Debt and Human Capital: Evidence from Student Loans

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Abstract

This paper investigates the effect of debt on human capital. Using data from a random sample of the universe of federal student loan borrowers in the US, we document a negative relationship between the level of undergraduate student debt and graduate school enrollment. We identify off variation in student debt: 1) within school by cohort, and 2) induced by large tuition changes that affect differentially students within the same school across cohorts. We find that $4,000 in higher debt reduces the probability of enrolling in graduate school by 1.3-1.5 percentage points relative to a 12% mean. This effect is largely driven by credit constraints, declines with family income, and is attenuated for students who had compulsory personal finance training in high school. The results highlight an important trade-off associated with debt-financing of human capital, and inform the debate on the effects of the large and increasing stock of student debt in the US.

Keywords: Student Debt, Human Capital, Postgraduate education, Credit constraints, Debt Overhang
JEL codes: D14, H52, H81, J24, I23

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I. Introduction

Student debt is currently the largest non-housing liability for U.S. households. As of the first quarter of 2016, U.S. households held approximately $1.4 trillion in student debt, more than credit card and auto loan liabilities. This amount represents a $1 trillion increase since the first quarter of 2004 (Lee, Van der Klaauw, Haughwout, Brown, and Scally (2014) and Lochner and Monge-Naranjo (2015)). The fast increase in student debt has attracted the interest of policymakers and academics, as large levels of debt may alter students' future consumption and investment decisions. As the White House Council of Economic Advisers (CEA (2016)) noted in 2016, student loans may induce credit constraints after graduation that *"may adversely affect some students' investment decisions and hinder others from successfully managing their debt."*

This paper studies the effect of student debt on the accumulation of human capital. In particular, we ask: does the level of undergraduate student debt affect an individual's propensity to enroll in a postgraduate degree? This effect is particularly important because it implies that the choice of financing of human capital (i.e., undergraduate student debt) distorts individual-level investment decisions and the total human capital that an individual can obtain. The dynamic effects of leverage on investment have long been recognized by the corporate finance literature in the context of physical capital (e.g., Myers (1977), Whited (1992), Albuquerque and Hopenhayn (2004)), and our paper applies this idea to human capital. On one hand, debt could decrease graduate enrollment. For instance, individuals with more debt could face tighter credit constraints in credit markets, which might limit their ability to invest. On the other hand, alternative models imply that debt could increase graduate enrollment. First, higher levels of debt might induce individuals to take up higher paying positions, in order to meet their debt service commitments and these positions may require a graduate degree. Second, higher levels of debt may induce individuals to delay repayment by enrolling in graduate school, and entering deferment and forbearance programs.

Finally, risk-shifting may lead to higher levels of graduate enrollment. When students are relatively close to bankruptcy due to a large debt burden, an option-like payoff structure may give them incentives to over-invest in risky projects with volatile payoffs.

Measuring the impact of student debt on graduate enrollment is particularly challenging for two reasons. First, the appropriate data, which must combine information on student debt with post graduation outcomes, are hard to obtain. For example, many commonly used survey data sources lack long time frames to examine outcomes, have small sample sizes, and are plagued by significant measurement error. We address this challenge by exploiting data from the National Student Loan Data System (NSLDS) that contain all federally guaranteed student loans issued under Title IV of the Higher Education Act of 1965. These data encompass more than 90% of all student debt in the U.S. across all types of schools and degrees from 1970 to 2015.

The second empirical challenge we face is that individuals who take on more student debt may be unobservably different in a way that is correlated with graduate enrollment. Indeed, in the data, the cross sectional relationship between undergraduate student debt and the propensity to enroll in a postgraduate degree is negative, but this relationship could be driven by unobservable omitted variables. For example, individuals who expect their future earnings to be high may be more likely to enroll in graduate school and to finance their undergraduate education with debt, which would bias estimates of the effect of debt on graduate enrollment toward zero. On the other hand, other factors could lead to a spurious negative correlation between student debt and graduate enrollment. For example, individuals who need to use more credit due to adverse family circumstances may be less likely to go to graduate school.

We use two complementary empirical strategies that strongly support a causal interpretation of the negative relation between debt and the propensity to enroll in graduate school. First, we identify off variation in student debt that remains after controlling for school by cohort fixed effects. This specification controls for cross-school heterogeneity in student ability, earnings potential, or family wealth. Moreover, this specification also rules out cross-cohort heterogeneity within the same school, which may be driven, for example, by differences in the business cycle at the time students graduate or by time variation in the quality of education within a school. In a specification saturated with calendar year and school by cohort fixed effects, we find that $4,000 in higher debt at graduation (equivalent to 0.34 standard deviations) reduces the probability that an individual enrolls in graduate school in the following eight years by 1.3 percentage points, an eleven per cent reduction off a baseline 12% probability. The magnitude of the effect decreases monotonically with family income, and is in fact indistinguishable from zero for the highest income quintile. The effect is also concentrated among undergraduates who attend more selective schools, as well as those who attend public and private not-for profit schools. These heterogeneous results suggest that leverage has unequal incidence and welfare implications at the household level, affecting disproportionally relatively poorer individuals with higher ability.

In our second empirical strategy we exploit changes in headline (in-state) tuition as a source of heterogeneity in student debt. At the margin, students may respond to tuition increases by taking on more debt, and indeed, previous work has identified school-level tuition changes as an important determinant of the rise in the aggregate level of student debt (Baum (2015), Mezza, Ringo, Sherlund, and Sommer (2016), Looney and Yannelis (2015a)). However, almost all schools–94% of all school by year pairs in our data–change their tuition on a yearly basis, and these changes are likely to be predictable by students and future applicants. As a result, a comparison of students across schools with and without tuition changes is likely to suffer from a selection bias. Instead, we focus on differences in student debt that arise from large changes in headline tuition at the school level. In our sample, 453 schools changed their headline undergraduate tuition by more than 50% in a given year. Relative to a sample of schools matched on observables, these schools held tuition fixed for at least three years prior to the large change, instead of gradually adjusting it on a yearly basis as the schools in the matched sample do. Together with their low frequency, this suggests that large changes in tuition are difficult to predict by applicants. However, schools that increase tuition are potentially able to attract better students over time who may differ in their level of debt as well as in the propensity to enroll in graduate school. Thus, we identify off variation in the level of debt at graduation for students who are already enrolled in the *same* undergraduate school in *different* cohorts during the year of a large tuition change. This strategy requires the weaker assumption that students enrolled in different years in the same school could not predict the large tuition changes differentially before enrolling.

We validate the power of the identification strategy by showing a negative relation in the level of undergraduate debt and the student's cohort at the time of a large tuition change–the cohort that just finished its first year, the cohort that just finished its second year, and so on. In turn, the reduced form relationship between the probability of enrolling in a graduate degree and a student's cohort at the time of a large tuition change is positive and monotonic. Demographic and financial student-level characteristics that are observable at the time of enrolling in an undergraduate degree such as family income, number of children, and gender do not exhibit any relation with the student's cohort at the time of a large tuition change, which provides support for the conditional independence assumption underlying this empirical strategy. By exploiting a panel of school-year level data, we also rule out that these effects are driven by changes in the quality of education induced by large tuition changes. Using the variation across cohorts at the time of a large tuition change as an instrumental variable for student debt, we find that $4,000 in higher student debt reduces the probability that an individual enrolls in graduate school by about 1.5 percentage points, statistically indistinguishable from the within-school by cohort coefficient.

Two distinct but non-mutually exclusive mechanisms can explain our results. First, individuals with more debt are likely to face tighter credit constraints in both federal and private credit markets, which would limit their ability to invest (Lochner and Monge-Naranjo (2011), Sun and Yannelis (2016)). Indeed, graduate students are likely to borrow to fund their studies: the proportion of graduate students who borrow is larger than the proportion of undergraduate students who borrow, and conditional on borrowing, the amount borrowed is also larger (Baum (2015)). Second, even assuming that a postgraduate degree is a riskless investment with a positive net present value, debt may induce an individual to under-invest in her own human capital because the benefits of the investment are first used to pay back creditors, following the analysis in Myers (1977) for investment by firms.

We perform three tests to differentiate between the two mechanisms and find strong support for the credit constraints channel. First, we consider whether our results vary following increases in the federal student debt borrowing limit. If binding credit constraints drive the negative relation between debt and graduate enrollment, this effect should be smaller in the years immediately following limit increases, when students become less constrained. Supporting our conjecture, we find that the negative effect of student debt on the propensity to attend graduate school is attenuated following federally mandated increases in government student debt caps.[1]

Second, we exploit the 1998 federal law change, which made federal student loans indefinitely non-dischargeable in bankruptcy. Under the under-investment channel, the relation between debt and graduate enrollment should become more negative after the law change. This is because after the policy change, bankruptcy is no longer available to eliminate the impact that student debt payments have on the profits from future investments. However, we find that the law change is not associated with significant changes in the relation between the level of student debt and the probability of attending graduate school. Therefore, this

[1]This result is also inconsistent with an alternative interpretation of our results based on behavioral biases such as debt aversion (e.g., Burdman (2005)). Indeed, under debt aversion, changes to the supply of credit such as increases in student borrowing limits should have no bearing on individual decision to enroll in a postgraduate degree.

result does not support the under-investment channel.

Third, we consider the role of financial education, which has been shown to significantly impact the debt behavior and the supply of credit of young borrowers (Brown, Collins, Schmeiser, and Urban (2014), Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016)). Under the credit constraints channel, the relation between debt and graduate enrollment should be weaker for financially educated students. Consistent with our conjecture, we find that the effect of undergraduate debt on graduate enrollment is strongly attenuated for borrowers who were required to take a financial education course. Moreover, our results suggest that mandatory high-school financial education can mitigate the role of credit constraints in the process of human capital accumulation, aside from more standard and potentially more expensive policies such as increasing federal grants or engaging in ex-post debt forgiveness programs.

Our results suggest that credit constraints matter for accumulation of human capital at the individual level. Although the welfare implications of debt-induced distortions are out of the scope of this project, previous work has shown that, on average, the returns to investments in education are high. In particular, while we cannot rule out that some postgraduate degrees offer consumption value (Lazear (1977)), a postgraduate education is likely to increase individual-level earnings on average (e.g., Avery and Turner (2012)) and to induce positive externalities (e.g., by increasing the supply of educated individuals to conduct research and development).[2] Moreover, we find a negative and monotonic relation between family income and the effect of student debt on the probability of attending graduate school. Since low family income students are least likely to over-invest in education, this further supports the conclusion that large student debt may lead to a sub-optimal level

[2]For example, as of 2015, approximately 90% of all graduate degrees are either Master's or professional degrees, e.g., law or medicine (Source: US Census: http://www.census.gov/hhes/socdemo/education/data/cps/historical/index.html). Further, more than 70% of all Master's degrees are in the areas of Business, Education, Health, Public Administration, and Engineering, areas typically associated with high returns and job opportunities (Source: US Department of Education).

of investment in postgraduate degrees. These distortions suggest a trade-off in the relation between debt-financed education and aggregate levels of human capital, and may thus impact the relation between human capital and income, tax revenue and fiscal balances.

Our paper contributes to several strands of the literature. First, we contribute to the literature that studies the consequences of the large and increasing stock of student liabilities (e.g., see Gicheva et al. (2011), Lochner and Monge-Naranjo (2011), Rothstein and Rouse (2011), Zhang (2013), Cooper and Wang (2014), Mezza, Ringo, Sherlund, and Sommer (2016), and Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016)). A related study is Scott-Clayton and Zafar (2016), which measures the effect of merit-based aid on outcomes that include future earnings and debt. Second, our work contributes to the literature on the returns to education and human capital (e.g., Goldin and Katz (2008), Carneiro, Heckman, and Vytlacil (2011), and Avery and Turner (2012)). Third, our paper contributes to the literature that studies the dynamics of aggregate human capital accumulation (e.g., Galor and Moav (2004), Lochner and Monge-Naranjo (2011), and Cordoba and Ripoll (2013)). Our paper shows that endogenous financing frictions that are induced by the level of debt play an important role in this process of human capital accumulation. Finally, our paper also contributes to the broader literature on the effects of household leverage on consumption and investment decisions, in particular after the 2008 recession (e.g., Mian and Sufi (2015)).

The rest of the paper is organized as follows. In Section II we describe the institutional background for student loans in the U.S. and our data. In Section III we present our empirical tests and results. In Section IV we explore heterogeneous effects to uncover the mechanism that underlies our main result. We conclude in Section V.

II. Student Loan Data

Student loans are currently the largest source of household debt in the United States, save mortgages. Interest rates are set by Congress and generally do not vary for borrowers within the same cohort, degree and loan type.[3] Over 40 million US households have student loan debt, and in 2012 71% of all students took on debt to finance their college education. Approximately 40% of all debt is held by graduate and professional students, who tend to have higher balances (Looney and Yannelis (2015b)). Student loans are almost completely non-dischargeable in bankruptcy as opposed to other types of consumer credit.

The main data source for our study is the National Student Loan Data System, henceforth referred to the NSLDS. The NSLDS is the main database that is used to administer all federal direct and federally guaranteed student loans, which as of 2008 comprise 92% of student loans in the United States. The data comprises billions of loan observations for over 70 million student loan borrowers since 1969, and is used in administrative tasks such as tracking loans disbursed and determining eligibility for different loan and repayment plans, as well as tracking defaulted borrowers and determining eligibility for special repayment plans. The analysis sample for this paper is constructed using a 4% random sample of the NSLDS. The sample is drawn using permutations of the last three digits of a borrower's identification to ensure that the same borrowers can be followed over time.

The NSLDS contains demographic and other data from the Free Application for Federal Student Aid (FAFSA) form, which all students who receive federal student loans are required to fill out. We obtain information from the last FAFSA filed by students. The data does not include information on degrees at the undergraduate or graduate level. Data on state level financial education requirements is obtained from Brown, Collins, Schmeiser, and Urban (2014) and merged to our main data based on students' home states address, obtained from the FAFSA form. We obtain tuition data at the school level from the Integrated Postsecondary Education Data System (IPEDS) operated by the Department of Education.[4] Tuition data is for Title IV eligible institutions' list tuition. Debt, tuition and income variables are winsorized at the 99% level.

[3]See Cox (2016) for a discussion of student loan interest rates.

[4]The borrower match rate is 88%, with match rates increasing over time. In 2008 we match 92% of schools, with coverage above 95% at all institution types except for-profits.

Data on institutional selectivity is obtained from Barron's Profile of American Colleges (2008), which classifies institutions as not competitive, less competitive, competitive, very competitive, highly competitive and most competitive based primarily on the fraction of applicants admitted. The majority of for-profits and community colleges are classified as non-competitive. Borrowers' institutions are identified by the last institution that they attended in the case of enrollment spells as multiple types of institutions. Looney and Yannelis (2015a) provide further information about the NSLDS as well as how variables are recorded from alternative data sources.

We measure graduate student enrollment from the NSLDS. We restrict the sample to borrowers with undergraduate loans who attain a four year undergraduate degree, and thus are eligible to attend postgraduate school. To ensure comparability of borrowers in different cohorts, the main outcome variable is an indicator of whether a borrower enrolls in graduate school within eight years of entry (as of 2007-08, the average time to complete a four year degree was six years and four months). Our analysis sample also includes borrowers who enter into repayment after 1987 and before 2009. After these two restrictions, our analysis dataset includes 265,006 individuals. All dollar figures are measured in 2014 dollars.

Table I displays selected summary statistics for the analysis dataset. We define undergraduate borrowing amounts as the sum of all undergraduate loans outstanding in the final year in which a borrower is enrolled in undergraduate studies.[5] According to the summary stats presented in Table I, 12% of borrowers in our sample enroll in a graduate degree in the eight years following graduation. Average debt at graduation is $18,560. In terms of demographics, 41% of our sample is male, which suggests that females are more likely to borrow among individuals who complete a four year degree. In our sample, 51% of all borrowers are classified as dependent, where dependency status is defined by observable variables such as the student's age or past military status. Finally, family income is $55,000

[5]Entry into borrowing typically occurs in students' first year. We allow borrowers to be in school for up to eight years after initial entry. That is, if borrowers are enrolled and borrowing for more than eight years, they are dropped from the sample.

per year on average. This suggests our main sample is selecting on a relatively poorer segment of the US population, which is consistent with the fact that individuals in our sample borrow to finance their studies.

III. The Effect of Student Debt on Graduate Education

A. Identification within school by cohort

We first measure the relation between post-graduate enrollment and undergraduate debt using a within school by cohort strategy. This strategy controls for any cross-school differences that may lead to similar levels of debt and propensities to enroll in graduate school. It also controls for any heterogeneity across cohorts in the same school, driven, for example, by the business cycle at the time of graduation.

To do so, we estimate the following cross-sectional regression:

$$Postgraduate_i = \beta Debt_i + X_i'\alpha + \gamma_{j(i),c(i)} + u_i, \tag{1}$$

where $Postgraduate_i$ is an indicator of whether student i is enrolled in a postgraduate degree eight years after graduating from her undergraduate degree, $Debt_i$ is the total debt of student i after the final year of undergraduate studies, X_i includes indicator variables for female individuals, for individuals with children, and for individuals who obtained an associate's degree. The $\gamma_{j(i),c(i)}$ term represents fixed effects for school j and cohort c. The coefficient of interest is β, the average relation between the level of undergraduate debt, in units of ten thousand dollars, and the propensity to enroll in graduate school.

In Table II column 1 we present the output of regression (1) where the $\gamma_{j(i),c(i)}$ term represents separate fixed effects for school j and for cohort c (i.e., two fixed effects that are not interacted). The coefficient of interest equals -0.0084, which implies that undergraduate debt is negatively correlated with the propensity to enroll in graduate school. Note that this result controls for the fact that different schools may attract different types of students, and for the fact that cohorts that graduate in different moments in time may face different labor markets. When we include controls in column 2, the magnitude of the estimated effect decreases to approximately -0.03. This reduction in the coefficient is likely driven by the fact that we include a control variable for whether an individual completed an associates degree. Borrowers with an associates degree prior to their bachelors are likely both to have less debt and to have lower academic achievement, and thus are likely to have a lower propensity to attend graduate school.

In column 3 we present the main regression output of the within school by cohort identification strategy, where we replace $\gamma_{j(i),c(i)}$ with graduation cohort c by school j fixed effects. The result suggests that there is a robust negative association between student debt and an individual's propensity to pursue a postgraduate degree within school-cohorts. In economic terms, the coefficient of interest in column 3 implies that \$4,000 in higher student debt is associated with a 1.3 percentage point reduction in the probability of attending graduate school, corresponding to an 11% reduction in the unconditional probability (relative to a baseline of 12%). The coefficient on $Debt_i$ remains negative and significant when we include student-level controls in column 4. The coefficients remain quite similar to the estimates in column 3, suggesting that the factors captured by the controls are also likely absorbed by the fixed effects.

The regression results in Table II are consistent with the notion that student debt deters investments in human capital. The results are strongly statistically significant and are robust to the inclusion of control variables that are likely to correlate with the level of undergraduate debt. Moreover, the economic magnitude of the effect, an 11% reduction in the unconditional probability, is relatively large. Next, we exploit our data to investigate who bears these effects. It is important whether the effects on graduate enrollment are for high or low ability students. Students with higher levels of academic achievement may be more likely to innovate and drive economic growth, and thus the welfare effects are potentially larger if the effects are driven by selective institutions. In Table III we present the output of regression (1) on subsamples of students by undergraduate school type and selectivity. Specifically, panel A breaks down the results by the institution control type, defined as for-profit, public, and private, while panel B breaks down the results by selectivity, as determined by Barron's primarily based on the fraction of students admitted. We group selectivity in three categories: non-selective schools, competitive and very competitive, and highly competitive and most competitive schools.

The regression output presented in panel A of Table III suggests that the association between undergraduate debt and the propensity to enroll in graduate school is largely driven by public and private non-profits schools. Notably, the relation between debt and graduate enrollment is insignificant at for-profit schools (see also Figure A.1 in the Internet Appendix). This result is intuitive, as for-profits tend to enroll students with lower academic achievements, and they may be less likely to have the academic qualifications to pursue graduate studies. We see similar effects by institutional selectivity. The effects are larger for moderately selective institutions in comparison to non-selective institutions, and the effects are strongest for the most selective institutions.

Selective schools tend to be more expensive and charge higher tuition than non-selective institutions (Hoxby (2009)) and students at selective schools may be constrained by large tuition payments and debt burdens. The fact that students at the most selective schools show the largest enrollment effects is consistent with credit constraints (see further discussion in Section IV), and may have important welfare implications. Indeed, welfare losses could be large if high ability students for whom returns to education are likely to be larger are unable to make investments in human capital due to borrowing constraints (Avery and Turner (2012)).

The rich set of fixed effects that we include in specification (1) absorbs time-invariant unobservable factors such as school quality and cohort-specific variation in economic conditions that may drive the relation between student debt and graduate studies (e.g., different cohorts within the same-school that graduate in different stages of the business cycle). However, the negative relation between the probability of enrolling in a graduate degree and student debt may be driven by time-varying student-specific heterogeneity that is unobservable to the econometrician. For example, family income affects educational attainment (Hoxby (1988)) and students with more debt could come from lower income families who are less likely to attend graduate school (a negative omitted variable bias). Alternatively, students with higher expected incomes are more likely to attend graduate school and to take on more debt (a positive omitted variable bias). In the next section we address these concerns through an alternative empirical strategy. This alternative strategy, using school specific tuition changes, results in very similar estimates of the effect of student debt on graduate enrollment.

B. Identification within school across cohorts exposed to large tuition changes

School-level tuition changes are one of the most important determinants of the rise in the level of student debt (Baum (2015)).[6] Most schools change their headline tuition by a small amount every year, and these changes are likely to be expected by students. As a result, it is likely that the pool of student borrowers is determined ex ante by the future changes in tuition. Additionally, these small annual tuition increases will be strongly correlated with temporal trends, and elite school have seen faster tuition growth over time which could bias simple estimates (Stephens and Yang (2014)). Thus, a simple comparison of students exposed to different tuition levels is an inadequate strategy to identify how student debt affects graduate school enrollment.

To avoid these concerns, we implement an empirical strategy that exploits large tuition

[6] In the Internet Appendix Figure **??** we confirm this positive correlation graphically by plotting average debt at graduation relative to tuition changes in $1,000 bins for all students in our sample.

changes as a source of heterogeneity in undergraduate student debt. In our data, 453 Title IV eligible schools changed their tuition by more than 50% in a given year. Universities exposed to these large changes are spread out across control types (113 Public schools, 278 Private not for profit, and 62 Private for profit) and academic years (Internet Appendix Figure A.4 plots the number of schools with large tuition changes by academic year). Some prominent examples of schools that increase tuition by 50% include University of Notre Dame, University of California Santa Barbara, Kansas State University, and University of Phoenix - Puerto Rico. In Section E below we provide more descriptive evidence about the nature of these large tuition changes and of the schools involved.

Students who are affected by the large tuition change are very unlikely to have predicted its timing when they were deciding whether to enroll. If this is true, the composition of the pool of students affected by the shock to tuition is likely to be independent of the shock itself. This assumption is likely to be true given the very low frequency of large tuition changes (there are 453 large tuition changes over a total sample of roughly 7,000 schools over a period of 21 years). Consistent with this assumption, in Section E below we show how universities with large changes in tuition keep their tuition relatively fixed and then do a one-time large change relative to a matched sample of schools that gradually adjust their prices. For example, in Internet Appendix Figure **??** we show an example of gradual tuition changes at New York University, the largest recipient of federal student loans in 2000, and an example of an abrupt tuition change at Arizona State University. This suggests that decisions to enroll are not likely to be affected by differential expectations about the timing of large future tuition changes simply because these changes are very hard to predict.

However, as we show in Section E, schools with large changes are likely to differ from schools without them. Thus, in our empirical strategy we compare the probability of enrolling in graduate school for students who are already enrolled at a school that changes its tuition by more than 50%, but who at the time of the change were in different grades. Intuitively, a large tuition increase after a student's freshman year would increase borrowing requirements by more than the same tuition increase after the student's sophomore year, and by more than the same tuition increase after her junior year. We therefore use the variation in the propensity to borrow following a large school-level tuition change that is induced by student's academic within-degree "grade," which we here denote as cohort (e.g., cohort 1 corresponds to all students who just finished their first year at the time of a tuition increase), as an instrument for the level of undergraduate debt.

We focus on percentage changes in tuition, rather than absolute changes, primarily because using absolute changes will weigh the variation in tuition for expensive private schools that raise tuition each year in a very predictable way (e.g., New York University in Figure **??**), where small year-to-year changes in tuition will qualify as large in absolute value. Moreover, focusing on large absolute changes will leave out of the analysis public schools that tend to have lower tuition and that house the majority of students in the US. We also note that all our results use headline tuition as defined by each university and not the actual tuition paid by students, which is likely to be correlated with other determinants of debt and enrollment in graduate school. For example, if schools raise tuition they may grant tuition reductions to high ability students, who may be more likely to go to graduate school. Using headline tuition–i.e., "sticker price"– avoids potential biases that may arise from this effect.

Formally, we estimate the effect of student debt on the propensity to enroll in a post-graduate degree using the following two-stage least squares regression:

$$(First\ Stage)\ Debt_i = \sum_{c=1}^{5} \pi_c \Delta Tuition_{j(i),t(i)} \times \delta^{c(i)}_{j(i),t(i)} + X_i'\omega_1 + \gamma_{j(i),t(i)} + \delta_{year(i)} + \eta_i, \quad (2)$$

$$(Second\ Stage)\ Postgraduate_i = \alpha + \beta \hat{Debt}_i + X_i'\lambda_1 + \gamma_{j(i),t(i)} + \delta_{year(i)} + \epsilon_i, \quad (3)$$

where $\Delta Tuition_{j(i),t(i)}$ is the change in headline in-state tuition for students in college j in year t (as the change in yearly tuition for year t relative to year $t-1$) for changes larger than 50% relative to the previous tuition level and zero otherwise, $\delta^{c(i)}_{j(i),t(i)}$ are cohort dummies that equal 1 for all students who finished their year c at school j in year $t-1$, and $\gamma_{j(i),t(i)}$ are year of tuition change t by school j fixed effects.[7] We control non-parametrically for differences in the characteristics of students across cohorts by limiting the comparison group within schools that change their tuition to students who belong to the cohorts that are in their first year in the eight years before the tuition change (i.e., students in their first year the year prior to the tuition change, students in their first year two years before the tuition change, and so on, up to and including students in their first year eight years before the tuition change). Moreover, we include sample-wide year dummies $\delta_{year(i)}$ which absorb any underlying trends that affect all students in our sample. The instrumental variables correspond to the interactions of the change in tuition $\Delta Tuition_{j(i),t(i)}$ multiplied with the cohort dummies, π_c. This non-parametric specification gives the most flexibility in estimating the relation between debt and tuition changes across grades. In Internet Appendix Table A.I we restrict the relation between tuition increase across grades and debt to be linear and use this to estimate a similar 2SLS model. We expect the first stage coefficients π_c to be *decreasing* in c (i.e., $\pi_1 > \pi_2 > \pi_3 \ldots$), the reduced form coefficients–linking the average differences in $Postgraduate_i$ across cohorts–to be *increasing* in c, and the second stage coefficient β to be positive.

In our data, a student's school is measured in their final undergraduate year. This induces

[7]For the small number of students exposed to more than one large tuition increase, we use the last one. As a robustness test, we report results of a regression that uses the same specification but with 25% tuition changes. See Section III.D.

error in the measurement of some students in early cohorts at the time of a tuition increase because some students transfer to a different university during their undergraduate studies, and transfers, which are unobservable in our data, almost always occur after the first year. To address this measurement error, we also estimate a grouped version of the first stage regression:

$$(First\ \ Stage)\ \ Debt_i = \sum_{g=1}^{2} \pi_c \Delta Tuition^{g(i)}_{j(i),t(i)} \times \delta^{g(i)}_{j(i),t(i)} + X_i' \omega_1 + \gamma_{j(i),t(i)} + \delta_{year(i)} + \eta_i, \quad (4)$$

where g indicates a group of cohorts. Specifically, we include cohorts one and two in group one ($g = 1$), cohorts three and four in group two ($g = 2$), and cohorts five through eight in the omitted category.

The exclusion restriction we assume is that any difference in the probability of enrolling in graduate school for students in different cohorts at the time of a large tuition increase is only driven by differences in the level of undergraduate debt across these cohorts. In other words, we assume that the terms $\Delta Tuition_{j(i),t(i)} \times \delta^{c(i)}_{j(i),t(i)}$ are conditionally orthogonal to the error term ϵ_i. As an example, consider two hypothetical students, Adam and Eve. When a school implements a large tuition increase in 2004, Eve has just completed her second year and Adam has just completed his third year. Eve was facing two years of higher tuition and Adam was facing one year of higher tuition. The exclusion restriction then states that the only factor that can lead to a difference in the probabilities of enrolling in graduate school for Adam and Eve is the difference in debt when they both finish their undergraduate studies. These differences in debt are in turn driven by the different exposure Adam and Eve have to the tuition increase due to the fact that they are in different grades at the time it occurred. We show evidence consistent with this assumption below.

C. Results

We present the results of our empirical tests in three parts. First, we focus on showing the effect of large changes in school-level tuition on the level of undergraduate student debt across cohorts. Second, we show evidence in support of the identification strategy that allows us to use these large tuition changes across cohorts as an instrumental variable for the level of debt. Finally, we present the reduced form relationship between the cohort at the time of a large tuition change and the probability to enroll in graduate school, as well as the IV estimates that combine the first stage and reduced form.

We start by documenting the effect of large tuition changes on student debt. For completeness, in column 1 of Table IV we start by showing the baseline relation between tuition changes (of all size) and debt. The coefficient implies that a $1,000 increase in tuition leads to $70 higher debt on average, across all individuals in our sample.

In columns 2 and 3 we present the coefficients of interest of the first stage regression (2). These coefficients highlight the key identifying feature of this empirical strategy: the exposure of student debt to the large tuition change depends on the student's cohort. For example, in column 3 we see that a $1,000 tuition increase leads to $150-$180 higher debt for students in cohorts one and two relative to students in cohorts six, seven, and eight (the omitted category). These first stage coefficients lend themselves to a graphical visualization. In Panel A in Figure 1 we show the first stage coefficients for the interactions of student cohort with tuition change (the $\pi_c \times \Delta Tuition_{j(i),t(i)}$ in regression (2) presented in column 2 of Table IV), which represent the differential level of debt for students attending the same undergraduate school in different grades during the year of a large tuition increase. Consistent with the intuition behind our identification strategy, the figure highlights a negative monotonic relationship between the academic grade at the time of a large tuition increase and the level of undergraduate debt for cohorts two and higher. Below we exploit this variation in student debt across cohorts at the time of a tuition increase to identify the effects of debt on graduate school enrollment.

We note that the coefficients for cohorts one and two plotted in Panel A of Figure 1 are not monotonically decreasing. The observed positive change between cohorts one and two is likely due to the measurement error pointed out above: we only observe students' graduation school, and students are most likely to transfer to another school after the first year. Hence, some students who are labeled as facing a tuition increase in their first year would in effect face fewer years of high tuition. In order to mitigate the effect of this measurement error, we estimate the grouped-cohort specification (4). Columns 4 and 5 in Table IV and panel B in Figure 1 show the estimates, which confirm the monotonic effect of tuition changes on the level of student debt across all grouped cohorts. For example, in column 5 we see that a \$1,000 tuition increase leads to \$170 higher debt for students in group one (cohorts one and two) relative to students in group three (cohorts 5-8, the omitted category).

Our identification assumption implies that in the absence of a large tuition change, students attending different cohorts at a school that changes its tuition enroll in graduate school at a similar rate. Although the assumption is not testable, we exploit the richness of our data and provide tests to support its validity. In particular, we estimate the first stage specification (2) replacing the left hand side variable *Debt* with student characteristics observable at the time of entering an undergraduate degree, such that family income, gender, number of children, and having an associate degree. An absence of a monotonic relation between these predetermined student characteristics and exposure to large tuition changes would provide support for the identification assumption. Indeed, the results, reported in Table V, show that predetermined student characteristics do not exhibit a monotonic relation or any relation at all, supporting the validity of our identification assumption. Moreover, it suggests that there is no observable evidence of differential selection across cohorts exposed to the tuition increase. This result supports our conjecture that decisions to enroll are not likely to be affected by differential expectations about the timing of large future tuition changes.

We have shown that students who have just completed earlier grades at the time of a tuition increase end up with relatively higher levels of student debt once they obtain their undergraduate degree, which is the first part of our identification strategy. We now turn to addressing the final part of our identification strategy, which is whether a higher level of student debt leads to changes in the probability of enrolling in graduate school. We plot the reduced form coefficients obtained by replacing the first stage regression (2) into the second stage regression (3) in Figure 2 (the actual coefficients are reported in Internet Appendix Table A.II). The coefficients plotted in Panel A show a positive and monotonic relation between a student's grade at the time of a tuition increase and the probability of attending graduate school. The difference between cohorts one and two is small and, again, likely to be contaminated by measurement error due to the fact that students transfer after their first year. To address this concern, Panel B presents the reduced form coefficients for the grouped cohorts specification, which do exhibit a monotonic relation throughout.

We combine the three parts of our identification strategy – a negative relationship between debt and cohorts at a school with a large tuition change, no difference in pre-determined covariates across cohorts, and a positive monotonic relationship between graduate school enrollment and cohort–to provide an estimate of the causal effect of undergraduate student debt on graduate school enrollment. As per equations (2) (first stage) and (3) (second stage), we instrument for the level of student debt with the interaction of the large tuition changes multiplied by the student's cohort at the time of a large tuition change.[8]

The IV estimates of the coefficient of interest (β) from regression (3) are reported in Table VI. The results in columns 1 and 2 correspond to the baseline regression using all cohorts separately (regression (2)) and the results in columns 3 and 4 correspond to the grouped first stage regression (4). The coefficients reveal a robust negative effect of student debt on

[8]As in any IV strategy, we also assume monotonicity. In this case, monotonicity translates into assuming that no student will end up with less debt when exposed to a higher tuition.

the probability of attending graduate school: a \$4,000 increase in student debt causes a 1.5 to 2.4 percentage point reduction in the probability of attending graduate school in the next eight years. To compare the magnitude of the estimates from the two empirical strategies, we perform a version of Hausman test. The results of these tests are reported in the last two rows of Table VI, and imply that we cannot reject the null that the difference between estimates is zero in all four specifications.

As in the within school by cohort identification strategy, the results are strongly statistically significant and economically important. To get a sense of the magnitude of this effect, note that \$4,000 in higher debt reduces the probability of attending graduate school by 15-20% relative to a 12% mean. The effect is also large relative to other factors that drive the probability of attending graduate school. For instance, women have a one percentage point higher probability of attending graduate school than men and having a child is associated with two percentage points lower probability of attending graduate school.

Our empirical strategy assumes a linear relationship between graduate enrollment and debt. However, we recognize that such a relationship may be inherently non-linear (e.g., debt starts to matter only when it reaches a sufficiently high level). Our empirical strategy does not lend itself well to tease out heterogeneous effects across levels of debt. But we can exploit our instrumental variables and make progress in inferring heterogeneous treatment effects from the first stage and reduced form coefficients. The first stage estimates reported in Table IV show that students in earlier cohorts at the time of a tuition change accumulate more debt. For instance, following a \$1,000 tuition increase, students in cohort 2 accumulate \$177 higher debt relative to students in cohorts six, seven, and eight, 62% higher than students in cohort 3 who accumulate \$109 more debt. The reduced form estimates, shown in Figure 2 and reported in Table A.II, are consistent with a relatively larger impact on graduate school enrollment for students in earlier cohorts–i.e., with a non-linear effect that is larger for students with more debt. Indeed, we find that *Postgraduate* is lower by 2.56% for students in cohort 2, 98% lower than the 1.29% reduction for students in cohort 3 (again, relative to students in cohorts six, seven, and eight). Thus, when we compare the effects on cohorts 2 and 3, we find that a 62% change in student debt leads to a 98% change in *Postgraduate*, suggesting a non-linear relation between graduate enrollment and debt. The relationship is harder to assess for students in higher grades given that coefficients are no longer significant (e.g., comparing cohorts 3 and 4). Given the inherent difficulties in extrapolating from individual coefficients, we interpret these conclusions with caution.

To further support the validity of this empirical strategy, we report the estimates of second stage regressions where we replace *Postgraduate* with predetermined student characteristics. The results are reported in Internet Appendix Table A.III. We find no effects of student debt on family income, gender, the number of children, and likelihood of having an associates degree. The results confirm that the differences in the probabilities of attending graduate school for students in early versus late grade cohorts at the time of a tuition increase are not driven by predetermined student characteristics.

D. Robustness

We perform several tests, reported in Table VII, that underscore the robustness of our results. In columns 1 and 2 we run the within school by cohort test (regression (1)) and include the duration of undergraduate studies as a control variable. This controls for concerns that undergraduate debt may be mechanically correlated with the duration of studies. In this specification, the effect of student debt on graduate studies is essentially unchanged, remaining negative and significant (and of a slightly larger magnitude). Inclusion of duration as a control in the IV specification could result in a bad control problem, as it may be causally affected by changes in tuition (e.g., Angrist and Pischke (2009)).

Second, we augment the within-school by cohort (in columns 3 and 4) and IV specifications (in columns 5 and 6) with state by year fixed effects. These fixed effects absorb any variation that can be related to state-specific business cycles. The results remain qualitatively unchanged. This result addresses the concern that large tuition increases in state schools are symptomatic of deep recessions at the state level, which may induce heterogeneous effects for cohorts graduating in different years irrespective of debt.

In columns 7 and 8 we change the dependent variable to an indicator of whether a borrower enrolls in graduate school within nine years of entering repayment (it is eight years in our main specification). The results are similar to the main specification and remain significant at the 1% level.[9] In Internet Appendix Table A.IV we include the regression output when we change the definition of large tuition changes to 25%, rather than 50% as in our baseline specification. The main results hold, and although the magnitude of the effect is larger it is not statistically distinguishable from the baseline tests. We also show that the results remain when we consider different sub-samples (Internet Appendix Table A.V) and different gender group (Internet Appendix Table A.VI).

E. Large tuition changes

Aside from the causal effect of debt on postgraduate enrollment, two additional interpretations of our IV results, driven by large changes in tuition, remain. The first one is that schools that change their tuition modify their offerings in a way that affects differentially students in earlier and later cohorts. For example, schools that increase their tuition may improve their offerings, inducing students in earlier cohorts to receive a better education that would lead them to a better labor market upon graduation and reduce the probability of enrolling in a postgraduate degree. A second interpretation is that our sample selection criteria induces heterogeneity across cohorts at the time of a large tuition increase that may drive the observed correlations. For example, large tuition changes could affect the decision

[9] In unreported results, we also run the main specification changing the dependent variable to an indicator of whether a borrower enrolls in graduate school within seven years of entering repayment, and the results remain robust.

to take on debt differentially across cohorts. Although we cannot fully rule these concerns out, they are somewhat mitigated to the extent that we do not see a systematic pattern of heterogeneity in observable characteristics across cohorts at the time of tuition changes (Table V). We next use the IPEDS school-year level panel to further investigate these two interpretations.

We obtain data from Delta Project, which constructs a panel from yearly IPEDS files, and allows us to analyze the evolution of school-year level variables (see Lenihan (2012)). We find a matched school for each of the 453 schools that changed tuition by more than 50% based on the minimal Euclidean distance by lagged tuition and lagged total enrollment within the same academic year, state, and control type (private, public, and for-profit). To minimize the effect of missing observations that distort the trend, we restrict the sample of schools to those where tuition is not missing for event years -3 to 3.

In Figure 3 we plot the evolution of average tuition in dollars for schools with a large change and for the matched sample. The figure shows that average tuition is relatively similar across samples before the large tuition change by construction, but it increases relatively smoothly throughout all event years for the matched sample. On the other hand, schools with large changes (gray bars) increase their tuition discontinuously in event year 0, and end up with a relatively higher tuition in the next three years. This suggests that schools go through large tuition changes after holding their tuition constant, instead of gradually adjusting it over time. Panel B in Table I presents descriptive statistics for schools with a large change and for the matched sample. Schools that experience a large tuition change charge lower tuition prior to a large tuition increase and spend less on instruction, research and student services. These schools also have fewer students, higher completion rates, lower percentage of students with loans, higher proportion of non-white students, and lower in-time completion rate. Differences in other outcome variables—such as student-to-faculty ratio, percentage of students with aid, admission rates, share returning students, share female, and

SAT scores—are close to zero.

We next investigate whether tuition changes are correlated with changes in school-level offerings that could affect students in different cohorts differentially. Whereas simple descriptive statistics are informative, this is an important test to address the validity of our identification strategy. Although in our estimation based on large tuition changes we identify off students who were already enrolled at the time of a large tuition increase, the concern remains that selection into our sample, which requires completion and borrowing, could be correlated with exposure to large tuition changes across cohorts. In Figure 4 we repeat the treated and matched sample plots with two school-level expenditure outcomes: expenditure in instruction, and expenditure in research. The plots suggest that schools with large changes seem to spend more than the matched sample, but that this difference does not seem to shift discontinuously after the large change. Moreover, the graphs suggest both types of schools are in different trends. If anything, the large increase in research expenditures after tuition increases would suggest that earlier cohorts, who are exposed to more cumulative spending in research, would have a higher propensity to enroll in a postgraduate degree, which goes against our main finding.

More formally, we run the following regressions at the school i event by year t level,

$$Y_{i,t} = \alpha_{c(i)} + \gamma Large\,change_i + \sum_{t=-3}^{3} \beta Large\,change_i \times \delta_t + \sum_{t=-3}^{3} \delta_t + \omega_\tau + \epsilon_{i,t}, \qquad (5)$$

where $y_{i,t}$ corresponds to several outcomes available in the IPEDS data. The coefficients of interest are the interactions of event time dummies δ_t and *Large change,* a dummy that equals one for schools exposed to large changes and zero for the matched sample. We identify off differences with respect to the matched pair, so we include matched pair fixed effects $\alpha_{c(i)}$, as well as event year (δ_t) and calendar year (ω_τ) fixed effects.

Results are presented in Table VIII. We maximize power to detect any difference by estimating OLS standard errors, without corrections for heteroskedasticity or within-cluster correlation. Because of this fact, we interpret statistical significance with caution. Note that not all school-year variables are populated in the data, which leads to differences in the number of observations. Column 1 of Table VIII replicates Figure 3, and shows that tuition increases by approximately $1,100 following a large tuition change. In columns 2 and 3 we see that the number of individuals who complete any degree and the fraction that take on debt does not change in a statistically significant way following the tuition change. This suggests that any selection effect in our IV strategy that is driven by completion of a 4-year degree or borrowing is likely to be small. We interpret this result with caution, as these variables are averaged within each school from a sample that also include students who registered after the tuition increase, and therefore are likely to be a selected sample relative to the pre-period. In columns 4 through 9 of Table VIII we see that indicators of school-level offerings and selection variables including admission rate, student to faculty ratio, the fraction of white and female students and the 25th percentile of SAT Math scores, do not change differentially across samples after the change in tuition in a statistically significant manner.

Because only 453 schools in our sample change their tuition by more than 50%, it is important to understand if and how these schools differ systematically from others in our sample. This helps in assessing the external validity of our estimates and their relation to an average treatment effect in the population. We therefore investigate level differences between 453 schools that change their tuition by more than 50% and all other schools in our sample. In Internet Appendix Table A.VII, we show the results of a cross sectional regression of variables measured in the academic year before a large tuition change of different school-level outcomes on *Large change,* a dummy that equals one for schools exposed to large changes and zero for all other schools. To compare schools in the same year, we include academic year fixed effects. Relative to schools that do not make large tuition changes, large tuition change schools have lower tuition, fewer students completing their degrees, fewer students taking debt, similar admission rates, fewer students per faculty, higher ratio of non-white students, similar ratio of female, and similar SAT Math scores as other schools in the same year.

Overall, we find that schools that increase tuition by more than 50% have kept it fixed for a number of years. Importantly, these schools do not seem to observably change their behavior in a way that would predict heterogeneous treatments across students in different cohorts in a manner consistent with our results, which lends further support to the identification assumption underlying our empirical strategy. As far as external validity is concerned, we find that schools that increase tuition seem to differ on some selected observables from the full sample but are remarkably similar on others.

IV. Heterogeneity and Mechanisms

In this section we conduct heterogeneity tests and investigate the mechanisms through which student debt may reduce the the probability of attending graduate school. We consider two non-mutually-exclusive mechanisms: the credit constraints mechanism and the under-investment/debt overhang mechanism.

First, a higher level of student debt may increase the cost of debt financing or even lead to exclusion from credit markets. Graduate students who need to borrow to finance their education or other expenses while they study may thus be unable to do so. For instance, large student debt may cause an individual to hit the federal student borrowing lifetime limit, and therefore to finance her graduate school through private, more expensive (non-subsidized) lenders. A larger stock of student debt can also lead to more defaults (Yannelis (2016)), which may impair individual's access to credit and employment (Liberman (2016), Bos, Breza, and Liberman (2016), Cohen-Cole, Herkenhoff, and Phillips (2016)). This is the *credit constraints* channel.

Higher student debt may also lead to under-investment via the debt overhang channel (Myers (1977)). Under this hypothesis, students choose not to undertake positive NPV investments in human capital due to existence of a large stock of non-dischargeable debt, as only part of the benefits from the new project are available to the student (the rest benefits the existing creditor). This is the *under-investment* channel.

On the other hand, students may undertake risky negative NPV investments in human capital due to the existence of large, non-dischargeable debt. This may happen when the investor is able to shift negative cash-flows from the project to the existing creditor. Since our main results clearly suggest that higher levels of student debt leads to lower investment in postgraduate degrees, we can rule out this *risk-shifting* channel.

We start by exploring the role of family income in the relation between student debt and postgraduate studies. Table IX reports estimates of equation (1), the most saturated version of the fixed effects OLS specification, by each family income quintile, and Figure 5 plots the coefficients. We do not estimate the IV coefficient by quintile because the instrument has very little power among high income individuals. Moreover, the difference between the IV and OLS coefficients in our main tests is not statistically significant, as reported above. As usual, the dependent variable is an indicator of whether student i is enrolled in a postgraduate degree eight years after graduating from her undergraduate degree. The results reveal a negative and monotonic relationship between family income and the effect of student debt on the probability of attending graduate school. A \$4,000 increase in student debt is associated with a 1.5 percentage point reduction in the probability of attending graduate school for students from the lowest family income quintile and with a 0.02 percentage point reduction in the probability of attending graduate school for students from the fifth family income quintile. Thus, the association between student debt and the probability of attending graduate school for the highest family income quintile is close to zero, both economically and statistically.

This result suggests an unequal incidence of the effect of student debt on future education, with a stronger effect for students from low income backgrounds. However, this result does not allow us to distinguish between the credit constraints and the under-investment channels. Indeed, family income mitigates the role of external financing in the credit constraints channel, but it also reduces the incentive to under-invest because individuals from high-income families are less likely to be close to bankruptcy.

We suggest three additional tests that allow us to differentiate between the two mechanisms. First, we consider increases in the federal student borrowing limit. Figure 6 shows the time series since 1970 of the median, 75th percentile and 95th percentile of student borrowing. In 1993 and 2007 federal borrowing limits were increased, alleviating borrowing constraints. The figure shows that borrowing increased sharply across the three plotted percentiles following increases in the borrowing limit, with a lag determined by the completion of students exposed to the new borrowing limit. If binding credit constraints are driving the negative correlation between debt and graduate enrollment, then we expect our results to be attenuated in the years immediately after limit increases. That is because, at that time, individuals who want to attend graduate school have higher loan limits and are therefore less affected by credit constraints. We also expect this effect to gradually fade away as inflation in tuition and general goods erodes the real value of the limit increases. Columns 1 and 2 in Table X report the results of our main tests interacting undergraduate debt with *Limit increase*, an indicator that equals one for the two cohorts that are enrolled immediately following the limit increase. We find that following federal student borrowing limit increases, the relationship between the level of student debt and the probability of attending graduate school is attenuated. Overall, this result supports the credit constraint channel.

Next, we use the 1998 change in federal rules concerning the treatment of student debt in personal bankruptcy. Specifically, after 1998 federal student loans became almost completely non-dischargeable in bankruptcy. Prior to 1998, student loans were dischargeable after seven years in repayment.[10] Students could, in principle, default and discharge student

[10] See Yannelis (2016) for a discussion of student loan bankruptcy.

debt, invest in a graduate degree and then enjoy full benefits from the new project. If the under-investment channel holds, the law change is expected to enhance the negative relation between debt and graduate enrollment. This is because after the policy change, bankruptcy is no longer available to eliminate the impact that student debt payments may have on profits from future investments. Columns 3 and 4 in Table X report the results of our main regression test when we interact the level of undergraduate debt (*Debt*) with *Non Dischargeable*, an indicator that equals one when student debt is fully non-dischargeable upon bankruptcy. We find that the law change is not associated with significant changes in the relationship between the level of student debt and the probability of attending graduate school. Therefore, this result does not support the under-investment channel and our results appear consistent with the credit constraints channel.

Last, we consider the role of financial education. Recent studies have shown that financial literacy has significant impacts on the debt behavior of young borrowers (e.g., Lusardi, Mitchell, and Curto (2010); Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016)). Students who take personal finance courses are less likely to be financially constrained for a number of reasons. First, borrowers with higher levels of financial education are more likely to be aware of alternative sources of credit, such as private student loans or home equity loans. Second, borrowers who took personal finance courses are more likely to avoid high interest debt such as credit card debt, which can negatively impact credit scores. Third, borrowers with financial education better understand credit scores and the implications on future borrowing, and thus are more likely to have access to credit. Finally, borrowers who took financial education course are less likely to default (Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016)), and thus are likely to have higher credit scores. Therefore, the credit constraints channel predicts a weaker relation between debt and graduate enrollment for financially educated students. Another possibility is that individuals in states with mandatory financial education are less likely to increase their debt following tuition increases.

In unreported results, we find no difference in the first stage coefficients among states that require financial education.

To study the interaction between the impact of debt on graduate enrollment and financial education we use data from Brown, Collins, Schmeiser, and Urban (2014) on state personal finance mandates for high school graduation. Columns 5 and 6 in Table X report the results of our main regression where we interact undergraduate debt with an indicator of whether an individual was required to take a personal finance course in the year that they graduate high school.[11] The results indicate that the effect of undergraduate debt on graduate enrollment is strongly attenuated for borrowers who were required to take a financial education course. This is consistent with the earlier evidence that credit constraints impact graduate enrollment, and that undergraduate debt affects graduate enrollment through a credit constraints channel.

We additionally can rule out two alternative mechanisms. First, student debt may increase the probability of attending graduate school if a student wants to postpone repayment of undergraduate student debt. This channel is inconsistent with our baseline results. Second, higher tuition may also cause lower lifetime wealth, which could induce less investment in education. However, the change in wealth is too small to explain our results given the magnitude of lifetime net present value of earnings across the distribution (see Avery and Turner (2012) for estimates of the distribution of lifetime earnings across education levels). A third possibility is that debt may also affect an individual's performance at school, causally reducing the probability that the student is accepted as a postgraduate student (e.g., Mullainathan and Shafir (2013)). Although the opposite effect is also possible (i.e., students with more debt may become more focused and become better students), we cannot test this channel because we have no information on courses taken or grades.

[11]State of residence is obtained from the last FAFSA form that a student filed. We assume that the student lived in this state at age 18, and the indicator measured whether students were required to take a personal finance course in the state of residence at 18 using data from Brown, Collins, Schmeiser, and Urban (2014). The list of states and the year in which the requirement was enacted are presented in Internet Appendix Table A.VIII.

To summarize, the results on borrowing limit increases, treatment of student debt in default, and financial education strongly support the credit constraints channel. We thus conclude that our baseline results are best explained by this mechanism.

V. Conclusion

In this paper we document that increased student debt causes individuals to forgo graduate school. Our results suggest that this effect arises because student debt exacerbates credit constraints, which restrict individuals' choice set in terms of feasible investments in human capital. The results are unequally distributed and affect lower income students who attend more selective schools disproportionately more. Moreover, more financial education and increases to the federal loan limit seem to alleviate these credit constraints.

Our results have two important implications. First, our results suggest that policymakers and academics should recognize that the choice of financing of investments in human capital with debt is not innocuous, and may reduce the total level of human capital relative to alternatives that do not tighten credit constraints. Second, our results speak to an unintended consequence of the fast and large increase in student debt in the U.S. during the past 10 years. While we do not intend to explain the entire time-series variation in graduate enrollment, we show evidence that is consistent with an aggregate effect on human capital in Internet Appendix Figure A.5, where we plot the evolution of undergraduate student debt and the number of graduate students. Indeed, the change in the slope of the level of debt post 2009 and the flattening slope in graduate school enrollment are consistent with our main result. While this increase in debt may have important future consumption effects, the effects that we document on investments in education may have first order implications in reducing the future supply of highly educated individuals to areas such as research and development and health. Future work should address the aggregate implications of increased student loan debt on human capital.

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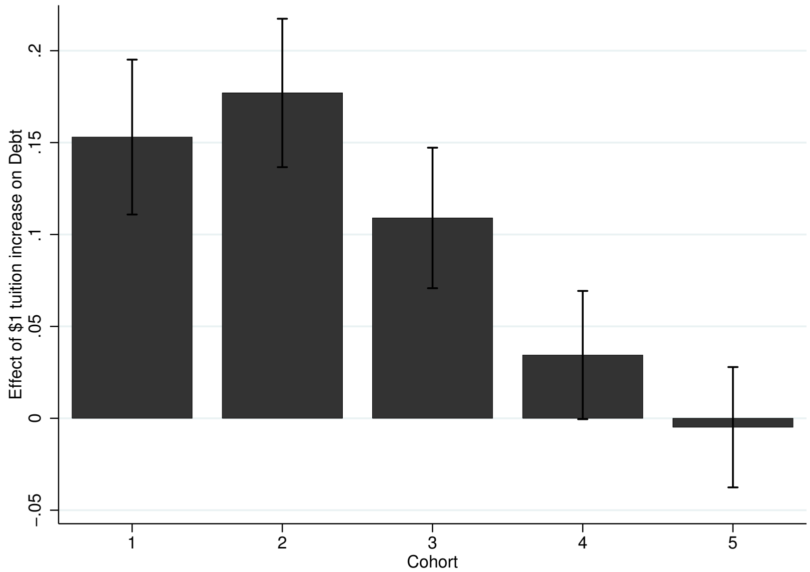
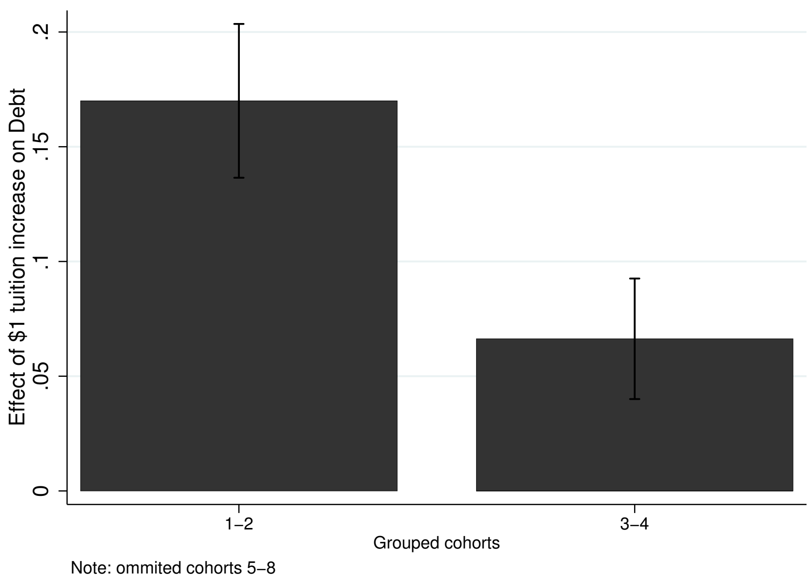
Figure 1: First stage estimates

This figure shows the effect of a $1 increase in tuition on the level of total undergraduate debt among students in different cohorts at the time of a large tuition increase in the same school. In panel A, the bars plot π_c coefficients of the first stage regression (2). Vertical lines plot 95% confidence intervals. In panel B, bars plot estimates from grouped first stage specification (4).



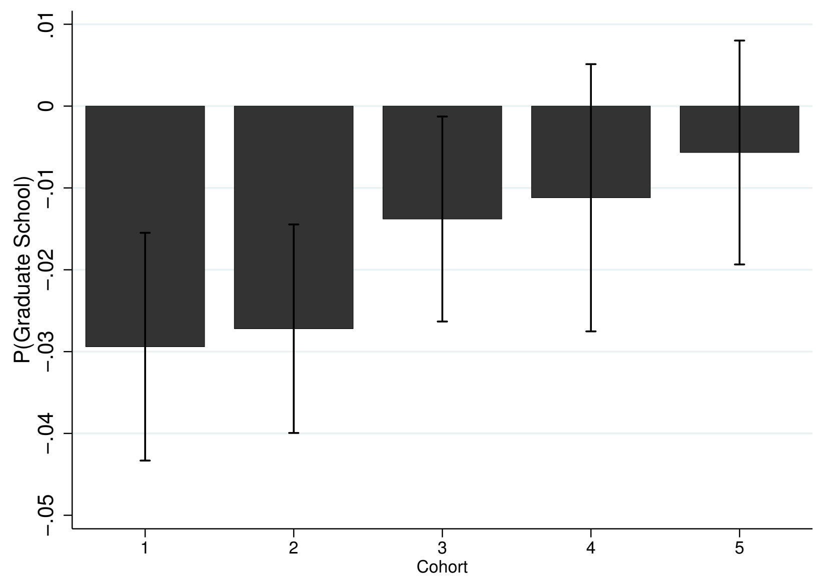
Panel A: Basic specification



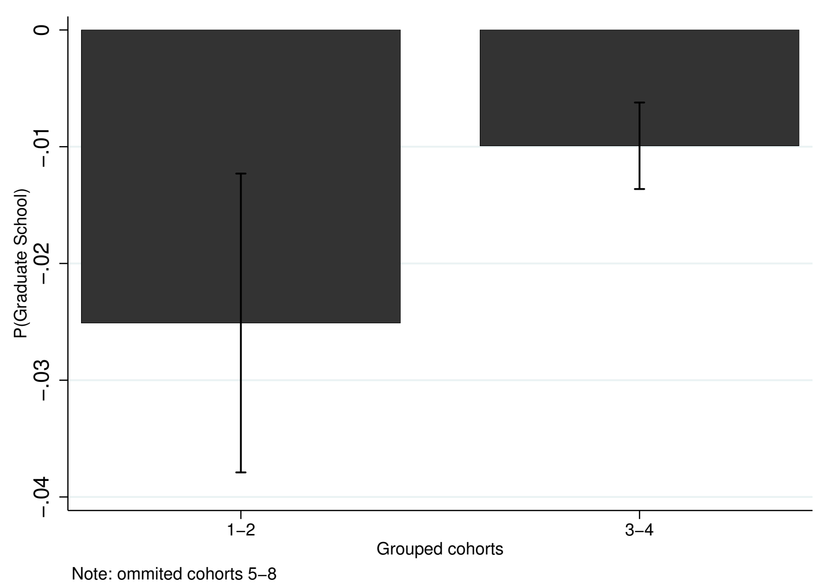
Panel B: Grouped specification

Figure 2: Reduced form estimates

This figure plots the coefficients from the reduced form specification of our IV empirical strategy. Panel A plots estimated coefficients π_c from the following specification: $Postgraduate_i = \sum_{c=1}^{5} \pi_c \Delta Tuition_{j(i),t(i)} \times \delta^{c(i)}_{j(i),t(i)} + X_i' \omega_1 + \gamma_{j(i),t(i)} + \delta_{year(i)} + \eta_i$. The coefficients show the effect of a $1 increase in tuition on the probability of enrolling in graduate school among students in cohort c at the time of a large tuition increase in the same school. Vertical lines plot 95% confidence intervals. In panel B, bars plot reduced form estimates from the grouped cohort specification.



Panel A: Basic specification



Panel B: Grouped specification

Figure 3: Large changes in tuition, matched sample

This figure shows the average tuition by event year centered at the time of a large tuition change for schools that change tuition and for a sample matched on the minimal Euclidean distance by lagged tuition and lagged enrollment within academic year, state, and control type.

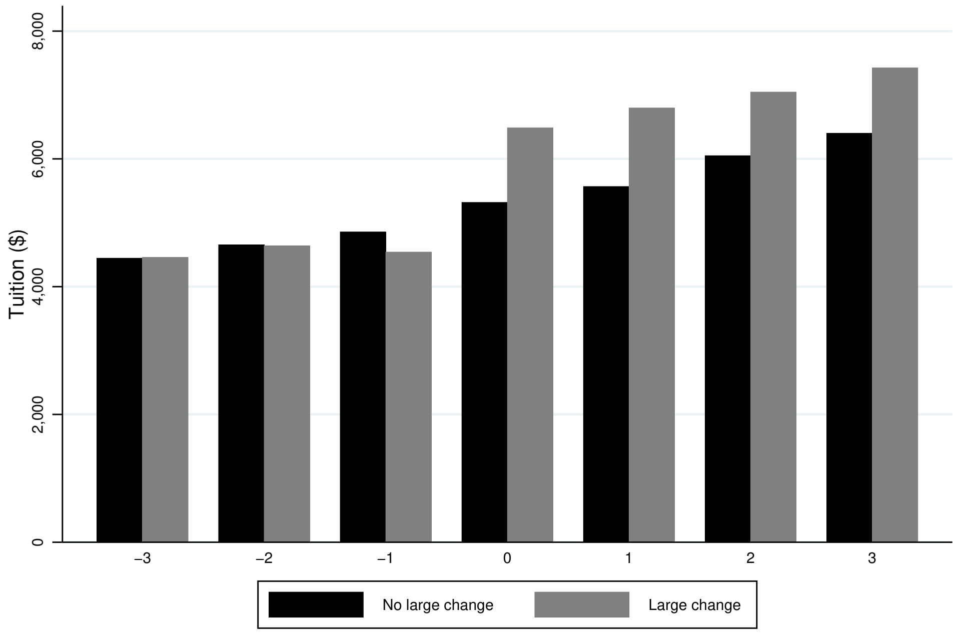
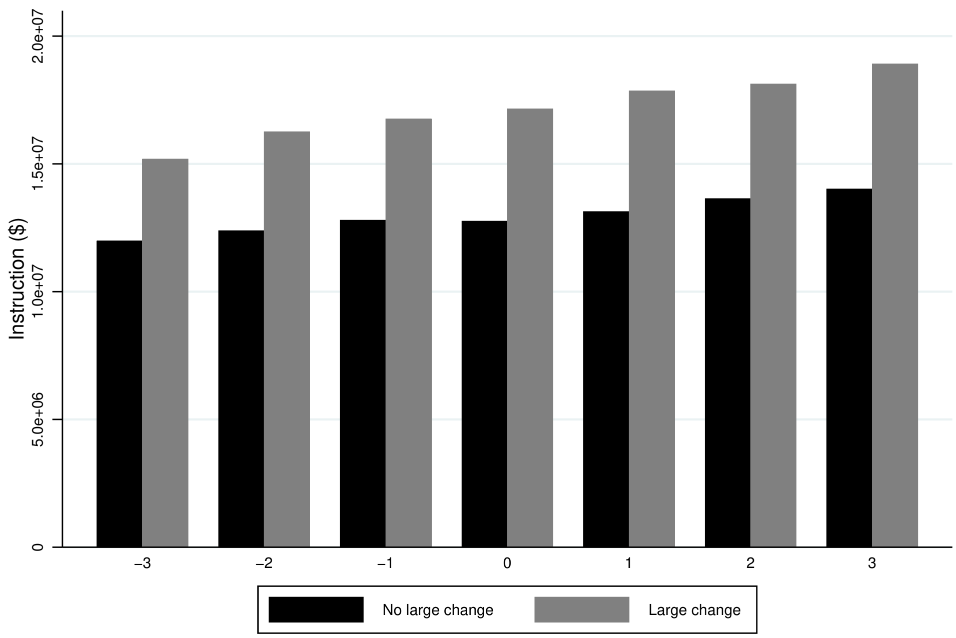


Figure 4: Matched sample: large changes in tuition and expenditures

This figure shows average expenditures in instruction and research by event year centered at the time of a large tuition change for schools that change tuition and for a sample matched on the minimal Euclidean distance by lagged tuition and lagged enrollment within academic year, state, and control type.



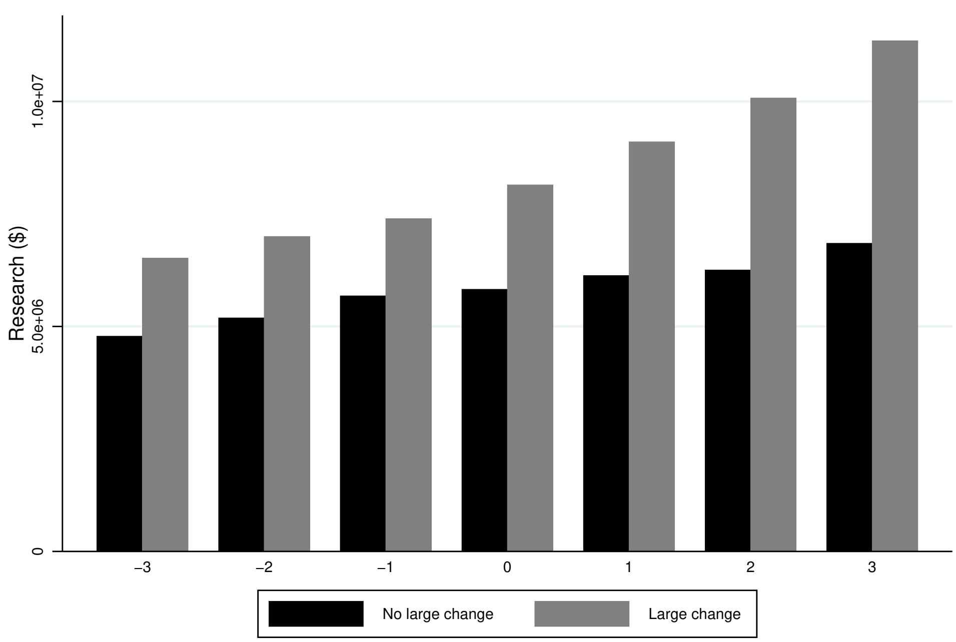


Figure 5: Student debt and graduate studies: the role of family income

This figure plots estimated coefficients β of equation (1) for five family income quintiles. The coefficients show the effect of a \$10,000 increase in student debt on the probability of being enrolled in a postgraduate degree within eight years after graduating from undergraduate degree. Regressions include graduation cohort by school fixed effect and student-level control variables. Vertical lines plot 5% confidence intervals.

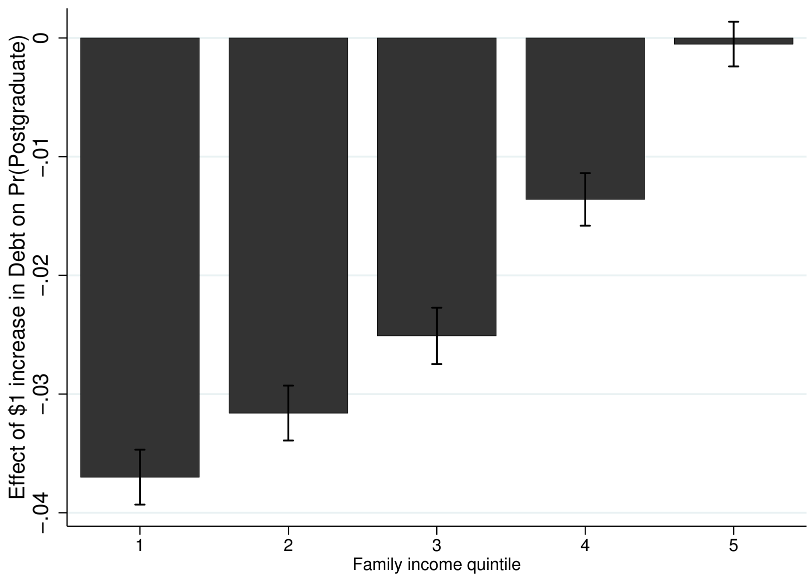


Figure 6: Evolution of undergraduate student debt and credit limit increase

This figure shows undergraduate student borrowing by repayment year. In 1993 and 2007 federal borrowing limits were increased, alleviating borrowing constraints. The figure shows that, following increases in borrowing limits, borrowing increased sharply. Source is Looney and Yannelis (2015) data appendix.

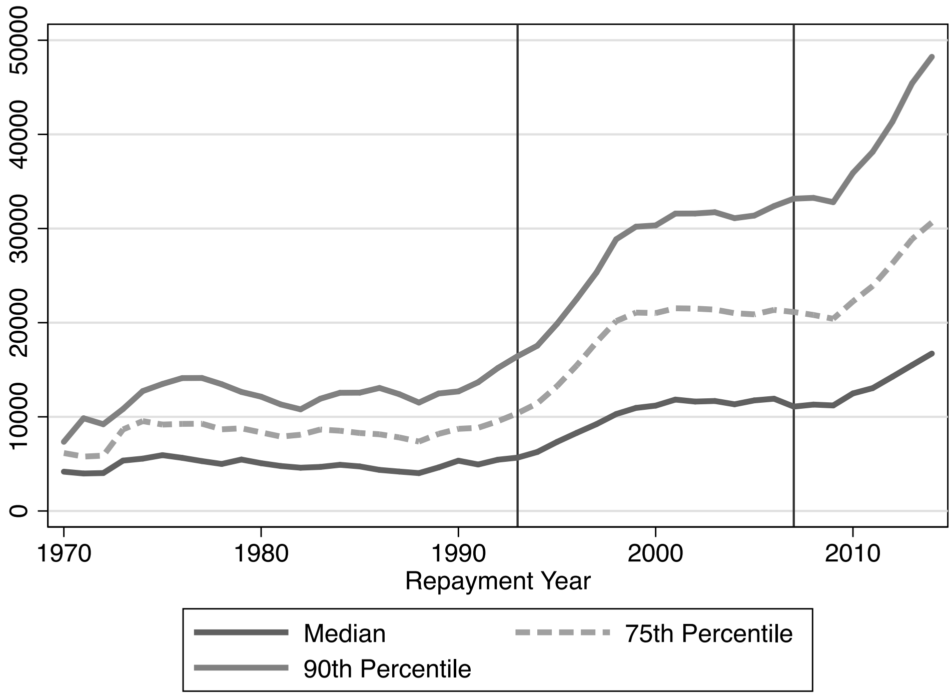


Table I: Summary statistics

Panel A shows the mean, standard deviation, minimum, and maximum of student-level variables from the main NSLDS sample. Panel B shows the mean and standard deviation of school-level variables from the Delta Project data differentially for schools that face a large tuition increase in our sample (453 schools) and those that do not. All dollar values are in 2014 dollars. Income, borrowing and tuition are winsorized at the 99% level. Panel A variables are defined in Section II. Panel B variables include *Lagged Tuition*, the nominal dollar value of in-state tuition and fees for full-time undergraduates (Sticker price); *Completions per 100 FTE students,* the number of total degrees, awards and certificates granted per 100 full time enrolled students; *Expenditures in instruction*, *Expenditures in research*, and *Expenditures in student services*, school level expenditures in millions of dollars; *Percentage with any aid*, the percentage of full-time first-time degree/certificate-seeking undergraduates receiving any federal aid; *Percentage with loan*, the percentage of full-time first-time degree/certificate-seeking undergraduates receiving a student loan; *Total enrollment*, total number of students enrolled; Admit *rate*, the fraction of full time applicants admitted; *Share returning students*, the fraction of students who have already completed first year; *Share non-white,* the fraction of total enrollment of non-white race; *Share* female, the fraction of total enrollment that is female; *SAT_M_25,* SAT Match 25th percentile score among admitted students; *In time,* the fraction of students graduating within 150% of normal time; *Student-fac ratio,* total enrollment divided by full and part time faculty.

Panel A: student-level variables

	Mean	SD	Min	Max
Postgraduate	0.1213	0.3265	0	1
Debt ($ 10,000)	1.856	1.193	0.000	7.839
Male	0.4129	0.4923	0	1
Children	0.2411	0.6571	0	9
Associate Degree	0.0471	0.2119	0	1
Dependent	0.5113	0.4999	0	1
Family income ($)	54,985.1	54,108.3	0	209,220
Entry tuition ($)	10,179.1	8,485.5	0	64,693
Limit increase	0.3388	0.4733	0	1

Panel B: school-level variables

	Large changes			No large changes		
	Mean	SD	Median	Mean	SD	Median
Lagged tuition ($)	4,310.2	6,455.7	2,356.0	9,657.0	7,649.7	7,593.0
Completions per 100 FTE students	37.8	127.2	22.9	31.9	133.0	24.5
Expenditures in instruction ($ mm)	13.2	31.9	2.9	28.5	87.1	6.1
Expenditures in research ($ mm)	7.0	20.8	0.2	28.4	88.0	0.9
Expenditures in student services ($ mm)	3.1	8.3	0.8	5.3	11.6	2.1
Percentage with any aid	82.65	62.40	89.00	84.93	30.30	91.00
Percentage with loan	52.91	64.94	49.00	59.54	33.97	64.00
Total enrollment	2,769.8	6,140.5	639.0	4,098.8	9,548.2	1,235.0
Admit rate	66.15	29.52	71.20	67.03	19.89	69.43
Share returning students	75.65	17.91	76.99	76.34	12.68	76.67
Share non-white	51.21	37.76	41.73	37.32	31.53	25.86
Share female	51.47	25.98	51.54	53.94	17.31	55.17
SAT_M_25	485.35	86.48	475.00	482.01	74.04	470.00
In time	47.63	26.01	45.61	52.49	65.37	50.83
Student-faculty ratio	15.20	11.50	11.62	15.92	76.21	12.28

Table II: Student debt and probability of enrolling in postgraduate school

This table reports estimates of equation (1). The dependent variable in each specification is an indicator of whether individual i enrolled in graduate school within eight years of entering into borrowing. $Debt_i$ is the total debt of student i in the final year of undergraduate studies measured in $10,000. Regressions in columns 1 and 2 include graduation cohort and school fixed effects; in columns 3 and 4 regressions include graduation cohort by school fixed effects. In column 2 and 4 regressions include student-level control variables. The inclusion of fixed effects is denoted beneath each column. The sample is restricted to individuals who complete a 4 year degree. All data comes from a 4% sample of the NSLDS. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*	(1)	(2)	(3)	(4)
Debt	-0.0084***	-0.0304***	-0.0335***	-0.0316***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Female		-0.0041***		-0.0024*
		(0.0012)		(0.0013)
Children		-0.0247***		-0.0259***
		(0.0009)		(0.0010)
Associate degree		-0.0502***		-0.0553***
		(0.0027)		(0.0029)
R^2	0.143	0.294	0.339	0.356
Obs.	265,006	265,006	265,006	265,006
Fixed effects				
School	Yes	Yes		
Cohort	Yes	Yes		
School×Cohort			Yes	Yes

Table III: Student debt and probability of enrolling in postgraduate school: estimates by school type

This table breaks down the results shown in the last column of Table II by school type. Panel A breaks down the results by the institution control type, for-profit, public and private. Panel B reaks down the results by selectivity. Selectivity is determined by Barron's. The lowest category is non-selective schools, competitive and very competitive are in the second group and highly competitive and most competitive schools are in the final group. Barron's classifies schools primarily based on the fraction of students admitted. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*	(1)	(2)	(3)
Panel A - Institution control types			
	For-profit	Public	Private
Debt	0.0017	-0.0312***	-0.0404***
	(0.0010)	(0.0007)	(0.0009)
R^2	0.352	0.277	0.474
Obs.	29,456	141,427	94,123
Panel B - Institution selectivity			
	Non-selective	Competitive	Highly competitive
Debt	0.0016	-0.0272***	-0.0452***
	(0.0010)	(0.0007)	(0.0009)
R^2	0.359	0.340	0.419
Obs.	32,545	140,386	92,075

Table IV: First stage results: the differential effect of tuition increases on student debt across cohorts

This table reports estimates of first stage regressions. Column 1 shows the relation between tuition increase and student debt. Columns 2 and 3 show the differential effect of tuition increases on different cohorts. Columns 4 and 5 show the effect of tuition increases on different groups of cohorts. All regressions include year of tuition change by school fixed effects, where the dummy for year of tuition change equals one for individuals who are enrolled at the institution of their undergraduate degree between one and eight years before the tuition increase. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Debt*	(1)	(2)	(3)	(4)	(5)
$\Delta Tuition$	0.0722***				
	(0.0082)				
$\Delta Tuition \times \delta^{c=1}$		0.151***	0.153***		
		(0.0213)	(0.0215)		
$\Delta Tuition \times \delta^{c=2}$		0.186***	0.177***		
		(0.0204)	(0.0206)		
$\Delta Tuition \times \delta^{c=3}$		0.109***	0.109***		
		(0.0196)	(0.0195)		
$\Delta Tuition \times \delta^{c=4}$		0.0376**	0.0344*		
		(0.0178)	(0.0178)		
$\Delta Tuition \times \delta^{c=5}$		-0.00288	-0.00486		
		(0.0166)	(0.0167)		
$\Delta Tuition \times \delta^{g=1}$				0.174***	0.170***
				(0.0171)	(0.0171)
$\Delta Tuition \times \delta^{g=2}$				0.0677***	0.0663***
				(0.0134)	(0.0134)
Controls	No	No	Yes	No	Yes
R^2	0.021	0.204	0.226	0.204	0.226
Obs.	265,006	265,006	265,006	265,006	265,006
F-Test		23.02	22.40	51.79	49.53

Table V: Tuition increases and student characteristics: placebo test

This table reports estimates of first stage regression (2), where *Debt* is replaced with student characteristics, such as family income, gender, number of children, and having an associate degree. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable:	Family income (1)	Female (2)	Children (3)	Associate degree (4)
$\Delta Tuition \times \delta^{c=1}$	1268.6*	-0.00238	-0.0000	0.00485
	(695.8)	(0.00793)	(0.0109)	(0.0045)
$\Delta Tuition \times \delta^{c=2}$	-1268.0**	0.0137*	0.0159	0.0169***
	(565.2)	(0.00702)	(0.0102)	(0.0042)
$\Delta Tuition \times \delta^{c=3}$	-1102.1*	0.00920	-0.0217**	0.0085
	(638.7)	(0.00675)	(0.00965)	(0.0038)
$\Delta Tuition \times \delta^{c=4}$	-1161.7**	0.00248	-0.000676	0.0022
	(534.5)	(0.00566)	(0.00926)	(0.0026)
$\Delta Tuition \times \delta^{c=5}$	-905.1*	0.00613	-0.00811	-0.0008
	(487.9)	(0.00659)	(0.00880)	(0.0029)
R^2	0.1230	0.1868	0.0492	0.2092
Obs.	265,006	265,006	265,006	265,006

Table VI: IV estimates of the effect of student debt on graduate studies

This table reports IV estimates of regression (3). First stage results are reported in Table IV. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*	(1)	(2)	(3)	(4)
Debt	-0.0488**	-0.0385**	-0.0613**	-0.0497*
	(0.0179)	(0.0184)	(0.0276)	(0.0280)
Controls	No	Yes	No	Yes
First stage	Cohorts	Cohorts	Groups	Groups
Obs.	265,006	265,006	265,006	265,006
Model	Large changes	Large changes	Large changes	Large changes
Hausman test statistic	1.09	0.28	0.07	0.03
Hausman test p-value	0.597	0.296	0.863	0.791

Table VII: Student debt and graduate studies: robustness

This table reports robustness tests for our OLS and IV specifications, as noted in each column. The dependent variable is an indicator of whether student i is enrolled in a postgraduate degree eight years after graduating from her undergraduate degree. $Debt_i$ is the total debt of student i in the final year of undergraduate studies. In columns 1 and 2, duration is included as a control variable. In columns 3, 4, 5 and 6 the regressions include state times cohort fixed effects. Columns 7 and 8 change the dependent variable to an indicator of whether a borrower enrolls in graduate school within nine years of entering repayment. All columns include graduation cohort by school fixed effects and student-level control variables. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Debt	-0.0637***	-0.0756***	-0.0360***	-0.0340***	-0.0761***	-0.0732***	-0.0335***	-0.0316***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0278)	(0.0288)	(0.0005)	(0.0005)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
School×cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State×year FE	No	No	Yes	Yes	Yes	Yes	No	No
Model	OLS	OLS	OLS	OLS	IV	IV	OLS	OLS
Obs.	265,006	265,006	265,006	265,006	265,006	265,006	265,006	265,006

Table VIII: School-year level matched sample

This table reports estimates of regression 5 ran at the school-year level on a panel of Title IV eligible institutions using the IPEDS data assembled by the Delta Project. *Large change* is a dummy that equals one for schools exposed to a large tuition change, defined as a change of 50% or higher, and zero for schools matched by minimizing Euclidean distance in lagged enrollment and lagged tuition within state, academic year of the large tuition increase, and control type (Private, Public, Private for Profit). δ_t are event year dummies, centered at zero the year of a tuition increase for schools with a large change. Omitted category is $t = -3$. Outcomes include *Tuition*, the nominal dollar value of in-state tuition and fees for full-time undergraduates (Sticker price); *Completions,* the number of total degrees, awards and certificates granted; *Loan pct*, the percentage of full-time first-time degree/certificate-seeking undergraduates receiving a student loan; *Admit rate*, the fraction of full time applicants admitted; *Student fac ratio,* total enrollmen divided by full and part time faculty; *In time,* the fraction of students graduating within 150% of normal time; *Fraction non-white,* the fraction of total enrollment of non-white race; *Fraction female*, the fraction of total enrollment that is female; *SAT_M_25,* SAT Match 25th percentile score among admitted students. OLS standard errors in parentheses. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable:	*Tuition*	*Completions*	*Loan pct*	*Admit rate*	*Student fac ratio*	*In time*	*Fraction non-white*	*Fraction female*	*SAT_M_25*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Large change $\times \delta_{-2}$	97.56	-22.70	-1.88	0.03	0.48	-0.05	-0.02	0.01	15.40
	(270.595)	(34.845)	(6.709)	(0.053)	(2.787)	(0.112)	(0.019)	(0.018)	(25.476)
Large change $\times \delta_{-1}$	260.79	-49.44	-1.44	0.06	1.43	-0.16	0.01	-0.01	20.52
	(287.271)	(36.913)	(6.986)	(0.056)	(2.761)	(0.120)	(0.020)	(0.019)	(26.654)
Large change $\times \delta_{0}$	1,138.90***	-1.92	1.36	0.03	-0.52	-0.08	-0.03	-0.03**	-15.92
	(247.300)	(33.115)	(6.287)	(0.049)	(2.480)	(0.103)	(0.018)	(0.017)	(22.510)
Large change $\times \delta_{1}$	1,193.22***	3.47	2.45	-0.01	0.46	-0.10	-0.00	-0.01	8.16
	(253.446)	(33.482)	(6.332)	(0.049)	(2.548)	(0.105)	(0.018)	(0.017)	(23.495)
Large change $\times \delta_{2}$	1,197.03***	10.38	6.45	-0.00	2.10	-0.13	-0.02	-0.01	-4.21
	(257.724)	(34.053)	(6.379)	(0.050)	(2.570)	(0.107)	(0.019)	(0.017)	(23.295)
Large change $\times \delta_{3}$	1,108.76***	16.36	7.02	0.02	-2.11	-0.04	-0.02	-0.01	-11.59
	(261.120)	(34.326)	(6.432)	(0.050)	(2.611)	(0.107)	(0.019)	(0.017)	(23.441)
R^2	0.803	0.904	0.511	0.773	0.328	0.303	0.753	0.542	0.702
Obs.	4,969	5,210	1,575	4,484	2,611	996	5,327	793	297

Table IX: Student debt and graduate studies: the role of family income

This table reports estimates of equation (1) for five family income quintiles. The dependent variable is an indicator of whether student i is enrolled in a postgraduate degree eight years after graduating from her undergraduate degree. $Debt_i$ is the total debt of student i in the final year of undergraduate studies. All columns include graduation cohort by school fixed effects and student-level control variables. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*					
Income quintile:	First	Second	Third	Fourth	Fifth
Average income (2014 dollars):	\$3.0K	\$16.8K	\$38.6K	\$72.2K	\$144.3K
	(1)	(2)	(3)	(4)	(5)
Debt	-0.0370***	-0.0316***	-0.0251***	-0.0136***	-0.0005
	(0.0012)	(0.0012)	(0.0012)	(0.00113)	(0.0010)
Controls	Yes	Yes	Yes	Yes	Yes
School×cohort FE	Yes	Yes	Yes	Yes	Yes
R^2	0.199	0.167	0.183	0.183	0.139
Obs.	53,181	52,316	49,759	52,526	57,209

Table X: Student debt and graduate studies: cross-sectional variation tests

This table reports estimates of equation (1). The dependent variable is an indicator of whether student i is enrolled in a postgraduate degree eight years after graduating from her undergraduate degree. $Debt_i$ is the total debt of student i in the final year of undergraduate studies. In columns 1 and 2, *Debt* is interacted with an indicator that equals one for the two cohorts that are enrolled immediately following the limit increase, *Limit increase*. In columns 3 and 4, *Debt* is interacted with $Non\ Dischargeable$, which is one when student debt is fully non-dischargeable upon bankruptcy. In columns 5 and 6, *Debt* is interacted with $Financial\ Education$, which indicates whether a state requires students to complete a mandatory personal finance year to graduate high school, in the year a student is 18, as determined by their state of residence from the FAFSA. The list of states and the date in which the requirement was enacted are presented in Internet Apendix Table A.VIII). All columns include graduation cohort by school fixed effects and student-level control variables. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*	(1)	(2)	(3)	(4)	(5)	(6)
Debt	-0.0336***	-0.0316***	-0.0431***	-0.0406***	-0.0341***	-0.0328***
	(0.0005)	(0.0005)	(0.0006)	(0.0006)	(0.0010)	(0.0010)
$Debt * Limit\ increase$	0.0089***	0.0077***				
	(0.0012)	(0.00122)				
$Debt * Non\ Dischargeable$			0.0008	-0.0018		
			(0.0083)	(0.0083)		
$Debt * Financial\ Education$					0.0073***	0.0060***
					(0.0020)	(0.0060)
Controls	No	Yes	No	Yes	No	Yes
School×cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.339	0.356	0.261	0.393	0.339	0.362
Obs.	265,006	265,006	265,006	265,006	265,006	265,006

Internet Appendix for

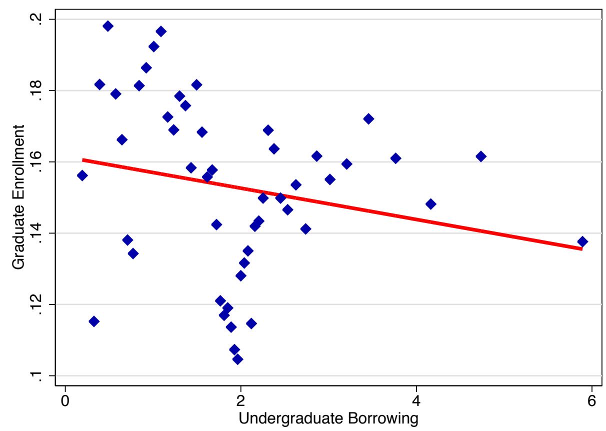
"Debt and Human Capital: Evidence from Student Loans,"

by Vyacheslav Fos, Andres Liberman, and Constantine Yannelis

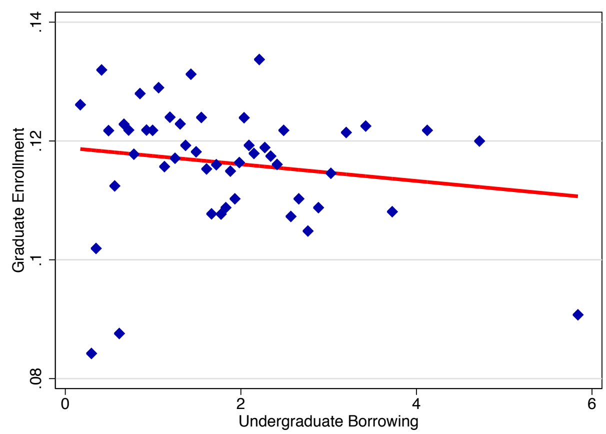
Figure A.1: Undergraduate borrowing and graduate enrollment across school types

This figure shows the relation between undergraduate borrowing and graduate enrollment for different institution types.

Panel A: Private schools



Panel B: Public schools



Panel C: Selective schools

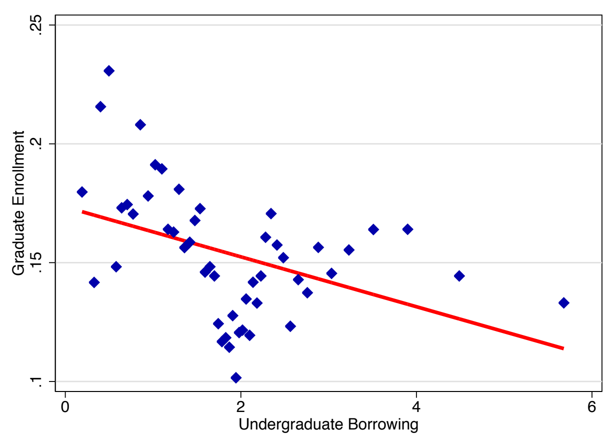


Figure A.2: Relation Between Tuition Changes and Borrowing

This figure shows total undergraduate debt at graduation in $1,000 bins of large tuition changes.

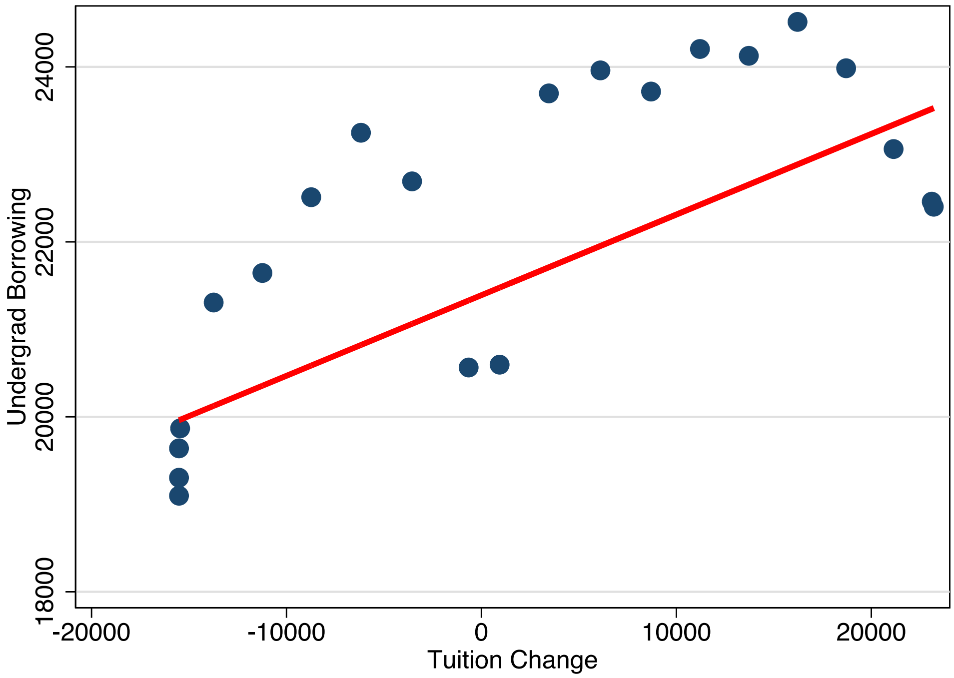


Figure A.3: Example of Large and Small Tuition Changes

This figure shows annaul average tuition for in-state students at New York University and Arizona State University. The source for tuition data is the Integrated Postsecondary Education Data System (IPEDS).

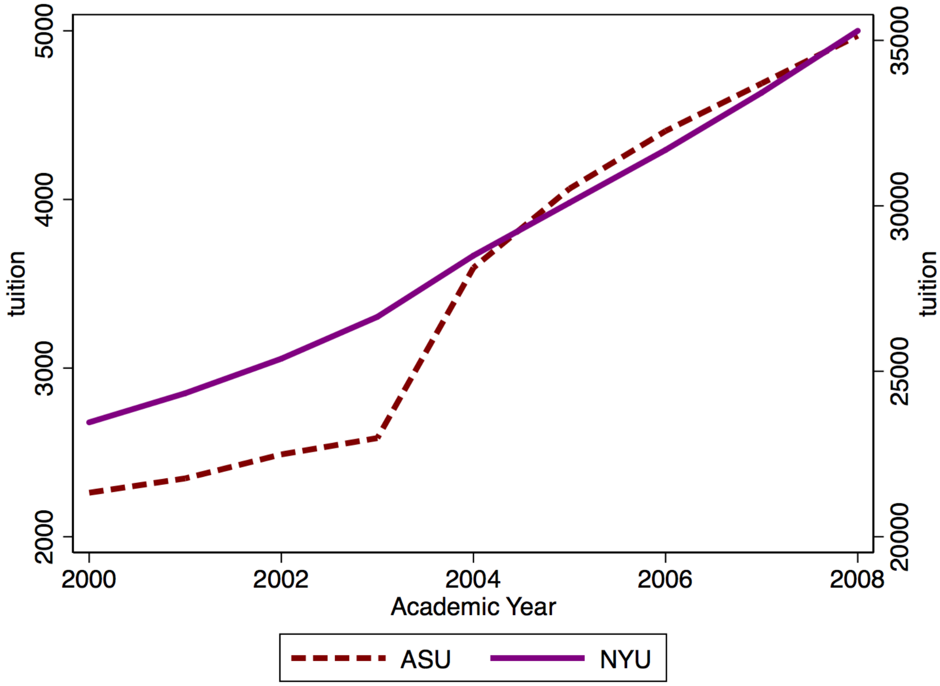


Figure A.4: Number of large tuition changes by year

This figure shows the number of large tuition changes in our sample (tuition changes by more than 50% relative to previous year) by repayment cohort.

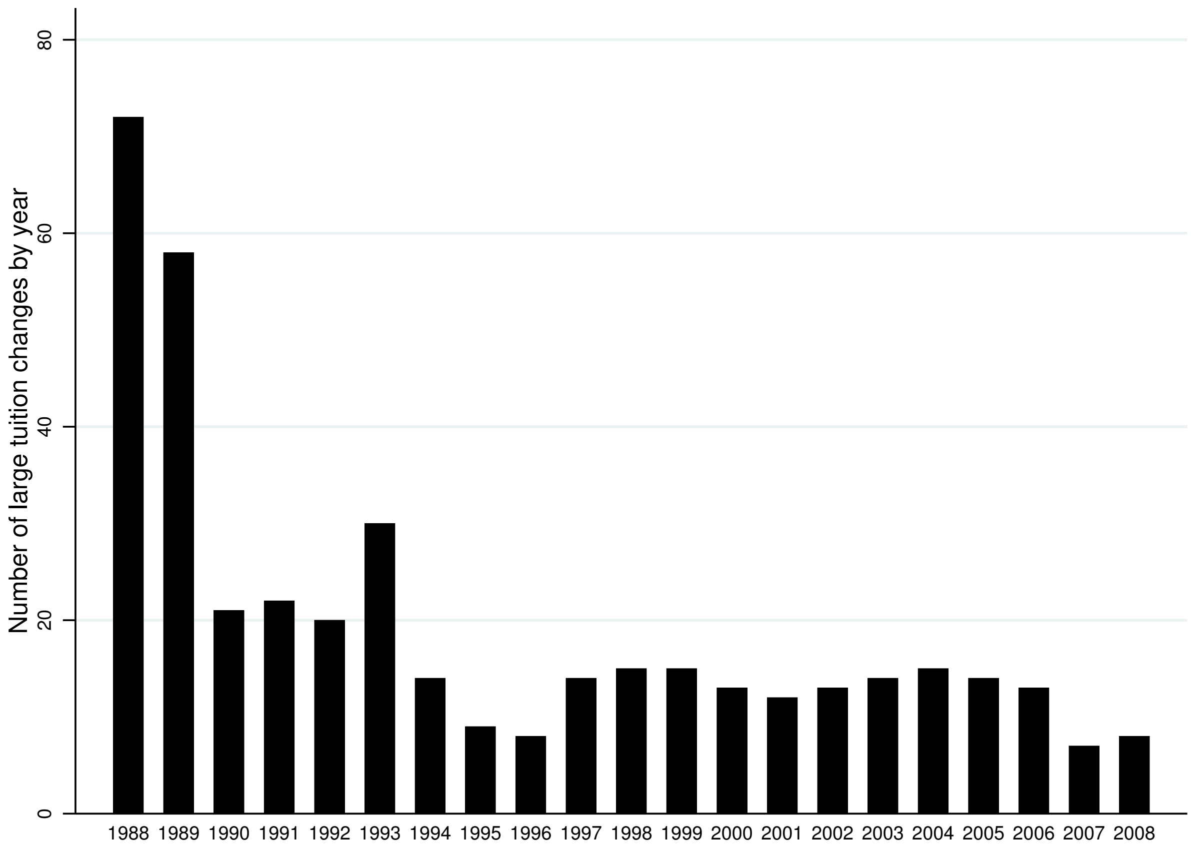


Figure A.5: Evolution of undergraduate student debt and number of postgraduate students

This figure shows changes in mean undergrad student debt in the year of repayment (right axis) and the number of graduate students. The source for undergraduate borrowing is Looney and Yannelis (2015). The source for graduate enrollment is the Integrated Postsecondary Education Data System (IPEDS).

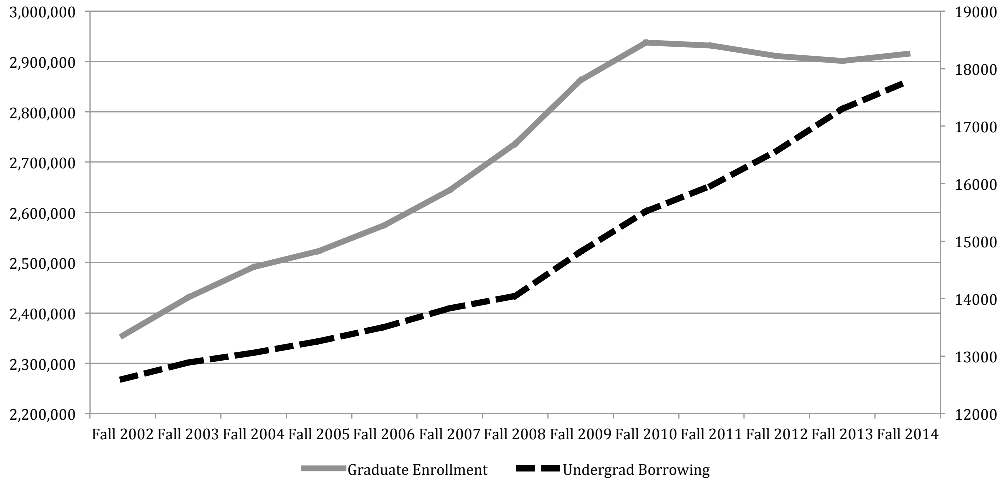


Table A.I: IV effects with a restricted linear relation

This table reports estimates of an IV regression of the causal effect of debt on the probability of enrolling in a postgraduate degree, where the instrument is the interaction of grade at the time of a tuition increase multiplied by the size of a tuition increase. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*		
	(1)	(2)
Debt	-0.0981***	-0.0705**
	(0.0301)	(0.0309)
Controls	No	Yes
Obs.	265,006	265,006

Table A.II: Tuition increases and student debt: reduced form

This table reports estimates of first stage regression (2), where *Debt* is replace with *Postgraduate*. Columns 1 and 2 show the differential effect of tuition increases on different cohorts. Columns 3 and 4 show the effect of tuition increases on different groups of cohorts. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable: *Postgraduate*	(1)	(2)	(3)	(4)
$\Delta Tuition \times \delta^{c=1}$	-0.0294***	-0.0291***		
	(0.0071)	(0.0071)		
$\Delta Tuition \times \delta^{c=2}$	-0.0272***	-0.0256***		
	(0.0065)	(0.0065)		
$\Delta Tuition \times \delta^{c=3}$	-0.0138**	-0.0129**		
	(0.0064)	(0.0063)		
$\Delta Tuition \times \delta^{c=4}$	-0.0112	-0.0109		
	(0.0083)	(0.0082)		
$\Delta Tuition \times \delta^{c=5}$	-0.0057	-0.0057		
	(0.0070)	(0.0069)		
$\Delta Tuition \times \delta^{g=1}$			-0.0262***	-0.0251***
			(0.0066)	(0.0065)
$\Delta Tuition \times \delta^{g=2}$			-0.0105*	-0.0099*
			(0.0055)	(0.0053)
Controls	No	Yes	No	Yes
R^2	0.224	0.234	0.224	0.234
Obs.	265,006	265,006	265,006	265,006

Table A.III: Student debt and student characteristics: placebo test

This table reports estimates of second stage regression (3), where *Postgraduate* is replaced with student characteristics, such that family income, gender, number of children, and having an associate degree. First stage results are reported in Table IV. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable:	Postgraduate	Female	Children	Dependents	Selectivity
	(1)	(2)	(3)	(4)	(5)
Debt	-3310.8	0.00490	0.0389	-0.0525	-0.0000
	(2352.8)	(0.0439)	(0.0598)	(0.0394)	(0.0004)
Controls	Yes	Yes	Yes	Yes	Yes
Obs.	265,006	265,006	265,006	265,006	265,006

Table A.IV: The effects of student debt on graduate studies: alternative definition of large tuition changes

This table reports estimates of second stage regression (3) where we replace the definition of large tuition changes with a 25% change relative to the previous year. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable:	*Postgraduate*	
	(1)	(2)
Debt	-0.0843***	-0.0911***
	(0.0143)	(0.0140)
Controls	No	Yes
First stage	Cohorts	Cohorts
Obs.	265,006	265,006

Table A.V: The effects of student debt on graduate studies: sub-sample analyses

This table reports estimates of second stage regression (3) where we split the sample based on sample period. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable:	*Postgraduate*			
Sample period:	Pre-1998	Post-1998	Pre-1998	Post-1998
	(1)	(2)	(3)	(4)
Debt	-0.0126**	-0.0136***	-0.0161**	-0.0210***
	(0.0063)	(0.0005)	(0.0065)	(0.0005)
Controls	No	No	Yes	Yes
Obs.	265,006	265,006	265,006	265,006

Table A.VI: The effects of student debt on graduate studies: the role of gender

This table reports estimates of second stage regression (3) where we split the sample based on student gender. All regressions include year of tuition change by school fixed effects. Heteroskedasticity-robust standard errors (in parentheses) are clustered at school-year level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable:	*Postgraduate*			
Gender:	Women	Women	Men	Men
	(1)	(2)	(3)	(4)
Debt	-0.0385***	-0.0366***	-0.0368***	-0.0340***
	(0.0007)	(0.0007)	(0.0009)	(0.0009)
Controls	No	Yes	No	Yes
Obs.	265,006	265,006	265,006	265,006

Table A.VII: Large tuition changes

This table reports estimates of cross sectional regressions of different school-level outcomes measured the academic year before large tuition change on *Large change*, a dummy that equals one for schools exposed to large changes and zero for all other schools . All regressions include academic year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Dependent variable:	Tuition	Completions	Loan Pct.	Instructional	Admit Rate	Student-to-faculty	Non-White	Frac. Female	SAT M 25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Large change	-3,557.62***	-288.39***	-16.16***	-10,985,080***	-0.04	-2.46*	0.07***	0.01	13.23
	(275.970)	(54.157)	(3.545)	(1,760,639)	(0.041)	(1.495)	(0.013)	(0.031)	(18.030)
Constant	4,181.59***	702.08***	54.10***	14,562,954***	0.70***	19.39***	0.21***	0.56***	483.96***
	(74.543)	(30.246)	(0.625)	(834,622)	(0.005)	(4.452)	(0.005)	(0.004)	(2.507)
Obs.	51,427	61,828	24,732	56,962	15,799	33,576	62,947	15,450	11,333

Table A.VIII: States requiring mandatory financial education

This table lists the US states that require mandatory personal finance education as a graduate requirement for high-school, and the year in which the requirement was established. Source: Brown, Collins, Schmeiser, and Urban (2014).

State	Year Required	State	Year Required
Alabama	None	Montana	None
Alaska	None	Nebraska	None
Arizona	2005	Nevada	None
Arkansas	2005	New Hampshire	1993
California	None	New Jersey	2011
Colorado	2009	New Mexico	None
Connecticut	None	New York	1996
Delaware	None	North Carolina	2007
Florida	2014	North Dakota	None
Georgia	2007	Ohio	None
Hawaii	None	Oklahoma	None
Idaho	2007	Oregon	2013
Illinois	1970	Pennsylvania	None
Indiana	None	Rhode Island	None
Iowa	2011	South Carolina	2009
Kansas	2012	South Dakota	2006
Kentucky	None	Tennessee	2011
Lousiana	2005	Texas	2007
Maine	None	Utah	2008
Maryland	None	Vermont	None
Massachussets	None	Virginia	2014
Michigan	1998	Washington	None
Minnesota	None	West Virginia	None
Mississippi	None	Wisconsin	None
Missouri	2010	Wyoming	2002