Tuition, Debt, and Human Capital[☆]

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Abstract

This paper investigates the effects of college tuition on student debt and human capital accumulation. We exploit data from a random sample of undergraduate students in the US and implement a research design that instruments for tuition with relatively large changes to the tuition of students who enrolled at the same school in different cohorts. We find that $10,000 in higher tuition causally reduces the probability of graduating with a graduate degree by 6.2 percentage points and increases student debt by $2,961. Higher tuition also reduces the probability of obtaining an undergraduate degree among poorer, credit constrained students. Thus, the relatively large increases in the price of education in the US in the last decade can affect the accumulation of human capital.

JEL classification: D14, H52, H81, J24, I23

Keywords: Tuition, Student Debt, Human Capital, Credit Constraints

1. Introduction

Between 2000 and 2017, the average yearly price of an undergraduate education in the United States increased by 58% in real terms. Large increases in university tuition may induce credit constrained students to substitute out of education, in the form of not enrolling in undergraduate or graduate programs, transferring to an easier or shorter degree, or dropping out of school (Hearn and Longanecker, 1985). Students may also rely on debt to finance the higher price of education, and, over the same time period, student borrowing increased from $250 billion to over $1.5 trillion (Lee, Van der Klaauw, Haughwout, Brown, and Scally, 2014; Looney and Yannelis, 2015).

In this paper we empirically investigate the effects of the level of university tuition on measures of human capital accumulation and on student debt. We find that higher tuition causally reduces the probability of graduating with a graduate degree and increases student debt. We also find that tuition reduces the probability of completing an undergraduate degree among students who are more likely to be credit constrained: lower income students, and those who have less access to credit markets.

Evidence on the effects of tuition on measures of human capital and student debt is hard to obtain for at least two reasons. First, data sources that link tuition, human capital outcomes, and debt outcomes at the student level are not easily available. And second, even when such data are available, a näive comparison of students exposed to different levels of tuition will not identify the causal effect of interest. For example, schools with higher levels of tuition are likely to be different in terms of quality, and thus attract different students, than those with lower tuition. Additionally, the availability of credit may itself lead universities to increase their prices, the so-called "Bennett hypothesis" (Lucca et al., 2018; Cellini and Goldin, 2014), which may bias the estimates due to reverse causality.

To overcome the empirical challenges, we exploit unique and novel data that links credit records for a random sample of individuals from the New York Fed Consumer Credit Panel (CCP) with their higher education enrollment and attainment records from the National Student Clearinghouse (NSC). In this paper, we exploit a sub-sample of that dataset that includes education and credit records for a random sample of individuals who enrolled in 4-year colleges between 2000 and 2014. The data detail all of the student's schools and degrees ever obtained throughout the student's life until 2014, as well as student debt balances and originations.[1] We link these data to IPEDS data at the school level to construct a measure of each student's total tuition bill for the first four years after enrolling in their first college. This measures a student's tuition bill had they chosen to remain in their first school for at least four years, the statutory time for this type of college. Actual tuition for the first college is likely to be endogenous to human capital and debt outcomes, and hence we do not consider a student's tuition bill during his actual time spent in the college. Our measure of total tuition bill increased from approximately $40,000 to $50,000 between 1998 and 2010 in constant 2014 dollars on average in our sample, and is on average higher for private and more selective schools.

We first run OLS regressions that control for cohort fixed effects and school fixed effects, and find that the 4-year tuition bill is positively correlated with student debt and negatively correlated with measures of human capital accumulation, including the probability of obtaining a Bachelors degree and a Graduate degree. However, even after controlling for cohort fixed effects and school fixed effects, the coefficients are likely to reflect heterogeneity that is unobserved to the econometrician, which complicates causal inference.

To obtain causal estimates of the effects of tuition on human capital outcomes and student debt, we exploit variation in the 4-year tuition bill induced by relatively large

[1]We refer to colleges and schools interchangeably.

tuition changes for students who enrolled at the same school in different years. Intuitively, students exposed to a relatively large tuition shock in their second year will have to pay a higher tuition for three years, while those exposed in their third and fourth years will have to pay the higher tuition for only two and one more years, respectively. In our baseline specification we focus on yearly tuition increases of at least $1,000, which affect about one quarter of our sample of students in the first four years after enrolling in their first college, but our results are qualitatively robust to alternative definitions of large tuition changes. The tuition shocks induce large variation in tuition across grades (a relevant first stage): students exposed to a tuition increase of more than $1,000 in their second, third, and fourth years end up with a total tuition bill that is respectively $6,250, $4,436 and $2,431 higher, all different from each other at conventional levels of statistical significance, than students exposed to the shock in their fifth year after enrolling. Predetermined variables are indistinguishable across cohorts at the time of a tuition shock, which provides support for the exclusion restriction.

Using the variation induced by differential exposure to large tuition changes, we produce two-stage least squares estimates of the effect of tuition on human capital outcomes and student debt accumulation. The two-stage least squares estimates show that higher tuition reduces human capital accumulation, but in a more nuanced way than suggested by the OLS results. A $10,000 higher tuition bill significantly lowers the probability of obtaining a graduate degree by 6.2 percentage points and leads to approximately $3,000 in a higher student debt balance four years after entry, both statistically significant results. In turn, the point estimates show that a higher tuition bill reduces the probability of obtaining a Bachelors degree by less than one percentage point and increases transfers to other schools by two percentage points, but these results are not significant at conventional levels.

To examine the mechanisms that can explain our results, we first explore whether tuition changes are correlated with changes in school-level offerings that could affect students in different cohorts differentially. A higher level of tuition may lead to improvements in the quality of education provided to students, so students with a larger exposure to the tuition increase (e.g., 1st or 2nd years) may receive a relatively better undergraduate education, which might change the probability of enrolling in a postgraduate degree. We find that schools that increase tuition by more than $1,000 have typically kept it relatively fixed for a number of years relative to other schools with similar observable characteristics that do not increase their tuition by more than $1,000. Importantly, schools with relatively large tuition increases do not change their practices, e.g. research and instruction expenses, after the tuition increase. Thus, changes in the quality of education are not likely to drive our baseline findings of the effects of higher tuition.

We consider two additional non-mutually-exclusive mechanisms that may explain our results. First, a higher tuition bill can cause credit constrained students to reduce their investments in human capital (e.g., Lochner and Monge-Naranjo, 2011). Second, students facing higher tuition may simply choose to substitute away from a more expensive education even when they are unconstrained, i.e., students have finite elasticities of demand for education.

To provide evidence for these mechanisms, we estimate heterogeneous treatment effects for three sub-samples that are a priori more likely to be credit constrained: lower income students, students without a credit score at the time of their first enrollment, and students exposed to tuition shocks before the 2007 federal credit limit increase, which ostensibly increased access to credit. Across all subsamples, we find that more credit constrained students respond to a higher tuition by dropping out of school without completing a Bachelors degree at a relatively higher rate. However, the negative effect of tuition on graduate school outcomes is indistinguishable across subgroups. Thus while a finite elasticity of demand for education can explain the effect of tuition on graduate school, credit constraints are likely to reduce the probability of completing an undergraduate degree.

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, including student debt (Looney and Yannelis, 2015; Mezza et al., Forthcoming; Brown et al., 2016; Mueller and Yannelis, 2019; Goodman et al., 2017; Amromin et al., 2016; Bleemer et al., 2017). A related study is Scott-Clayton and Zafar (2016), which measures the effect of West Virginia's merit-scholarship program on student debt and other financial outcomes. Rothstein and Rouse (2011) also argue that students are constrained by debt after college, and that this impact their labor market outcomes. Our paper provides causal evidence that tuition increases lead to increases in student debt.

Second, our work contributes to a literature on credit constraints and college completion. There is significant debate about the role of credit constraints in the college drop-out decision. Cameron and Taber (2011) note that difficulties arise in determining how credit access affects educational outcomes, as many data sources provided poor measures of which individuals are credit constrained. For example, Carneiro and Heckman (2002) argue that credit constraints play a role in completion, albeit a minor one, while Stinebrickner and Stinebrickner (2008) argue that credit constraints play a smaller role in drop-out decisions. Keane and Wolpin (2001) argues that while credit constraints play little role in the college attendance decision, they do play a role in labor market and consumption outcomes post college. Our findings are consistent with the view that credit constraints play a role in the drop-out decision and that tuition shocks lead to the accumulation of additional debt. At the same time, our findings indicate the both constrained and unconstrained students are less likely to attend a graduate schools, indicating that a higher cost of education deters investment in graduate degrees.

Third, our paper contributes to a literature that studies the dynamics of aggregate human capital accumulation (Galor and Moav, 2004; Lochner and Monge-Naranjo, 2011; Cordoba and Ripoll, 2013). We show that tuition levels can have important aggregate and distributional effects on the accumulation of human capital, and potentially on subsequent investment decisions due to higher levels of student debt.

The rest of the paper is organized as follows. In Section 2 we describe our data. The empirical strategy is presented in Section 3. In Section 4 we describe the results. In Section 5 we explore heterogeneous effects to study the mechanisms behind our findings. We conclude in Section 6.

2. Empirical Setting and Data

In this section we present our data, describe our sample selection procedure and the construction of variables, and provide summary statistics.

2.1. College attendance and attainment

The bulk of our analysis leverages a unique match of two unusually rich administrative datasets: the New York Fed Consumer Credit Panel (CCP) and the National Student Clearinghouse (NSC). The resulting panel dataset is a large, individual-level anonymized dataset that includes educational and credit outcomes. The CCP constitutes a 5% random sample of individual-level consumer credit records, sourced from the Equifax credit bureau. The NSC constitutes individual-level postsecondary education records, that includes detailed information on enrollment and degree attainment. This unique matched dataset allows us to observe the student debt of each individual over time, as well as educational enrollment and attainment over time for a random sample of 225,000 individuals (CCP-NSC). To maximize the match between NSC and CCP, we exploit a stratified random sampling method based on the coverage of the NSC data, where we over-sample cohorts starting from the 1980 birth year.

For each student, we identify the institution where the student was first enrolled. The motivation for doing this is that the college path that a student chooses can potentially be correlated with tuition shocks and future education and student debt. The richness of the NSC data enables us to observe the college enrolled in at any point in time, which we exploit to construct a "transfers" variable. This variable captures whether a student moves away from their first school. In addition, we observe the type of school at each point of time (public/private, 2-year/4-year). We identify as outcomes whether a student attained a Bachelors degree and a post-Bachelors degree in any school (graduate school) later in life.

2.2. Tuition

We obtain tuition data at the school level from the Integrated Postsecondary Education Data System (IPEDS) of the US Department of Education. Tuition data are available for the list of Title IV eligible institutions.

We measure an individual's tuition bill as the sum of the realized tuition for in-state residents as reported in the IPEDS data in the first four years following entry to her first undergraduate college. Because one possible effect of changes to tuition is that students drop out or transfer to a different school, this way of defining tuition measures a student's effective tuition bill had they chosen to stay in their first school for four years. Notably, our measure of tuition bill does not depend on the actual time the student spends in college, which is likely to be endogenous to the student's educational outcomes. For example, if schools raise tuition, they may grant tuition reductions to high ability students, who may be more likely to graduate, to enroll in graduate school, and to end up with less student debt. Using headline tuition (i.e., "sticker price") avoids potential biases that may arise from this effect.

For each school, we identify the largest year-over-year tuition increases between 2000 and 2014. If the largest tuition increase exceeds \$1,000, we define the year of the largest tuition increase as the tuition shock year (for robustness, we consider tuition increase thresholds of \$800, \$900, \$1,100, and \$1,200). Thus, a school can experience only one tuition shock during the sample period.

2.3. Zipcode income and college selectivity

We observe detailed measures of geography for each individual at each quarter from our CCP data. While we do not observe each individual's income, we use 2001 zipcode earnings data from the Internal Revenue Service (IRS) to create measures of neighborhood income at the point where we first see the individual in CCP. We typically first see individuals in the CCP data at ages 17-21. In most cases, this is before their first college enrollment, and we consider this zipcode of origin as their home zipcode. We match to 2001 income data at the zipcode level to obtain a measure of their home zipcode income. We use this home zipcode income as a proxy for family income and refer to it as such in the paper.

Finally, we match Barron's selectivity rankings as of 2001 for four-year colleges to our CCP-NSC panel. Barron's ranks four-year colleges into six categories (1-highest, 6-lowest) based on institutional characteristics such as acceptance rate, median entrance exam (SAT, ACT), GPA for the freshman class, and percentage of freshmen who ranked at the top of their high school graduating classes. Following standard practice, we group the colleges in the top three categories into a single category ("selective") and the rest of the colleges as "non-selective".

2.4. Sample selection and summary statistics

We make three sample selection restrictions. First, we consider only students whose first enrollment was a 4-year undergraduate college, which reduces the sample by approximately

70%. Next, we restrict the data to students who are matched to schools with non-missing tuition data in IPEDS. For these students we are able to compute the 4-year expected tuition bill. Finally, the coverage of the NSC data improves markedly from 2000. Thus, we consider only cohorts that enroll in 2000 or later. This results in a total sample of 58,648 students, which we refer to as the analysis dataset.

Table 1 displays selected summary statistics for the analysis dataset. Panel A reports demographic and school-level variables and Panel B reports tuition variables. Panel A shows that in our sample, 67.7% of students are first enrolled in a public school and 26.9% are first enrolled in a private non-profit school. 60.6% of students' first enrollment is in a selective school. In terms of demographics, the average age at entry is 19.5. The median home zipcode income is $69,039 per year on average. According to US Department of Education data, the average time to complete a four year degree was six years and four months in the 2007-08 school year.

According to the summary stats presented in Panel A, 49.7% of students in our sample graduate with a Bachelors degree and 11.55% of students in our sample complete a graduate degree. The fact that roughly half of our sample completes a degree is consistent with the National Center for Education Statistics data, which report that 60% of all undergraduates complete a bachelors degree within six years. The average outstanding student debt balance after the first 4 years after the first college enrollment is $11,989. This closely matches administrative data used in Looney and Yannelis (2015), who report undergraduate balances between $8,470 in 2000 and $17,780 in 2014.

Panel B in Table 1 shows summary statistics for tuition variables. Across first-enrollment institutions in our sample, the average (median) yearly tuition change is $385.1 ($262.9), corresponding to 4.09% (2.87%) percentage change. In the final sample of students, the typical tuition bill for the first four years after entering college is

$51,605 in constant 2014 dollars. Approximately one third of students see annual tuition changes of more than $800, and 4.5% of students see tuition changes greater than $2,000. Approximately 26% of students see a tuition shock of $1,000 or more in a given year. Conditional on a tuition change above $1,000, students see a $1,710 change in tuition. This amounts to a 15.47% increase in average annual tuition. Schools exposed to these large changes are spread out across different college-types (131 Public colleges, 779 Private not for profit, and 101 Private for profit) and academic years.

Figure 1 shows average tuition bill in the top panel and student debt four years after entry in the bottom panel for cohorts that entered school between 2002 and 2013. Both series have been rising steadily over time. Student debt has increased at a faster rate, approximately doubling over the time period, while tuition has risen by roughly 50%.

Figure 2 shows the number of $1,000 shocks between 2005 and 2014. The average number of schools per year exposed to tuition increases of at least $1,000 is 92. We see a roughly uniform distribution over time, with notable spikes in 2007 and 2009.[2] In the Appendix, we show the distribution of students by college entry cohort (figure A1), grade (figure A2), and state (table A2). The cohort refers to the number of years after entry. A student in his second year after entry is in grade 2, in the third year after entry is in grade 3, and so on. The number of students by cohort tracks enrollment patterns, while the distribution of students by state tracks population.

[2]Regarding 2009, public college and university charges are sensitive to the level of funding provided by state governments. Tuition and fees tend to rise more rapidly when state appropriations decrease or grow at very slow rates. Strained state budgets across the country that year (largely due to the recession that preceded) led to severe cutbacks in institutional funding, causing increased reliance on the other major source of revenue, tuition and fees. The 2007 spike corresponds to the federal student borrowing limit increase that took place that year and may have been contributed by capitalization of the loan limit increase into higher tuition prices (Lucca et al., 2018; Cellini and Goldin, 2014).

3. Empirical Strategy

We start our analysis of the effects of tuition on debt and human capital by providing OLS estimates of the relationship between tuition and outcomes. Then we describe our main empirical strategy based on large tuition shocks.

3.1. Tuition and Outcomes

Consider a model that relates individual i's outcomes, such as the level of student debt and the probability of obtaining a bachelors or a graduate degree, to the total tuition bill at her first college j during the first four years after entry:

$$y_i = \beta \text{Tuition}_{c(i)j(i)} + \gamma_{c(i)} + \gamma_{j(i)} + u_i. \quad (1)$$

Here, y_i is the outcome of interest for individual i, $\gamma_{c(i)}$ are cohort fixed effects defined by the individual's year of the first college entry, and $\gamma_{j(i)}$ are first college fixed effects. $Tuition_{c(i)j(i)}$ represents total tuition bill of individual i (who belongs to cohort $c(i)$) in his first college $j(i)$ during the first four years after college entry and u_i is the error term. In various tables in our paper, we refer to "$Tuition_{c(i)j(i)}$" as "*Tuition years 1-4*" to make it more explicit that it represents total tuition bill of an individual during the first four years after his first college entry. Note that the tuition bill varies at the college-cohort level, not at the college-individual level. Moreover, note that this is a cross-sectional regression, with one observation per student, and therefore all variables depend on i as noted in the regression model.

Table 2 presents OLS estimates of model (1). Our primary outcome variable of interest is *Debt*, which measures the total student debt after the first 4 years of college enrollment, in units of $10,000. We additionally explore measures of human capital accumulation, *Graduate school*, an indicator that equals one if the student has completed a graduate degree, *Bachelors*, an indicator variable that equals one for students who graduate with a Bachelors degree,and *Transfers*, an indicator variable that equals one for students who transfer to a different undergraduate school. All results in this paper cluster standard errors at the school level.[3]

These regressions account for average differences in the level of tuition across schools and for differences in average tuition over time for all schools, on average. The table documents significant associations between all outcome variables and $Tuition$. For example, a $10,000 higher tuition is associated with a 3.30% lower probability of obtaining a graduate degree, a 2.28% lower probability of graduating with a Bachelors degree, and a 1.19% higher probability of transferring between schools. These effects are not negligible relative to the outcome means, ranging from 28% of the sample mean for the probability of obtaining a Graduate degree to 4.59% of the sample mean for the probability of obtaining a Bachelors degree. Moreover, a $10,000 higher tuition is associated with a $424 increase in student debt balances measured four years after entry, representing almost 3.5% of the outcome's sample mean.

The results presented in Table 2 indicate that higher tuition levels are correlated with a reduction in the accumulation of human capital, while increasing student's debt burden. However, these results are likely to reflect heterogeneity over time in school and student quality that is unobservable to the econometrician, a selection bias that complicates inference about the causal effect of tuition on outcomes. For example, students that enter colleges with a higher tuition bill may come from families with higher income, which affects educational attainment (Hoxby, 1988) and debt. These students may therefore be more likely to graduate and to attend a graduate school.

[3]We have also tried clustering at the college-by-cohort level, the level of our treatment, which reduces standard errors and increases the precision of our estimates. These results are available upon request.

More importantly, schools that raise tuition may be different from other schools in a time-varying fashion. For example, these schools may be having financial difficulties, which could impact faculty retention and education provision. Thus, a simple comparison of students exposed to different tuition levels is an inadequate strategy to identify how tuition affects investments in human capital or the accumulation of student debt. In the next subsection we present our empirical strategy to isolate plausibly exogenous variation in tuition across students.

3.2. Large tuition shocks

In our main empirical strategy we exploit students' heterogenous exposure to large year-over-year changes in headline tuition at the school level.

Our main concern is that schools that increase their tuition by at least $1,000 are likely to be different from schools that do not, both because of the type of students they attract and the quality of the education they provide.[4] In our empirical strategy we therefore compare outcomes for students who are enrolled in different cohorts at a school that increases its tuition by more than $1,000.

We define *grade* (g) as the number of years the student is away from his year of first college enrollment when the shock occurs. Students who are not exposed to a large tuition shock are not assigned a grade. This variable can be interpreted as an inverse measure of exposure to the tuition shock. Intuitively, when a tuition shock hits, a lower grade implies that the student faces a larger number of years paying a higher tuition and is therefore more exposed to the shock. We thus exploit the variation in the total tuition bill across different grades induced by exposure to a large, school-level tuition change to identify the

[4]Indeed, Internet Appendix Table A1 shows that students in schools exposed to shocks come from higher income neighbourhoods, accumulate more student debt, and are more likely to attend a graduate school. Schools exposed to large tuition changes charge higher tuition and are more likely to be private than public.

effect of tuition on outcomes.

We run two stage least squares regressions (2SLS) where the second stage corresponds to equation (1) and the first-stage regression is given by:

$$\text{Tuition}_{c(i)j(i)} = \sum_{\tau=2}^{4} \pi_\tau \mathbb{1}_{g(i)=\tau} + \gamma_{c(i)} + \gamma_{j(i)} + \gamma_{j(i)} \times \mathbb{1}_{g(i)\in\{2,3,4,5\}} + \varepsilon_i, \tag{2}$$

where $\mathbb{1}_{g(i)=\tau}$ are grade dummies that equal 1 for all students who are τ years away from their entry at the time of a large tuition shock in a school that faces such a shock, $\gamma_{j(i)}$ are school of entry fixed effects, and $\gamma_{c(i)}$ are cohort fixed effects defined by entry year. π_τ are the first stage coefficients of interest.

We measure our effects for students who are in grades 2, 3, and 4 at the time of the tuition shock, and use the group of students who are in grade 5 at the time of the shock as the omitted category. We make this specification choice for two reasons. First, students enrolling precisely at the time of a large tuition change or after, i.e., in grades one, zero, or negative, can modify their school choices based on the tuition increase. This could endogenously modify the sample of students and potentially bias our estimates. Second, we limit the control group to students who enrolled at most 5 years before the shock to increase comparability.

To operationalize this choice, we assign a separate fixed effect to students who are in grades 2 through 5 of a school with a large tuition shock, $\gamma_{j(i)} \times \mathbb{1}_{g(i)\in\{2,3,4,5\}}$. These modified fixed effects are also included in the second stage equation (1). Students in schools exposed to tuition shocks who are not assigned grades between 2 and 5 help identify cohort fixed effects, but they do not affect the coefficients of interest, π_τ.

Our empirical strategy recovers the causal effect of tuition shocks on human capital accumulation decisions and student debt if the instrument predicts tuition (the "relevance condition") and if tuition increases affect outcomes only through changes to the tuition bill (the "exclusion restriction"). We address each of these assumptions next.

3.2.1. Relevance condition: the first stage

Table 3 column 1 presents estimates of the first stage. The coefficients of interest (π_τ in equation (2)) are also plotted in Figure 3. As the table and figure show, differences in exposure to a large tuition increase across grades lead to large differences in these cohorts' four-year tuition bills. The differences in tuition bills across grades are statistically different from each other (as evidenced in the p-value of zero in the last row of the table, as well as the non-overlapping confidence intervals in the figure). In particular, a student who is exposed to at least a $1,000 tuition increase in year 2 after her initial enrollment ends up with about $6,252 in higher four-year tuition bill than a student exposed to the same tuition increase in year 5 (the omitted category). As Figure 3 shows the relation between the number of years since school entry of cohorts at the time of large tuition increase and their four-year tuition bill is negative and monotonic, and statistically different across cohorts. The results suggest that our instrument captures an intuitive and transparent source of variation in four-year tuition across grades and satisfies the relevance assumption.

Moreover, as is evident from the R^2 reported in Table 3, across-grade differences in exposure to large tuition increases explain 99% of the variation in four-year tuition. This suggests that these large tuition changes are relatively infrequent and are followed by a much more stable path for tuition that is captured by the cohort and school fixed effects.

3.2.2. Exclusion restriction

The exclusion restriction translates to the assumption that differences in exposure to the large tuition increases affect differences in outcomes only through the effect on the four-year tuition bill.

As an example, consider two hypothetical students, Adam and Alex, who are enrolled in a school that implements a large tuition increase in 2004. In that year, Alex has just completed his second year and Adam has just completed his third year. As a result of the tuition shock, Alex and Adam face two and one more year of higher tuition, respectively. The exclusion restriction states that any difference in observed outcomes for Adam and Alex is only due to the difference in the total tuition bill they face after a large tuition increase. We next show evidence consistent with the exclusion restriction.

First, we investigate whether there is evidence of strategic bunching of students across different grades. That is, we investigate whether differences in exposure to the large tuition increases are "as good as randomly assigned" across grades within a school. Figure A2 in the Internet Appendix reports the average number of students in each year since entry at the time of a large tuition increase. The average number of students in grades 2 through 4 is approximately 1,550 and does not exhibit substantial variation across grades. Thus, we do not find evidence indicating strategic bunching of students across different grades.

Second, we verify that the quality of students does not vary across grades. Note that a priori this is unlikely because enrollment choices were made prior to the tuition shock, and we do not condition our sample on student's choices to complete their degree or remain in their initial school, as both are outcomes of interest. We compare students' characteristics across grades 2, 3, and 4 at the time of a tuition shock relative to students in grade 5, and relative to the average characteristics of all students in the sample with the same year of entry to their first school. Specifically, we estimate equation (2) replacing the dependent variable by different measures of student and school quality. Columns 2 through 5 in Table 3 show that there are no differential relationships between grades differently exposed to large tuition increases by student age, family income, school type (public and private), and school selectivity.

Public school (column 4) and school selectivity (column 5) are constant at the school level. Therefore, these regressions are estimated without school fixed effects. The coefficients across all three grades are different from zero for both outcomes, which reflects average differences between schools exposed to shocks and those that are not. But importantly, the coefficients are not different from each other across grades at conventional levels of significance. This is shown in the last row of Table 3, which shows the p-value for a statistical test of the hypothesis that the coefficients on all three dummies are equal, i.e. Grade 2 = Grade 3 = Grade 4. The p-value is close to zero for column 1, strongly rejecting the null of equality and denoting a strong first stage, but is large for all other columns. Overall, we find no systematic differences in the number of students as well as their characteristics across different grades at the time of large tuition increases.

An additional assumption we make in the context of IV estimation is of monotonicity. Given our setting with multiple instruments (three dummies), we follow Angrist and Pischke (2009) and assume that each instrument makes treatment (a higher total tuition bill) more likely and never less likely. This assumption is unlikely to be contentious in our setting, and a violation would require tuition shocks to lead to a *lower* total tuition bill for a subset of borrowers. Mogstad et al. (2019) argue that a weaker assumption, conditional (on all other instruments) monotonicity, is also sufficient for estimation of a LATE, with the added benefit of not requiring the assumption of homogeneous treatment effects. Under any of these assumptions, our estimates can be interpreted as a weighted average of the local average treatment effects identified by each instrument, that is, of the causal effect of a higher total tuition bill on students who end up with higher tuition because they enrolled in a later cohort.

4. Main Results

In this section we present our main result: the causal effects of tuition on human capital accumulation decisions and student debt. Table 4 reports estimates of two stage least squares regressions (2SLS) where the second stage corresponds to equation (1) and the first-stage corresponds to equation (2).

Column 1 in Table 4 reveals that changes in tuition have a strong positive effect on *Debt*, which measures the outstanding student debt balance in the first 4 years after entering first college. This effect is both economically and statistically significant. A $10,000 increase in tuition bill translates into $2,961 in student debt, suggesting that about 30% of a tuition bill increase is financed through student debt.

Column 2 shows that tuition increases have a significant negative effect on *Graduate school*, an indicator that equals one for students who have completed a graduate degree. A $10,000 increase in tuition bill causes the probability of completing a graduate degree to drop by 6.18 percentage points. The effect is highly significant, at the 1% level. The magnitude of the effect is quite large, given that the unconditional probability of completing a graduate degree is 11.55%. Thus, a $10,000 increase in tuition bill can essentially reduce the probability of completing a graduate degree by more than half.

Columns 3 and 4 turn to human capital accumulation at the undergraduate level. Column 3 shows a small and statistically insignificant negative effect of tuition on the completion of a *Bachelors* degree. Similarly, column 4 shows a positive but insignificant effect of tuition on *Transfers*. These results suggest that the effects of tuition on graduate studies are not driven on average by a failure to complete a Bachelors degree, or transferring to a lower quality school. They also speak to the debate on credit constraints and college completion, and are largely consistent with Keane and Wolpin (2001), Carneiro and Heckman (2002) and Stinebrickner and Stinebrickner (2008) who argue that credit constraints play only a small role in completion decisions on average but play a larger role in later life human capital and consumption choices. These average effects may also mask important heterogeneity. We investigate heterogeneous effects on lower income and credit constrained subsamples in the next section. Overall, our findings indicate that higher tuition leads to a larger student debt balance and a lower probability of completing a graduate degree.

4.1. Robustness: large tuition threshold

In our main specification we identify the largest tuition increase for each school and then refer to that increase as a large tuition shock if the increase exceeds $1,000. We show that our results are robust to different definitions of "large tuition shocks." Specifically, we consider $800, $900, $1,100, and $1,200 thresholds. Table 5 reports the results. The evidence shows that the effects of tuition on *Debt* and *Graduate school* remain significant. Importantly, the economic magnitudes of the coefficients are very similar to magnitudes of those from the main specification. The effects of tuition on *Bachelors* and *Transfers* remain statistically and economically insignificant. Overall, we find that our main findings remain unchanged when we consider these various thresholds.

5. Heterogeneity and Mechanisms

In this section we investigate the mechanisms through which higher tuition may change student debt accumulation and investments in human capital. We consider three non-mutually-exclusive mechanisms: changes in the quality of education, changes in the demand for human capital induced by a higher price of education, and credit constraints. In the process, we explore treatment heterogeneity across different sub-populations and periods.

First, a higher level of tuition may lead to changes in the quality of education provided to students. For example, schools may hire better lecturers or may provide additional resources to students such as computing facilities or tutors. If, by raising tuition, universities significantly increase spending on instruction and research then students with larger exposure to tuition increase may receive a more valuable undergraduate education. In turn, this might change the probability of enrolling in a postgraduate degree. Second, higher tuition could reduce investments in human capital as long as the demand for human capital is not inelastic to price changes. For instance, students exposed to a tuition increase may transfer to less expensive institutions or drop out. Alternatively, these students may complete the bachelors degree they are already enrolled in, but reduce their investment in graduate degrees. And third, students may be credit constrained and unable to secure the funds necessary to finance their education and other expenses while they study.

Understanding the economic mechanisms that drive the effects of tuition increases on human capital accumulation is important because different mechanisms imply different policy responses. For example, if the credit constraints mechanism is in play, the effects of higher tuition on human capital accumulation might be mitigated by increasing the federal student borrowing lifetime limits. Alleviating credit constraints would allow students to obtain their desired, presumably higher level of education. In contrast, it would not be effective if students reduce investments in human capital because a higher tuition makes these investments less attractive.

5.1. Effects on the quality of education

We start by exploring whether tuition changes are correlated with changes in school-level offerings that could affect students in different cohorts differentially. To address this question, we obtain data from the Delta Project, which constructs a school-level panel from yearly IPEDS files and allows us to analyze the evolution of school-year level variables (see Lenihan, 2012). To operationalize, we match each school that changed tuition by more than $1,000 to another school 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 could distort the trend, we restrict the sample of schools to those where tuition is not missing for event years -3 to 3.

In Figure 4 we plot the evolution of average tuition in dollars for schools with a large change (larger than \$1,000) and for the matched sample. The figure shows that, by construction, average tuition is relatively similar across the two samples before the large tuition change. On the other hand, schools with large changes (gray bars) increase their tuition discontinuously in event year zero, 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.

In Figure 5 we repeat the treated and matched sample plots with two school-level expenditure outcomes: expenditures in instruction, and expenditures in research (measured in units of \$100,000 dollars). 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 not on different trends.

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

$$Y_{j\kappa} = \alpha_{c(j)} + \gamma Large\ Change_j + \sum_{\kappa=-3}^{3} \beta Large\ Change_j \times \delta_\kappa + \sum_{\kappa=-3}^{3} \delta_\kappa + \omega_t + \epsilon_{jt}, \qquad (3)$$

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

Results are presented in Table 6. 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 across columns.

Column 1 of Table 6 replicates Figure 4, and shows that tuition increases by approximately $1,000 following a large tuition change and is roughly maintained in the following years. In column 2 we see that the number of individuals who complete any degree does not change in a statistically significant way following the tuition change. Using fraction of students that take on debt as the dependent variable, column 3 shows that the economic magnitude of the coefficients does not change before and after a large tuition increase. For instance, the coefficient of $\mathit{Large\ Change}_j \times \delta_{-2}$ is very similar to the coefficient of $\mathit{Large\ Change}_j \times \delta_1$. In columns 4 through 9 of Table 6 we see that indicators of school-level offerings and selection variables including admissions rate, student to faculty ratio, percentage of students graduating within 150% of statutory time, the fraction of non-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.

Overall, we find that schools that increase tuition by more than $1,000 have kept it relatively fixed for a number of years. Importantly, these schools do not seem to observably change their practices in a way that would predict heterogeneous treatments across students in different cohorts in a manner consistent with our results. Thus, these results suggest that changes in the quality of education are not likely to drive the results.

5.2. Heterogeneity by income

We next explore the role of income in the relation between tuition and outcome variables. In this specification we augment regression (1) by adding the interaction term "$\text{Tuition}_{c(i)j(i)} \times \text{Low Income}_i$", where "Low Income" indicates students whose family income (as defined in Section 2) is below the 25th percentile:

$$y_i = \beta_0 \text{Tuition}_{c(i)j(i)} + \beta_1 \text{Tuition}_{c(i)j(i)} \times \text{Low Income}_i + \gamma_{c(i)} + \gamma_{j(i)} + u_i. \tag{4}$$

In this model we interpret the coefficients β_0 and $\beta_0 + \beta_1$ as the effect of tuition on outcome y_i for high- and low-income individuals, respectively, and β_1 as the differential effect for low-income students. To estimate causal heterogeneous effects, we augment the first stage regression (2) where the excluded variables include the standard indicators of the year after entry at the time of a tuition shock, as well as new variables that interact these indicators with Low Income:

$$\text{Tuition}_{c(i)j(i)} = \sum_{\tau=2}^{4} \pi_\tau \mathbb{1}_{g(i)=\tau} + \sum_{\tau=2}^{4} \pi_\tau \mathbb{1}_{g(i)=\tau} \times \text{Low Income}_i + \gamma_{c(i)} + \gamma_{j(i)} + \gamma_{j(i)} \times \mathbb{1}_{g(i)\in\{2,3,4,5\}} + \varepsilon_i. \tag{5}$$

Table 7 reports the regression output. The results reveal that the effect of tuition on student debt and graduate education is similar for students from low income areas and high income areas. In contrast, we find significant differences in the effects of tuition on low income students for *Bachelors* and *Transfers*. Specifically, the interaction term indicates that a \$10,000 increase in tuition bill translates into a decrease of 1.59 percentage points in the likelihood of graduation with a Bachelors degree and 1.30 percentage points increase in the probability of transfer to a different undergraduate school for students from low income areas relative to students from high income areas (both significant at 5% level). Thus, higher tuition affects both the likelihood of graduating with a Bachelors degree and the likelihood of a transfer between schools for poorer students.

This result suggests that students from low income neighborhoods and high income neighborhoods respond differently to higher tuition. Students from high income neighborhoods, who are less likely to be financially constrained, accumulate more debt, do not reduce their completion rates, but do reduce enrollment in graduate schools. Thus, higher tuition seems to deter these students from investing in graduate education.

Relative to students from high income neighborhoods, students from low income neighborhoods, who are more likely to be relatively financially constrained, accumulate similar amounts of debt and experience similar decreases in graduate school enrollments, but are significantly less likely to complete a Bachelors degree and are significantly more likely to transfer to other schools. This finding suggests that limited financial resources are likely to contribute to the negative effect of higher tuition on undergraduate degrees. Overall, there is an unequal incidence of the effect of tuition on human capital accumulation, with a stronger negative effect for students from low income backgrounds.

5.3. Heterogeneity by access to credit

To shed more light on the role of credit constraints, we next consider two additional sources of heterogeneity. While all the students in our sample have a credit score after graduation, only about 50% of them have a credit score at the time they enroll in a Bachelors degree. Since having a credit score is positively associated with having access to credit, we test whether the effect of higher tuition on outcome variables depends on whether a student has a credit score. To do so, we repeat the analysis from Table 7, replacing the "Low Income" indicator with "Has Score", which indicates students with a credit score at the time of the enrollment. The results are reported in table 8.

The results reveal that the effect of tuition on student debt is stronger for students with a credit score. Specifically, a $10,000 increase in tuition bill translates into an additional $1,559 in student debt for students with a credit score (significant at 1% level), which corresponds to more than 50% of the effect on students without a credit score at the time of the enrollment. This finding is consistent with the fact that having a credit score is a good proxy for less binding credit constraints. Students with a credit score are approximately 1%

more likely to complete a Bachelors degree than students without a credit score (significant at 10% level). Thus, the results indicate that having access to credit mitigates some of the negative effects of higher tuition on human capital accumulation.

Interestingly, the effect of higher tuition on "Graduate school" is similar for students with and without credit scores. Similar to the heterogeneity in income, this result is consistent with the fact that higher tuition deters constrained and unconstrained students from investing in a graduate education, suggesting a more elastic demand for graduate programs.

We next consider increases in the federal student borrowing limit. Figure 6 shows the time series of the median, 75th percentile, and 95th percentile of student borrowing since 1970. 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.

To study if the limit increase changed student's response to higher tuitions, we repeat the analysis from Table 7, replacing the "Low Income" indicator with "Limit Increase", an indicator that equals one for the cohorts exposed to a tuition shock following the limit increase. The results are presented in Table 9. We find that the effect of higher tuition on student debt and graduate education did not change following the 2007 federal student borrowing limit increase. However, similarly to the heterogeneity in having a credit score or being high income, we find that less binding credit constraints—as measured by 2007 federal student borrowing limit increase—contribute to 5.9% higher completion rates (significant at 10%). While these results should be interpreted with caution due to several confounding factors that affected economic activity during financial crisis, our results suggest that the 2007 credit limit increase had a positive effect on the accumulation of human capital by increasing Bachelors completion rates.

Overall, our findings are consistent with two mechanisms driving the response of students to higher tuition. First, higher costs of education deterred students from investing in a graduate education, even when credit constraints are not likely to be binding. Second, binding credit constraints produce significant differences in the completion of a Bachelors degree, suggesting that credit constraints have significant effects on investments in undergraduate education. The evidence is not consistent with changes in the quality of education following tuition increases driving students' human capital accumulation decisions.

6. Conclusion

In this paper we investigate the effects of higher tuition on human capital accumulation and student debt. We document that increased tuition shocks are absorbed via higher levels of student debt, and cause individuals to forgo additional human capital investment through graduate school. We find that tuition reduces college completions among lower income students and among those with less access to credit at enrollment. This suggests that higher tuition reduces the probability of completing undergraduate degrees among credit constrained students. However, credit constraints do not change the effect of tuition on graduate school outcomes, which suggests that all students choose to invest less in a more expensive education, i.e., students have a finite elasticity of demand for education.

Our results inform the debate on some consequences of the fast and large increase in tuition levels in the U.S. during the past 10 years, which has attracted considerable interest from policy-makers and academics. We show evidence that is consistent with an aggregate effect of tuition on investments in human capital and, moreover, our results can also partially explain the contemporaneous time-series increase in student debt, which itself may induce distortions in future consumption and investment choices.

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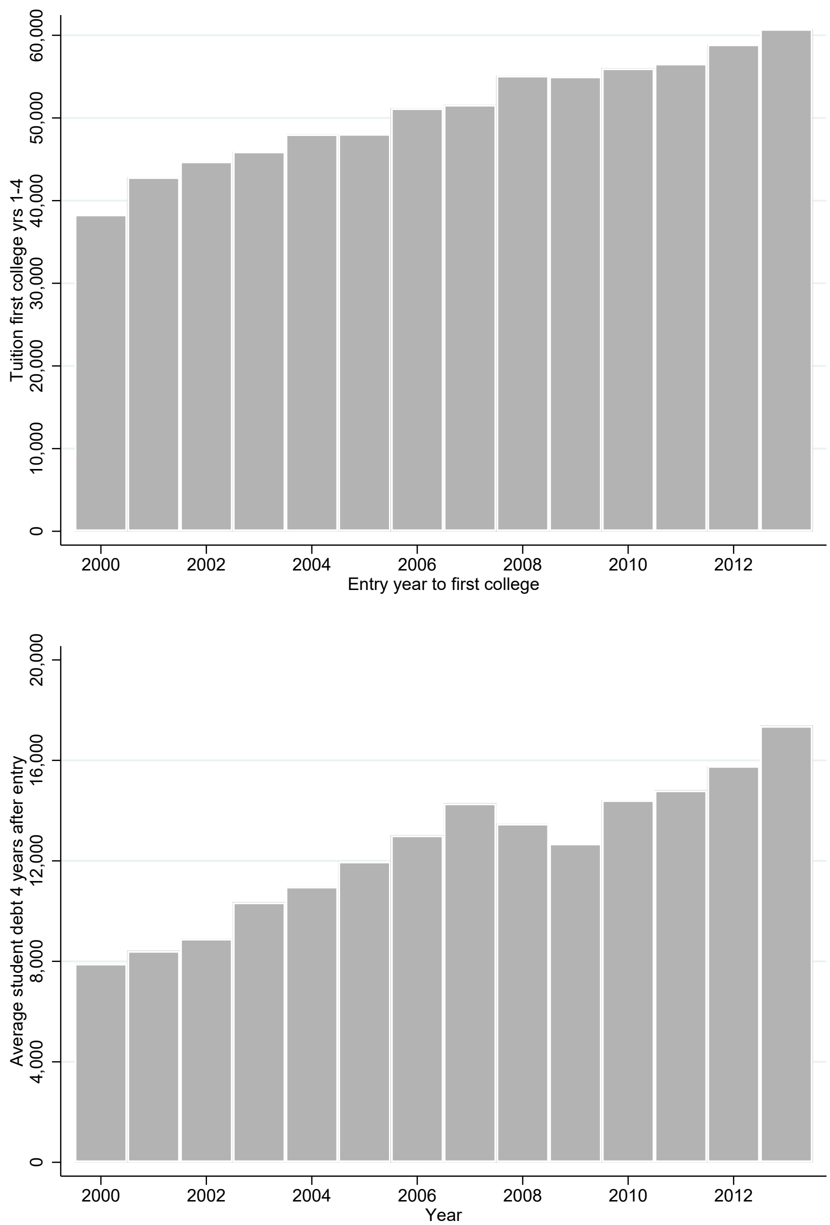
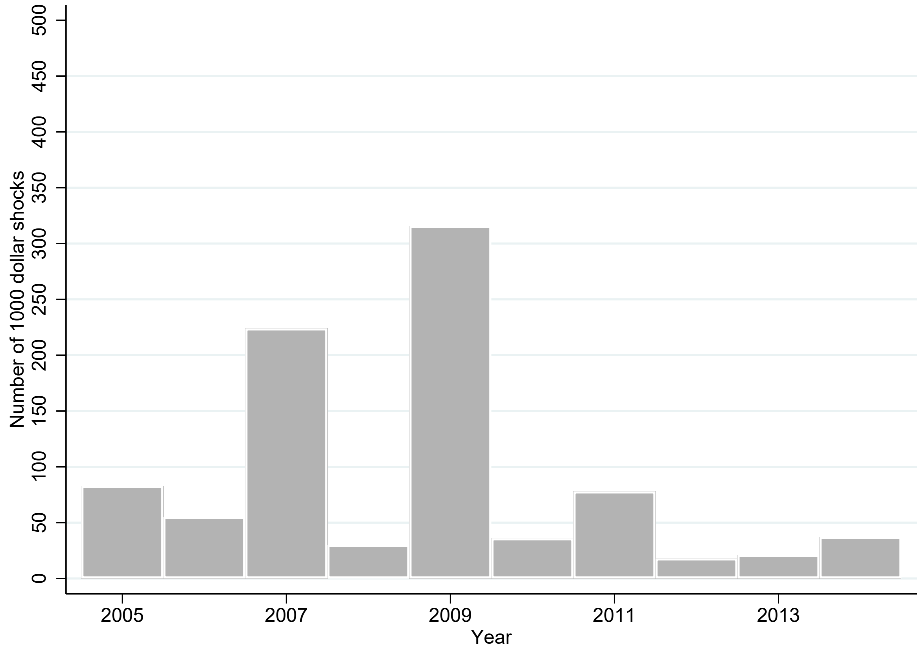
Figure 1: **The evolution of 4-year tuition bill and student debt.**



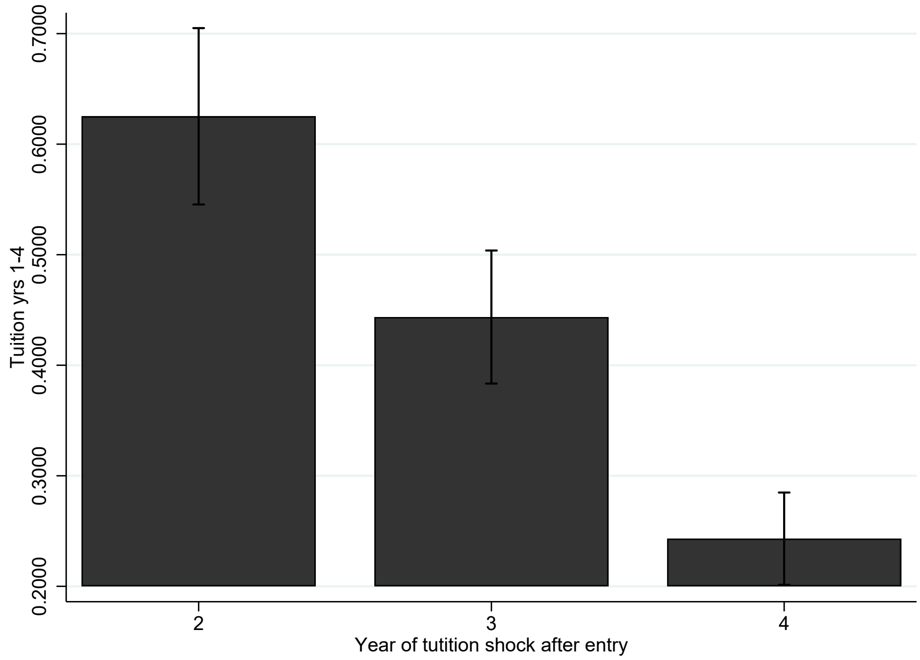
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows changes in average 4-year tuition bill (top panel) and student debt (bottom panel) during our sample period.

Figure 2: **Number of large tuition changes by year.**



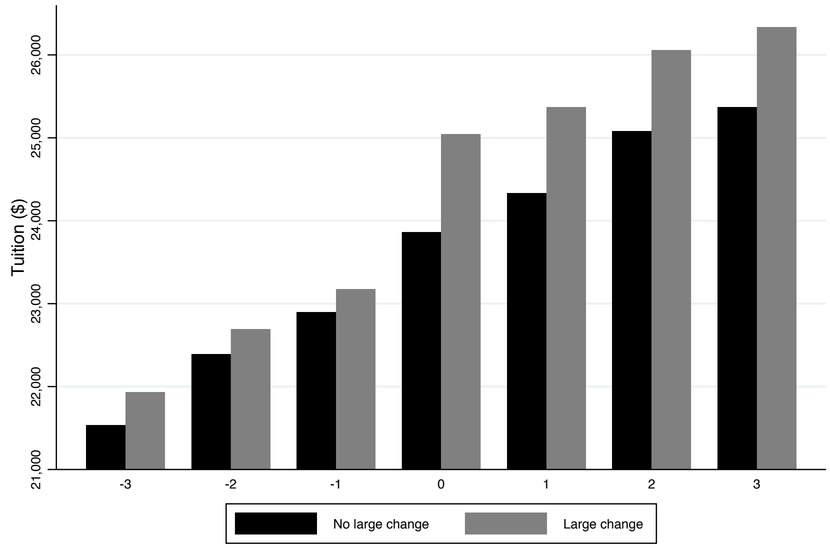
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows yearly frequency of $1,000 shocks to tuition in our sample.

Figure 3: **First Stage Estimates.**



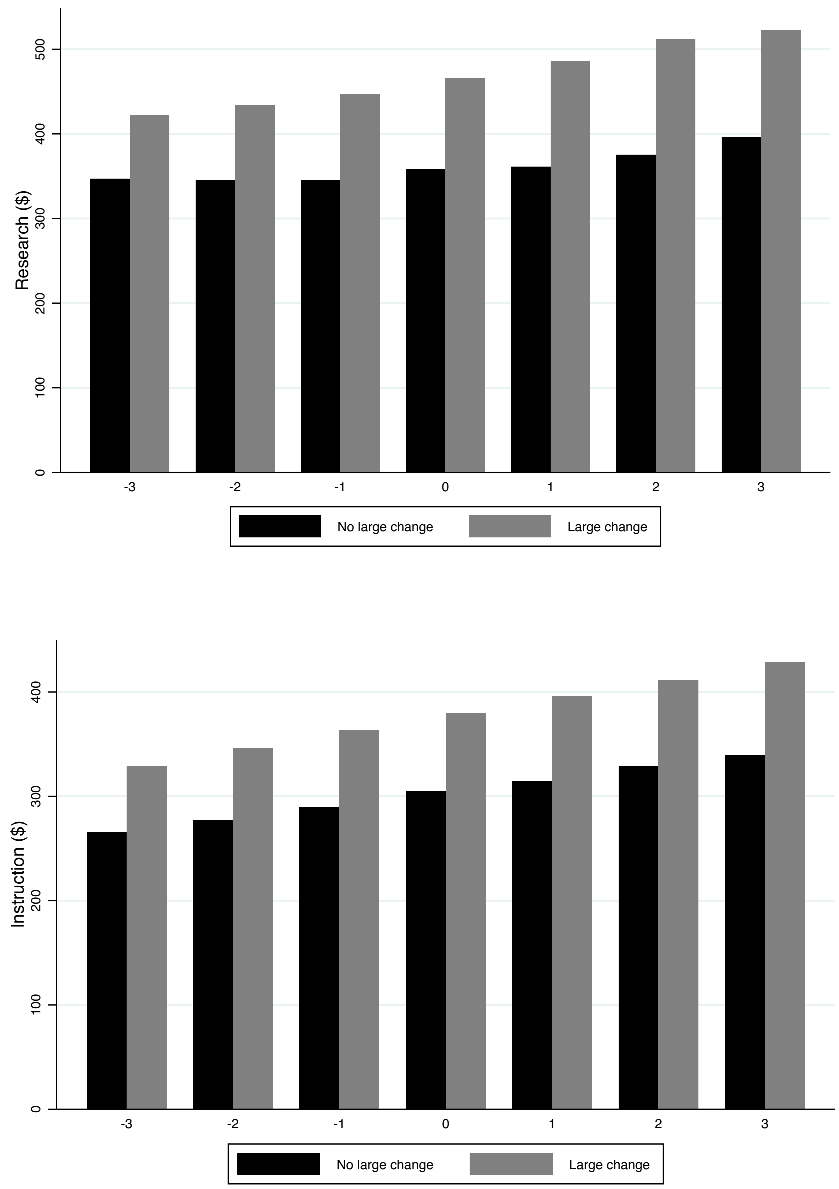
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows the effect of a $1 tuition shocks on tuition bill across grades at the time of a large tuition increase (based on column 1 in table 3). Vertical lines plot 95% confidence intervals.

Figure 4: **Large increases in tuition, matched sample.**



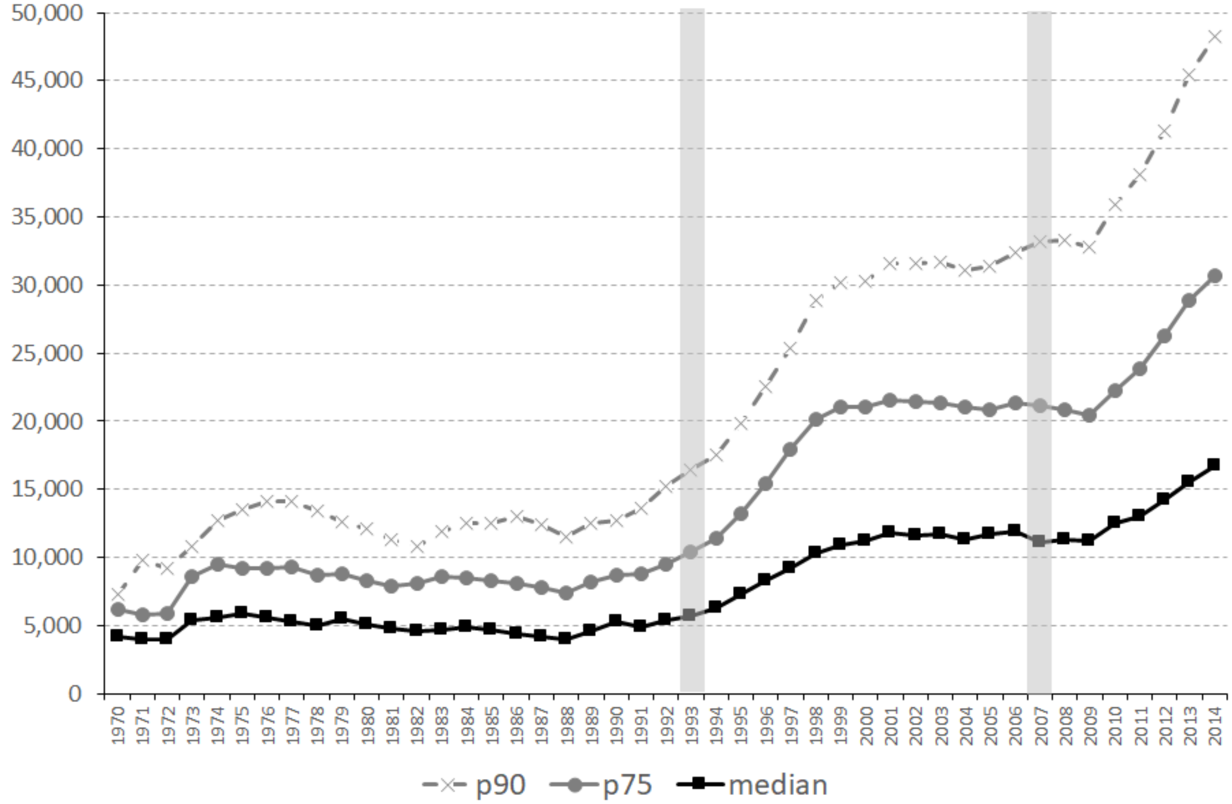
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows the average tuition by event year centered at the time of a large tuition increase (at least $1,000 increase in tuition). Black bars correspond to schools that experience a large tuition increase. Grey bars correspond to a sample of matched schools, identified within academic year, state, and school type based on the minimal Euclidean distance by lagged tuition and lagged enrollment.

Figure 5: **Expenditures in research and instruction.**



Source: Integrated Postsecondary Education Data System. This figure shows average expenditures in research (top panel) and instruction (bottom panel) measured in units of $100,000 dollars 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 6: **Evolution of undergraduate student debt and credit limits.**



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: Looney and Yannelis (2015) data appendix.

Table 1: **Descriptive Statistics.**

Variable	Mean	SD	Median	N
Panel A: Demographic and School-level variables				
First school is public	0.6766	0.4678	1.0000	58,641
First school is private non-profit	0.2693	0.4436	0.0000	58,641
First school is selective	0.6063	0.4886	1.0000	58,673
Age at entry	19.5349	3.3099	18.0000	58,673
Median household income ($10,000)	6.9039	3.6364	6.1577	56,728
Bachelors	0.4965	0.5000	0.0000	57,394
Graduate school	0.1155	0.3197	0.0000	57,394
Debt ($10,000)	1.1989	1.9807	0.3500	53,356
Panel B: Tuition variables				
Yearly tuition change	385.1	676.3	262.9	514,619
Percent yearly tuition change	0.0409	0.3410	0.0287	514,619
Total tuition years 1-4 after entry ($10,000)	5.1605	4.6106	3.1051	48,534
Max percent tuition change by student	0.1214	0.6899	0.0774	53,347
Max tuition change by student	1,015.1	1,014.5	778.5	53,347
Fraction $800 or higher	0.3171	0.4653	0.0000	58,648
Fraction $900 or higher	0.2903	0.4539	0.0000	58,648
Fraction $1,000 or higher	0.2606	0.4390	0.0000	58,648
Fraction $1,100 or higher	0.2380	0.4259	0.0000	58,648
Fraction $1,200 or higher	0.2109	0.4080	0.0000	58,648
Fraction $2,000 or higher	0.0450	0.2074	0.0000	58,648
Yearly tuition change conditional on $1,000 shock	1,709.9	851.5	1,556.4	13,895
Percent yearly tuition change conditional on $1,000 shock	0.1547	0.4348	0.0912	13,895

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports descriptive statistics. All variables are defined in section 2.

Table 2: **OLS regressions.**

Dependent variable:	*Debt* (1)	*Graduate school* (2)	*Bachelors* (3)	*Transfers* (4)
Tuition years 1-4	0.0424*	−0.0330***	−0.0228***	0.0119**
	(0.0238)	(0.0036)	(0.0046)	(0.0053)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	48,406	47,415	47,415	48,422
R^2	0.15	0.12	0.35	0.10

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of equation 1. The dependent variables are *Debt*, which measures the total student debt after the first 4 years of college enrollment, in units of $10,000, *Graduate school*, an indicator that equals one if the student has completed a graduate degree, *Bachelors*, an indicator variable that equals one for students who graduate with a Bachelors degree, and *Transfers*, an indicator variable that equals one for students who transfer to a different undergraduate school. All regressions include cohort and school fixed effects, defined by entry year and by entry school respectively. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 3: **First stage and predetermined outcomes.**

Dependent variable:	*Tuition yrs 1-4* (1)	*Age entry* (2)	*Median income* (3)	*Public school* (4)	*Selective* (5)
Grade 2 (g=2)	0.6252***	−0.0539	0.1127	−0.3683***	0.1279***
	(0.0407)	(0.1195)	(0.1692)	(0.0377)	(0.0383)
Grade 3 (g=3)	0.4436***	−0.0789	0.1251	−0.3489***	0.1182***
	(0.0307)	(0.1310)	(0.1505)	(0.0366)	(0.0361)
Grade 4 (g=4)	0.2431***	−0.0276	0.0037	−0.3517***	0.1259***
	(0.0213)	(0.1125)	(0.1621)	(0.0427)	(0.0378)
Fixed effects:	Cohort, School	Cohort, School	Cohort, School	Cohort	Cohort
Observations	48,291	48,291	46,972	48,539	48,539
R^2	0.99	0.20	0.20	0.05	0.01
p-value	0.00	0.90	0.68	0.62	0.87

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of the first stage regression (2). The outcome variables are *Tuition yrs 1-4*, student age, family income, school type (public and private), and school selectivity. *Tuition yrs 1-4* measures total in-district tuition and fees as per the IPEDS dataset for each student, from entry-year until year 4 (in units of $10,000). Regression in columns 1,2, and 3 include cohort and (modified) school fixed effects, defined by entry year and by entry school respectively. The outcome variables in columns 4 and 5 are school level variables, therefore these regressions are estimated without school fixed effects. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 4: **The effect of tuition on human capital and debt accumulation.**

Dependent variable:	*Debt* (1)	*Graduate school* (2)	*Bachelors* (3)	*Transfers* (4)
Tuition years 1-4	0.2961**	−0.0618***	−0.0068	0.0219
	(0.1311)	(0.0219)	(0.0270)	(0.0279)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	48,275	47,279	47,279	48,291

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of two stage least squares regressions (2SLS) where the second stage corresponds to equation 1 and the first-stage corresponds to equation 2. First stage results are reported in column 1 of Table 3. The dependent variables are *Debt*, which measures the total student debt after the first 4 years of college enrollment, in units of $10,000, *Graduate school*, an indicator that equals one if the student has completed a graduate degree, *Bachelors*, an indicator variable that equals one for students who graduate with a Bachelors degree, and *Transfers*, an indicator variable that equals one for students who transfer to a different undergraduate school. All regressions include cohort and school fixed effects, All regressions include cohort and school fixed effects, defined by entry year and by entry school respectively. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 5: **Alternative definitions of large tuition changes.**

Dependent variable:	*Debt* (1)	*Graduate school* (2)	*Bachelors* (3)	*Transfers* (4)
Panel A: $800 increase				
Tuition years 1-4	0.3222**	−0.0816***	0.0117	0.0049
	(0.1357)	(0.0233)	(0.0287)	(0.0295)
Observations	48,242	47,240	47,240	48,258
Panel B: $900 increase				
Tuition years 1-4	0.3483***	−0.0708***	0.0060	0.0127
	(0.1339)	(0.0228)	(0.0282)	(0.0291)
Observations	48,266	47,267	47,267	48,282
Panel C: $1,100 increase				
Tuition years 1-4	0.3252**	−0.0628***	−0.0007	0.0180
	(0.1282)	(0.0217)	(0.0255)	(0.0266)
Observations	48,287	47,290	47,290	48,303
Panel D: $1,200 increase				
Tuition years 1-4	0.2764**	−0.0605***	0.0006	0.0302
	(0.1300)	(0.0216)	(0.0251)	(0.0265)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	48,301	47,307	47,307	48,317

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table repeats the analysis in table 4 where we replace the definition of large tuition changes with a $800, $900, $1,100, and $1,200 change. All regressions include cohort and school fixed effects, All regressions include cohort and school fixed effects, defined by entry year and by entry school respectively. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 6: **School-year level matched sample.**

Dependent variable:	*Tuition* (1)	*Completions* (2)	*Loan pct* (3)	*Admit rate* (4)	*Student fac ratio* (5)	*In time* (6)	*Fraction non-white* (7)	*Fraction female* (8)	*Sat-M-25* (9)
Large change $\times \delta_{-2}$	34.71	0.00	-1.52**	-0.00	-0.55	0.01*	0.00	-0.00	7.02***
	(83.994)	(0.005)	(0.650)	(0.006)	(1.127)	(0.005)	(0.006)	(0.006)	(1.679)
Large change $\times \delta_{-1}$	58.93	0.00	-1.07*	-0.01*	1.07	0.01	0.01	0.00	6.25***
	(82.673)	(0.005)	(0.640)	(0.006)	(0.910)	(0.005)	(0.006)	(0.006)	(1.665)
Large change $\times \delta_0$	1,024.14***	0.01	-1.09*	-0.01	-1.37	0.03***	0.01	-0.01	6.27***
	(81.721)	(0.005)	(0.644)	(0.006)	(1.082)	(0.005)	(0.005)	(0.005)	(1.663)
Large change $\times \delta_1$	811.55***	-0.00	-1.45**	-0.01**	-0.37	0.01**	0.01**	-0.00	8.18***
	(83.149)	(0.005)	(0.640)	(0.006)	(0.901)	(0.005)	(0.006)	(0.006)	(1.687)
Large change $\times \delta_2$	753.95***	0.00	-1.28*	-0.01*	-0.86	0.02***	0.01*	0.01	6.39***
	(87.745)	(0.005)	(0.685)	(0.007)	(1.164)	(0.006)	(0.006)	(0.006)	(1.793)
Large change $\times \delta_3$	682.59***	0.00	-0.59	-0.01	0.23	0.01**	0.01*	0.00	6.73***
	(90.446)	(0.005)	(0.696)	(0.007)	(0.969)	(0.006)	(0.006)	(0.006)	(1.850)
R^2	0.966	0.345	0.592	0.534	0.306	0.652	0.725	0.533	0.829
Observations	15,102	15,097	14,771	12,813	11,181	13,694	15,097	12,422	9,898

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of regression (3) 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 $1,000 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_κ are event year dummies, centered at zero the year of a tuition increase for schools with a large change. Omitted category is $\kappa = -3$. 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 enrollment 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.

Table 7: **Heterogeneous 2SLS estimates: The Role of Income.**

Dependent variable:	*Debt* (1)	*Graduate school* (2)	*Bachelors* (3)	*Transfers* (4)
Tuition years 1-4	0.2926^{**}	-0.0613^{***}	-0.0027	0.0172
	(0.1327)	(0.0219)	(0.0270)	(0.0280)
Tuition years 1-4 x Low Income	-0.0166	-0.0018	-0.0159^{**}	0.0130^{**}
	(0.0319)	(0.0041)	(0.0067)	(0.0058)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	48,275	47,279	47,279	48,291

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table repeats the analysis in table 4, where first stage instrumental variables and instrumented tuition variable are interacted with the indicator of low income. *Low income* indicates zip codes where median individual income is below 25th percentile (as measured in 2001 based on data provided by the Federal Reserve). All regressions include cohort and school fixed effects, All regressions include cohort and school fixed effects, defined by entry year and by entry school respectively. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 8: **Heterogeneous 2SLS estimates: The Role of Access to Credit.**

Dependent variable:	*Debt* (1)	*Graduate school* (2)	*Bachelors* (3)	*Transfers* (4)
Tuition years 1-4	0.2662**	−0.0631***	−0.0063	0.0201
	(0.1240)	(0.0219)	(0.0270)	(0.0280)
Tuition years 1-4 x Has Score	0.1559***	0.0036	0.0083*	0.0003
	(0.0207)	(0.0032)	(0.0046)	(0.0049)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	48,227	47,232	47,232	48,243

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table repeats the analysis in table 4, where first stage instrumental variables and instrumented tuition variable are interacted with the indicator of having a credit score at the time of enrollment. All regressions include cohort and school fixed effects, All regressions include cohort and school fixed effects, defined by entry year and by entry school respectively. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 9: **Heterogeneous 2SLS estimates: The Role of Federal Credit Limits.**

Dependent variable:	*Debt* (1)	*Graduate school* (2)	*Bachelors* (3)	*Transfers* (4)
Tuition years 1-4	0.3078*	−0.0604**	−0.0473	0.0555
	(0.1634)	(0.0280)	(0.0319)	(0.0348)
Tuition years 1-4 x Limit increase	−0.0010	0.0014	0.0589*	−0.0475
	(0.1466)	(0.0250)	(0.0310)	(0.0318)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	48,275	47,279	47,279	48,291

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table repeats the analysis in table 4, where first stage instrumental variables and instrumented tuition variable are interacted with *Limit increase*, the indicator that equals one for the cohorts that are enrolled following federal 2007 student borrowing limit increase. All regressions include cohort and school fixed effects, All regressions include cohort and school fixed effects, defined by entry year and by entry school respectively. Standard errors (in parentheses) are clustered at the school level. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.