. Loan limit increases do not affect constrained community college entrants' likelihood of earning a bachelor's degree but do have positive, marginally significant effects on associate degree receipt. Appendix Tables C.9 and C.10 presents estimated effects on additional measures of attainment, including enrollment by type of institution and cumulative credits earned by type of institution.

[64]Appendix Figure C.9 shows that the effects on credit card use one year after entry are driven by the two cohorts exposed to loan limit increases at this point. Note that 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 are net of this effect, as we control for baseline credit card holding. It also worth noting that the Credit Card Accountability Responsibility and Disclosure Act of 2009 (which became effective in 2010) restricted credit card availability for those under age 21. This restriction could have affected credit card availability for 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.

[65] 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).

[66] Estimates for community college entrants suggest that these students experience a statistically significant 6 percent decrease in earnings three years after entry and statistically significant increases in the probability of having nonzero earnings two and three years after entry.

utilization and earnings from employment indicates that greater student loan availability reduces constrained students reliance on other sources of liquidity during school.

C. Effects of loan limit increases on constrained students' earnings, employment, and credit outcomes

Given that loan limit increases result in constrained students having both more debt and more human capital, we next consider impacts on credit and labor market outcomes using the CCP/Equifax data and Texas data, respectively. We focus on these outcomes beginning four years after entry—a point when many students have left college.

We first consider how additional borrowing affects student loan repayment. While the increases in federal loan limits resulted in higher average loan balances—which could lead to increased difficulty in making minimum payments—constrained students also acquired more human capital. As shown in Figure 7, 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 eight years after entry (Panel A). Panel A of Table 8 shows that the reductions in delinquency rates are similar in each of these years, 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 eight years later.

We define student loan default as at least 360 days of missed payments.[67] Constrained students facing higher limits are significantly less likely to default on their loans at any point over the four through eight years after college entry (Panel B of Figure 7). Year-by-year effects range from a 1.2 percentage point (18 percent) reduction in default rates four years after entry to a 0.9 percentage point (10 percent) reduction eight years after entry. Taken together, the significant reductions in delinquency and defaults suggest that even with higher student debt, the increase in human capital experienced by constrained students who were able to obtain access higher loan limits outweighed any increases in student loan payment obligations.[68]

[67] Servicers are not required to report a federal loan as in payment delinquency until at least 90 days (one quarter) of payments are missed, and loans are considered in default once 270 days of payments are missed. However, servicers are not required to (and generally do not) report default until 360 days of missed payments. See https://studentaid.ed.gov/sa/repay-loans/default and https://www.gao.gov/assets/670/665709.pdf.

[68] Barr, Bird, and Castleman (2019) also find a negative relationship between student debt and student loan default 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. However, the authors examine effects on

Higher loan limits do not appear to affect labor force participation in later years, when most students would have left college (Panel C of Table 8 and Figure 8).[69] However, they enable constrained students to earn significantly more six through eight years after entry, with estimates implying an increase of 4 to 6 percent (Panel B of Table 8 and Figure 8).[70] This result is again consistent with binding credit constraints preventing students from making beneficial investments in human capital when loan limits are lower.[71]

We next examine effects of loan limit increases on other types of debt, which can address the question of whether borrowers might substitute on-time student loan payments at the expense of other debt payments. The potential spillovers from student loan debt to other credit products is also a key concern with respect to financial stability. 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 9 and Figure 9).[72] Thus, the larger student loan balances induced by the limit increases do not appear to have had negative spillovers to payment behavior on other types of loans.

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student loan payment difficulties over the short run, while we show that the relationship between student debt and repayment persists over a longer period.

[69] 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, which suggests that these concerns are unlikely to affect our results.

[70] Estimated effects on (unconditional) earnings levels show similar patterns (Appendix Table C.11). We find no significant effects on community college entrants' earnings or employment in later years (Appendix Table C.12).

[71] Consistent with our result that higher loan limits have positive effects on constrained students' earnings in later years, we also find evidence that students who were able to borrow more were also able to live in more affluent neighborhoods after college. We observe address information (Zip code, state, county, census tract, census block) from CCP/Equifax, which we use to merge time-varying neighborhood characteristics from other sources (e.g., Zip code house prices from Zillow and Zip code incomes from Internal Revenue Service Statistics of Income records) to examine as outcomes. Specifically, Appendix Table C.13 shows that constrained borrowers who gain access to higher loan limits live in neighborhoods with significantly higher average wage income and average adjusted gross income, as well as significantly higher housing values.

[72] According to the CCP/Equifax data documentation, before 2011, the "delinquency on any debt" measure could reflect student loan delinquencies as well, but upon inspection of the data through 2011, individuals delinquent on student loans were equally, if not more, likely to have a value of zero for this measure, suggesting student loans are not included in the measure throughout our sample period.

All else equal, higher levels of student debt could reduce borrowers' ability to finance other investments, such as home and vehicle purchases.[73] Thus, we next examine the effect of loan limit increases on other aspects of constrained borrowers' balance sheets. We examine effects on mortgage debt (Panel B of Table 9 and Panel B of Figure 9) and auto loan debt (Panel C of Table 9 and Panel C of 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.[74] Higher limits generate small increases in the probability of having an auto loan in the fourth and fifth year after entry.[75] Altogether, we find no evidence that additional borrowing adversely affects constrained students' financial well-being after college.[76]

C. Dose-response estimates of size-of-limit increases

Table 10 presents estimates from the dose-response specification (equation (3)). The coefficient of interest is the interaction of an indicator for constrained status interacted with the cumulative loan limit increase.[77] 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, relative to the 2005 cohort. For brevity, we present

[73] 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 (Panel A of Appendix Table C.14). On the other hand, improved student loan repayment, 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 more plentiful among higher earners, unless higher debt payment burdens reduce savings rates.

[74] Panels B and C of Appendix Table C.14 show 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.

[75] We find no evidence that higher loan limits affected auto loan amounts (Panel D of Appendix Table C.14).

[76] Although credit score algorithms are proprietary, many of our outcomes are typical inputs into credit score calculations, and our results thus far imply a likely improvement in credit scores. Indeed, we find statistically significant positive effects of loan limit increase on Equifax Risk Scores (a type of credit score) and negative effects on the probability of having a score in the bottom quintile (Panels E and F of Appendix Table C.14). For context, the magnitude of this effect is around a quarter of the estimated effect of having a bankruptcy flag removed from one's credit report (Dobbie et al., *forthcoming*). However, these results may be driven by the fact that 2004 constrained entrants have significantly lower scores relative to unconstrained students when they are early in their college careers, and, thus, we view these results with caution.

[77] The benefit (and drawback) of this specification is that it imposes more structure on the relationship between loan limits and the outcome, allowing the size of the effect to vary with the limit increase, which is particularly useful for the CCP/Equifax data where there are relatively few pre-policy years.

estimates pertaining to four-year entrants in the Texas data; Appendix E contains additional results, including estimates for community college entrants.

Panel A estimates suggest that for every $1,000 increase in statutory loan limits over a student's first four years, constrained four-year entrants in the Texas sample increase borrowing by $339 and constrained students in the CCP/Equifax sample increase borrowing by $461. Effects on cumulative borrowing six years after entry are of a similar magnitude for Texas four-year entrants ($342) but grow to $624 per $1,000 limit increase for the CCP/Equifax sample.

Panel B shows that estimated effects on attainment and earnings from this specification yield similar conclusions to our earlier findings, with higher loan limits resulting in significant increases in years of enrollment, credits attempted, degree receipt, and earnings. Estimates for the CCP data (Panel C) also yield similar conclusions to results presented thus far, with higher loan limits leading to a significant reduction in student loan delinquency and defaults and no evidence of effects on repayment of other debt, having a mortgage, or having an auto loan.

D. Instrumental variables estimates of the effect of student debt

Finally, we present estimates from IV models (equation (4)) 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.[78] 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.

Table 11 reports estimated effects of student debt on four-year entrants' attainment and earnings eight years after college entry.[79] In addition to significantly increasing years of enrollment and cumulative credits attempted, an additional $1,000 of student debt increases

[78] IV models using the interaction between constrained and the aggregate limit increases as the excluded instrument provide similar estimates (see Appendix E).

[79] Appendix Table C.15 displays IV estimates for community college entrants. Results suggest that a $1,000 increase in student loan debt results in just under 0.2 more years of enrollment and approximately 5 additional credits earned. Constrained community college entrants are approximately 2 percentage points more likely to complete an associate degree per $1,000 increase in student debt. While we do not find statistically significant increases in community college entrants' earnings eight years after college enrollment, estimates are too imprecise to rule out modest effects.

four-year entrants' bachelor's degree receipt by 2.6 percentage points and their annual earnings by 2.5 percent. These effects are similar in magnitude to the estimated effect of similar-sized increases in grant aid on degree receipt and earnings found by Bettinger et al. (2019) and Denning, Marx, and Turner (2019), further supporting the interpretation that increased access to student loans facilitate human capital investment by easing liquidity constraints.

In Table 12, we provide estimated effects on financial outcomes for borrowers in the CCP/Equifax sample eight years after entry. A $1,000 increase in student debt significantly reduces student loan delinquencies by 0.9 percentage point and defaults by 1.1 percentage points. We again find no evidence that student loan debt affects homeownership. The estimated 95 percent confidence interval excludes effects larger than a 0.5 percentage point reduction in homeownership per $1,000 increase in debt, which is substantially smaller than the 1.8 percentage point reduction per $1,000 in student debt found by Mezza et al. (2020). As mentioned earlier, Mezza et al. (2020) look 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 home ownership.

D. Robustness

Our estimates are robust to the exclusion of covariates and different sample restrictions for the Texas sample (Appendix Table C.16) and CCP/Equifax sample (Appendix Table C.17).[80] Estimates are the least precise when we exclude controls for baseline characteristics and school fixed effects, but point estimates remain similar in magnitude (Panel B of Table C.16 and Panel A of Table C.17) and adding back in school fixed effects results in estimates that are very similar to our main results (Panel C of Table C.16).

In the remainder of the robustness tests, we vary the estimation sample. We obtain similar results for the Texas sample when we omit the first cohort (Panel D of Table C.16), include students who initially enrolled in a nonprofit institution (Panel E of Table C.16), impose the same sample restrictions as the CCP/Equifax sample (Panel F of Table C.16), and use the 2001

[80] Estimates for the pooled Texas sample using our preferred specification are provided in Panel A of Table C.16 to facilitate comparisons.

sample restrictions in all years (Panel G).[81] Finally, our results are robust to 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 (Panel H of Table C.16 and Panel C of Table C.17). Dose-response estimates are also similar when we use a specification that allows for five years of loan limit increases or discounts future borrowing by national persistence rates (not shown).

6. Conclusion

Much of the recent public discussion surrounding student borrowing has focused on the potential *negative* effects of resulting debt burdens, particularly on household balance sheets.[82] 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. Students who borrow more because of higher loan limits 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. Additional student loans also benefit constrained borrowers' financial positions in the years after college. 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 (2018). Increased borrowing could lower public revenue through four potential channels: (1) foregone tax revenues due to a reduction in

[81] As discussed in Section 3, we use a slightly different sample definition for the 2001 entry cohort due to it being 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. The CCP sample is restricted to students who were under age 20 at entry and excludes cohorts that entered before 2004.

[82] For example, in 2013 and 2014, respectively, the *New York Times* featured articles titled "Heavy Load of Student Loan Debt Is Weighing on the Economy, Too" and "Ripple Effects from Rising Student Debt."

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.[83] 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 eight years after entry persists, we estimate that the government will fully recoup expenditures on additional student loans within 9 to 12 years after entry.[84]

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 students *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.

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[83] As we explain further in Appendix F, 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 a borrower 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.

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

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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				
Gender = male	0.45	0.46	0.46	0.46
Race = white	0.52	0.43	0.34	0.34
Race = URM	0.41	0.49	0.60	0.61
Age	18.1	18.0	18.1	18.1
Financial aid received in entry year (2018$)				
Federal Stafford loans	$3,580	$3,567	$2,474	$2,044
Total loans	$3,586	$3,592	$2,569	$2,195
Federal Pell Grant	$1,745	$1,806	$2,100	$2,113
TEXAS Grant	$1,201	$1,473	$1,419	$1,754
Other grants	$1,686	$2,127	$1,956	$2,321
Work study	$167	$134	$206	$185
EFC (2018$)	$9,992	$11,986	$6,586	$6,768
COA (2018$)	$17,026	$20,367	$16,413	$18,076
Number of students	21,700	19,235	22,956	9,705
B. Texas Sample, community college entrants				
Demographics				
Gender = male	0.52	0.50	0.55	0.53
Race = white	0.43	0.44	0.44	0.44
Race = URM	0.54	0.53	0.54	0.54
Age	18.6	18.4	18.7	18.5
Financial aid received in entry year ($2018)				
Federal Stafford loans	$3,535	$3,561	$2,119	$1,941
Total loans	$3,539	$3,573	$2,166	$1,991
Federal Pell Grant	$1,983	$1,868	$1,870	$1,516
TEXAS Grant	$222	$283	$212	$202
Other grants	$574	$545	$515	$497
Work study	$80	$62	$58	$41
EFC (2018$)	$7,261	$7,929	$5,307	$6,890
COA (2018$)	$13,051	$13,497	$11,384	$12,544
Number of students	7,372	10,450	15,520	10,316
C. CCP/Equifax Sample				
Total loans	$3,588	$4,010	$2,151	$2,219
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	0.27
Has a Credit Score	0.15	0.15	0.26	0.26
Number of Accounts	0.23	0.22	0.43	0.41
Has a Credit Card	0.09	0.10	0.15	0.17
Has an Auto Loan	0.02	0.02	0.04	0.04
Number of Students	41,539	65,858	16,525	22,692

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. 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 eighth year after entry. Total loans in the CCP/Equifax sample are winsorized at the 99th percentile.

Table 4: Loan limit increases and baseline student characteristics

	(1) Predicted graduation rate	(2) Male	(3) White	(4) URM	(5) EFC	(6) Age
A. Texas sample, four-year college entrants (N = 74,132)						
Dependent variable mean	0.555	0.457	0.414	0.519	9026	18.08
Constrained × cohort ∈ {2006,2007,2008}	0.006	0.011	0.008	-0.020	1720	0.04
	(0.005)	(0.010)	(0.014)	(0.014)	(1206)	(0.03)
B. Texas sample, community college entrants (N = 43,122)						
Dependent variable mean	0.321	0.526	0.440	0.536	6639	18.58
Constrained × cohort ∈ {2006,2007,2008}	-0.004	-0.002	-0.011	0.012	-938	0.16
	(0.002)+	(0.009)	(0.010)	(0.010)	(364)*	(0.16)

	(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 = 145,616)						
Dependent variable mean	0.204	0.181	0.279	0.116	0.027	18.50
Constrained × cohort ∈ {2006,2007,2008}	-0.002	-0.002	0.010	-0.008	0.001	0.03
	(0.005)	(0.005)	(0.011)	(0.004)*	(0.002)	(0.01)**

Note: See Table 3 notes for sample definitions. Each column within a panel includes estimates from separate regressions; dependent variable indicated in column heading. 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 8 years of college entry on the other characteristics displayed in this table 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; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

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 = 74,132)							
Dependent variable mean	$2,977	$6,018	$9,631	$13,487	$16,245	$17,708	$18,633
Constrained × cohort ∈ {2006,2007,2008}	175	740	1285	1760	1819	1856	1873
	(59)**	(230)**	(424)**	(559)**	(635)**	(668)**	(685)**
	{0.682}	{0.049}	{0.016}	{0.008}	{0.029}	{0.037}	{0.031}
B. Texas sample, community college entrants (N = 43,122)							
Dependent variable mean	$2829	$4456	$6116	$7874	$9485	$10,708	$11,652
Constrained × cohort ∈ {2006,2007,2008}	86	249	547	802	1056	1118	1074
	(25)**	(83)**	(127)**	(193)**	(252)**	(294)**	(317)**
	{0.800}	{0.600}	{0.268}	{0.123}	{0.047}	{0.027}	{0.035}
C. CCP/Equifax sample (N = 145,616)							
Dependent variable mean	$3379	$9846	$16,990	$23,748	$30,307	$35,750	$39,770
Constrained × cohort ∈ {2006,2007,2008}	311	574	1130	1600	2037	2768	3043
	(12)**	(87)**	(182)**	(261)**	(362)**	(478)**	(503)**
	{0.361}	{0.506}	{0.266}	{0.170}	{0.112}	{0.087}	{0.098}

Note: See Table 3 notes for sample definitions. Each column within a panel contains estimates from separate regressions; dependent variable is cumulative total student loans *X* years after entry, where the value of *X* is indicated in column heading. Total loans in the CCP/Equifax sample are winsorized at the 99th percentile. 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 race (white, URM), age at entry, EFC at entry, and gender. 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; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$; p-values from wild-t cluster bootstrap in brackets.

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

X =	1	2	3	4	5	6	7	8
A. Enrollment X years later								
Dependent variable mean	0.877	0.793	0.728	0.532	0.311	0.203	0.147	0.114
Constrained × cohort ∈ {2006,2007,2008}	0.044	0.047	0.050	0.014	-0.002	-0.012	-0.018	-0.007
	(0.007)**	(0.007)**	(0.009)**	(0.010)	(0.009)	(0.007)	(0.006)**	(0.004)
	{0.003}	{0.008}	{0.008}	{0.321}	{0.672}	{0.106}	{0.033}	{0.018}
B. Cumulative years of enrollment								
Dependent variable mean	1.88	2.67	3.40	3.93	4.24	4.44	4.59	4.71
Constrained × cohort ∈ {2006,2007,2008}	0.04	0.09	0.14	0.16	0.15	0.14	0.12	0.12
	(0.01)**	(0.01)**	(0.02)**	(0.03)**	(0.03)**	(0.04)**	(0.04)**	(0.04)**
	{0.002}	{0.001}	{0.004}	{0.006}	{0.006}	{<0.001}	{<0.001}	{0.005}
C. Cumulative credits attempted								
Dependent variable mean	51.65	73.12	92.09	103.61	109.53	113.11	115.58	117.43
Constrained × cohort ∈ {2006,2007,2008}	2.81	4.25	5.36	5.69	5.68	5.53	5.29	5.33
	(0.61)**	(0.87)**	(1.14)**	(1.20)**	(1.21)**	(1.23)**	(1.20)**	(1.18)**
	{0.001}	{<0.001}	{0.008}	{<0.001}	{<0.001}	{0.005}	{<0.001}	{0.004}
D. Any degree or credential								
Dependent variable mean	<0.001	0.016	0.180	0.381	0.478	0.527	0.557	0.578
Constrained × cohort ∈ {2006,2007,2008}	-0.0004	0.001	0.033	0.039	0.049	0.052	0.050	0.046
	(0.001)	(0.002)	(0.009)**	(0.011)**	(0.010)**	(0.011)**	(0.011)**	(0.010)**
	{0.620}	{0.823}	{0.031}	{0.022}	{0.009}	{0.009}	{0.003}	{0.008}
E. Bachelor's degree								
Dependent variable mean			0.162	0.355	0.446	0.491	0.517	0.534
Constrained × cohort ∈ {2006,2007,2008}			0.036	0.043	0.054	0.058	0.056	0.053
			(0.009)**	(0.011)**	(0.011)**	(0.012)**	(0.011)**	(0.011)**
			{0.015}	{0.023}	{0.003}	{0.002}	{<0.001}	{<0.001}
F. Associate degree								
Dependent variable mean		0.012	0.015	0.023	0.032	0.039	0.046	0.053
Constrained × cohort ∈ {2006,2007,2008}		-0.001	-0.001	-0.004	-0.008	-0.007	-0.008	-0.009
		(0.002)	(0.002)	(0.003)	(0.003)*	(0.003)*	(0.003)*	(0.003)**
		{0.387}	{0.489}	{0.264}	{0.041}	{0.076}	{0.115}	{0.049}

Note: Texas sample, see Table 3 notes for definition (N = 74,132). Each column within a panel contains estimates from separate regressions; dependent variable is indicated in the subpanel heading, measured X years after entry, where the value of X is indicated in column heading. All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for race (white, URM), age at entry, EFC at entry, and gender. Robust standard errors, clustered by entry institution, in parentheses; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$; p-values from wild-t cluster bootstrap 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 = 145,616)			
Dependent variable mean	0.517	0.584	0.615
Constrained × cohort ∈ {2006,2007,2008}	-0.023	-0.007	-0.010
	(0.006)**	(0.006)	(0.007)
	{0.066}	{0.480}	{0.381}
B. Credit card balance (conditional on having 1+ cards)			
Dependent variable mean	$1361	$1816	$2151
Constrained × cohort ∈ {2006,2007,2008}	-83	-79	-27
	(28)*	(42)+	(55)
	{0.016}	{0.036}	{0.662}
Observations	75,230	85,061	89,569
C. Any earnings X years after entry (N = 74,132)			
Dependent variable mean	0.807	0.796	0.798
Constrained × cohort ∈ {2006,2007,2008}	-0.019	-0.015	-0.001
	(0.005)**	(0.006)*	(0.006)
	{0.066}	{0.051}	{0.926}
D. Ln(earnings) X years after entry			
Dependent variable mean	8.595	8.826	9.044
Constrained × cohort ∈ {2006,2007,2008}	-0.048	-0.028	-0.043
	(0.025)+	(0.025)	(0.025)+
	{0.020}	{0.394}	{0.049}
Observations	59,802	58,981	59,172

Note: Texas and CCP/Equifax samples, see Table 3 notes for definitions. See Table 5 notes for specification. Each column within a panel contains estimates from separate regressions; dependent variable is indicated in the subpanel heading, measured X years after entry, where the value of X is indicated in column heading. Robust standard errors, clustered by entry state, in parentheses; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$; p-values from wild-t cluster bootstrap in brackets.

Table 8: Effects of loan limit increases on student loan repayment and labor market outcomes

X =	4	5	6	7	8
A. Student loan delinquency (N = 145,616)					
Dependent variable mean	0.095	0.110	0.118	0.119	0.113
Constrained × cohort ∈ {2006,2007,2008}	-0.014	-0.015	-0.008	-0.009	-0.008
	(0.004)**	(0.004)**	(0.004)*	(0.004)*	(0.004)+
	{0.141}	{0.118}	{0.281}	{0.070}	{0.064}
B. Student loan default (N = 145,616)					
Dependent variable mean	0.067	0.082	0.092	0.095	0.094
Constrained × cohort ∈ {2006,2007,2008}	-0.012	-0.016	-0.008	-0.006	-0.009
	(0.003)**	(0.003)**	(0.003)*	(0.003)*	(0.004)*
	{0.135}	{0.113}	{0.479}	{0.150}	{0.049}
C. Any earnings X years after entry, four-year entrants (N = 74,132)					
Dependent variable mean	0.803	0.796	0.785	0.778	0.771
Constrained × cohort ∈ {2006,2007,2008}	-0.008	-0.006	-0.002	-0.005	-0.009
	(0.007)	(0.006)	(0.008)	(0.007)	(0.007)
	{0.395}	{0.632}	{0.701}	{0.660}	{0.389}
D. Ln(earnings) X years after entry, four-year entrants					
Dependent variable mean	9.414	9.750	9.972	10.124	10.247
Constrained × cohort ∈ {2006,2007,2008}	-0.010	0.010	0.043	0.059	0.046
	(0.019)	(0.024)	(0.020)*	(0.016)**	(0.016)**
	{0.533}	{0.630}	{0.016}	{0.056}	{0.097}
Observations	59,513	58,987	58,199	57,686	57,114

Note: Texas and CCP/Equifax samples, see Table 3 notes for sample definitions. See Table 5 notes for specification. Each column within a panel contains estimates from separate regressions; dependent variable is indicated in the subpanel heading, measured X years after entry, where the value of X is indicated in column heading. Student loan borrowers are classified as delinquent if they have a positive past due balance for 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; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$; p-values from wild-t cluster bootstrap in brackets.

Table 9: The effect of loan limit increases on constrained students' other financial outcomes

X =	4	5	6	7	8
A. Delinquent (60+ days past due) on any loans					
Dependent variable mean	0.105	0.112	0.118	0.122	0.124
Constrained × cohort ∈ {2006,2007,2008}	0.001	0.004	0.002	-0.001	-0.004
	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
	{0.775}	{0.016}	{0.775}	{0.742}	{0.461}
B. Has a mortgage					
Dependent variable mean	0.038	0.061	0.090	0.121	0.155
Constrained × cohort ∈ {2006,2007,2008}	0.005	0.004	0.001	0.002	0.001
	(0.003)+	(0.003)	(0.004)	(0.004)	(0.003)
	{0.082}	{0.353}	{0.859}	{<0.001}	{0.761}
C. Has an auto loan					
Dependent variable mean	0.259	0.311	0.356	0.398	0.434
Constrained × cohort ∈ {2006,2007,2008}	0.018	0.014	0.007	0.008	0.004
	(0.004)**	(0.005)**	(0.005)	(0.006)	(0.006)
	{0.082}	{0.024}	{0.541}	{0.064}	{0.625}

Note: CCP/Equifax sample, see Table 3 notes for definition (N = 145,616). See Table 6 notes for specification. Robust standard errors, clustered by entry state, in parentheses; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$; p-values from wild-t cluster bootstrap in brackets.

Table 10: Effects of loan limit increases on attainment, earnings, and financial outcomes, dose-response specification

A. Cumulative student loans, credit card holding one year after entry

	(1) Four years after entry (Texas)	(2) Six years after entry (Texas)	(3) Four years after entry (CCP)	(4) Six years after entry (CCP)	(5) Has credit card
Constrained x AggLimExp ($1k)	339	342	461	624	-0.004
	(94)**	(103)**	(62)**	(80)**	(0.001)**
	{0.045}	{0.041}	{0.052}	{0.048}	{0.075}
Observations	74,132	74,132	145,616	145,616	145,616

B. Attainment and earnings outcomes, eight years after entry

	(5) Cumulative years enrolled	(6) Cumulative credits attempted	(7) Any degree receipt	(8) Ln(earnings)	(9) Zip code mean AGI
Constrained x AggLimExp ($1k)	0.02	1.07	0.007	0.008	242
	(0.01)**	(0.23)**	(0.002)**	(0.003)*	(74)**
	{0.052}	{0.041}	{0.050}	{0.153}	{0.097}
Observations	74,132	74,132	74,132	57,114	145,616

C. Financial outcomes, eight years after entry

	(10) Ever delinq. student loan	(11) Ever default on student loan	(12) Delinquent on any debt	(13) Any mortgage	(14) Any auto loan
Constrained x AggLimExp ($1k)	-0.005	-0.006	-0.001	0.0004	-0.0001
	(0.001)**	(0.001)**	(0.001)	(0.001)	(0.001)
	{0.141}	{0. 025}	{0.371}	{0.696}	{0.947}
Observations	145,616	145,616	145,616	145,616	145,616

Note: Columns 3, 4, 5, 9, and Panel C contain estimates from the CCP/Equifax sample; remaining columns contain estimates from the Texas four-year entrant sample. See Table 3 notes for sample definitions. Total loans in the CCP/Equifax sample are winsorized at the 99th percentile. All specifications control for the level effect of AggLimExp and Constrained. See Table 5 notes for description of additional control variables used Robust standard errors, clustered by entry institution (columns 1, 2, 5, 6, and 7) or entry state (columns 3, 4, 5, 9, and Panel C), in parentheses; + $p < 0.1$, * $p < 0.05$, **$p < 0.01$; p-values from wild-t cluster bootstrap in brackets.

Table 11: IV estimates of the effect of student loans on four-year entrants' attainment and earnings, eight years after college entry

	(1) Total years enrolled	(2) Total credits earned	(3) Any degree	(4) Bachelor's degree	(5) Associate degree	(6) Ln(earnings)
Cumulative loans ($1k)	0.066	3.01	0.026	0.030	-0.005	0.025
	(0.031)*	(1.31)*	(0.012)*	(0.014)*	(0.003)+	(0.013)*
Observations	74,132	74,129	74,129	74,129	74,129	57,110

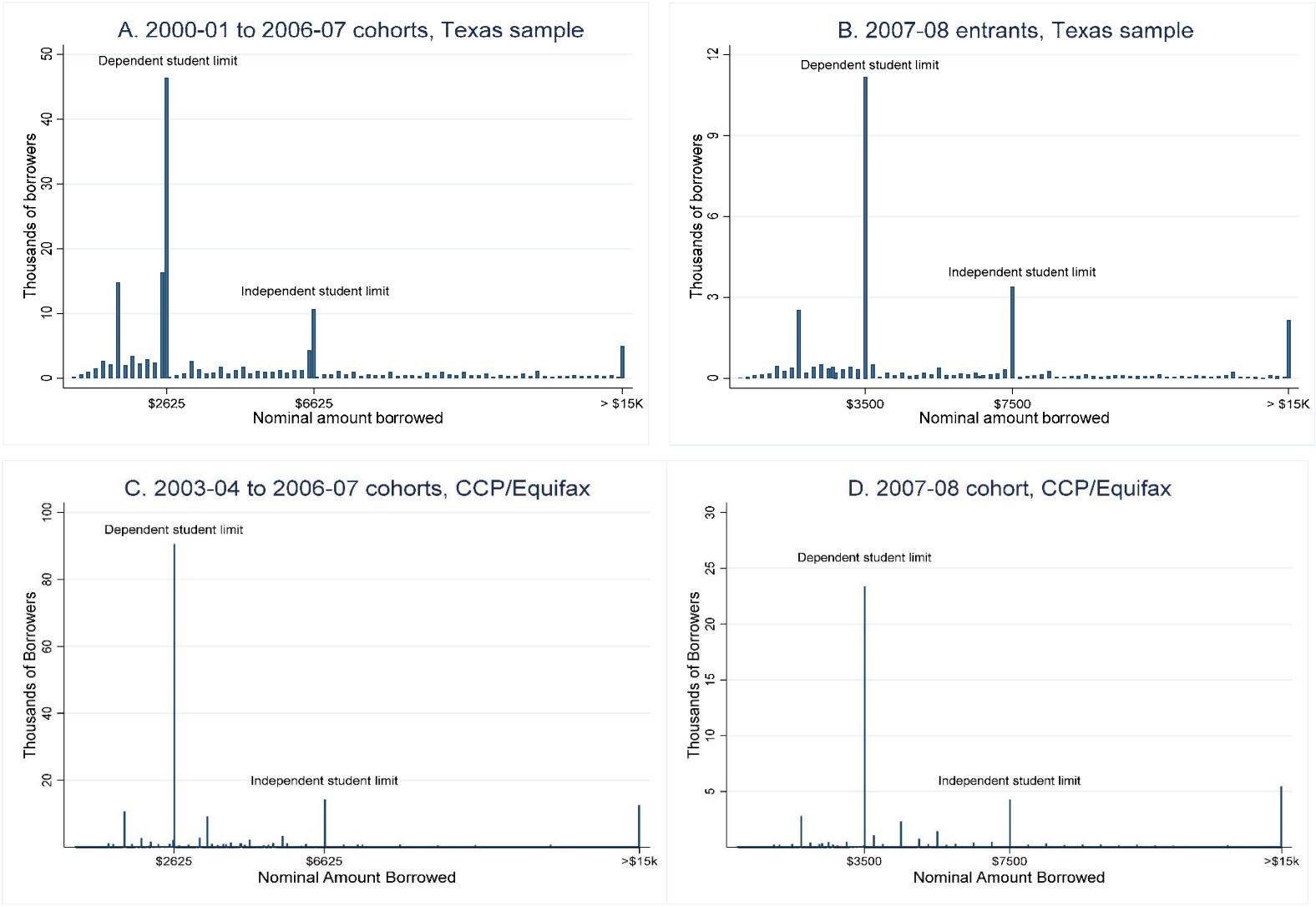
Note: Four-year entrants, Texas sample, see Table 3 notes for definition. Each column contains estimates from separate regressions; dependent variable is indicated in the column heading, measured eight years after entry. All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for race (white, URM), age at entry, EFC at entry, and gender. The interaction between constrained at entry and belonging to the 2006 through 2008 entry cohorts serves as excluded instrument. Robust standard errors, clustered by entry institution, in parentheses; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table 12: IV estimates of the effect of student loans on selected financial outcomes, eight years after college entry

	(1) Delinquent (stud. loans)	(2) Default (stud. loans)	(3) Any delinquent debt	(4) Any mortgage	(5) Any auto loan
Cumulative loans ($1k)	-0.010	-0.012	-0.003	0.001	0.003
	(0.003)**	(0.003)**	(0.003)	(0.002)	(0.004)
Observations	145,616	145,616	145,616	145,616	145,616

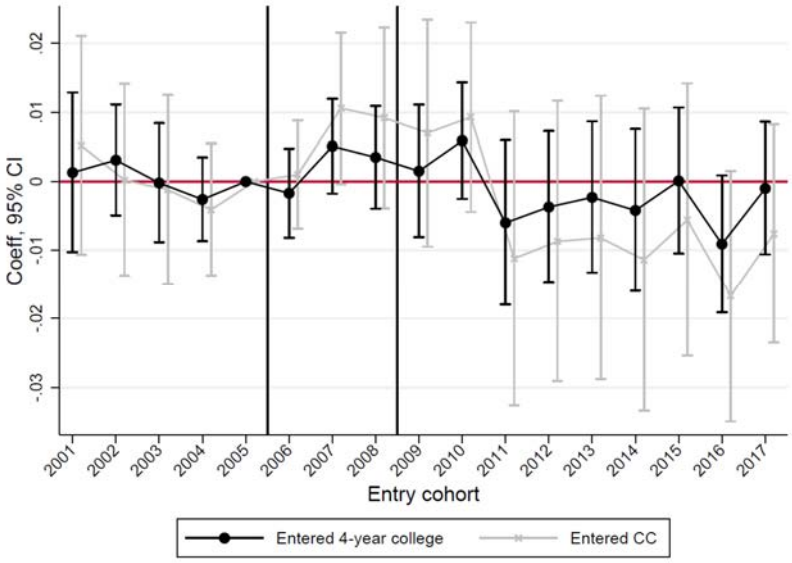
Note: CCP/Equifax sample (N = 145,616), see Table 3 notes for definition. Each column contains estimates from separate regressions; dependent variable is indicated in the column heading, measured 8 years after entry. All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, state at entry fixed effects, and age fixed effects. Cumulative loans measured in years 1-4. The interaction between constrained at entry and belonging to the 2006 through 2008 entry cohorts serves as excluded instrument. First-stage F-Statistic is 37.58. Robust standard errors, clustered by entry state, in parentheses; + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Figure 1: The distribution of entry year borrowing



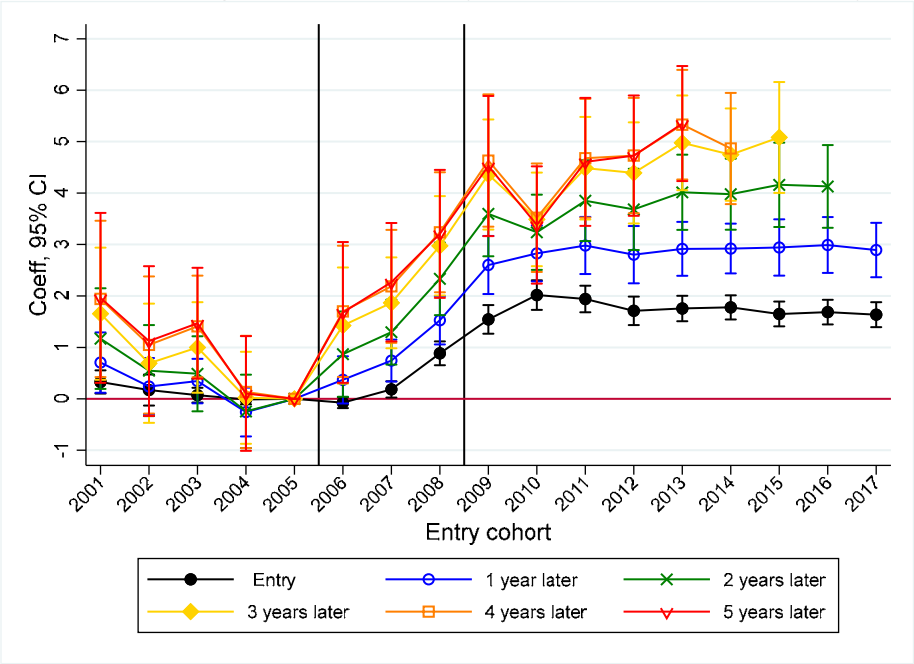
Notes: See Table 3 notes for sample definitions.

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



Note: Texas sample, see Table 3 notes for definition. 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 8 years of college entry on the characteristics displayed in Table 4 (Panels A and B) and school of entry fixed effects. Confidence intervals based on robust standard errors, clustered by entry institution.

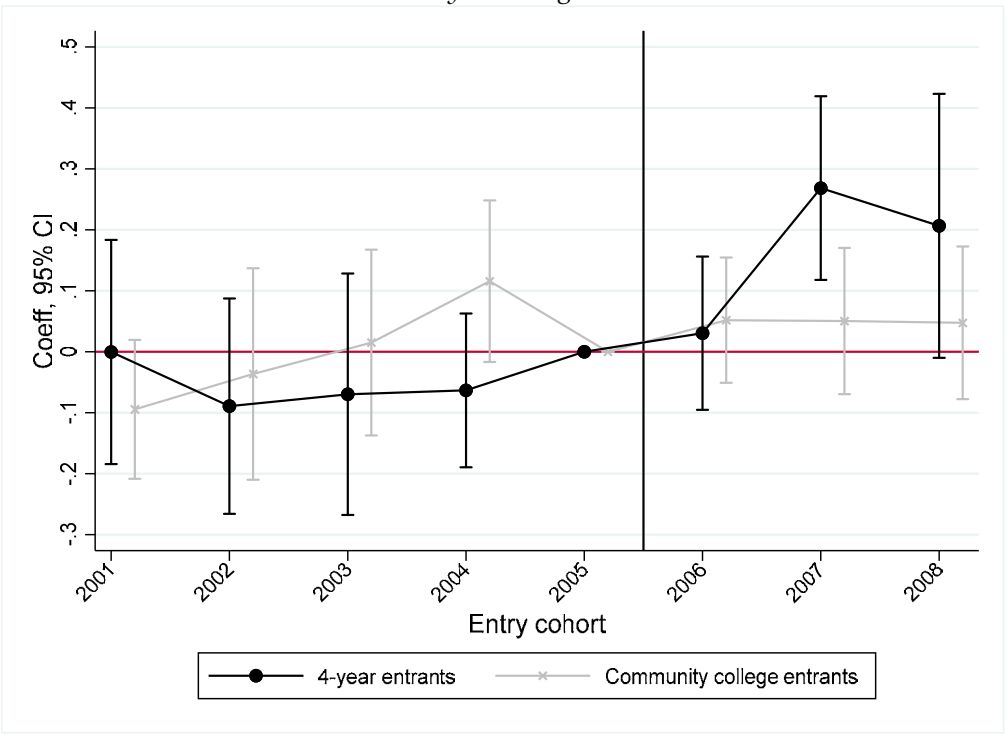
Figure 3: The effect of being constrained at entry on cumulative student loans by entry cohort



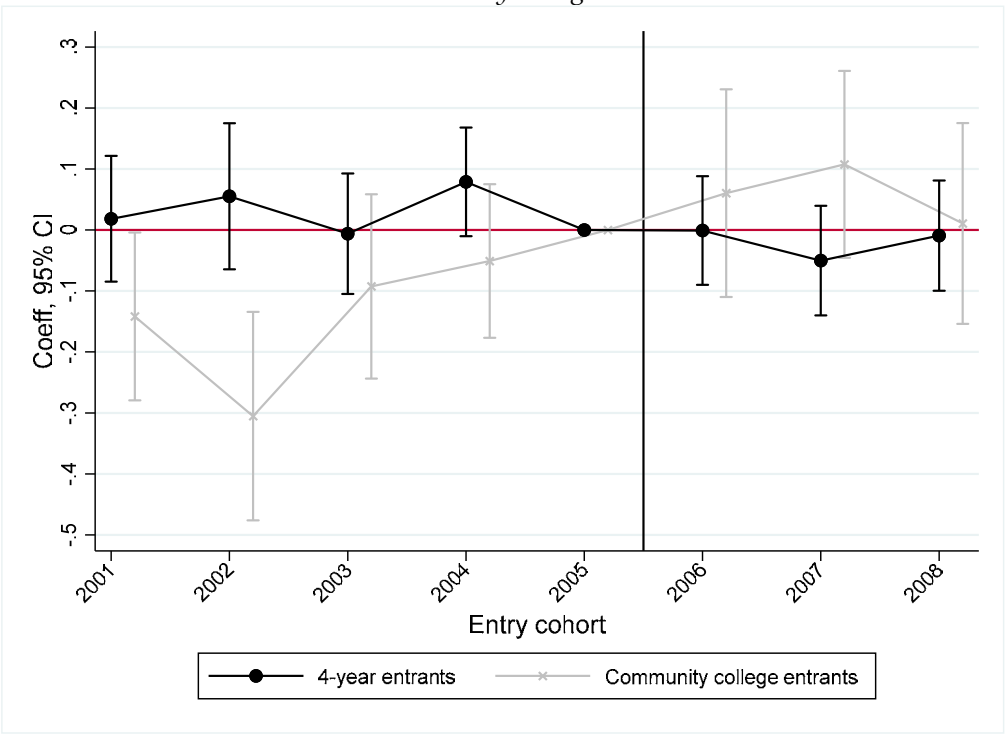
Note: Texas sample, see Table 3 notes for definition. Coefficients and 95% confidence intervals from regressions of cumulative borrowing on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, entry school fixed effects, and controls for race (white, URM), age at entry, EFC at entry, 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 8 years after entry

A. Four-year college enrollment



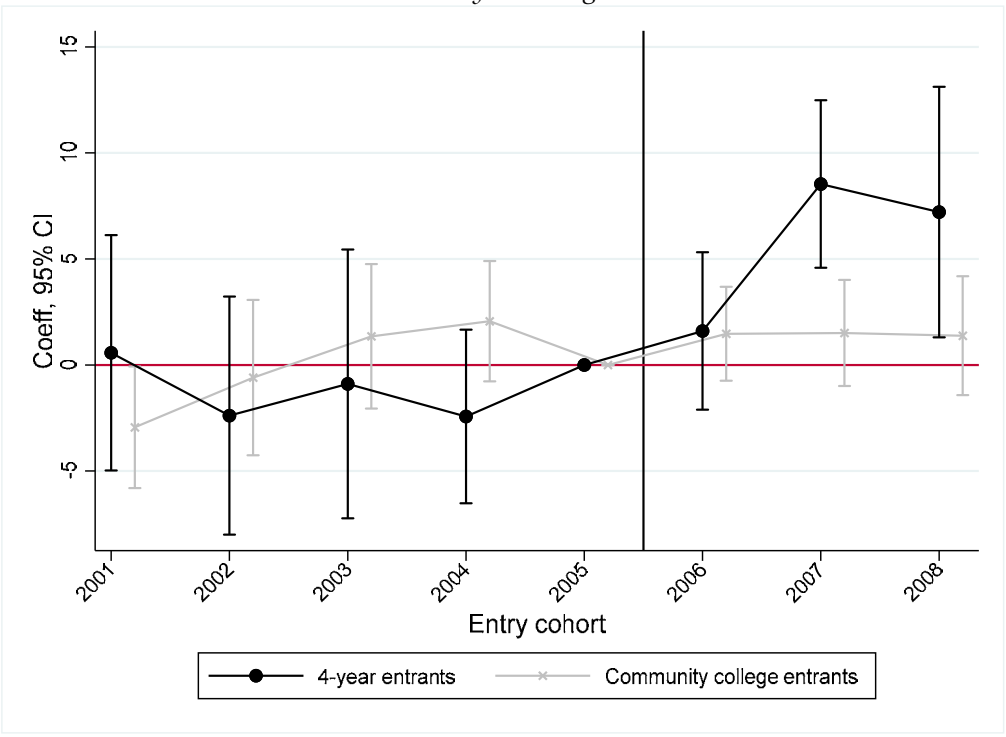
B. Community college enrollment



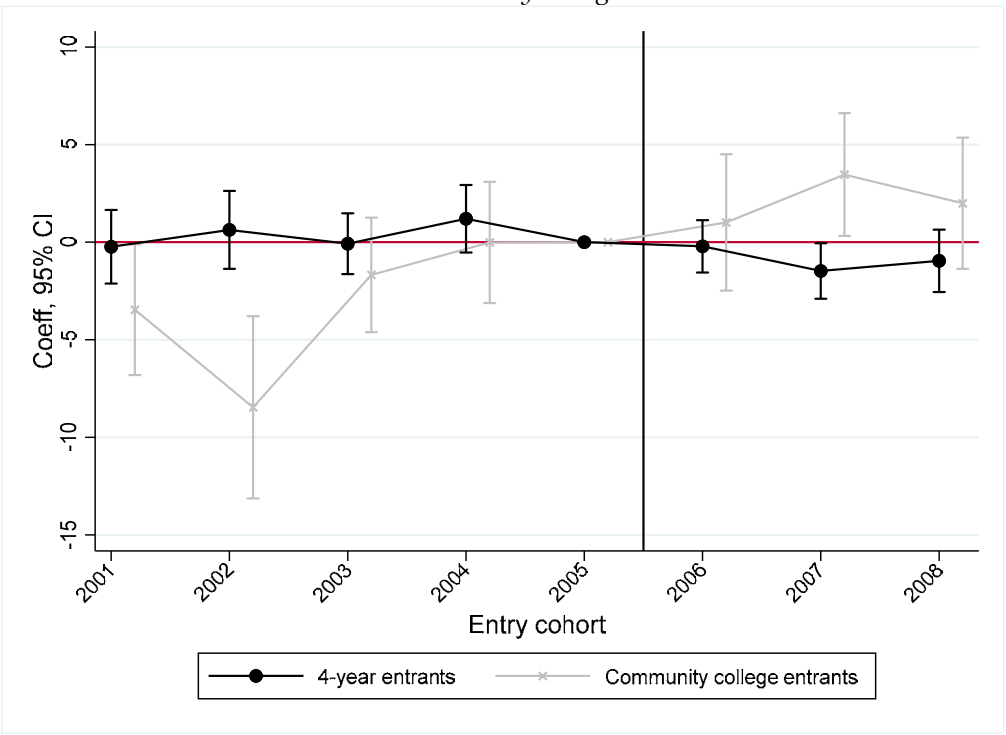
Note: Texas sample, see Table 3 notes for definition. See Figure 3 notes for specification.

Figure 5: Effects of loan limit increases on cumulative credits attempted 8 years after entry

A. Four-year college credits



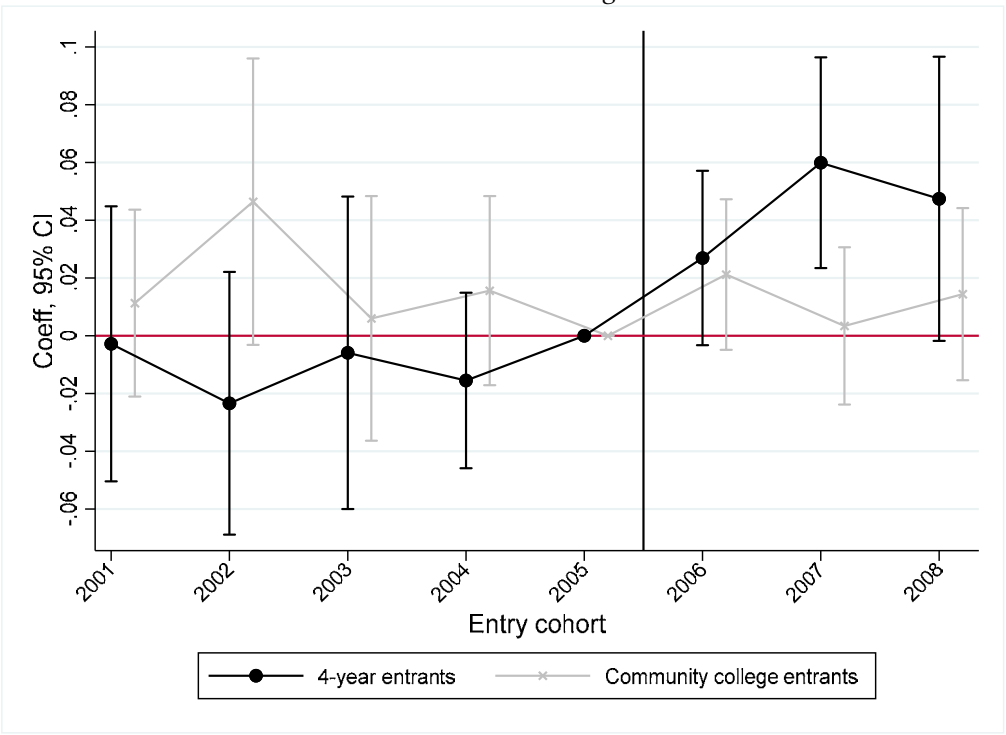
B. Community college credits



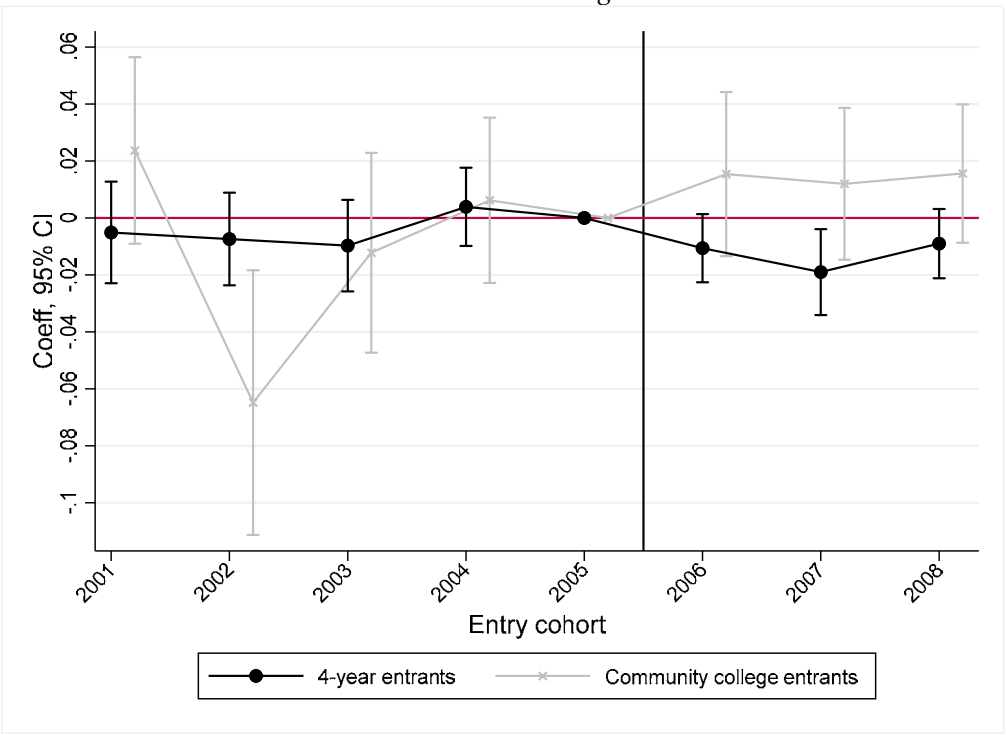
Note: Texas sample, see Table 3 notes for definition. See Figure 4 notes for specification.

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

A. Bachelor's degree



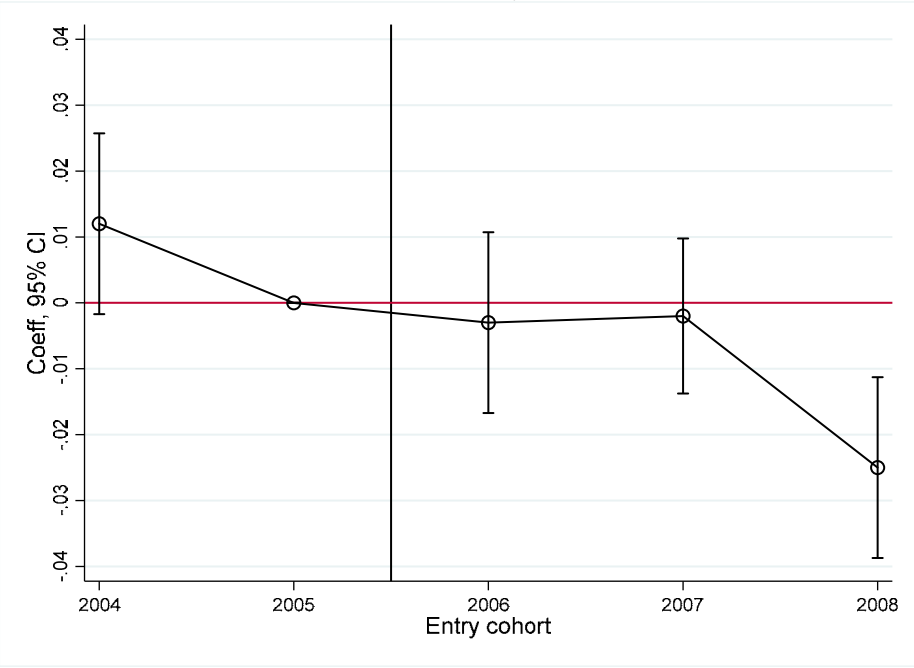
B. Associate degree



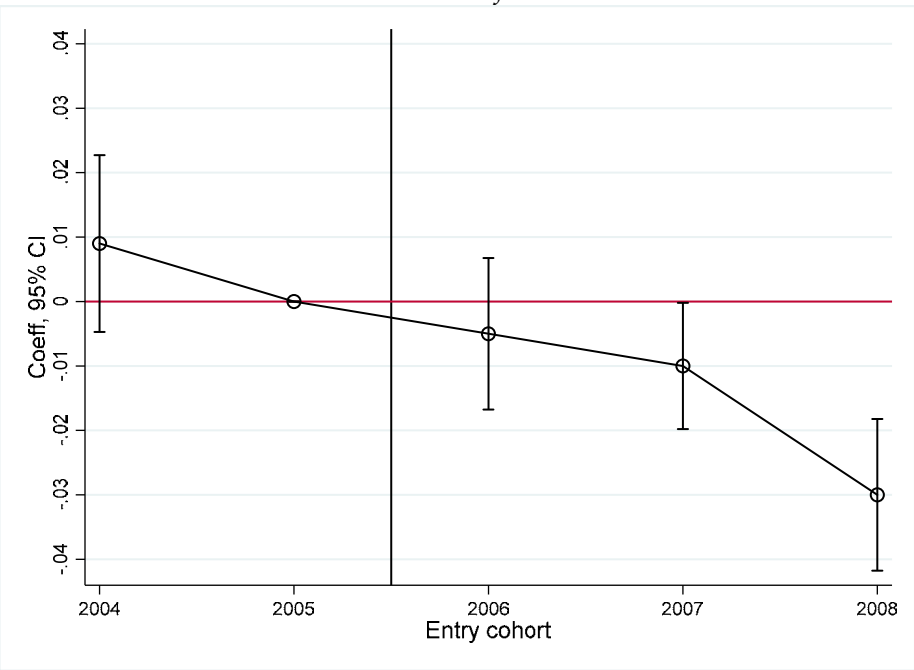
Note: Texas sample, see Table 3 notes for definition. See Figure 4 notes for specification.

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

A. Ever delinquent



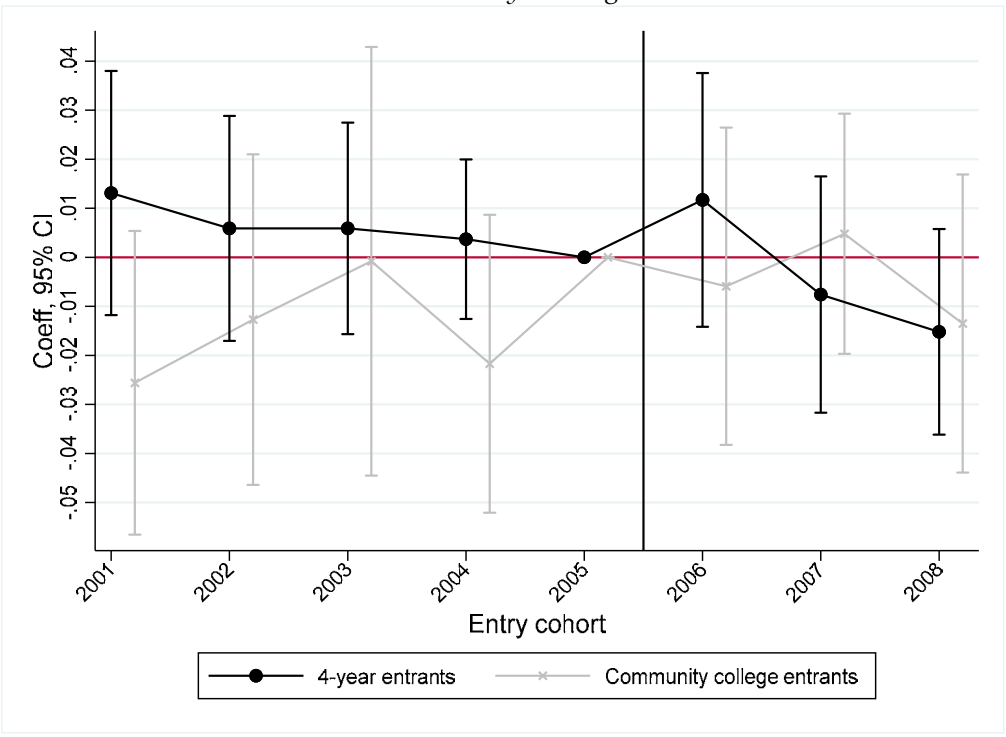
B. Ever default



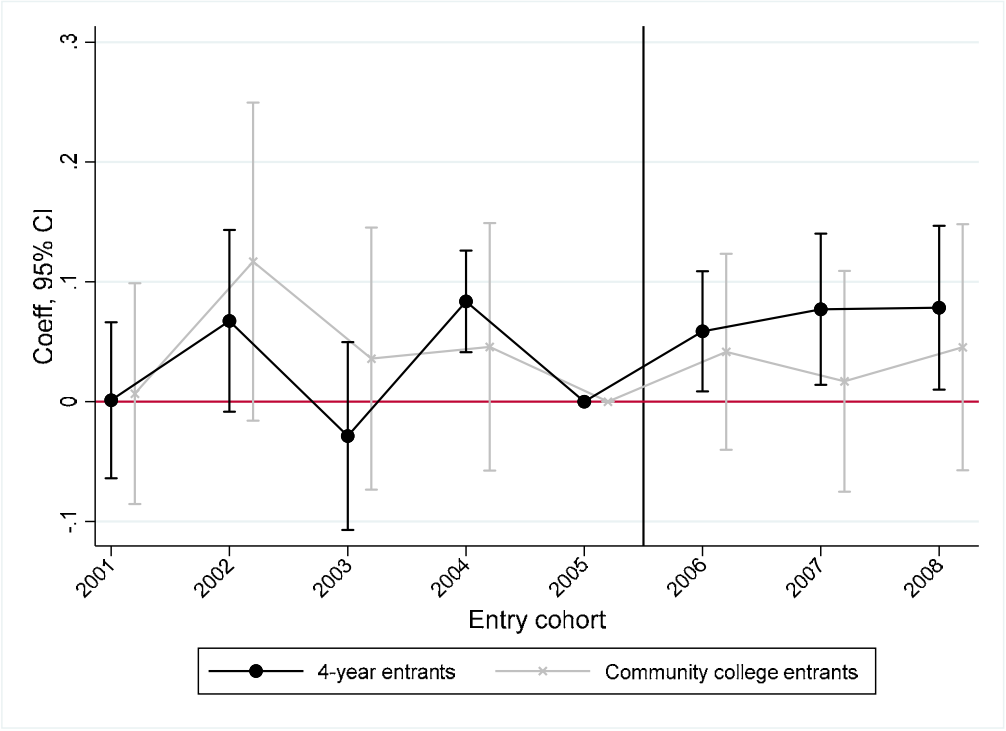
Note: CCP/Equifax sample, see Table 3 notes for definition. Outcomes are measured four through eight years after initial borrowing. Coefficients and 95% confidence intervals from regressions of the indicated outcome on the interaction between being constrained at entry and entry cohort (with 2005 serving as omitted category). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, 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. Confidence intervals based on robust standard errors, clustered by entry state.

Figure 8: Effects of loan limit increases on labor market outcomes eight years after entry

A. Any earnings



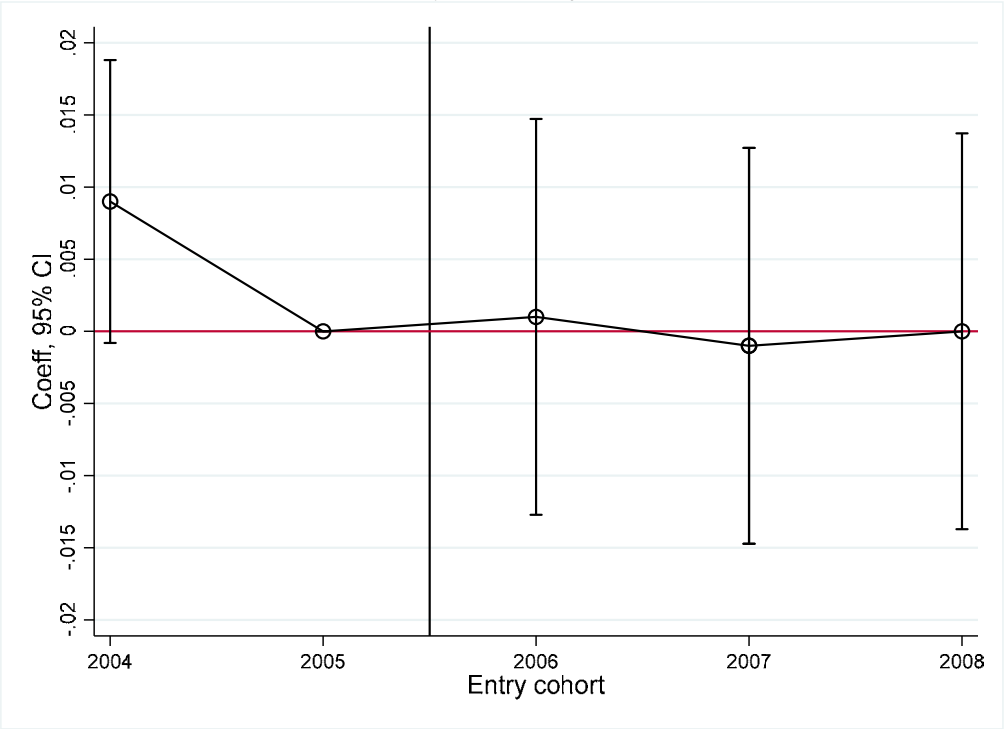
B. Ln(earnings)



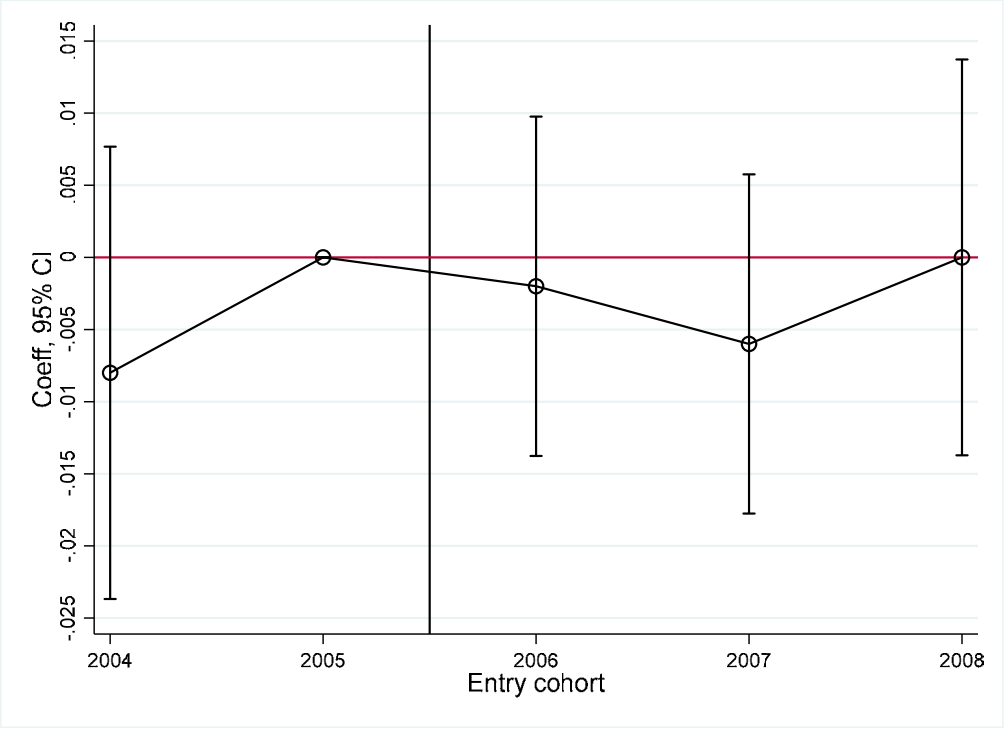
Note: Texas sample, see Table 3 notes for definition. See Figure 4 notes for specification.

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

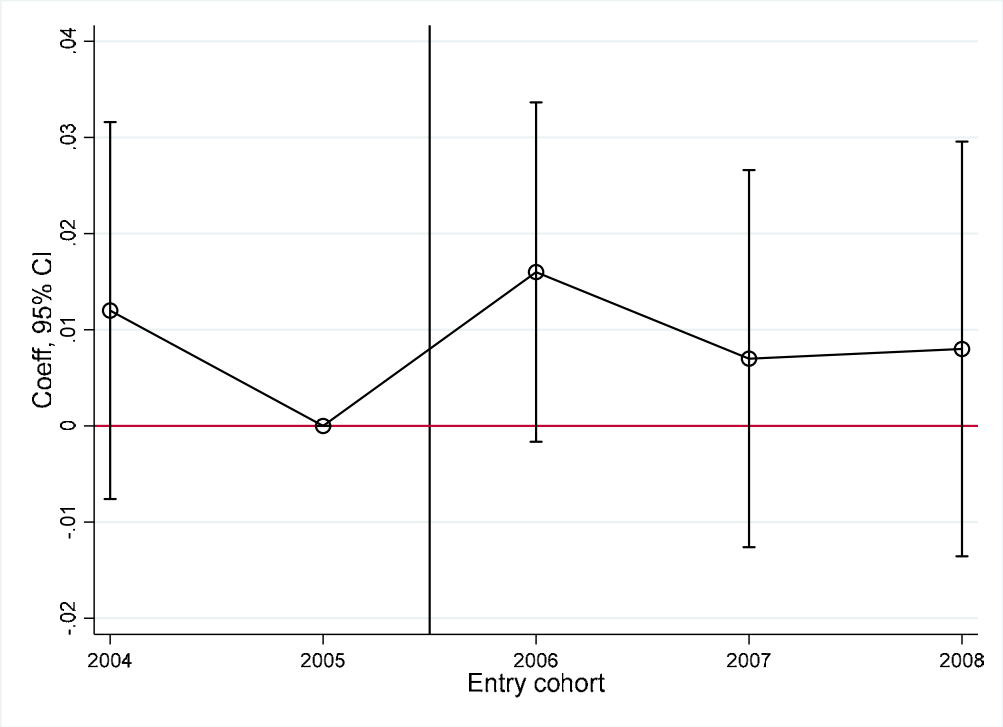
A. Delinquent on any debt



B. Has a mortgage



C. Has an auto loan



Note: CCP/Equifax sample, see Table 3 notes for definition. See Figure 7 notes for specification.