Taking It to the Limit:
Effects of Increased Student Loan Availability*

Sandra E. Black
Columbia University

Jeffrey T. Denning
Brigham Young University

Lisa J. Dettling
Federal Reserve Board of Governors

Sarena Goodman
Federal Reserve Board of Governors

Lesley J. Turner
Vanderbilt University

March 2020

Abstract

There is a widely held perception that there is a student debt crisis in the United States. However, little is known about the longer-run effects of increased student debt on longer-run student outcomes such as earnings, and, importantly, credit health. We use variation in student borrowing induced by the expansion of borrowing limits in the 2000s that differentially affected entry cohorts over time and detailed administrative data on student outcomes and credit information to identify the effect of increased educational borrowing on credit constrained students' education attainment, earnings, credit score and credit history, and borrowing behavior. We find that access to more student debt improves student outcomes by increasing degree completion, raising earnings, and importantly, by improving loan repayment and other credit outcomes.

* Email: sblack@columbia.edu, jeffdenning@byu.edu, lisa.j.dettling@frb.gov, sarena.f.goodman@frb.gov, and lesley.j.turner@vanderbilt.edu. Any views or interpretations expressed in this paper are those of the authors and do not necessarily reflect the views of the Federal Reserve Board of Governors. The conclusions of this research do not necessarily reflect the opinion or official position of the Texas Education Research Center, the Texas Education Agency, the Texas Higher Education Coordinating Board, the Texas Workforce Commission, or the State of Texas. We thank seminar participants at Brigham Young University, Duke University, Vanderbilt University, the Association for Education Finance and Policy 2020 annual meeting, National Tax Association 2019 annual meeting, and the WEAI 2019 conference for helpful comments and suggestions.

1. Introduction

There is growing concern of a student debt crisis in the United States. Outstanding student loan debt now exceeds $1.5 trillion (Federal Reserve Bank of New York 2019) and over half of U.S. undergraduates rely on federal student loans to finance college (U.S. Department of Education 2018). Although student debt is intended to help fund human capital investments, it may also distort borrowers' post-college decisions (e.g., choice of career or where to live) or have negative spillovers to other parts of the economy (e.g., by delaying homeownership or harming credit worthiness). Thus, a key question is whether student debt negatively impacts borrowers' longer-run outcomes and the economy more broadly.

In contrast to concerns over the potentially negative impacts of student debt, existing research suggests that attending college is a good investment (Card 1999; Barrow and Malamud 2015), and that on average, borrowing to finance college should be more than offset by higher earnings later on (Avery and Turner 2012). However, surprisingly little is known about the effects of increased borrowing (holding college prices and students' other resources constant) on longer-run outcomes such as earnings and, importantly, financial well-being. We use policy-driven borrowing increases caused by increases in federal student loan limits to provide the first evidence on the effect of increased access to student loans for credit constrained students on both short- and longer-run attainment and labor market outcomes (e.g., degree completion and earnings) financial well-being (e.g., homeownership and credit-worthiness), and use of other debt (e.g., credit cards and auto 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 cannot be offered as collateral for a loan, the private market will underprovide credit (Friedman 1955). Hence, in classic models of human capital investment, increasing access to credit should also increase human capital. Existing evidence on the presence and magnitude of credit constraints is mixed.[2] Our paper advances this line of research by providing a direct test of the existence and importance of credit constraints for investments in higher education – the first of this sort for a broad population.[3]

A key challenge in identifying the effect of increased access to student loans in the United States is that eligibility is essentially universal and loan limits only vary with class standing and whether a student is classified as traditional or non-traditional.[4] We overcome this limitation by studying the only policy changes to federal loan limits in recent history, which led to staggered increases in student loan limits across entry cohorts in the mid-2000s. Our difference-in-differences identification strategy compares changes in the outcomes of students who entered before and after the loan limit increases between two groups – students who were likely

[2] See, for example, 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, Sidibe (2018).
[3] Past research examines effects of increases in credit access in Chile (Solis 2017), South Africa (Gurgand, Loresnceau, Melonio 2011), and U.S. community college students (Dunlop 2013; Weiderspan 2016), or uses smaller policy discontinuities (Goodman, Isen, Yannelis 2018; Denning and Jones *forthcoming*).
[4] Non-traditional or "independent" students are either 24 or older, have children or other dependents, or are married. All other students are considered traditional or "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.

constrained in their borrowing by statutory limits and those who were likely unconstrained by loan limits.

A second challenge to examining the full effects of student loan debt is access to a sufficiently long and detailed panel of borrowers, which we overcome by leveraging two large administrative data sets that contain 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. student loan borrowers.

Taking advantage of the large change in loan limits combined with these detailed datasets, our work provides the first comprehensive picture of the effects of changes in student loan limits on both short- and long-run outcomes for a broad population of students. Importantly, in addition to identifying the effect on educational attainment and work behavior while in school, we also provide estimates of long-run effects on wages and financial well-being. As a result, we can address the question of whether student debt has negative effects on later homeownership and borrowing behavior.

A key identifying assumption for our empirical strategy is that, in the absence of policy changes to federal loan limits, differences between the outcomes for constrained and unconstrained students would have been similar in the years before and after the loan limit increases. Balance tests across a range of baseline socioeconomic characteristics and parallel trends in outcomes for cohorts prior to the limit increases provide support for this assumption.

Focusing on dependent students, who represent 51% of undergraduate students in recent years (Fountain 2019), we first show that loan limit increases significantly increased 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 by more than $1,800, while community college entrants' accumulated approximately $1,000 more in student debt.[5] These effects are driven by increases in federal Stafford Loans, and we can rule out all but small changes in other types of student loans. 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. Similarly, loan limit increases led to significant reductions in credit card use among constrained students early in their college careers, suggesting the increased student loan debt reduced students' reliance on (typically higher cost) credit card debt.

The increase in loan availability also led to significant increases in 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 greater annual earnings.[6]

Under the assumption that increases in loan limits only increase constrained students' attainment vis-à-vis increases in their borrowing, instrumental variables estimates suggest that an additional $1000 of student debt significantly increases four-year entrants' bachelor's degree receipt by a 2.7 percentage points and their annual earnings by 2.5 percent. These effects are

[5] The effect of loan limit increases on borrowing overwhelmingly occur in the first four years after college entry.
[6] Although we find some evidence of increased attainment among community college entrants, these students do not appear to benefit from higher earnings.

similar in magnitude to the estimated effect of similar-sized increases in grant aid (Bettinger et al. 2019; Denning, Marx, Turner 2019), further supporting the interpretation that increased access to student loans facilitate human capital investment by easing liquidity constraints.

We next exploit the rich credit report data to examine the effect of increased loan availability on borrowers' balance sheets and financial well-being. We find that access to higher loan limits result in 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" would complete college. Thus, despite having higher student debt, these borrowers appear to be better able to manage their debt burdens. Moreover, even though indebtedness is an input into credit scoring models, these borrowers' credit scores are higher than those of their peers.[7]

Since debt-to-income ratios are used in underwriting, higher levels of student debt could reduce one's ability to finance other investments, such as home and vehicle purchases. Despite the fact that constrained borrowers exposed to higher loan limits accumulate more student debt, their post-college mortgage and auto loan take-up rates are statistically indistinguishable from those of their unconstrained peers. These borrowers were also no more likely to be delinquency or in collections on other types of debt. Overall, this suggests the increased student borrowing did not have spillover effects on other types of borrowing, which is a key concern in debates over the potential threats to financial stability stemming from student loans.[8]

[7] Credit scores refer to the Equifax Risk Score. For more information on the information used in credit scoring, see Federal Reserve Board (2007) and https://www.experian.com/blogs/ask-experian/do-student-loans-affect-credit-score/.
[8] See, for example, Feiveson et al (2018).

Student debt is an important input to many economic models of educational investment. Our setting offers an additional advantage relative to the existing literature, namely that we can directly study the effects of credit constraints in the United States. The policy variation we use for identification directly increased how much constrained students could borrow, holding constant other factors (e.g., the price of college, dischargability of debt, access to grant aid). This variation allows us to document the comprehensive consequences of increases in student debt that are driven by a relaxation of credit constraints.

Our research contributes to several literatures in economics. First, a small but growing body of empirical research examines the short-run effects of student borrowing using credible experimental or quasi-experimental variation. Findings suggest that increased borrowing due to changes in loan offers, messages, and eligibility leads to increased short-run attainment.[9] Our study looks beyond short-run educational outcomes, linking student-loan generated increases in short-run attainment to improvements in longer-term outcomes including degree receipt, earnings, and post-college financial well-being.[10]

Another branch of the literature examines the effects of student borrowing on labor market and other lifecycle outcomes after college.[11] However, in all of these studies, variation in student

[9] See, for instance, Dunlop 2013; Wiederspan 2016; Solis 2017; Barr, Bird, and Castleman 2019; Denning 2019; Marx and Turner 2019.
[10] While many papers examine one or two of these outcomes, we are unique in our ability to examine all of them.
[11] Increases in student loans have been linked to post-college job choice (Rothstein and Rouse 2011; Gervais and Ziebarth 2019), co-residence with parents (Dettling and Hsu 2018), graduate education (Chakrabarti et al. 2020), and entrepreneurship (Krishnan and Wang 2019). Rothstein and Rouse (2011) identify these effects using variation in "no loan" policies which replace student loans with grant aid in students' financial aid packages at one highly selective school. Krishnan and Wang (2019) study entrepreneurship by comparing borrowers before and after the change in federal law that made student loan debt to no longer be dischargeable in bankruptcy. Dettling and Hsu (2018) show that otherwise-similar borrowers with higher student loan balances and poor payment histories were more likely to move back in with their parents as young adults. Chakrabarti et al. (2020) use within-school, across-cohort variation in undergraduate tuition to identify effects of increased student loan debt on graduate school enrollment. Gervais and Ziebarth (2019) use a regression kink design and data on student borrowers from the early

debt is not driven by changes in student loan access, but rather changes in college prices, grant aid, other sources of funding for college, bankruptcy protections for student loans, or a combination of these factors. In contrast, we examine the effects of loan limit-driven increases in student debt, holding constant these other factors. Thus, we provide the first evidence on effects of student debt due to increased liquidity.[12]

Public and policy makers' interest in the effects of student debt on borrowers' balances sheets has primarily focused on homeownership. The two studies that use credible quasi-experimental variation to examine the effects of student debt on homeownership find opposite signed effects, which likely reflect the source of identifying variation (i.e., changes in tuition prices in the case of Mezza et al. (*forthcoming*) or changes in grant aid and loan limits in the case of Goodman et al. (2018)). In both papers, however, the research designs abstract from changes in human capital investment and their results therefore reflect whether the identifying variation affected student debt through an increase or decrease in liquidity. In contrast, our paper shows that increased loan access can finance constrained students' human capital investment, and as a result, additional borrowing has very little effect on homeownership, at least over the medium run.

Our study suggests that credit constraints are important; increased access to credit while in college leads to increased college completion and higher earnings with little if any negative

1990s, when loan eligibility was, in some cases, determined by EFC. The authors conclude that increased borrowing leads to higher wages, but this finding is sensitive to the specification or time frame considered and the underlying sample is selected based on a potentially endogenous outcome (college graduation).

[12] Goodman et al. (2018) and Denning (2019) analyze effects of a change in statutory annual loan limits that occur when a student become old enough to be considered financially independent (age 24). However, under this policy rule, both federal loan and Pell Grant limits increase. Further, the loan limits we analyze affect a broader, arguably higher interest, population of undergraduates while Goodman et al. (2018) and Denning (2019) can only identify the effect of changes in loan limits and grant aid for students close to completing their undergraduate studies.

effect on creditworthiness later in life. These findings directly inform policy debates concerning future changes in annual statutory loan limits. Over half of all Stafford Loan borrowers borrowed at the statutory limit in recent years and thus, policy changes affecting loan limits would affect millions of U.S. undergraduates.[13] Our estimates, which derive from administrative records from over 100 colleges and universities and a nationally representative sample of undergraduate borrowers, are the first to characterize effects of limit increases on student outcomes. Altogether, our findings suggest, an additional dollar of student loan availability improves borrowers' education and future financial well-being, and has negligible negative spillovers to other financial markets.

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

Student debt has become an increasingly important component of household balance sheets, reflecting rising enrollments and college costs and, in recent years, over half of U.S. undergraduates relied on student loans to help finance their education, with the share growing over time (U.S. Department of Education 2018). The vast majority of these loans were originated under one of two federal lending programs established under Title IV of the Higher Education Act of 1965—the Federal Direct Loan (DL) Program and the (now-defunct) Federal Family Education Loan (FFEL) Program.

[13] More than one-half of Stafford Loan borrowers—or 3.8 and 4.5 million students, respectively—borrowed at the statutory limit in both 2007-08 and 2011-12. For more, see https://nces.ed.gov/pubs2016/2016408.pdf.

Undergraduate Stafford Loans, the main type of federal loan, feature standardized terms and interest rates.[14] Unlike 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: subsidized loans, which are need-based, and unsubsidized loans, which are not. Subsidized loans do not accrue interest while the borrower is attending college on at least a half-time basis and have slightly lower interest rates.

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 a student may receive, as well as eligibility for the federal Pell Grant, but does not affect the overall amount a student can borrow or her unsubsidized loan eligibility. Continuing students must resubmit a FASFA each year.

Important for our research design, statutory annual Stafford Loan limits only vary with academic level and can only be changed by federal legislation. Statutory limits are also higher for students classified as financially independent according to Department of Education guidelines, which are either older students, students who are married, or students with dependents. The difference in the amount that dependent and independent undergraduate students can borrow remained unchanged during the period we examine.

[14] Federal student loan interest rates are set by federal law. Private student loan interest rates are generally higher. Starting in 2013, interest rates were pegged to the rate for the 10-year Treasury note plus 2.05 percent, with a cap of 8.25 percent. Between 2007 and 2012, interest rates ranged from 3.4 to 6.8 percent and were set by Congress each year. 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 prior to 2007.

Only two changes to 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 provided for an increase in 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, generate our identifying variation.

Table 1 summarizes borrowing limits for dependent students by college entry cohort and level. The maximum amount a student could borrow in their first year of college increased from $2625 in the 2007 and earlier academic years to $3500 in 2008 and again to $5500 in 2009. Second year students could borrow $3,500 before 2008, $4500 in 2008, and $6500 in later years. There was no change in the maximum amount upper level students could borrow until 2009 when the $5500 limit was increased to $7500.

Table 2 shows the cumulative effect of these policy changes on aggregate borrowing limits by entry cohorts. Even though increases in loan limits did not occur until 2008, students in the 2006 entry cohort who persisted into their fourth year would have experienced a $2000 increase in their aggregate limit. Students in the 2007 entry cohort who returned for a second year saw a $1000 increase in their loan limits and by their fourth year would have been able to borrow $5000 more than fourth-year students who entered college in 2005. Students who entered

college in 2008 saw a cumulative increase in loan limits of $7875 and those who entered in 2009 and later years saw a $9875 increase.[15]

3. Data and sample

Our analysis uses two separate sources of administrative data: the Texas Education Data and the credit report data. These two datasets act as complements, allowing us to explore an array of outcomes and document important mechanisms linking increased liquidity to longer-run outcomes. Unfortunately, due to the data agreements, we are unable to link these two data sets.

A. Texas Education Data

Our first data set is provided by the Texas Higher Education Coordinating Board (THECB) and is drawn from the population of students who entered a public higher education institution in Texas beginning in 2001.[16] Texas provides a near ideal setting to study the effects of student loans for several reasons. First, Texas is the second largest state in terms of college enrollment, containing 8 percent of all postsecondary students in the United States (US Department of Education 2019). The state's large public college system has 60 public community colleges and 37 public universities which, combined, served more than 1.2 million students in Fall 2018 (Texas Higher Education Coordinating Board 2019). Texas is similar to the country as a whole in terms of undergraduate tuition, household income, educational

[15] As we discuss in the following section, our main analyses focus on students who entered college before the start of the Great Recession (i.e., those who entered in the fall of 2007 or earlier).
[16] We accessed the Texas administrative data through the University of Houston Education Research Center.

appropriations per student, and educational attainment, but has slightly lower graduation rates and a higher fraction of Hispanic enrollees.

The Texas Education Data includes student-level information on college enrollment, credits attempted, GPA, major, degree receipt, and financial aid (grants, loans, and work-study aid).[17] Importantly, we observe whether and how much each student borrows on an annual basis and can distinguish between loans from different sources. Student-level records are then linked to Texas Workforce Commission (TWC) data, which includes quarterly earnings for individuals employed in jobs covered by Unemployment Insurance in Texas.[18]

Our main analysis sample includes first-time college students who entered in the 2001 through 2008 academic years.[19] Our main analyses focus on dependent students. Independent students are subject to higher maximum and, as discussed in Section 5, there are few independent students who appear to be constrained by loan limits. Independent students who are constrained do not appear to respond to increases in loan limits by increasing borrowing.

B. Consumer Credit Panel

Our second data set is the Federal Reserve Bank of New York Consumer Credit Panel ("CCP/Equifax").[20] The CCP/Equifax is an individual-level panel dataset of consumer credit reports obtained from Equifax - one of the three main credit bureaus in the United States. The

[17] 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.
[18] UI records cover employers who pay at least $1500 in gross wages to employees or have at least one employee during twenty different weeks in a calendar year. We winsorize wages at the 99th percentile.
[19] The administrative data contains an indicator for first time in college (FTIC) status; however, this information is incomplete. We define a student's first year in college if they are listed as FTIC when they first appear in the data set. For students who are never flagged as FTIC students, we include those who are classified as a first-year student and not listed as a transfer student when first observed and first enroll after the first year in our dataset (2001). We show that our estimates are robust to limiting our sample to only students listed as FTIC at entry.
[20] Additional information about the dataset, including sampling and methodology, can be found in Lee and van der Klaauw (2010).

data have been collected quarterly since 1999 and consist of five percent random sample of all U.S. consumers with credit histories.[21] 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 the current residence (down to the Census Block), year of birth, and the Equifax risk score (a type of credit score). Additional information on the CCP/Equifax data can be found in Online Appendix A.2.

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 dataset. We assume that the first year of observed borrowing at or below the first-year Stafford Loan limit is the first year the individual attended school.[22] We define academic year (AY) borrowing as loans originated in June through July of the academic year in question and focus on cohorts that began borrowing (and presumably entered college) in the 2004 through 2008 academic years.[23] We omit from our analyses borrowers who were over age 20 at entry in order to ensure we are capturing undergraduate (as opposed to graduate) borrowers.

[21] The sampling frame is based on social security numbers, so that once a consumer establishes a credit history and enters the sample, they remain 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. We cannot differentiate between private and federal student loans in the credit report data. Private student loans represent a very small share of student debt - less than 10 percent in recent years (Baum et al. 2019) - and the vast majority of students that finance their education with private loans also use federal loans (Bureau CFP 2012). Consistent with these national patterns, private student loans make up a negligible share of undergraduate student borrowing in the Texas data.

[22] 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 NCES PowerStats, available at: https://nces.ed.gov/datalab/powerstats).

[23] Prior to 2004, there is incomplete coverage of student loans in our dataset, so the 2003-04 cohort is the first cohort for whom we can reliably observe first year borrowing in the CCP/Equifax data set.

We use the panel structure of the CCP/Equifax data to assemble credit and neighborhood information up to 8 years after a borrower likely entered college.[24] The credit file outcomes we examine include a broad array of information on individuals' balance sheets, such as student loan payment delinquency and balances, credit scores, indicators for having credit cards, auto loans, or a mortgage, and the corresponding balances. In all time periods, 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—like zip code house prices from Zillow and zip code incomes from IRS/SOI records—to examine as outcomes.

C. Identifying constrained borrowers

Our identification strategy compares changes in outcomes for those students who would have been constrained prior to the limit increase to the change in outcomes for students who were unconstrained. To implement this, we must identify those students who would be constrained by the pre-policy change loan limits. Conceptually, constrained students are those who would optimally borrow more if they faced higher loan limits for federal borrowing.

In the years before loan limits were increased, we consider students borrowing an amount exactly equal to the federal limit ($2625 for dependent students) to be 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

[24] 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 removes individuals who pass away over this time period. This process also removes any incorrectly duplicated records which can appear (typically for a limited period of time) when new accounts are opened and have not yet been linked to an existing credit record.

($3500 for dependent students).[25] Unconstrained students are those who borrowed but did not borrow at the maximum pre-policy limit.

Figure 1 shows the raw distribution of first year borrowing by cohort in the Texas (Panel A) and CCP/Equifax data (Panel B). Vertical lines indicate statutory limits. We first show the distribution for 2001 through 2007 entry cohorts in the Texas data and 2004 through 2007 cohorts in CCP/Equifax – years where the dependent limit equaled $2,625. The second panel shows the distribution for the 2008 entry cohort in each data set, when the limit was $3,500. (Both panels also highlight the statutory limit for financially independent student borrowers.) There is clear evidence of bunching at the dependent student limit in both samples.

D. Summary Statistics

Table 3 presents summary statistics for the Texas analysis sample by constrained status and entry cohort, separately for students who initially enrolled in a four-year public institution (Panel A) and those who entered a community college (Panel B).[26] We group entry cohorts by whether they were potentially affected by loan limit increases (those who entered between 2006 and 2008) or entered college early enough that they would not have seen an increase in borrowing limits in their first four years of college (those who entered between 2001 and 2005).

Constrained four-year entrants are more likely to be white and are less likely to be classified as an underrepresented minority (Black, Hispanic, American Indian or Native

[25] Results are quite similar if we do not adjust the prior year limit for inflation. To allow for measurement error, students within $50 of the statutory limit to be counted as constrained. This is primarily because in some cases, the loan amount recorded is net of origination fees

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

Alaskan) than unconstrained four-year entrants. Further, constrained borrowers have more student debt but receive similar levels of federal and state grants. Constrained borrowers also have higher average EFCs, suggesting that they come from families with slightly higher incomes. Although it is possible for students to 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.[27]

4. Empirical Strategy

Our identification strategy relies on a comparison between the outcomes of constrained and unconstrained borrowers who entered college in cohorts that were and were not affected 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 are defining our treatment and control groups based on first-year borrowing behavior. Students who entered after 2005 experienced substantial increases in cumulative loan limits (Table 2). We use this variation in a difference-in-differences framework.[28]

Unfortunately, the second increase in loan limits coincided with the Great Recession. We address concerns about differential selection into college enrollment and into borrowing after

[27] This is partially mechanical as we restrict our sample to students who borrowed at or below the first-year limit in their year of entry. Most students exhaust their federal Stafford Loans before taking-up other student loans, so most borrowers with non-Stafford debt would likely be excluded by this sample restriction.

[28] Lucca, Naduald and Shen (2019) use variation in institutional exposure to loan limit increases (based on institution-wide borrowing prior to the policy change) to examine effects on net tuition. They conclude that increases in loan limits were partially captured by institutions through increased prices. This 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.

the start of the Great Recession by focusing on cohorts that entered college prior to the Great Recession (i.e., 2008 and earlier entry cohorts).[29]

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 college*. This inherently excludes students who were so credit constrained that the 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 upgrade to more selective or more expensive colleges).[30] However, students who are not constrained at college entry may become constrained over time if there are changes in income or expenses. 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 *s*, $Cons_i$ is an indicator for being constrained at entry (i.e., whether a student borrows at the first-year federal Stafford Loan limit in her first year), $\mathbf{X_i}$ is a vector of baseline characteristics (which vary across data sets), δ_c are entry cohort fixed effects, and δ_s are entry school fixed effects (when examining outcomes in the Texas data) or state fixed effects (when examining outcomes in the CCP/Equifax data).[31] The "treatment" of interest is the interaction between belonging to a

[29] Barr and Turner (2013) show that college enrollment increased by roughly 2-3 percentage points as a result of the Great Recession. We present alternative specifications in the appendix that include post Great Recession entry cohorts, or exclude the years directly affected by the recession (2009 – 2011) with broadly similar results.
[30] 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); these results are discussed in the following section.
[31] Models estimated using the CCP data do not include school fixed effects as it is not possible to identify what school a student was enrolled in when she borrowed in this data. When estimating models using the Texas data, baseline characteristics include race (white versus under-represented minority versus other), age at entry, EFC at

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 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 that were unaffected by the increase in loan maxima should be similar.[32]

Our second specification pools the three treated cohorts:

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

This is our preferred specification, since for most outcomes, we cannot reject the hypothesis that the estimated (null) effects of being constrained in the pre-period are equal nor can we reject the hypothesis that the estimated effects of being constrained in the 2006 through 2008 entry cohorts are equal.[33] We present cluster-robust standard errors (clustered by entry institution when using

entry, and gender (male versus female). Baseline characteristics are much more limited in the CCP and for these models, X_i is an indicator for whether the borrower was younger than 19 years old when first observed.

[32] In order to motivate our focus on students who enter college before the Great Recession and to provide as complete a picture as possible, we do not limit our sample to 2008 and earlier entrants when estimating event-study models. It also allows us to be agnostic as to the relationship between increases in loan limits and increases in borrowing by constrained students.

[33] For most outcomes, we cannot reject the hypothesis that estimated (null) effects for untreated cohorts are equal (i.e., $\hat{\gamma}^c$ from equation (1) for $c \in \{2001,2002,2003,2004\}$). Likewise, in most cases we cannot reject the hypothesis of equivalent (non-null) estimated effects for treated cohorts (i.e., $\hat{\gamma}^c$ from equation (1) for $c \in \{2006,2007,2008\}$). The online appendix contains estimates of η^c from the below specification, in which the effect of being constrained is allowed to vary across treated cohorts, and corresponding *p*-values from a test of the hypothesis $\eta^{2006} = \eta^{2007} = \eta^{2008}$:

$$Y_{isc} = \beta_1 Cons_i + \sum_{c=2006}^{2008} \eta^c(\mathbf{1}[Cohort = c] \times Cons_i) + \mathbf{X_i}\boldsymbol{\beta_x} + \delta_c + \delta_s + \epsilon_{isc}$$

the Texas data and state of entry when using the CCP/Equifax) as well as *p*-values from the wild cluster bootstrap where clusters are defined based on entry cohort by constrained status.[34]

Finally, we present estimates from instrumental variables (IV) models that relate student borrowing to later outcomes. In this case, 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., $\mathbf{1}[Cohort \in \{2006,2007,2008\}] \times Cons_i$) serve as excluded instruments in a first stage equation that takes the form of equation (2). The second stage equation is:

$$Y_{isc} = \beta_1 Cons_i + \lambda \widehat{D}_{isc} + \mathbf{X_i}\boldsymbol{\beta_x} + \delta_c + \delta_s + \epsilon_{isc} \qquad (3)$$

Under the assumption that loan limit increases only affect constrained students' outcomes vis-à-vis effects on the amount borrowed, λ will represent the causal effect of student loan debt on student outcomes.

A. Evaluating the key identifying assumptions

We rely on observed borrowing in a student's first year to proxy for whether a student is constrained. If the composition of students who borrow the maximum is changing relative to those borrowing less than the maximum at the same time as the change in borrowing limits in ways that would lead these students to have systematically better or worse outcomes, our approach would not identify causal effects of changes in borrowing limits.

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

To address this concern, we show that observable characteristics of constrained relative to unconstrained students do not change differentially after the expansion of credit limits in the Texas sample.[35] 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, and 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.[36] We find no evidence of differential changes in characteristics for the constrained students relative to the unconstrained students.[37]

Table 4 presents the estimated effects for individual baseline characteristics and the predicted probability of graduation from equation (2). Panel A displays estimates for students who initially enrolled in a four-year public institution in Texas while Panel B shows estimates for community college entrants.[38] For four-year college entrants, we find no significant differences in baseline characteristics including race, gender, age, EFC, or the composite measure. We find some significant differences among community college entrants, which suggest that results pertaining to this population may represent an underestimate of the effect

[35] Unfortunately, outside of age, we do not observe any predetermined characteristics in the CCP data. Thus, we focus on the Texas Education data to test for changes in the relative composition of constrained versus unconstrained students.
[36] We also exclude the vector of baseline characteristics ($\mathbf{X_i}$) from these models; estimates are robust to their inclusion.
[37] Similar figures for individual baseline characteristics can be found in the online appendix.
[38] Pooled estimates can be found in the online appendix.

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.[39]

5. Results

We first document how student borrowing and financing for college changes for constrained students. 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. Borrowing among constrained students in 2005 and earlier entry cohorts is, if anything, trending downwards. There is a significant increase in borrowing among constrained students in cohorts that entered after 2005 who had access to increased loan limits, and these increases are larger for those who were exposed to larger limit increases.[40]

Turning 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 (in $1000) 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 $1800 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. Impacts on constrained community college student

[39] We document changes in other financial aid in the Appendix. Focusing on financial aid in a student's first year, we find no changes in Pell Grant aid, TEXAS grant aid (the largest state aid program), other grant aid, or work study that are significant at the five percent level for four-year college entrants. Overall, we interpret our results to suggest that for this population, loans increased while other financial aid did not.

[40] Online Appendix displays similar estimates for independent students, a group for whom loan limit increases did not appear to have any significant effect on cumulative borrowing. The Appendix shows separate estimates for four-year and community college entrants in the dependent student sample. Loan limit increases have large effects on cumulative borrowing for four-year entrants compared to community college entrants.

entrants follow a similar pattern (Panel B). However, constrained community college students have smaller increases in borrowing, amounting to around $1000 in additional debt. Panel C displays estimates from the CCP/Equifax sample. Effects on cumulative borrowing in this sample are similar in magnitude to the Texas sample with impacts approaching $1800, although the effects appear to grow in the fifth and sixth years after college entry. Importantly, this increase in borrowing can reflect two forces at work: borrowing more in each year of education as well as increased borrowing because of increased reenrollment in later years.[41]

Table 6 incorporates the CCP/Equifax data to examine the effects of expanded loan limits on a different potential source of financing for college—credit cards. We find that when constrained students have access to higher federal loan limits, and as a result borrow more, they are also significantly less likely to have a credit card in their year of entry and in the subsequent year. Credit card holders also carry significantly lower balances over the same period. Specifically, credit card use falls by 4 percentage points (11 percent) in the year of entry and 2 percentage points (4 percent) in the next year, while cardholders' monthly balances fall by $121 (19 percent) and $51 (5 percent) in these years, respectively.[42] Up to six years after entry, constrained students remain significantly less likely to have a credit card.

It is important to note that our first stage results are notable in and of themselves—offering students more student loans substantially increases constrained students' borrowing, all other factors—including tuition costs and grants—held constant. This is prima facie evidence

[41] The Appendix shows that first, increases in cumulative total loans shown in Table 5 are due to increases in Federal Stafford Loans, and second, there are no statistically significant change in state loans. This highlights the fact that variation in borrowing is driven by changes in federal policy rather than (potentially endogenous) changes in the availability of other types of loan aid.

[42] Balances are reported at an arbitrary point in the monthly billing cycle and thus reflect a combination of new spending and revolving debt.

of the existence of binding credit constraints. Having established that increasing borrowing limits leads to increases in borrowing for constrained students, we next investigate what effect this had on human capital accumulation.

A. Effects of loan limit increases on constrained students' 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 also separately examine effects on enrollment in four-year institutions versus community colleges. Panel A shows that 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 than students in earlier entry cohorts. There is no effect of loan limit increases on years spent in four-year institutions for community college entrants. Panel B shows that the increase in years of enrollment for constrained four-year entrants did not come at the expense of a reduction of years of enrollment in community college. Among community college entrants, significant pre-trends 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 affected cohorts 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 8 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 there is no evidence of significant increases in degree receipt.[43]

Table 7 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 4 to 5 percentage points. This leads to an additional 0.15 years of enrollment 5 years after entry (Panel B) and 5.5 more credits attempted (Panel C). Higher federal loan limits also increased the probability that students received any degree within 6 years by 5.2 percentage points; this comes primarily through a 5.8 percentage point increase in the probability of bachelor's degree receipt and 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 suggests that loan limit increases led to a lasting increase in graduation rates rather than simply a retiming of degree receipt.

Community college entrants also experienced increases in educational attainment, although evidence of pre-trends in these outcomes in Figures 4 through 6 lead us to view these estimates with caution. Table 8 shows an increase of approximately 0.16 years of enrollment and

[43] The Online Appendix shows estimated effects on the certificate receipt within 8 years of entry. We find no evidence that loan limit increases had significant effects on this outcome for either four-year or community college entrants.

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.

The effects on borrowing and educational attainment provide direct evidence on the presence of binding constraints for human capital investment for both four-year college entrants and community college students. The smaller increases in degree receipt for community college students may be due to the smaller effect of increases in loan limits on borrowing, as the increase in student loans in this group is around 60 percent the size of the increase among four-year entrants. The lack of an effect on bachelor's degree receipt in this group also provides evidence that higher loan limits did not increase the likelihood that community college students "upgrade" to a four-year degree program.

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 labor and credit market outcomes using the Texas data and the CCP/Equifax data, respectively. We first consider effects on four-year entrants' labor market outcomes; estimates are shown in Table 9. Constrained four-year entrants are significantly less likely to have any earnings in the first two years after initial enrollment, suggesting that additional student loans may allow students to spend less time working while in college. Among students who are employed, earnings are 3 to 5 percent lower, although these effects are only statistically significant one and three years after entry.[44]

[44] Estimates from equation (1) for effects on ln(earnings) and employment in early years are shown in Appendix Figures X through X.

Looking further ahead, we find no evidence that students who gain access to higher loan limits earlier in their college careers are less likely to be working. Because we proxy for labor supply with whether a student has any earnings reported to the state UI system, a differential probability of observing earnings in later years could reflect differences in the likelihood of employment or differences the probability of leaving the state or being self-employed. The fact that we find small, statistically insignificant effects on the likelihood of having earnings in later years suggests that these concerns are not likely to affect our results. Moreover, outcomes in the CCP/Equifax data are not subject to this concern as borrowers can be tracked across states.

Constrained students earn significantly more 6, 7, and 8 after entry, likely due to the increases in human capital accumulation. Earnings rise by 4 to 6 percent over these years. This is again consistent with binding credit constraints preventing students from making beneficial investments in human capital when loan limits are low. For community college entrants (Table 10), we find no significant effects on earnings or employment in later years.

We also consider outcomes on neighborhood quality using the CCP/Equifax data. Table 11 considers the effect of additional borrowing on measures of zip code-level income. Constrained borrowers who gain access to higher loan limits live in neighborhoods with significantly higher average wage income and significantly higher average adjusted gross income (AGI) , suggesting that students who were able to borrow more were able to live in more affluent neighborhoods after college.[45]

[45] They also live in neighborhoods where a significantly higher percentage of residents earn more than $50,000 and $100,000 (Appendix Table X).

We next consider how additional borrowing affects loan repayment (Table 12). While the increases in federal loan limits resulted in constrained students having higher average loan balances -- which could lead to more difficulty making minimum payments -- this group also has higher average human capital. We find that constrained students with higher loan limits are significantly less likely to be delinquent (more than 180 days late) on their student loan payments at any given point in time starting four years after college entry. Eight years after entry, these students are 1.7 percentage points (6 percent) less likely to have ever been delinquent with respect to their student loan payments.

We define student loan default as occurring after 360 days of missed payments.[46] Constrained students are significantly less likely to default on their loans four through eight years after college entry. Eight years after entry, higher loan limits decrease constrained students' likelihood of ever defaulting on a student loan by 2.3 percentage points (11 percent). These results suggest that even with higher student debt, the increase in earnings experienced by constrained students who were able to access higher loan limits outweighed the increase in student loan payments.[47]

Finally, we consider the effect of loan limit increases on other aspects of borrowers' balance sheets (Table 13). Increased borrowing increases constrained borrowers' average credit

[46] 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.

[47] 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 are only able to look at effects on student loan payment difficulties over the short-run, while we can show that the relationship between student debt and repayment persists over a longer period. [47]

scores and significantly reduces the likelihood of having a credit score in the bottom quintile (Panel B).[48] We find no evidence of effects on the likelihood of having any non-student-loan delinquent debt or debt that has been placed in collections, which suggests that the larger student loan balances did not have spillover effects to payment behavior on other types of loans. We also find no effect on the probability of having a mortgage (which is a proxy for homeownership), mortgage size (both the level, and a "pseudo" loan to value ratio, which scales the mortgage size by local house prices), having an auto loan, or auto loan amounts. Altogether, we find no evidence that additional borrowing has adverse effects on constrained students' financial well-being after college.

Our finding of negligible impacts on the likelihood of having a mortgage is particularly interesting because it is often asserted that the rise in student loan debt has decreased homeownership among young adults. We do find not any effect of increased borrowing limits on the probability of having a mortgage between 6 to 8 years after college entry; while it may have an effect in the longer term, it seems more likely that the increase in wages and improvement in student loan repayment and credit scores would actually lead to increased home ownership.

D. Instrumental variables estimates of the effect of student debt

[48] We also find no effect on the likelihood of having a credit score (Appendix Table X). For context, Dobbie et al (forthcoming) find that the removal of a bankruptcy flag – arguably the most derogatory item that can be included in a credit report – increases credit scores by 10 points. Dettling and Hsu (2017) find that every $1 increase in the minimum wage (which raises household wages by $250) leads low-skill workers' credit scores to increase by 1 point. Thus, a 3 point increase measured just 4 years after a traditional student would graduate from college arguably represents an economically sizeable effect on credit scores.

Finally, we present estimates from instrumental variables models (equation (3)) in which the interaction between being constrained and being in a cohort exposed to loan limit increases serves as the excluded instrument for student debt. Under the assumption that loan limit increases only affect constrained students' outcomes vis-à-vis effects on the amount borrowed, the coefficient on amount borrowed will represent the causal effect of student loan debt on student outcomes. Even if this assumption is not met, this scaling allows for comparison with estimated effects of other sources of financial aid or student loans in other settings.

Table 14 presents estimated effects of student debt on attainment and earnings eight years after college entry. A $1000 increase in student debt leads to a 0.07 increase in years enrolled and 3 additional credits earned by constrained four-year entrants (Panel A). These students are 3 percent more likely to earn a bachelor's degree and have 2.5 percent higher earnings.

Among community college entrants (Panel B), $1000 increase in student loan debt results in 0.17 more years of enrollment and 5.4 additional credits earned. Constrained community college entrants are 2.1 percentage points more likely to complete an associate degree per $1000 increase in student debt. While we do not find statistically significant increases in earnings 8 years after college enrollment, these estimates are too imprecise to rule out moderate effects.

Finally, we examine financial outcomes for borrowers in the CCP sample (Table 15). A $1000 increase in student debt results in a significant reduction in student loan delinquencies and defaults, with magnitudes around 1 percentage point and 1.3 percentage points respectively. Students who borrow more have significantly higher credit scores (1.7 points) and

are 1 percentage point less likely to have a bottom quintile credit score, about 20 percent of the estimated effect of removing a bankruptcy flag from one's credit report (Dobbie et al. *forthcoming)*.

We find no evidence that student loan debt has any effect on the probability of having a mortgage. The estimated 95 percent confidence interval rules out effects larger than a 0.3 percentage point reduction in homeownership per $1000 increase in debt, which is substantially smaller than the 1.8 percentage point reduction per $1000 in student debt found by Mezza et al. (*forthcoming*). As mentioned earlier, Mezza et al. (*forthcoming*) look at the effects of student debt that are due to increases in tuition. 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 have any effect on young adults' likelihood of owning a home.

E. Robustness and Alternative Specifications

Estimates using the Texas data are robust to the exclusion of covariates and different sample restrictions (Table 16). Panel A replicates our main estimates. 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). Adding back in school fixed effects results in estimates that are very similar to our main results (Panel C).

The remainder of the specifications vary the sample used for estimation. Estimates are quite similar when we omit the first cohort (Panel D), include non-profit students, and imposes the

same sample restrictions as the CCP sample.[49] In Panel G, we present estimates from a sample that uses a narrower definition of unconstrained borrowers by excluding those who borrow less than half of the Stafford Loan limit (approximately $1300) in their entry year. Panel H uses the sample restrictions from 2001 in all years. In both cases, the results are quite similar.

We also present estimates from a more parametric specification:

$$Y_{isc} = \beta_1 Cons_i + \theta(AggLimExp_c \times Cons_i) + \mathbf{X_i}\boldsymbol{\beta_x} + \delta_c + \delta_s + \epsilon_{isc} \quad (4)$$

Where $AggLimExp_c$ is the aggregate amount available to borrow if a student borrowed the maximum for four consecutive years (Table 1) and $\mathbf{X_i}$ includes the main effect of $AggLimExp_c$. Under the assumption that the relationship between the aggregate limits and borrowing is linear and that increases in statutory aggregate limits only affect students who are constrained at entry, estimates of θ will represent the policy response of increasing borrowing *limits* (rather than borrowing amounts) on student outcomes.

Table 17 presents estimates from this specification, which scales the change in outcomes by a $1000 change in aggregate borrowing limits. For brevity, we present estimates for the combined sample of four-year and community college entrants in the Texas data; results for each group separately can be found in the Online Appendix. Estimates from this specification yield similar conclusions, with the notable exception of log earnings, where we find no statistically significant effect.

6. Conclusion

[49] As discussed in Section 3, we use slightly different sample definitions in 2001 due to it being the first year in our sample. Enrollment and outcomes for students in nonprofit institutions are only observed starting in 2004. The CCP sample is restricted to students who were less than 20 years old at entry and excludes cohorts that entered before 2004.

Much of the recent public discussion surrounding the student borrowing has focused on the potential *negative* impacts of resulting debt burdens, particularly on borrowers' future financial well-being.[50] Our study is able to provide insight not only on the effects of credit limit increases on human capital but also on household finance. We find that increased access to student loans increases human capital accumulation in the United States. Student loans reduce the amount of in-college work and credit card debt. Borrowing by constrained four-year students makes them more likely to (re)enroll in college, graduate from college, have higher earnings, and live in higher-income neighborhoods. We also find that additional student loans are beneficial to household finances. Increased borrowing leads to *reduced* student loan delinquency and default which could appear to be counterintuitive without the effects on human capital accumulation. Increased borrowing had no effect on mortgages, mortgage balances, or other sources of debt.

Our study documents the existence and effects of a classic market failure – binding credit constraints for human capital investment. Further, we can document 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 may be "overborrowing," our results 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.

[50] 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."

In addition to the policy insight gained from this paper, our results are perhaps most 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 loans not only increases borrowing, it increases human capital.

References

Avery, C. & Turner, S. (2012). "Student Loans: Do College Students Borrow Too Much - Or Not Enough?" *Journal of Economic Perspectives*, 26(1), 165–192.

Barr, A. C., Bird, K., & Castleman, B. L. (2019). The effect of reduced student loan borrowing on academic performance and default: Evidence from a loan counseling experiment. EdWorkingPaper: 19-89. Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/gc0r-gv02.

Barrow, L. and Malamud, O. (2015). Is College a Worthwhile Investment? *Annual Review of Economics* 7, 519-555.

Baum, S., Ma, J., Pender, M., & Libassi C.J. (2019). Trends in Student Aid 2019, New York: College Board.

Belzil, C., Maurel, A., & Sidibé, M. (2017). "Estimating the Value of Higher Education Financial Aid: Evidence from a Field Experiment," NBER Working Paper 23641.

Bertrand, M., E. Duflo, and S. Mullainathan. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics*, 119(1): 249–275.

Bettinger, E., Gurantz, O., Kawano, L., Sacerdote, B., & Stevens, M. (2019). "The Long-Run Impacts of Financial Aid: Evidence from California's Cal Grant," *American Economic Journal: Economic Policy*, 11(1), 64-94.

Barr, A., & Turner, S. E. (2013). Expanding enrollments and contracting state budgets: The effect of the Great Recession on higher education. *The ANNALS of the American Academy of Political and Social Science*, *650*(1), 168-193.

Bureau, C. F. P. (2012). *Private student loans. Report to the Senate Committee on Banking, Housing, and Urban Affairs, the Senate Committee on Health, Education, Labor, and Pensions, the House of Representatives Committee on Financial Services, and the House of Representatives Committee on Education and the Workforce*. ERIC Clearinghouse.

Brown, M., Scholz, J.K., and Seshadri, A. (2012). A New Test of Borrowing Constraints for Education. *Review of Economic Studies,* 79(2): 511-538.

Card, D. E. (1999). The Causal Effect of Education on Earnings. *Handbook of Labor Economics,* volume 3A (Ashenfelter, O. and Card, D.E., ed.), 1801-1863.

Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). "Bootstrap-Based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics*, *90*(3), 414-427.

Cameron, S. V. & Taber, C. (2004). "Estimation of Educational Borrowing Constraints Using Returns to Schooling," *Journal of Political Economy*, 112: 132-182.

Carneiro, P. & Heckman, J. J. (2002). "The Evidence on Credit Constraints in Post-Secondary Schooling," *Economic Journal,* 112: 705-734.

Chakrabarti, R., Fos, V., Liberman, A., & Yannelis, C. (2020). "Tuition, Debt, and Human Capital." Federal Reserve Bank of New York Staff Report no. 912.

Denning, J. T. (2019). Born under a lucky star financial aid, college completion, labor supply, and credit constraints. *Journal of Human Resources*, *54*(3), 760-784.

Denning, J. T., & Jones, T. R. (2019). Maxed Out? The Effect of Larger Student Loan Limits on Borrowing and Education Outcomes. *Journal of Human Resources*, 0419-10167R1.

Denning, J. T., Marx, B. M., and Turner, L. J. (2019). "ProPelled: The Effects of Grants on Graduation, Earnings, and Welfare," *American Economic Journal: Applied Economics,* 11(3): 193-224.

Dettling, L. J., & Hsu, J. W. (2017). "Minimum Wages and Consumer Credit: Impacts on Access to Credit and Traditional and High-Cost Borrowing," Finance and Economics Discussion Series 2017-010. Washington: Board of Governors of the Federal Reserve System, https://doi.org/10.17016/FEDS.2017.010r1.

Dettling, L. J., & Hsu, J. W. (2018). Returning to the nest: Debt and parental co-residence among young adults. *Labour Economics*, *54*, 225-236.

Dobbie, W., Goldsmith-Pinkham, P, Mahoney, N., & Song, J. (forthcoming). Bad Credit, No Problem? Credit and Labor Market Consequences of Bad Credit Reports. *Journal of Finance.*

Dunlop, E. (2013). "What do Stafford Loans Actually Buy You? The Effect of Stafford Loan Access on Community College Students," CALDER Working Paper 94.

Federal Reserve Bank of New York. (2019). "Quarterly Report on Household Debt and Credit 2019: Q1," New York, NY: Federal Reserve Bank of New York

Federal Reserve Board. (2007). "Report to the Congress on Credit Scoring and Its Effects on the Availability and Affordability of Credit." Board of Governors Federal Reserve System Report. Available at: https://www.federalreserve.gov/boarddocs/rptcongress/creditscore/creditscore.pdf and Experian (2019) https://www.experian.com/blogs/ask-experian/do-student-loans-affect-credit-score/

Feiveson, L., Mezza, A., and Sommer, K. (2018) "Student Loan Debt and Aggregate Consumption Growth," FEDS Notes. Washington: Board of Governors of the Federal Reserve System, https://doi.org/10.17016/2380-7172.2127.

Fountain, J. H. (2019). "The Postsecondary Undergraduate Population: Student Income and Demographics." Congressional Research Service Report R45686.

Friedman, M. (1955). *The Role of Government in Education.* Rutgers University Press.

Gervais, M. and Ziebarth, N. L. (2019). "Life After Death: Postgraduation Consequences of Federal Student Loans," *Economic Inquiry* 57, 1342-1366.

Goodman, S., Isen, A., & Yannelis, C. (2018). A day late and a dollar short: Liquidity and household formation among student borrowers. FEDS Working Paper No. 2018-025. Available at SSRN: https://ssrn.com/abstract=3187675 or http://dx.doi.org/10.17016/FEDS.2018.025.

Gurgand, M., Lorenceau, A. J., & Mélonio, T. (2011). Student loans: Liquidity constraint and higher education in South Africa. *Agence Française de Développement Working Paper*, (117).

Krishnan, K. and Wang, P. (2019). "The Cost of Financing Education: Can Student Debt Hinder Entrepreneurship?" *Management Science* 65(10), 4522-4554.

Lee, D., & Van der Klaauw, W. (2010). An introduction to the FRBNY consumer credit panel. *FRB of New York Staff Report*, (479).

Lochner, L. J. & Monge-Naranjo, A. (2011). "The Nature of Credit Constraints and Human Capital," *American Economic Review*, 101(6), 2487–2529.

Lucca, D. O., Nadauld, T., & Shen, K. (2019). Credit supply and the rise in college tuition: Evidence from the expansion in federal student aid programs. *The Review of Financial Studies*, *32*(2), 423-466.

Marx, B. M., & Turner, L. J. (2018). Borrowing Trouble? Human Capital Investment with Opt-In Costs and Implications for the Effectiveness of Grant Aid. *American Economic Journal: Applied Economics*, 10(2), 163-201.

Marx, B. M., & Turner, L. J. (2019). Student loan nudges: Experimental evidence on borrowing and educational attainment. *American Economic Journal: Economic Policy*, *11*(2), 108-41.

Mezza, A., Ringo, D., Sherlund, S., & Sommer, K. (2020). Student loans and homeownership. *Journal of Labor Economics*, *38*(1), 215-260.

Rothstein, J., & Rouse, C. E. (2011). Constrained after college: Student loans and early-career occupational choices. *Journal of Public Economics*, *95*(1-2), 149-163.

Solis, A. (2017). Credit access and college enrollment. *Journal of Political Economy*, *125*(2), 562-622.

Stinebrickner, R., & Stinebrickner, T. (2008). The effect of credit constraints on the college drop-out decision: A direct approach using a new panel study. *American Economic Review*, *98*(5), 2163-84.

Texas Higher Education Coordinating Board. (2019). 2019 Texas Public Higher Education Almanac: A Profile of State and Institutional Performance and Characteristics. Available at: http://reportcenter.thecb.state.tx.us/agency-publication/almanac/2019-texas-public-higher-education-almanac/.

U.S. Department of Education. (2018). "Digest of Education Statistics." Washington, DC: National Center for Education Statistics.

U.S. Department of Education. (2019). "Digest of Education Statistics." Washington, DC: National Center for Education Statistics.

Wiederspan, M. (2016) Denying Loan Access: The Student-Level Consequences When Community Colleges Opt Out of the Stafford Loan Program. *Economics of Education Review*, 51, 79–96.

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

Notes: Combined subsidized and unsubsidized Stafford Loan limits. Independent undergraduate students can borrow an additional $4000 ($5000 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 at	Increase in total loan limit relative to 2005 cohort by years since entry		
Entry cohort	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

Notes: 2005 cohort = 2004-05 entry cohort. The 2nd through 4th 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, constrained status, and type of college at entry

	Constrained borrowers		Unconstrained borrowers	
Entry cohort =	*2001-2005*	*2006-2008*	*2001-2005*	*2006-2008*
A. Four-year college entrant				
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
A. Community college entrant				
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

Notes: The sample is limited to 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.

Table 4: Treatment is uncorrelated with baseline student characteristics

	(1) Pred. grad rate	(2) Male	(3) White	(5) URM	(6) Age	(7) EFC
A. Four-year college entrants (N = 74,132)						
Dependent variable mean	0.555	0.457	0.414	0.519	18.08	9026
Constrained × cohort ∈ {2006,2007,2008}	0.006	0.011	0.008	-0.020	0.04	1720
	(0.005)	(0.010)	(0.014)	(0.014)	(0.03)	(1206)
B. Community college entrants (N = 43,122)						
Dependent variable mean	0.321	0.526	0.440	0.536	18.58	6639
Constrained × cohort ∈ {2006,2007,2008}	-0.004	-0.002	-0.011	0.012	0.16	-938
	(0.002)+	(0.009)	(0.010)	(0.010)	(0.16)	(364)*

Notes: The sample is limited to 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, and first entered a four-year college (Panel A) or community college (Panel B). 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. 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. Robust standard errors, clustered by entry institution, 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. Entered 4-year institution (N = 74,132)							
Constrained × cohort ∈ {2006,2007,2008}	0.175	0.740	1.285	1.760	1.819	1.856	1.873
	(0.059)**	(0.23)**	(0.424)**	(0.559)**	(0.635)**	(0.668)**	(0.685)**
	{0.682}	{0.049}	{0.016}	{0.008}	{0.029}	{0.037}	{0.031}
B. Entered community college (N = 43,122)							
Constrained × cohort ∈ {2006,2007,2008}	0.086	0.249	0.547	0.802	1.056	1.118	1.074
	(0.025)**	(0.083)**	(0.127)**	(0.193)**	(0.252)**	(0.294)**	(0.317)**
	{0.800}	{0.600}	{0.268}	{0.123}	{0.047}	{0.027}	{0.035}
C. CCP sample (N = 145,616)							
Dependent variable mean							
Constrained × cohort ∈ {2006,2007,2008}	0.214	0.364	0.718	1.000	1.224	1.761	1.793
	(0.009)**	(0.075)**	(0.150)**	(0.231)**	(0.293)**	(0.394)**	(0.437)**
	{0.359}	{0.490}	{0.215}	{0.171}	{0.158}	{0.114}	{0.149}

Notes: The sample for estimates in Panels A and B is limited to 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 for estimates in Panel C is limited to student borrowers under age 20 that 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. Each column within a panel contains estimates from separate regressions; dependent variable is cumulative total student loans ($1k) 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 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 at entry fixed effects and an indicator for age less than 19 at 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: Effects of loan limits on credit card debt

Years since entry =	0	1	2	3	4	5	6
A. Any credit card (N = 145,616)							
Dependent variable mean	0.378	0.517	0.584	0.615	0.641	0.656	0.675
Constrained × cohort ∈ {2006,2007,2008}	-0.041	-0.022	-0.006	-0.009	-0.017	-0.016	-0.013
	(0.007)**	(0.006)**	(0.006)	(0.007)	(0.007)*	(0.006)*	(0.006)*
	{0.057}	{0.030}	{0.626}	{0.206}	{0.076}	{0.038}	{0.034}
B. Credit card balance(conditional on having 1+ cards)							
Dependent variable mean	$652	$1020	$1396	$1731	$1961	$2125	$2275
Constrained × cohort ∈ {2006,2007,2008}	-121	-51	-56	-19	89	84	41
	(20)**	(23)*	(34)+	(44)	(59)	(82)	(62)
	{0.024}	{0.025}	{0.024}	{0.692}	{0.275}	{0.579}	{0.621}
Observations	55,084	75,230	85,061	89,569	93,345	95,449	98,263
C. Credit card utilization ratio (conditional on having 1+ cards)							
Dependent variable mean	0.485	0.529	0.541	0.533	0.515	0.504	0.489
Constrained × cohort ∈ {2006,2007,2008}	-0.005	-0.014	-0.035	0.001	0.008	0.006	-0.003
	(0.010)	(0.007)*	(0.030)	(0.007)	(0.006)	(0.008)	(0.006)
	{0.755}	{0.559}	{0.328}	{0.905}	{0.385}	{0.326}	{0.519}
Observations	55,084	75,230	85,061	89,569	93,345	95,449	98,263

Notes: The sample is limited to student borrowers under age 20 that 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. 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, state at entry fixed effects, and an indicator for age less than 19 at entry. 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 7: The effect of loan limit increases on constrained students' educational attainment: Four-year entrants

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}

Notes: The sample is limited to student borrowers who first enrolled in a four-year 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 (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 8: The effect of loan limit increases on constrained students' educational attainment: Community college entrants

X =	1	2	3	4	5	6	7	8
A. Enrollment X years later								
Dependent variable mean	0.680	0.504	0.405	0.334	0.259	0.199	0.158	0.131
Constrained × cohort ∈ {2006,2007,2008}	0.050	0.039	0.037	0.033	0.008	0.001	-0.001	0.003
	(0.012)**	(0.011)**	(0.01)**	(0.012)**	(0.011)	(0.009)	(0.006)	(0.008)
	{0.009}	{0.017}	{0.012}	{<0.001}	{0.372}	{0.903}	{0.925}	{0.532}
B. Cumulative years of enrollment								
Dependent variable mean	1.68	2.18	2.59	2.92	3.18	3.38	3.54	3.67
Constrained × cohort ∈ {2006,2007,2008}	0.05	0.09	0.13	0.16	0.17	0.17	0.17	0.17
	(0.01)**	(0.02)**	(0.03)**	(0.04)**	(0.04)**	(0.05)**	(0.05)**	(0.06)**
	{0.010}	{0.011}	{0.010}	{0.008}	{0.004}	{0.001}	{0.006}	{0.004}
C. Cumulative credits attempted								
Dependent variable mean	37.80	47.87	55.84	62.19	66.79	70.14	72.71	74.79
Constrained × cohort ∈ {2006,2007,2008}	2.52	3.58	4.52	5.21	5.39	5.49	5.51	5.58
	(0.59)**	(0.71)**	(0.94)**	(1.15)**	(1.27)**	(1.29)**	(1.29)**	(1.33)**
	{0.056}	{0.027}	{0.016}	{0.027}	{0.030}	{0.027}	{0.041}	{0.023}
D. Any degree or credential								
Dependent variable mean	0.047	0.091	0.133	0.187	0.231	0.261	0.282	0.299
Constrained × cohort ∈ {2006,2007,2008}	0.001	0.006	0.006	0.014	0.012	0.018	0.014	0.017
	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)+	(0.011)	(0.011)
	{0.934}	{0.783}	{0.676}	{0.290}	{0.462}	{0.285}	{0.366}	{0.238}
E. Bachelor's degree								
Dependent variable mean			0.019	0.055	0.094	0.121	0.139	0.153
Constrained × cohort ∈ {2006,2007,2008}			-0.001	0.003	-0.003	0.001	0.0004	-0.0004
			(0.004)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
			{0.920}	{0.814}	{0.765}	{0.896}	{0.971}	{0.962}
F. Associate degree								
Dependent variable mean		0.053	0.079	0.099	0.113	0.124	0.133	0.141
Constrained × cohort ∈ {2006,2007,2008}		0.006	0.010	0.015	0.016	0.020	0.019	0.021
		(0.005)	(0.006)	(0.007)*	(0.008)*	(0.008)*	(0.009)*	(0.008)*
		{0.631}	{0.388}	{0.188}	{0.204}	{0.111}	{0.120}	{0.098}

Notes: The sample is limited to student borrowers who first enrolled in a Texas community college, were classified as dependent students, and borrowed at or below the federal Stafford Loan maximum for first-year students (N = 43,122). 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 9: The effect of loan limit increases on constrained students' labor market outcomes: Four-year entrants

X =	1	2	3	4	5	6	7	8
A. Ln(earnings) X years after entry								
Dependent variable mean	8.595	8.826	9.044	9.414	9.750	9.972	10.124	10.247
Constrained × cohort ∈ {2006,2007,2008}	-0.048	-0.028	-0.043	-0.010	0.010	0.043	0.059	0.046
	(0.025)+	(0.025)	(0.025)+	(0.019)	(0.024)	(0.02)*	(0.016)**	(0.016)**
	{0.020}	{0.394}	{0.049}	{0.533}	{0.630}	{0.016}	{0.056}	{0.097}
Observations	59,802	58,981	59,172	59,513	58,987	58,199	57,686	57,114
B. Any earnings X years after entry								
Dependent variable mean	0.807	0.796	0.798	0.803	0.796	0.785	0.778	0.771
Constrained × cohort ∈ {2006,2007,2008}	-0.019	-0.015	-0.001	-0.008	-0.006	-0.002	-0.005	-0.009
	(0.005)**	(0.006)*	(0.006)	(0.007)	(0.006)	(0.008)	(0.007)	(0.007)
	{0.066}	{0.051}	{0.926}	{0.395}	{0.632}	{0.701}	{0.660}	{0.389}
Observations	74,132	74,132	74,132	74,132	74,132	74,132	74,132	74,132

Notes: The sample is limited to student borrowers who first enrolled in a four-year 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. 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 10: The effect of loan limit increases on constrained students' labor market outcomes: Community college entrants

X =	1	2	3	4	5	6	7	8
A. Ln(earnings) X years after entry								
Dependent variable mean	8.920	9.135	9.307	9.453	9.614	9.757	9.868	9.954
Constrained × cohort ∈ {2006,2007,2008}	0.007	0.003	-0.063	-0.037	0.012	-0.024	-0.021	0.0004
	(0.027)	(0.032)	(0.029)*	(0.028)	(0.031)	(0.026)	(0.027)	(0.031)
	{0.841}	{0.912}	{0.024}	{0.474}	{0.529}	{0.363}	{0.350}	{0.979}
Observations	36,419	35,307	34,488	34,114	33,769	33,405	33,020	32,665
B. Any earnings X years after entry								
Dependent variable mean	0.844	0.818	0.800	0.791	0.783	0.774	0.763	0.757
Constrained × cohort ∈ {2006,2007,2008}	0.012	0.022	0.021	0.010	0.005	0.011	0.013	0.006
	(0.008)	(0.009)*	(0.009)*	(0.01)	(0.01)	(0.011)	(0.009)	(0.008)
	{0.026}	{0.011}	{0.011}	{0.269}	{0.423}	{0.352}	{0.143}	{0.451}
Observations	43,122	43,122	43,122	43,122	43,122	43,122	43,122	43,122

Notes: The sample is limited to student borrowers who first enrolled in a Texas community college, were classified as dependent students, and borrowed at or below the federal Stafford Loan maximum for first-year students. 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 11: The effect of loan limit increases on constrained students' neighborhood quality

X =	4	5	6	7	8
A. Zip code mean wage income					
Dependent variable mean	$42,519	$43,342	$44,496	$45,913	$47,355
Constrained × cohort ∈ {2006,2007,2008}	787	837	731	660	915
	(216)**	(221)**	(238)**	(255)*	(266)**
	{0.029}	{0.010}	{0.018}	{0.064}	{0.027}
B. Zip code mean AGI					
Dependent variable mean	$59,130	$60,668	$63,137	$65,514	$67,817
Constrained × cohort ∈ {2006,2007,2008}	1682	1799	1546	1222	1510
	(364)**	(366)**	(406)**	(444)**	(444)**
	{0.025}	{0.027}	{0.076}	{0.143}	{0.043}

Notes: The sample is limited to student borrowers under age 20 that 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 (N = 145,616). 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, state at entry fixed effects, and an indicator for age less than 19 at entry. 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 12: The effect of loan limit increases on constrained students' student loan repayment outcomes

X =	4	5	6	7	8
A. Currently delinquent					
Dependent variable mean	0.095	0.110	0.118	0.119	0.113
Constrained × cohort ∈ {2006,2007,2008}	-0.015	-0.016	-0.009	-0.010	-0.008
	(0.004)**	(0.004)**	(0.004)*	(0.004)*	(0.005)+
	{0.197}	{0.154}	{0.230}	{0.070}	{0.025}
B. Ever delinquent					
Dependent variable mean	0.161	0.204	0.241	0.271	0.293
Constrained × cohort ∈ {2006,2007,2008}	-0.015	-0.012	-0.015	-0.017	-0.017
	(0.005)**	(0.006)*	(0.006)*	(0.006)*	(0.006)**
	{0.364}	{0.442}	{0.197}	{0.104}	{0.165}
C. Currently in default					
Dependent variable mean	0.067	0.082	0.092	0.095	0.094
Constrained × cohort ∈ {2006,2007,2008}	-0.012	-0.017	-0.009	-0.007	-0.010
	(0.004)**	(0.004)**	(0.004)*	(0.003)*	(0.004)*
	{0.171}	{0.148}	{0.381}	{0.130}	{0.067}
D. Ever in default					
Dependent variable mean	0.094	0.126	0.156	0.182	0.203
Constrained × cohort ∈ {2006,2007,2008}	-0.016	-0.020	-0.020	-0.021	-0.023
	(0.004)**	(0.005)**	(0.005)**	(0.005)**	(0.004)**
	{0.239}	{0.255}	{0.305}	{0.050}	{0.082}

Notes: The sample is limited to student borrowers under age 20 that 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 (N = 145,616). 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 being 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). All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, state at entry fixed effects, and an indicator for age less than 19 at entry. 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 13: The effect of loan limit increases on constrained students' other financial outcomes

	(1) Credit score	(2) Bottom 20% credit score	(3) Any debt delinquent/ collections	(4) Any mortgage	(5) Mortgage amt. (cond.)	(6) Pseduo LTV	(7) Any auto loan	(8) Auto loan amt. (cond.)
A. 6 years after entry								
Dependent variable mean	644.8	0.308	0.103	0.090		0.082	0.356	$12,947
Constrained × cohort ∈ {2006,2007,2008}	1.7	-0.017	0.001	0.001		0.003	0.007	-312
	(1.3)	(0.006)**	(0.005)	(0.004)		(0.003)	(0.005)	(177)+
	{0.294}	{0.085}	{0.866}	{0.815}		{0.592}	{0.611}	{0.243}
Observations	145,616	145,616	145,616	145,616		145,616	145,616	51,874
B. 8 years after entry								
Dependent variable mean	654.9	0.302	0.110	0.155		0.143	0.434	
Constrained × cohort ∈ {2006,2007,2008}	3.1	-0.019	-0.005	0.002		0.002	0.005	
	(1.2)*	(0.006)**	(0.005)	(0.004)		(0.003)	(0.006)	
	{0.321}	{0.036}	{0.198}	{0.675}		{0.723}	{0.680}	
	145,616	145,616	145,616	145,616		145,616	145,616	145,616

Notes: The sample is limited to student borrowers under age 20 that 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. Each column within a panel contains estimates from separate regressions; dependent variable is indicated in the subpanel heading, measured either 6 years after entry (Panel A) or 8 years after entry (Panel B). Credit score = Equifax risk score. Mortgage and auto loan size measured at origination. Pseudo LTV = mortgage size/zip code median house price. All specifications also include an indicator for being constrained at entry, cohort entry year fixed effects, state at entry fixed effects, and an indicator for age less than 19 at entry. 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 14: IV estimates of the effect of student loans on attainment and earnings, 8 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)
A. 4-year college entrants						
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
B. Community college entrants						
Cumulative loans ($1k)	0.165	5.41	0.016	-0.0004	0.021	0.001
	(0.044)*	(1.45)**	(0.009)+	(0.008)	(0.009)*	(0.037)
Observations	43,122	43,121	43,121	43,121	43,121	32,664

Notes: [Add notes]

Table 15: IV estimates of the effect of student loans on financial outcomes, 8 years after college entry

	(1) Ever delinquent (stud. loans)	(2) Ever in default (stud. loans)	(4) Any debt delinquent/ collections	(5) Credit score	(6) Credit score in bottom 20%	(7) Any mortgage	(8) Any auto loan
Cumulative loans ($1k)	-0.010	-0.013	-0.003	1.74	-0.010	0.001	0.003
	(0.004)**	(0.004)**	(0.003)	(0.69)*	(0.004)**	(0.002)	(0.003)

Notes: N = 145,616

Table 16: Robustness of attainment and earnings estimates

	(1) Cum. student loans	(2) Cum. years enrolled	(3) Cum. credits attempted	(4) Any degree	(5) Ln(earnings)
A. Main sample and specification					
Constrained × cohort ∈ {2006,2007,2008}	1.365	0.144	5.68	0.042	0.036
	(0.498)**	(0.029)**	(0.86)**	(0.008)**	(0.014)*
Observations	117,254	117,254	117,254	117,254	89,779
B. No controls					
Constrained × cohort ∈ {2006,2007,2008}	2.233	0.204	7.31	0.043	0.021
	(0.967)*	(0.076)**	(2.66)**	(0.024)+	(0.030)
Observations	117,254	117,254	117,254	117,254	89,779
C. Only school FE					
Constrained × cohort ∈ {2006,2007,2008}	1.310	0.128	5.27	0.041	0.039
	(0.511)*	(0.031)**	(0.93)**	(0.009)**	(0.014)**
Observations	117,254	117,254	117,254	117,254	89,779
D. Omit 2001 cohort					
Constrained × cohort ∈ {2006,2007,2008}	1.550	0.129	5.17	0.040	0.027
	(0.512)**	(0.032)**	(0.87)**	(0.008)**	(0.015)+
Observations	104,753	104,753	104,753	104,753	80,217
E. Include nonprofits					
Constrained × cohort ∈ {2006,2007,2008}	1.367	0.147	5.31	0.043	0.032
	(0.469)**	(0.027)**	(0.82)**	(0.007)**	(0.013)*
Observations	132,031	132,031	132,031	132,031	99,216
F. CCP sample restrictions					
Constrained × cohort ∈ {2006,2007,2008}	2.021	0.092	3.94	0.027	0.021
	(0.579)**	(0.039)*	(1.00)**	(0.008)**	(0.016)
Observations	85,528	85,528	85,528	85,528	65,896
G. Unconstrained borrow $1300+					
Constrained × cohort ∈ {2006,2007,2008}	1.824	0.159	6.39	0.050	0.050
	(0.544)**	(0.037)**	(1.15)**	(0.010)**	(0.017)**
Observations	100,885	100,885	100,885	100,885	77,567
H. 2001 sample restrictions in every year					
Constrained × cohort ∈ {2006,2007,2008}	1.349	0.124	5.14	0.042	0.043
	(0.526)*	(0.031)**	(0.94)**	(0.009)**	(0.014)**
Observations	113,569	113,569	113,569	113,569	87,071

Notes: The sample is limited to 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 (N = 117,254). Each cell within a panel contain estimates from

separate regressions; dependent variable indicated in column heading. All outcomes are measured 8 years after college entry. [Add details for various specifications.]

Table 17: Effects of statutory loan limit increases on attainment, earnings, and financial outcomes

A. Cumulative student loans

	(1) 4 years after entry (Texas)	(2) 6 years after entry (Texas)	(3) 4 years after entry (CCP)	(4) 6 years after entry (CCP)
Constrained x AggLimExp ($1k)	0.261	0.240	0.258	0.374
	(0.051)**	(0.058)**	(0.056)**	(0.075)**
	{0.034}	{0.044}	{0.144}	{0.124}
Observations	117,254	117,254	145,616	145,616

B. Attainment and earnings outcomes, 8 years after entry

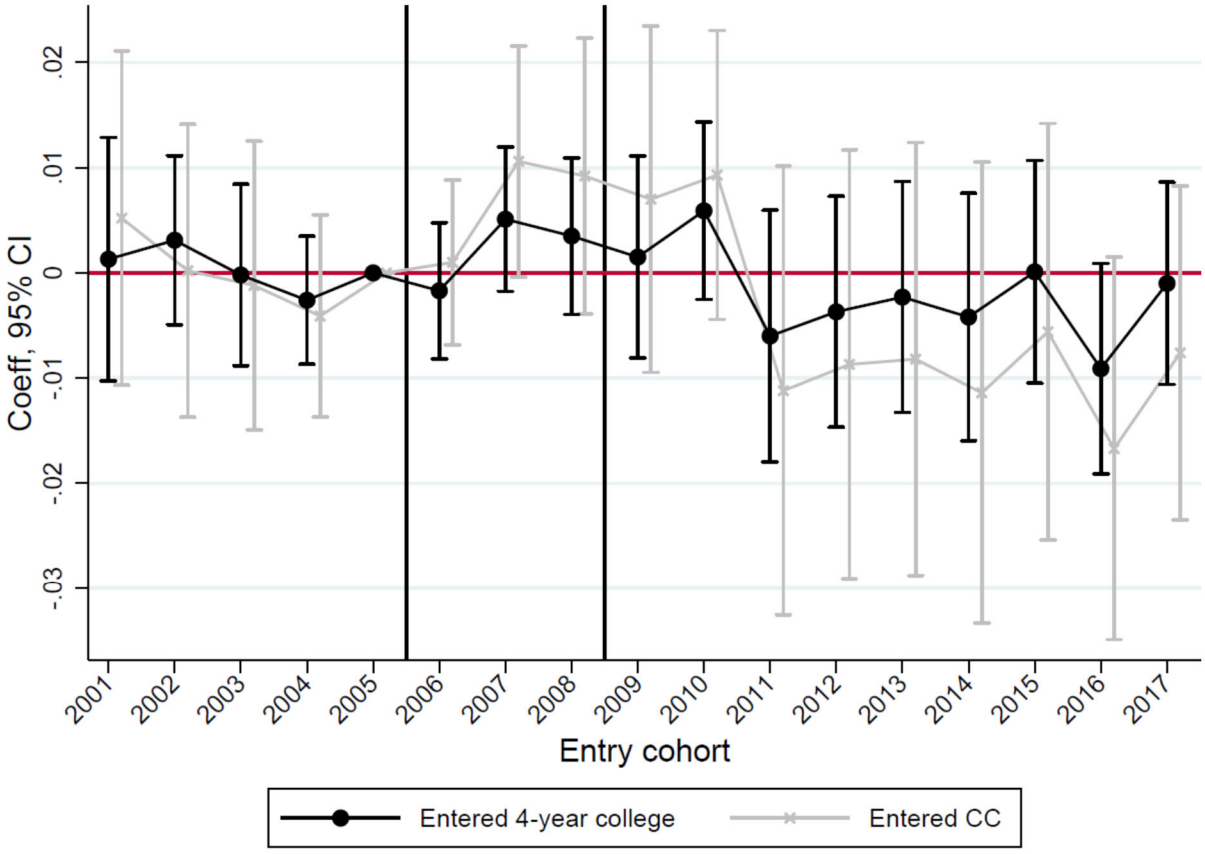
	(5) Cumulative years enrolled	(6) Cumulative credits attempted	(7) Any degree receipt	(8) Ln(earnings)	(9) Zip code mean AGI
Constrained x AggLimExp ($1k)	0.016	0.898	0.005	0.003	242
	(0.007)*	(0.22)**	(0.002)**	(0.003)	(74)**
	{0.124}	{0.048}	{0.105}	{0.538}	{0.097}
Observations	117,254	117,254	117,254	89,779	145,616

C. Financial outcomes, 8 years after entry

	(10) Ever delinquent	(11) Ever in default	(12) Bottom 20% credit score	(13) Any mortgage	(14) Any auto loan
Constrained x AggLimExp ($1k)	-0.003	-0.004	-0.003	0.0004	0.0001
	(0.001)**	(0.001)**	(0.001)**	(0.001)	(0.001)
	{0.303}	{0.152}	{0.153}	{0.560}	{0.943}
Observations	145,616	145,616	145,616	145,616	145,616

Notes:

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



Notes:

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

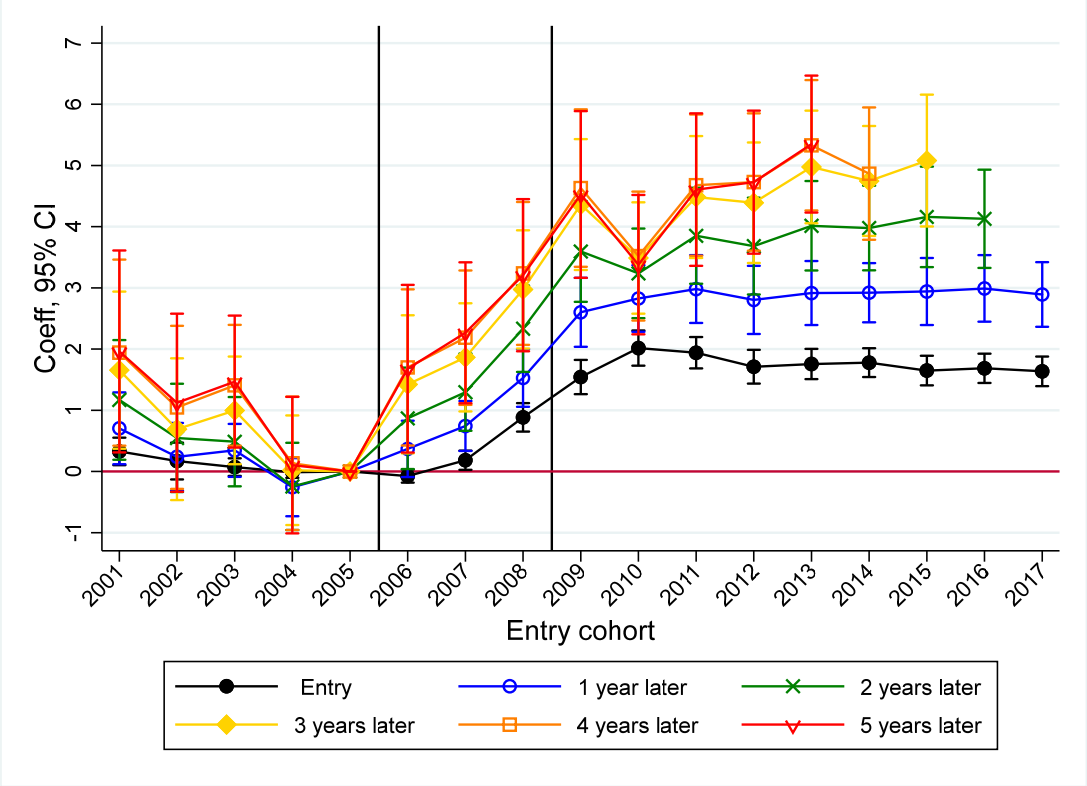
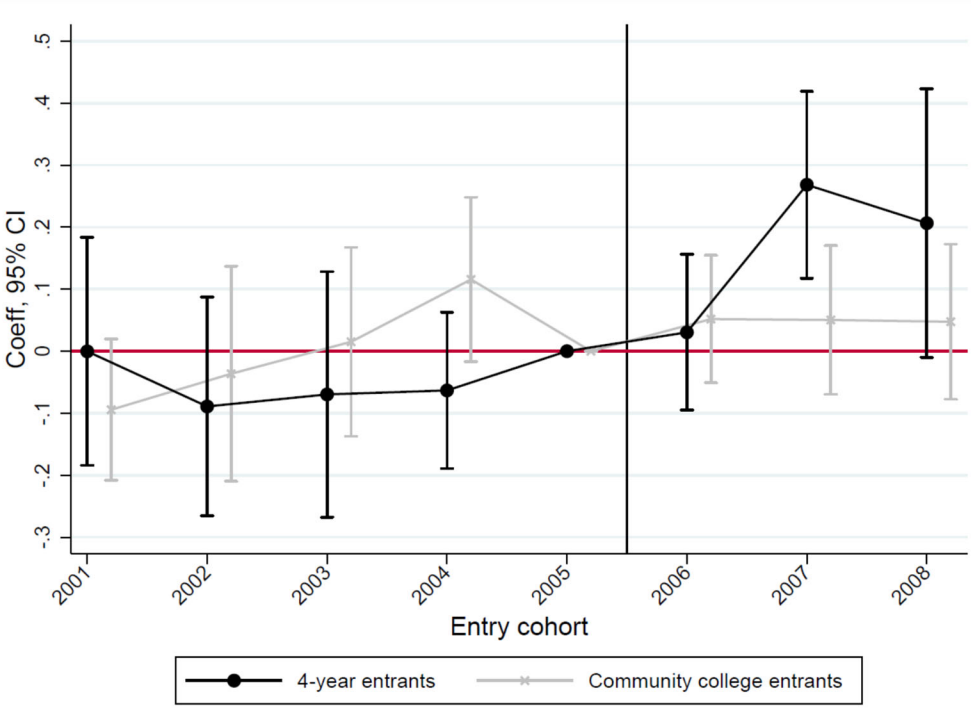
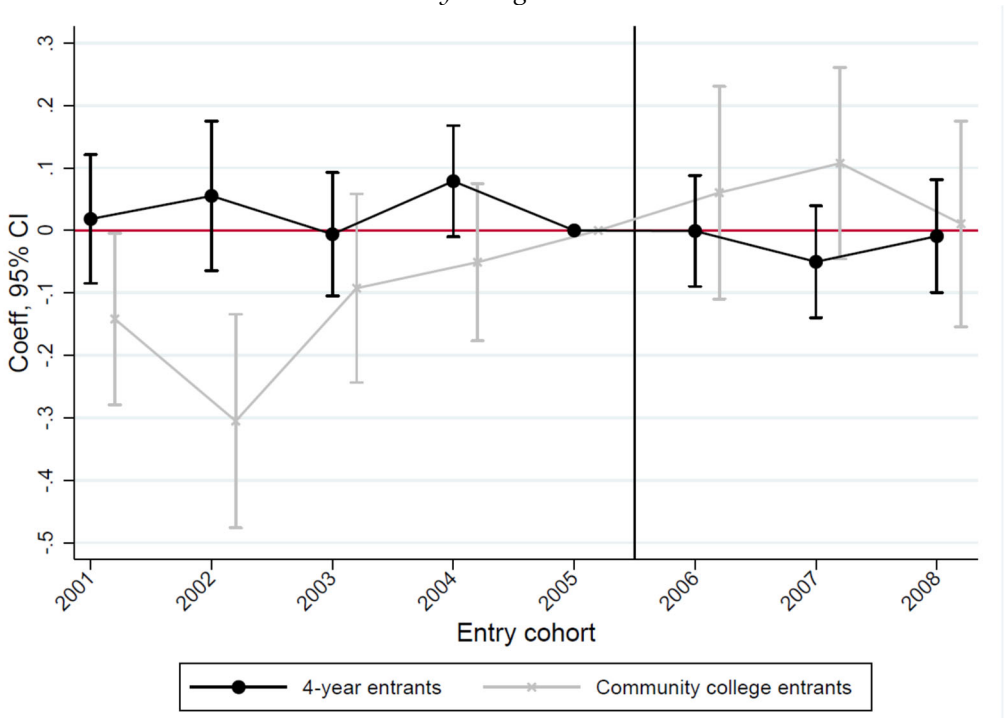


Figure 4: Effects of loan limit increases on cumulative years of enrollment 8 years after entry

A. Four-year college enrollment



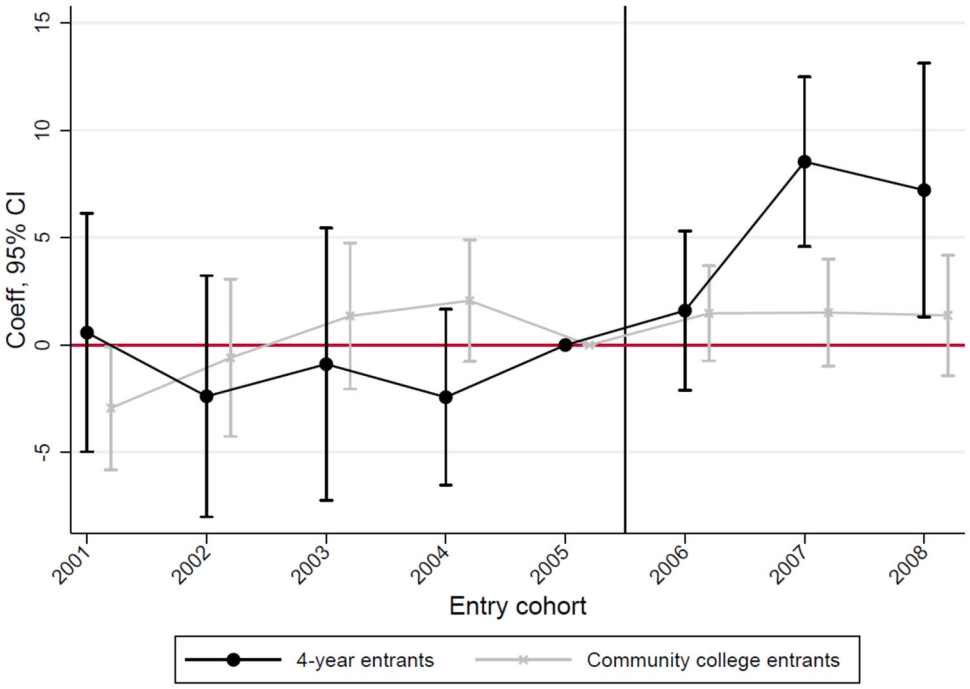
B. Community college enrollment



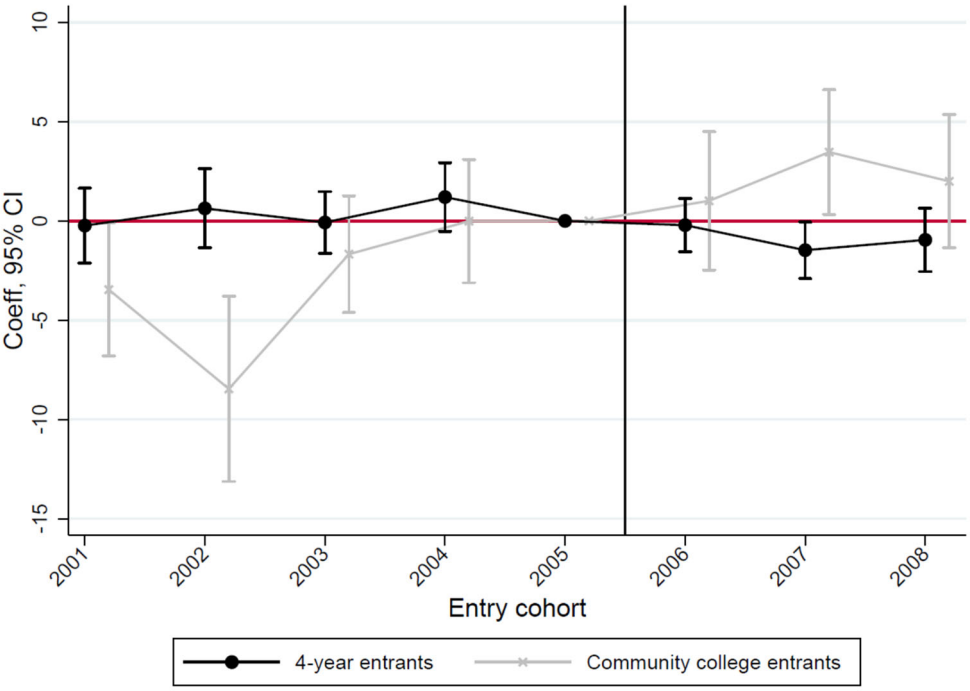
Notes:

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

A. Four-year college credits



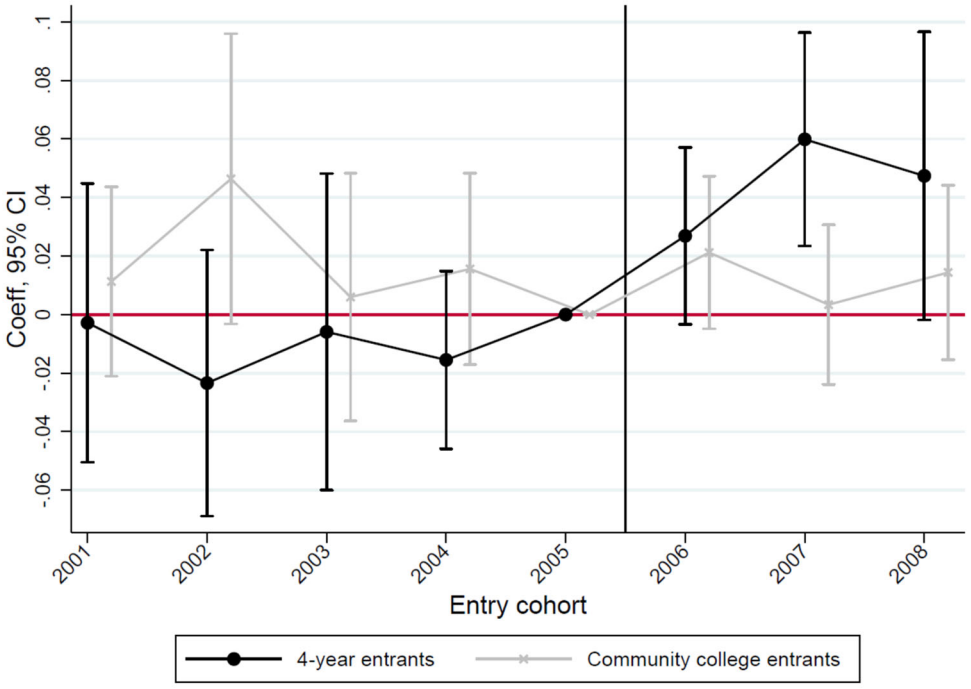
B. Community college credits



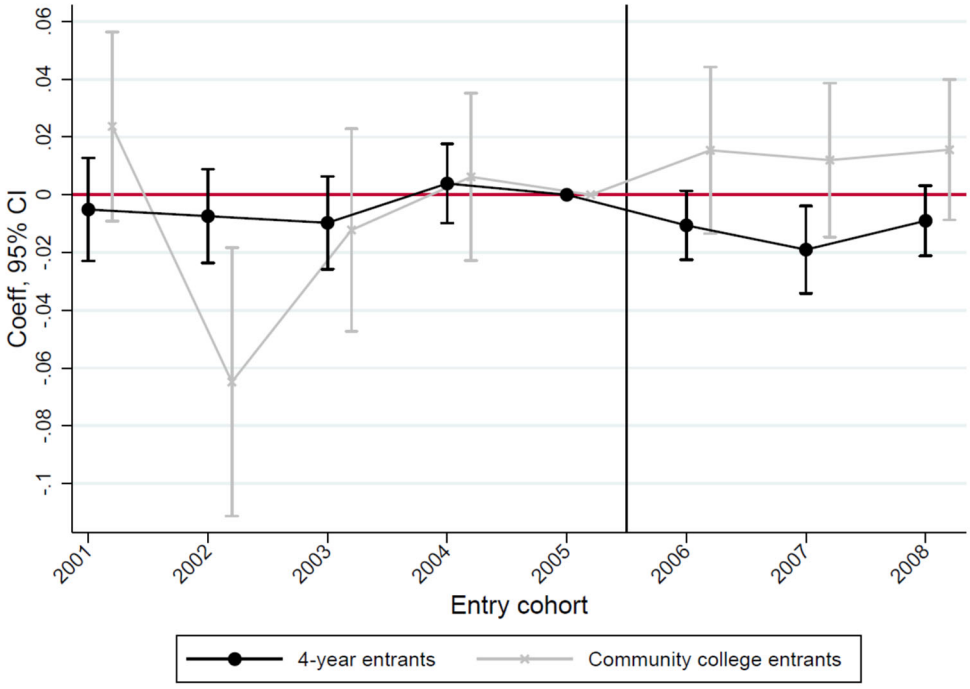
Notes:

Figure 7: Effects of loan limit increases on degree receipt 8 years after entry

A. Bachelor's degree



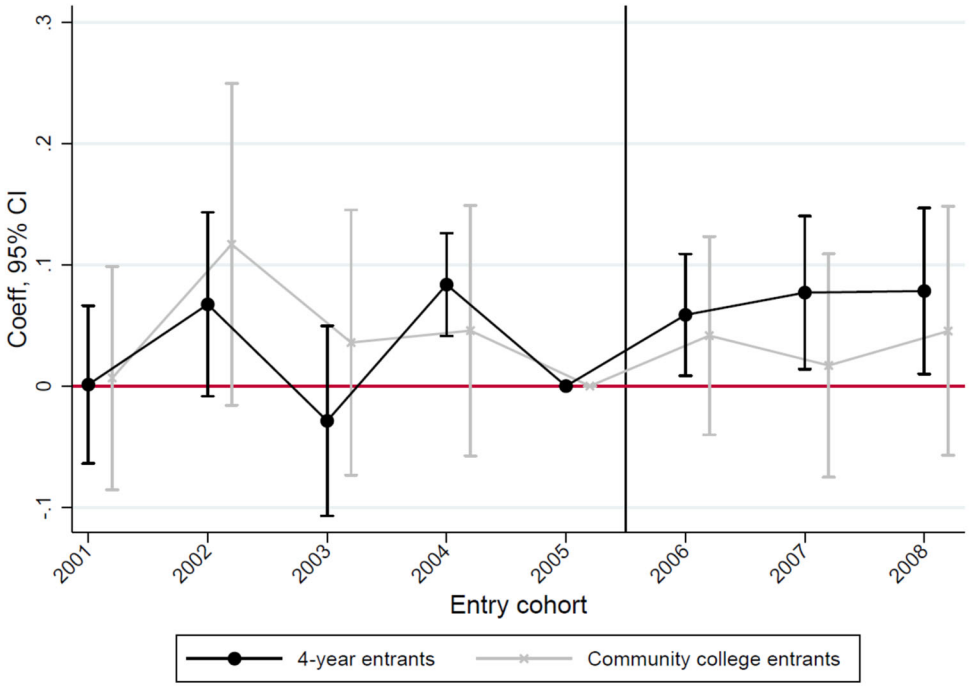
B. Associate degree



Notes:

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

A. Ln(earnings)



B. Any earnings