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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 United States and implement a research design that instruments for realized tuition with relatively large changes to the advertised tuition of students who enrolled at the same school in different cohorts. We find that \$5,000 in higher tuition causally reduces the probability of graduating with a graduate degree by 3.1 percentage points and increases student debt by \$1,480. Higher tuition also reduces the probability of obtaining an undergraduate degree among poorer students.

Key words: tuition, student debt, human capital, credit constraints

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1. Introduction

Between 2000 and 2017, the average yearly price of 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, which may have negative effects on long term financial and economic outcomes (Bleemer et al., 2021; Chakrabarti et al., 2020). 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).

The effect of tuition on human capital accumulation decisions is a priori not clear. Consider, for example, the effect of tuition increases on bachelor's degrees. Universities could use funds raised from tuition increases to boost the quality of education, which could result in higher bachelor's degree completion rates. At the same time, higher tuition could force credit-constrained students to transfer to an easier (and cheaper) degree or drop out of school. The effect of tuition on the decision to attain a graduate degree is also a priori not clear. On one side, if higher tuition results in higher student debt, students could have stronger incentives to enroll in graduate degrees because enrollment defers students' debt payments. On the other side, binding credit constraints could increase dropouts and decrease demand for graduate education. Therefore, whether higher tuition encourages or discourages human capital accumulation is an open empirical question.

In this paper, we empirically investigate the effects of the level of university tuition on measures of human capital accumulation and on student debt, using linked administrative data on education and 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. This has the potential to increase income inequality.

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 naïve 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 prices, the so-called “Bennett hypothesis” (Lucca et al., 2018; Cellini and Goldin, 2014; Kargar and Mann, 2018), 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). 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 tuition changes for students who enrolled at the same school in different years. The intuition behind this strategy rests on the fact that students enrolled in different years will see a differential impact from large tuition changes. For example, a student exposed to a large tuition change in their second year will have to pay higher tuition for three years, while a student exposed in their fourth year will only have to pay higher tuition for one additional year.

In this paper, we exploit a sub-sample of the linked CCP-NSC data that includes

education and credit records for a random sample of individuals who enrolled in 4-year colleges between 2000 and 2015. The data detail all of the student's schools and degrees ever obtained throughout the student's life until 2015, as well as student debt balances and originations.¹ We link these data to the Integrated Postsecondary Education Data System (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 her actual time spent in the college. Our measure of a student's 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 bachelor's 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.

We next turn to causal estimates of the effects of tuition on human capital outcomes and student debt, using large tuition increases for students who enrolled at the same school in different years. Intuitively, students exposed to a relatively large tuition increase in their second year will have to pay higher tuition for three years, while those exposed in their

¹We refer to colleges and schools interchangeably.

third and fourth years will have to pay higher tuition for only two and one more years, respectively.

In our baseline specification, we first identify the largest year-over-year tuition increases between 2000 and 2015 for every school. If the largest tuition increase exceeds \$1,000, we then define the year of the largest tuition increase as the “event” year.² Thus, a school can experience only one large tuition increase event during the sample period. This allows us to assign students who are enrolled at a school exposed to a large tuition increase to one specific ‘grade’, defined as the number of years after entry where the student is situated at the time of the tuition increase. We set a threshold (\$1,000 in our baseline) to focus on economically meaningful tuition increases. Among the sample of large tuition increases defined this way, yearly tuition increases on average by \$1,709, which corresponds to a 15.47% increase. Thus, these tuition increases are not routine tuition increases. Indeed, we show that after a school experiences a large tuition increase, tuition dynamics become similar to tuition dynamics of matched schools (see Figure 5).

In addition to being economically important, large tuition increases affect a sizeable population: about 10% of our sample of students experienced a large tuition increase during the first four years after enrolling in college. Importantly, large tuition increases induce large variation in tuition across students who were enrolled in the same institution at the time of the increase, but in different years. This generates a strong first stage to explore outcomes. Students exposed to a large tuition increase 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 for students exposed to the tuition increase in their fifth year after enrolling. Predetermined variables are indistinguishable across years at the time of a tuition increase, which provides support

²For robustness, we consider thresholds of \$800, \$900, \$1,100, and \$1,200.

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

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 \$5,000 higher tuition bill significantly lowers the probability of obtaining a graduate degree by 3.1 percentage points and leads to approximately \$1,500 in 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 bachelor's degree by less than one percentage point and increases transfers to other schools by two percentage points, but these results are not statistically significant at conventional levels.

We examine three non-mutually-exclusive mechanisms that may explain our results. We first explore whether tuition changes are correlated with changes in school-level offerings that could affect students in different grades differentially. A higher level of tuition may lead to improvements in the quality of education provided to students, so students with greater exposure to the tuition increase (e.g., 1st or 2nd years) may receive a relatively better undergraduate education, which might change the probability of bachelor's degree completion as well as the likelihood of enrolling in a postgraduate degree. Second, a higher tuition bill can cause credit-constrained students to reduce their investments in human capital (e.g. Lochner and Monge-Naranjo, 2011). Finally, 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 understand the last two mechanisms, we estimate heterogeneous treatment effects

for a sub-sample that is a priori more likely to be credit constrained: lower-income students. We find that more credit constrained students respond to higher tuition by dropping out of school without completing a bachelor's degree at a relatively higher rate than less credit constrained students. However, the negative effect of tuition on graduate school outcomes is indistinguishable across subgroups and persists for all three groups. Thus, whereas a finite elasticity of demand for education can explain the effect of the cost of undergraduate education on graduate school, credit constraints are likely to reduce the probability of completing an undergraduate degree.

When we consider the first mechanism, the quality of education, we find that schools with relatively large tuition increases do not change expenditure in research in a statistically significant way. However, expenditure in instruction does seem to pick up following the tuition change. This suggests that tuition increases could lead to improvements in the quality of education, which would predict better student outcomes. Our findings suggest, however, that the increase in quality of education is not sufficient to compensate for the negative effects of credit constraints and the cost of education on investments in human capital.

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 (Lustig and Van Nieuwerburgh, 2006; Rothstein and Rouse, 2011; Looney and Yannelis, 2015; Cadena and Keys, 2015; Mezza et al., 2020; Black et al., 2020; Brown et al., 2016; Amromin et al., 2016; Scott-Clayton and Zafar, 2016; Goodman et al., 2017; Bleemer et al., 2021; Lucca et al., 2018; Mueller and Yannelis, 2019a; Ebrahimian and Wachter, 2020; Chakrabarti et al., 2020). Relatedly, Luo and Mongey (2019) study the impact of student debt, and build a model with a mechanism similar to Rothstein and Rouse (2011). They use survey data and exploit the composition

of grants versus loans at schools, finding that higher debt causes graduates to accept jobs with higher wages. Bleemer et al. (2021)³ and Mezza et al. (2020)⁴ use variation in tuition, and study how student debt affects homeownership. Black et al. (2020) use variation from expansions in federal loan limits, along with administrative schooling, earnings, and credit records, finding that increased student loan availability increases student debt and improves degree completion and later-life earnings.⁵

³Our paper differs from Bleemer et al. (2021) because we use different data, different identification strategy, and ask different research questions. The authors use state-cohort variation in public college tuition increases, and primarily focus on homeownership. They conduct a state-cohort level difference-in-differences. In one table, they explore enrollment outcomes using data from the ACS, and find ambiguous effects of tuition on college enrollment, years of post-high school schooling, and BA degree attainment rates. Our paper has three main differences: (a) Data: Bleemer et al. (2021) uses state-cohort level tuition changes, and do not observe the college of the students, their results are potentially confounded by measurement error and other state-cohort level changes such as education policy changes, funding changes etc. In contrast, our unique matched individual-level dataset on educational and credit market outcomes allows us to place a student in their first school and focus on the individual student’s educational outcomes after tuition shocks faced by the institution (while the student is in school). Thus, we are using matched individual level education-credit outcomes data. While they use individual level credit outcomes data, they rely on state-cohort level education data. They are not able to connect an individual’s credit outcomes with education outcomes, meaning they are not able to assess whether higher tuition faced by an individual affects educational outcomes of that individual. (b) Method: Our identification strategy is different. Instead of using tuition directly, our identification variation comes from comparing students within a school who belong to different grades and hence are exposed to different tuition bills. The identification strategy is different and their tuition variable is likely endogenous, being confounded with other observable and unobservables. For example, they cannot rule out extensive margin changes (or composition changes within schools) that may correlate with tuition changes, unlike us. (c) Outcome differences: Bleemer et al. (2021) do not look at post-Baccalaureate attainment, which is the primary focus of our paper. Their focus is homeownership.

⁴Mezza et al. (2020) focus on homeownership as an outcome, not graduate school enrollment. Mezza et al. (2020) use a different identification strategy, instrumenting for debt (different endogenous variable) using an interaction of state-level public university tuition and an indicator of whether an individual attended a public university before turning 23, comparing individuals who went to college with those who did not, which is of course an endogenous choice. They study how tuition affects enrollment in different types of undergraduate degrees and major choice, mainly to assess threat to their identification strategy. They further use enrollment indicators as controls. The data they use is limited compared to ours. Their sample consists of 34,891 individuals between ages of 23 and 31 in 2004 who they observe biennially between June 2007 and June 2003 and then in December 2004, June 2007, December 2008 and finally biennially between June 2010 and June 2014. In contrast, our final sample consists of 58, 648 students (of all ages) who went to school between 2000 and 2015 and we observe all their educational and credit records.

⁵Black et al. (2020) use data from a single large state, Texas, and a loan limit increase in 2007 and 2008. The difference between their result and ours is intuitive given the differences in variation used, and what is being picked up. Increasing loan limits does increase debt, but it also gives borrowers more cash on hand. Increasing tuition increases debt, but does not give borrowers more cash on hand, since the increase in debt is used to pay tuition. Goodman et al. (2017) have an extensive discussion of liquidity versus debt effects. The two different sources of variation are useful in terms of different policy counterfactuals. For example,

Our paper contributes to this literature by providing causal evidence that tuition increases lead to increases in student debt and reductions in human capital accumulation. Our findings also highlight the role of sample selection in evaluating the relationship between student liabilities and various outcome variables. Specifically, we find that credit-constrained students drop out of schools when tuition increases, implying that studies based on students who have obtained a bachelor's degree could suffer from sample selection biases because credit-constrained students are more likely to be excluded from those studies.

Second, our work contributes to a literature on credit constraints and college completion. There is a 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 provide poor measures of which individuals are credit-constrained. For example, Carneiro and Heckman (2002) and Chakrabarti et al. (2020) 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) argue 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 dropout decision and that large tuition increases lead to the accumulation of additional debt and reduction in the probability of a graduate degree. At the same time, our findings indicate that both constrained and unconstrained students are less likely to attend graduate

if Congress is considering increasing federal loan limits, there would be shocks to both liquidity and debt, and hence the Black et al. (2020) variation is more appropriate. If Congress is considering loan forgiveness or other forms of effective price reductions, then our estimates are more appropriate. Additionally, they focus on a single state Texas and are unable to track students who leave Texas. In contrast, our sample is a representative nationwide sample and we can observe students regardless of their movement across state boundaries.

schools, indicating that a higher cost of undergraduate education deters investment in graduate degrees.

Finally, our paper contributes to a literature that studies the aggregate dynamics of human capital accumulation (Galor and Moav, 2004; Lustig and Van Nieuwerburgh, 2006; Lochner and Monge-Naranjo, 2011; Cordoba and Ripoll, 2013; Cadena and Keys, 2015; Ebrahimian and Wachter, 2020). We show that through its effect on debt, bachelors' completion and graduate school enrollment, tuition 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. In Section 6 we discuss implications for household debt, delinquencies, and graduate enrollment and we conclude in Section 7.

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, attainment and debt

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 anonymized 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). In addition to student debt, we observe other forms of consumer debt over time for each student, such as mortgage debt, credit card debt and auto debt. For each of these forms of debt, we also observe delinquencies over time. 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.⁶

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 in time (public/private, 2-year/4-year). We identify as outcomes whether a student attained a bachelor’s degree and a post-bachelor’s 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 Title IV eligible institutions.

⁶Our dataset carries several advantages relative to other databases because it covers a larger number of individuals (225,000) and is representative of the whole population and of the college population every year. Moreover, our sample covers a balanced set of cohorts in that we see both young and older cohorts.

We measure an individual’s tuition bill as the sum of the realized sticker tuition for in-state residents as reported in the IPEDS data in the first four years following entry to his or her first undergraduate college. The use of this measure guards against a number of potential concerns. One possible effect of changes to tuition is that students drop out or transfer to a different school. Our way of defining tuition measures a student’s sticker tuition bill had they chosen to stay in their first school for four years. That is, 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. Additionally, 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 sticker tuition (i.e., “sticker price”) avoids potential biases that may arise from this effect.

We construct a sample of large tuition increases as follows. First, for each school, we identify the largest year-over-year tuition increase between 2000 and 2015. This way, for each school, we identify one tuition increase that had the largest economic effect on the cost of undergraduate education. Second, if the largest tuition increase exceeds \$1,000, we define the year of the largest tuition increase as the “event” year (for robustness, we consider thresholds of \$800, \$900, \$1,100, and \$1,200). The choice of \$1,000 as our baseline value of “large tuition” increase is to ensure that the magnitude is large enough to be economically meaningful.⁷ As we will show, the average yearly tuition increase in the sample of large tuition increase events is \$1,709, corresponding to a 15.47% change in the tuition (see table 1). Thus, these tuition increases are not likely to be routine tuition increases.

⁷To emphasize the importance of this step, we also considered \$100 threshold in the definition of the largest tuition increase. The results in Internet Appendix Table A7 indicate weak first stage results. Indeed, the instrument does not pass the F -test criteria because the F -statistic is 4.267, which is clearly below 10. Due to the weak instrument problem, the estimates of the effects on *Debt* and *Graduate School* are considerably larger than for larger tuition increases. This is precisely why we adopt the second step in the definition of a large tuition increase and require the largest tuition increase to be also large in absolute sense.

2.3. Zip code income and college selectivity

We observe detailed measures of geography for each individual in each quarter from our CCP data. While we do not observe each individual's income, we use 2001 zip code earnings data from the Internal Revenue Service (IRS) to create measures of neighborhood income at the point where we first see the individual in the 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 zip code of origin as their home zip code. We match 2001 income data at the individual's home zip code level to obtain a measure of their home zip code income. We use home zip code 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 in the literature, we group the colleges in the top three most selective 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 only consider students in our sample whose first enrollment was a 4-year undergraduate college. Thus, our sample is a random sample of 4-year undergrads whose first college was a 4 year college during 2000-2015. Our strategy entails dropping non-college goers and students whose first college was a 2-year or less than 2-year 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. 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 zip code income is \$69,039 per year on average. Figure A3 shows that the median home zip income exhibits little variation across sample period. 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 bachelor's 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 bachelor's degree within six years. The average outstanding student debt balance after the first 4 years since 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 a 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 26% of students attend schools that increase tuition

by \$1,000 or more in a given year during the sample period. 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, 779 private non-profit, and 101 private for-profit) and academic years.

Figure 1 shows the average tuition bill in the top panel and student debt four years after entry in the bottom panel for cohorts that entered school during our sample period. 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 large tuition increases. The average number of schools per year exposed to a large tuition increase is 92. We see a roughly uniform distribution over time, with notable spikes in 2007 and 2009 (for robustness, we report main results when we drop large tuition increases in 2005, 2007, 2009, and 2011).⁸ In the Internet Appendix, we show the distribution of students by college entry cohort (Figure A1), grade (Figure A2), and state (Table A2). The grade refers to the number of years after entry where the student is at the time of the large tuition increase. A student in his or her second year after the entry is in grade 2, in the third year after the 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.

Figure 3 shows the distribution of largest tuition increases. We find that most of largest tuition increases are between \$100 and \$2,000. For some schools, the largest tuition

⁸Public 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 in 2009 (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).

increase is larger than \$2,000. As indicated above, in our main specification we use a \$1,000 threshold for the largest tuition increase to be an event. We perform two robustness tests to show that the choice of this particular threshold is inconsequential. First, Figure 3 shows no jump in the distribution of largest tuition increases around the \$1,000 threshold. Second, for robustness, we consider thresholds of \$800, \$900, \$1,100, and \$1,200 threshold and find similar results (see section 4.1).

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 differential exposure by students to large tuition increases.

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. $\text{Tuition}_{c(i)j(i)}$ represents the 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 " $\text{Tuition}_{c(i)j(i)}$ " as "*Tuition years 1-4*" to make it more explicit that it represents the total tuition bill of an individual during the first four years after his or her 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 outcome variables of interest 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 bachelor’s 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 college level.⁹

These regressions account for average differences in the level of tuition across colleges and for differences in average tuition over time within colleges. The table documents significant associations between all outcome variables and *Tuition*. For example, a \$5,000 higher tuition level is associated with a 1.88% lower probability of obtaining a graduate degree, a 1.06% lower probability of graduating with a bachelor’s degree, and a 0.61% higher probability of transferring between schools. These effects are not negligible relative to the outcome means, ranging from 14% of the sample mean for the probability of obtaining a graduate degree to 2.30% of the sample mean for the probability of obtaining a bachelor’s degree. Moreover, \$5,000 in higher tuition levels is associated with a \$245 increase in student debt balances measured four years after entry, representing almost 1.8% 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 and an increase in student’s debt

⁹We 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.

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 graduate school, and less likely to take on debt. Further, education quality improvements could lead, rather than lag tuition increases.

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 who faced different tuition bills 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 increases

Our main concern is that schools that experience large tuition increases 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.¹⁰ In our empirical strategy, we therefore compare outcomes for students who are enrolled in different cohorts at a school that experiences a large tuition increase. We implement this strategy by comparing students, already enrolled, who are affected by the same shock, but in different cohorts. We exploit the fact that students earlier in their academic career will be more affected by a tuition increase. For example, if a school raises tuition, second-year students will pay for two additional years

¹⁰Indeed, Internet Appendix Table A1 shows that students in schools exposed to large tuition increases come from higher-income neighborhoods, 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.

relative to fourth-year students.

We define *grade* (g) of a student as his/her number of years since enrollment at the time the large tuition increase occurs. Intuitively, when a tuition increase happens, a lower grade implies that the student faces a larger number of years paying higher tuition and is therefore more exposed to the tuition increase. 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 effect of tuition on outcomes.

We run two-stage least squares regressions (2SLS) where the second stage corresponds to an augmented version of equation (1):

$$y_i = \beta \text{Tuition}_{c(i)j(i)} + \gamma_{c(i)} + \gamma_{j(i)} + \gamma_{j(i)} \times \mathbb{1}_{g(i) \in \{2,3,4,5\}} + u_i, \quad (2)$$

and the first-stage regression is given by:

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

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 increase in a school that faces such an increase in tuition, $\gamma_{j(i)}$ 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 increase, and use the group of students who are in grade 5 at the time of the tuition increase 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 one, 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 5 years before the tuition

increase to increase comparability.¹¹

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 increase, $\gamma_{j(i)} \times \mathbb{1}_{g(i) \in \{2,3,4,5\}}$. These modified fixed effects are also included in the second stage equation (2). Students in schools exposed to large tuition increases who are not in grades between 2 and 5 help identify cohort fixed effects, but they do not affect the coefficients of interest, π_τ . Importantly, this choice of fixed effects implies that the estimates are based on the variation within the group of students who are in grades 2 through 5 at the time of the large tuition increase.

We next show that the identification is not coming from tuition increases per se, but rather from how tuition increases affect different cohorts. To show that, we repeat the estimation of regression (2), while replacing *Tuition* with an indicator for students in grades 2 through 5 at the time of a large tuition increase, $\mathbb{1}_{g(i) \in \{2,3,4,5\}}$, thus it takes a value of 1 for students who are 2, 3, 4, 5 years away from entry in the school at the time of the tuition shock. All other students get a value of zero. Note that, as explained above, the main 2SLS specification is based on the variation *within* this group of students, who are all exposed to a large tuition increase to a varying degree. In contrast, in this specification we treat grades 2 through 5 symmetrically. An insignificant coefficient for $\mathbb{1}_{g(i) \in \{2,3,4,5\}}$ would imply that outcomes for this group of students are indistinguishable from outcomes for other students within affected schools (because the regression includes school of entry fixed

¹¹Note that students admitted 5 years prior to a large tuition increase are assigned to grade 5 regardless of whether they have completed the program by that time. Using actual graduation time for the purpose of grade assignment (e.g., comparing students who remain in a program longer and students who complete a degree on time) could lead to several identification issues. This is precisely why our definition of grade is not based on whether a student actually remains in a program a certain number of years. Instead, we use predicted/simulated grades, calculated based on the admission year. Students admitted 5 years prior to a large tuition increase are assigned to grade 5 regardless of whether they have completed the program by that time, dropped out or transferred to another school. In other words, we count 5 years from the entry year of students in a school regardless of where the student is in grade 5 or what the graduation status of the student may be.

effects). Indeed, Internet Appendix Table A5 shows no significant differences in outcome variables for these students and other students within affected schools. Therefore, it is not the exposure to a tuition increase, but rather the differential exposure to a tuition increase drives the results. These results show that our inferences are based on cohort dummies that capture the differential effect of large tuition increases on cohorts within the group of students in grades 2 through 5 at the time of a large tuition increase.

Our empirical strategy recovers the causal effect of tuition increases 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, and acknowledge potential limitations.

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 (3)) are also plotted in Figure 4. 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 a large tuition increase in year 2 after her initial enrollment ends up with about \$6,270 in a higher four-year tuition bill than a student exposed to the same tuition increase in year 5 (the omitted category). As Figure 4 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. An F -test indicates

that the instruments pass conventional rule of thumb F -statistic tests.

Moreover, as is evident from the R^2 reported in Table 3, across-grade differences in exposure to large tuition increases and the cohort and school fixed effects 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 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 large tuition increase, 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. While this restriction is an assumption, 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 (about 4,650 in total) and this number does not exhibit substantial variation across grades. Thus, we do not find evidence indicating strategic bunching of

students across different grades. This finding is plausible, because in our analysis, students who are affected by the large tuition change need to be already enrolled at the time of a large tuition change. Hence, following the example above, for selection concerns to drive the results, Adam and Alex would need to have differential expectations about the precise timing of large *future* tuition changes when they were making the decision to enroll, which is unlikely given the facts that tuition changes have not happened yet and they will often depend on economic and political factors that have not yet materialized and students typically are poorly informed about financial matters (Brown et al., 2016; Mueller and Yannelis, 2019b).

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 increase, 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 increase 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 (3) 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 type (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 large tuition increases 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.¹²

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 large tuition increases 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 levels because they enrolled in earlier cohorts.

Potential threats to identification primarily come from selection. For example, educational quality improvements could lead, rather than lag tuition increases. If students

¹²In columns 4 and 5 the regressions do not include school fixed effects, implying that coefficients are capturing cross-school differences. Indeed, private school tuition is higher and rose at a considerably higher rate, implying that the tuition hikes are less likely for public school students, as column 4 in table 3 shows. Similarly, selective schools have higher tuition and higher tuition increases, which is consistent with positive numbers in column 5. The picture in columns 4 and 5 is also consistent with the summary statistics in Internet Appendix Table A1, where we find that in our sample, public schools were less likely to be exposed to large tuition increases than private schools and selective schools were more likely to face such increases compared to less selective schools.

select colleges based on these improvements, and these are correlated with outcomes like graduate student enrollment, then effects may not be driven by a causal effect of tuition increases. Further, we cannot fully rule out that there are quality improvements contemporaneous with tuition changes, immediately affecting outcomes. In section 5 we assess these threats by exploiting the timing of tuition increases and changes in education quality.

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 (2) and the first-stage corresponds to equation (3).

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 the first college. This effect is both economically and statistically significant. A \$5,000 increase in a student's tuition bill translates into a \$1,745 increase in student debt, suggesting that about 35% of a tuition bill increase is financed through student debt. Between 2000 and 2017, the average tuition bill increased by more than \$20,000 (figure 1). Assuming an undergraduate student population of 15 million students, our estimates imply that this tuition increase resulted in about \$105 billion in additional 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 \$5,000 increase in tuition bill causes the probability of completing a graduate degree to drop by 2.59 percentage points. The effect is significant, at the 5% level. Thus, a \$5,000 increase in tuition bill can essentially reduce the probability of completing a graduate degree

by more than 22%, implying that thousands of students did not enroll in graduate schools due to higher undergraduate tuition bill.

Columns 3 and 4 turn to human capital accumulation at the undergraduate level. Column 3 shows a small and statistically insignificant positive effect of tuition on the completion of a *Bachelors* degree. Similarly, column 4 shows a positive but insignificant effect of tuition on *Transfers*. The absence of an effect on transfers is consistent with equal enrollment faced by grades 2,3,4 as seen in Internet Appendix Figure A2.¹³ Our results indicate that there is no effect of tuition on *Bachelors* and a negative effect of tuition on *Graduate school*. These two results are not inconsistent with each other. Bachelors may be considered a basic/core degree, so despite tuition shocks, students continue to earn this degree (column 3) and foot the higher tuition bill with higher student debt (column 1). However, because of the higher tuition, a lower share of these students go on to pursue higher education (column 2). Thus a higher tuition leads to a lower probability of graduate education (column 2). The students pursuing graduate education rack up a higher volume of debt compared to what they would have accrued in the absence of a tuition shock thus further adding to the debt volume (column 1).

¹³Internet Appendix Table A3 presents the reduced form relationship between instruments and outcome variables. The results show that students with greater exposure to tuition increase in grades 2, 3 and 4 (relative to students in grade 5, which is the omitted category) accumulate more debt and are less likely to complete a graduate degree. The effects on the completion of a *Bachelors* degree are insignificant across all instruments. The effect on *Transfers* is positive and weakly significant for one out of three instruments. It is worth noting that the coefficients corresponding to the three grades (grades 2, 3, 4) do not follow the monotonic pattern we observe for the first stage results (table 3, column 1). The reason is that the coefficients corresponding to the grades in the reduced form not only reflect the direct causal effect of tuition shock on an outcome variable, but also the causal effects of other outcome variables that are affected by the tuition shock. Consider, for example, the coefficient of Grade 4 in column 1 in Internet Appendix Table A3. This coefficient reflects not only the direct causal effect of tuition on debt, but also the causal effect of tuition on Transfers and therefore Debt. That is, by responding to the tuition increase with a decision to transfer to a different school, students affect their student debt balances. Therefore, because students can respond to tuition increases in several ways (e.g. increase debt, transfer to a different school), the reduced form estimates may not necessarily be monotonic. For the coefficients in column 1 to be monotonic, one would need to assume that Debt is the only outcome variable that can be affected by tuition increase. Similar condition would need to hold for other outcome variables. Our empirical strategy does not require making this implausible assumption.

These results suggest that the effects of tuition on graduate studies are not driven on average by a failure to complete a bachelor’s 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), Stinebrickner and Stinebrickner (2008), and Chakrabarti et al. (2020) 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 high-income subsamples in the next section.

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. Note that the step that involves the \$1,000 threshold merely determines whether the largest tuition increase experienced by a school is large enough to be economically meaningful. We show that our results are robust to different tuition increase thresholds. 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 the magnitudes 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.

4.2. Robustness: the timing of large tuition increases

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. Figure 2 shows that some of the sample years—namely 2005, 2007, 2009, and 2011—exhibit higher frequencies of large tuition increases. We next show that our results are not driven by any of these large tuition increase clusters by estimating equation (2) after excluding large tuition increases in each of these years.

Table 6 reports the results. The evidence shows that the effects of tuition on *Debt* and *Graduate school* do not alter signs and remain statistically significant. Importantly, the economic magnitudes of the coefficients are similar to magnitudes 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 exclude large tuition increases in these respective years from our sample.

Another potential concern is that some of the outcome variables are censored for students who were enrolled during the later part of our sample period and then were exposed to a large tuition increase. For instance, the measurement of graduate school outcomes could be different for students who were exposed to a large tuition increase in 2002 than for students who were exposed to a large tuition increase in 2012—we would see the 2002 cohort for a long enough time to observe their graduate school enrollment (if any) while this may not be the case for the 2012 cohort, leading to a censoring of the graduate school outcome for this cohort.

To address this concern, we repeat the analysis while considering large tuition increases events during 2000-2010 period. That is, we retain the full sample while ignoring large tuition increases during the later part of the sample period. The latest cohort exposed to tuition increase in this analysis is the cohort that was in grade 2 at the time of the 2010

increase, that is the cohort that entered school in 2009. Since our data go until 2015, the 2009 entry cohort has a fair amount of time to graduate. So censoring should not be an issue here. Internet Appendix Table A6 reports additional results that are consistent with the main results, suggesting that censoring of outcome variables does not have a large effect on the analysis results.

4.3. Robustness: falsification test

We next ask whether there are differences in the dynamics in outcome variables across grades prior to the year of the large tuition increase. To address this question, we replace the year of the largest tuition increase, t , with $t - 5$. If there are differential trends in outcome variables between students in different cohorts prior to the event year, the placebo test will estimate significant effects on outcomes.

Table 7 reports the results and shows no differences in *Tuition* across grades. Further, we find no effects of instrumented tuition on the outcome variables. This placebo test therefore mitigates the concern that there are built-in differences between grades that can drive the results in the absence of exposure to a large increase in tuition.

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, higher tuition levels 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 a tuition increase may receive a more valuable undergraduate education. In turn, this might change the probability of graduation as well as the probability of enrolling in a postgraduate degree (e.g., Black et al., 2005; Black and Smith, 2006; Dillon and Smith, 2020).

Second, higher tuition could reduce investments in human capital as long as the demand for human capital is not inelastic to the cost of education. For instance, students exposed to a large tuition increase may transfer to less expensive institutions or drop out. Importantly, even if these students complete the bachelor's degree they are already enrolled in, they could reduce investments in graduate degrees. Since large tuition increases affect the cost of undergraduate education but not the cost of graduate education, the effect of higher tuition on graduate degree attainment would suggest a *dynamic* relation between tuition and investments in human capital.¹⁴

Third, students may be credit-constrained and unable to secure the funds necessary to finance their education and other expenses while they study. Similarly to the cost of education mechanism, credit constraints may induce students to transfer to less expensive institutions or drop out. Alternatively, these students may complete the bachelor's degree they are already enrolled in, but reduce their investment in graduate degrees.

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

¹⁴We argue that large tuition increases are not likely to affect the cost of graduate education. First, a student who is enrolled in a bachelor's degree at the time of a large tuition increase can apply to any graduate school and not necessarily to the school that experienced a large tuition increase. Moreover, since students typically spend several years in the labor force before returning to a graduate school, heterogenous cohort-based exposures to large tuition increases are likely to wash out by the time students consider the decision to obtain a graduate degree.

of higher tuition on human capital accumulation might be mitigated by increasing 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 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 experiences a large tuition increase to another school. We conduct the matching based on the minimal Euclidean distance using lagged tuition and lagged total enrollment within the same academic year, state, and institution 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 5 we plot the evolution of average tuition in dollars for schools with a large change and the matched sample. The figure shows that the differences in average tuition across these two types of schools were stable in years before the large tuition change. On the other hand, schools with large changes (gray bars) increase their tuition discontinuously in event year zero, and continue to show relatively higher tuition in the next three years. After the initial jump at period 0, the differences in tuition remains stable across years 0-3 indicating that after a large tuition increase, tuition remained stable over years and there was no clustering of tuition shocks. This suggests that before schools go through large tuition changes, differences in tuition across these schools and other schools remain stable.

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

$$Y_{j\kappa} = \alpha_{c(j)} + \gamma \text{Large Change}_j + \sum_{\kappa=-3}^3 \beta_{\kappa} \text{Large Change}_j \times \delta_{\kappa} + \sum_{\kappa=-3}^3 \delta_{\kappa} + \omega_t + \epsilon_{jt}, \quad (4)$$

where Y corresponds to several outcomes available in the IPEDS data. The coefficients of interest are the β s, 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 include matched pair fixed effects $\alpha_{c(j)}$ to measure differences with respect to the matched pair, as well as event year (δ_{κ}) and year (ω_t) fixed effects. Standard errors are clustered at school year level.¹⁵

Results are presented in Table 8. 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 8 replicates Figure 5, and shows that tuition increases by approximately \$1,000 following a large tuition change and is roughly maintained in the following years. Moreover, we find no difference in tuition dynamics for the two groups of schools prior to the year of a large tuition increase.

Next, we consider two outcome variables that indicate how schools allocate resources obtained from large tuition increases. Columns 2 and 3 report changes in expenditures in instruction, and expenditures in research (measured in units of dollars per student).

Whereas we find that expenditure in research does not change in a statistically significant way following the tuition change, expenditure in instruction does seem to pick up following the tuition change. Thus, our findings indicate that tuition increases could lead to improvements in the quality of education. Improvements in the quality of education

¹⁵Furthermore, Internet Appendix Table A8 shows that we obtain similar results when we use double-clustered (at school and year levels) standard errors.

would predict better student outcomes, which we consider next. Using the fraction of students that take on debt as the dependent variable, column 5 shows that the economic magnitude of the coefficients does not change before and after a large tuition increase. For instance, the coefficient of $Large\ Change_j \times \delta_{-2}$ is very similar to the coefficient of $Large\ Change_j \times \delta_1$. In columns 6 through 11 of Table 8 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 the 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 experience a large tuition increase have tuition at a similar level to tuition of matched schools prior to large tuition increases. Importantly, schools do not seem to observably change practices in a way that would predict heterogeneous treatments across students in different cohorts in a manner consistent with our results. Whereas we find that schools with relatively large tuition increases do not change expenditure in research in a statistically significant way, expenditure in instruction does seem to pick up following the tuition change. This suggests that tuition increases could lead to improvements in the quality of education, which would predict better student outcomes. Our findings suggest, however, that the increase in quality of education is not sufficient to compensate for the negative effects of credit constraints and the cost of education on investments in human capital.¹⁶

¹⁶While there is some controversy, many studies argue that increases in expenditure improve education quality. For example, Cellini et al. (2010) and Jackson et al. (2016) argue that educational spending increases the quality of education. There is also a large literature on class size and student outcomes, largely finding positive effects of smaller class sizes (Angrist and Lavy, 1999). Higher funding amounts can be used to hire more faculty, and lower class sizes. For the relationship between the quality of the post-secondary education and future outcomes see Black et al. (2005), Black and Smith (2006) and Dillon and Smith (2020).

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 (2) 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:

$$y_i = \beta_0 \text{Tuition}_{c(i)j(i)} + \beta_1 \text{Tuition}_{c(i)j(i)} \times \text{Low Income}_i + \gamma_{c(i)} + \gamma_{j(i)} + u_i. \quad (5)$$

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 estimate two first stage regressions, one for each endogenous variable. First, we augment the first stage regression (3) where the excluded variables include the standard indicators of the year after entry at the time of a tuition shock, with variables that interact these indicators with Low Income:

$$\text{Tuition}_{c(i)j(i)} = \sum_{\tau=2}^4 \pi_{\tau} \mathbb{1}_{g(i)=\tau} + \sum_{\tau=2}^4 \pi_{\tau} \mathbb{1}_{g(i)=\tau} \times \text{Low Income}_i + \gamma_{c(i)} + \gamma_{j(i)} + \gamma_{j(i)} \times \mathbb{1}_{g(i) \in \{2,3,4,5\}} + \varepsilon_i. \quad (6)$$

In the second first stage regression, we estimate specification (6), where the dependent variable is $\text{Tuition}_{c(i)j(i)} \times \text{Low Income}_i$:

$$\begin{aligned} \text{Tuition}_{c(i)j(i)} \times \text{Low Income}_i &= \sum_{\tau=2}^4 \pi_{\tau} \mathbb{1}_{g(i)=\tau} + \sum_{\tau=2}^4 \pi_{\tau} \mathbb{1}_{g(i)=\tau} \times \text{Low Income}_i \quad (7) \\ &+ \gamma_{c(i)} + \gamma_{j(i)} + \gamma_{j(i)} \times \mathbb{1}_{g(i) \in \{2,3,4,5\}} + \varepsilon_i. \end{aligned}$$

Table 9 reports second stage 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 between low-income students and high-income students for *Bachelors* and *Transfers*. Specifically, the interaction term indicates that a \$5,000 increase in a student's tuition bill translates into a decrease of 0.70 percentage points in the likelihood of graduation with a bachelor's degree and 0.66 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 differentially affects both the likelihood of graduating with a bachelor's degree and the likelihood of a transfer between schools for poorer and richer students.¹⁷

We would like to note that the results for *Bachelors* and *Graduate school* for low income are also not inconsistent with each other. It is possible that a large tuition increase reduces the probability of Bachelors degree for low income students relative to high income students (column 3). At the same time, the low income students that earn Bachelors are more likely to pursue a graduate degree (which makes sense as they are motivated enough to earn Bachelors despite higher tuition burden), so there is no difference in the probability of graduate education between low and high income students and both type of students, on average, reduce graduate education similarly (column 2). Thus, the results in columns (2) and (3) are consistent with each other and can be explained by such a composition effect.

This result suggests that students from low-income neighborhoods and high-income neighborhoods respond differently to higher tuition for undergraduate outcomes. 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 bachelor's degree and are significantly more likely to transfer to other schools. These changes take place despite the fact that schools increase spending on

¹⁷Estimates of the reduced form model are reported in the Internet Appendix Table A4.

instruction and therefore are likely to offer higher quality education.¹⁸

Our findings show that for low-income students (relative to high-income students) debt levels do not increase, while bachelors completion rates and transfers are impacted. One possibility is that low-income students are credit constrained and have already maxed out their federal loan and other borrowing options. These credit constrained students may need to drop out and transfer to cheaper institutions, or institutions that offer financial aid. The negative impact on bachelors degrees is commensurate with the increase in transfers, which is consistent with credit constraints being an important channel.¹⁹

This finding suggests that limited financial resources are likely to contribute to the negative effect of higher tuition on undergraduate education for students from low-income neighborhoods. 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 who are likely to be financially constrained. This finding has important implications, and suggests that tuition increase can widen both educational and economic inequalities.

6. Implications for Household Debt, Delinquencies, and Gradual Enrollment

6.1. *Implications for Household Debt and Delinquencies*

We next turn to the effects of tuition increases on short and medium-term household finances. Table 10 presents estimates of our main specification, in which the outcomes

¹⁸Since the data on net tuition paid by students is not available at the individual level, we have to work with sticker tuition. Additionally, net tuition is heavily dependent on characteristics of students and hence likely to be confounded with student characteristics, which is not the case with sticker tuition. There is a possibility, however, that the difference between sticker price tuition and actual tuition could drive these results. The results for students from high-income neighborhoods—who are less likely to qualify for need-based aid and thus are more likely to pay the full sticker price—are remarkably similar to the main results, suggesting that the bias due to the difference between sticker price and actual tuition is not large.

¹⁹Untabulated results suggest that students are not going to worse schools, as defined by 2-year institutions or less selective schools.

are replaced with indicators of debt balances and financial distress. The first three rows explore mortgage loans, the next three rows explore credit card debt, while the final three rows show results for auto loans. Each triplet first explores the extensive margin of debt in the first row. The second row presents balances, while the third row presents results regarding delinquency. The first column shows results eight years after college entry, the second column shows results six years after college entry, while the third column shows results four years after entry, and the final column shows results at age thirty.

We begin the analysis from mortgages. Eight years after enrollment, we see a 4.2 percentage point reduction in having a mortgage. This suggests that a tuition increase of \$5,000 reduces the probability of holding a mortgage by 2.1 percentage points. There is a slightly larger effect at age 30. Both effects are significant at the five percent level. Effect sizes are small four and six years after college entry, and statistically insignificant. We see similar effects for mortgage balances, although the effects are only significant at conventional levels at age thirty. We do not find effects on mortgage delinquencies, and our standard errors can rule out economically meaningful effect sizes. Thus, our findings are consistent with higher tuition having a negative effect on the likelihood of having a mortgage as well as mortgage balances.

Turning to credit cards, we do not see a significant effect on opening credit cards, probably because most of individuals in our sample have a credit card (see table 1). We see a significant negative effect on credit card balances at age 30, and insignificant negative point estimates four, six, and eight years post enrollment. We also see positive effect on delinquency, which are significant six and eight years post enrollment. An additional \$5,000 in tuition is associated with a 2.4 percentage point increase in credit card delinquency eight years post enrollment. The results for credit card outcomes are consistent with lower consumption due to higher student debt payments.

Finally, we consider the effects of tuition on auto loans. We find no effect on the likelihood of having an auto loan. We see marginally significant effects on auto loan balances eight years post enrollment. Similar to the effects on credit card delinquency, we document positive effects of tuition on auto loan delinquency, which are significant six and four years post enrollment. An additional \$5,000 in tuition is associated with a 1.05 percentage point increase in auto loan delinquency six years post enrollment.

Overall, the results are generally indicative of higher tuition leading to lower consumption and more financial distress. Therefore, higher tuition not only reduces investments in human capital and leads to higher student debt balances, but also adversely affects a wide range of household finance outcomes. Our analysis suggests that education acts as a mechanism leading to lower durable and non-durable consumption later in life when there is a large tuition increase faced by students in college.

While some of our effects are ambiguous or underpowered, they largely point to tuition increases reducing consumption. This is consistent with higher payments leading to less available cash on hand to make payments on other loans, or to finance consumption. For example, Mueller and Yannelis (2019b) find that lowering student loans payments lowers delinquencies and increases auto lines. Di Maggio et al. (2017) find that lower interest payments also increase consumption.

6.2. Implications for Graduate Enrollment

The results show that tuition increases have a significant negative effect on the number of students that complete a graduate degree. Figure 1 shows the average tuition bill for cohorts that entered school during our sample period and indicates that tuition has risen by roughly 50%. Given such a significant increase in tuition, we next reconcile these results with the overall trend in graduate enrollment. We exercise caution when making such analysis because our regressions include cohort fixed effects, implying that the estimates

cannot be affected or explained by aggregate trends in post-bachelor’s education.

Internet Appendix Figure A4 plots the number of post baccalaureate students in the U.S. The figure indicates that there was an increase in the number of graduate students during 2000-2010 period (the first half of our sample period). This increase was driven by U.S residents, rather than international students. During 2010-2019, the number of U.S. residents slightly declined and then rebounded, whereas the number of international students—who are more likely to have sufficient financial resources to finance graduate education—increased. Thus, higher tuition and the resulting high levels of student debt could contribute to the stagnation in the growth of the number of U.S. residents going to graduate school.

7. 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. 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.

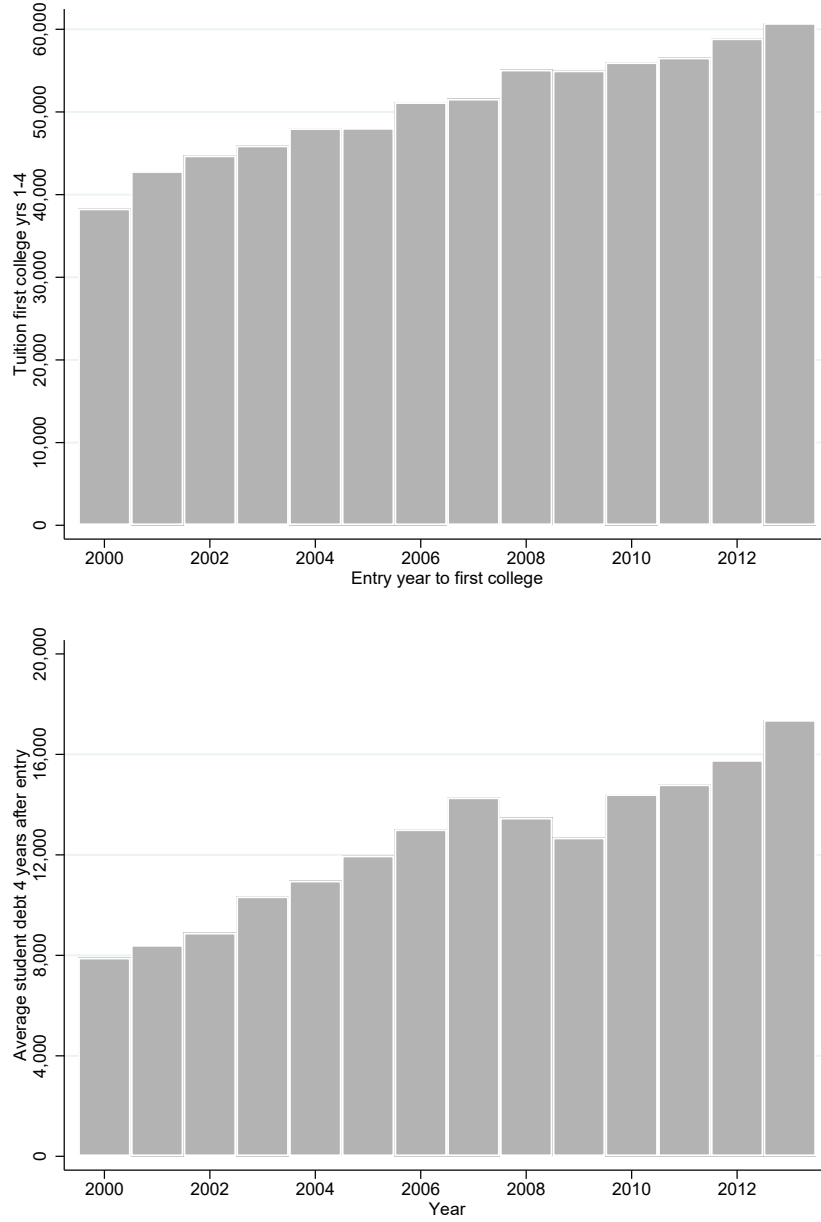
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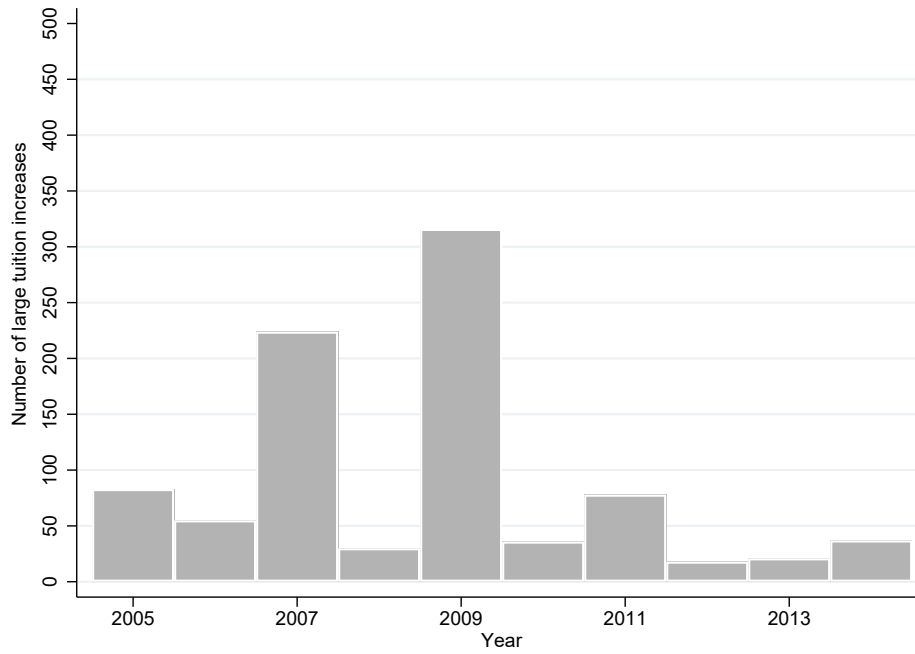
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Figure 1: **The evolution of 4-year tuition bill and student debt.**



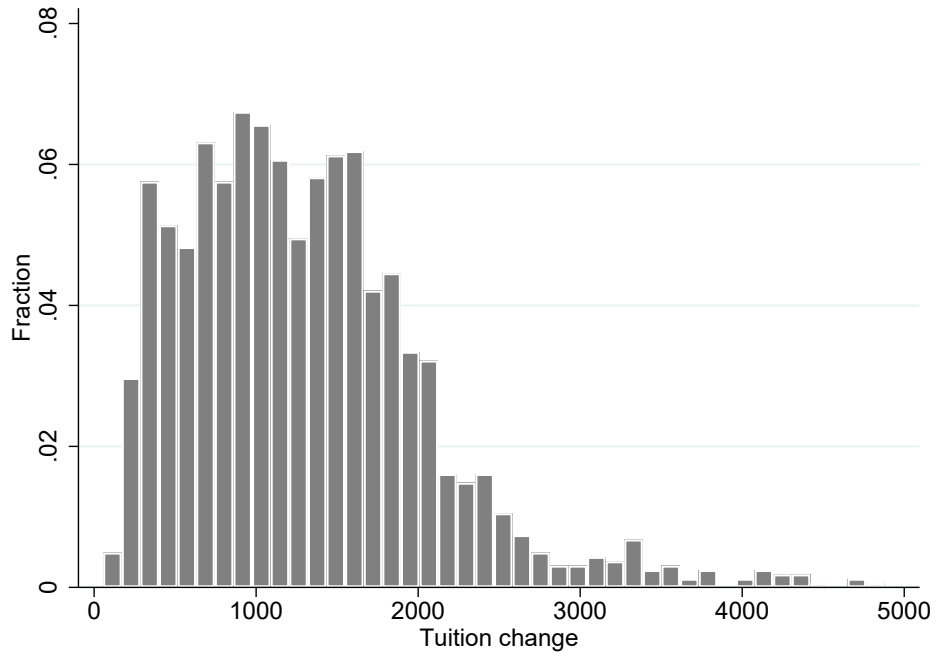
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows changes in average 4-year tuition bill (top panel) and student debt (bottom panel) during our sample period.

Figure 2: Number of large tuition increases by year.



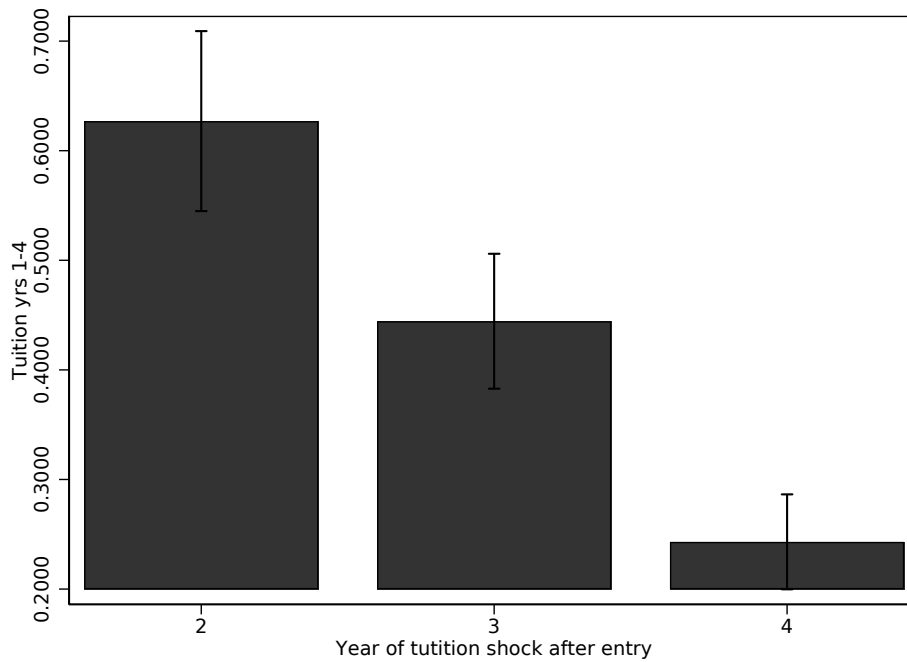
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows yearly frequency of large tuition increases in our sample.

Figure 3: **The size of large tuition increases.**



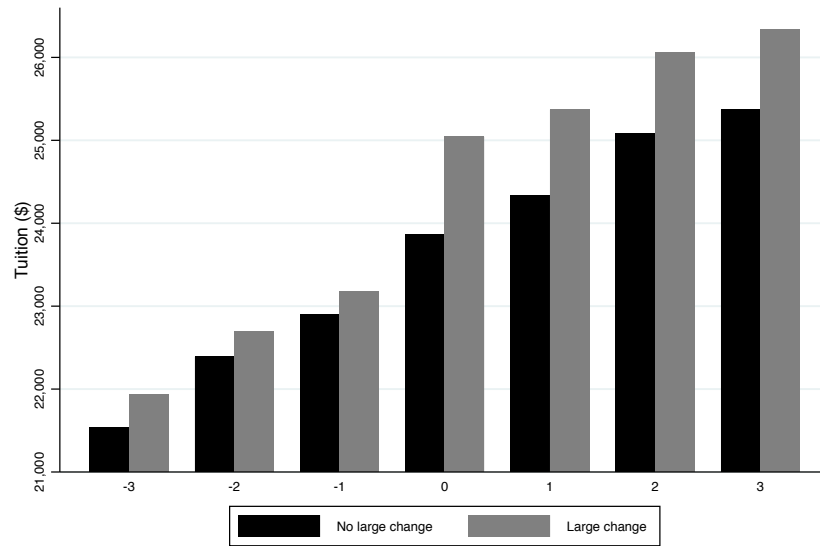
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows the distribution of large tuition increases in our sample.

Figure 4: **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 5: 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. Gray bars correspond to schools that experience a large tuition increase. Black 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.

Table 1: **Descriptive statistics.**

Variable	Mean	SD	Median	N
<i>Panel A: Demographic and School-level variables</i>				
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
<i>Panel B: Tuition variables</i>				
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 a large tuition increase	1,709.9	851.5	1,556.4	13,895
Percent yearly tuition change, conditional on a large tuition increase	0.1547	0.4348	0.0912	13,895
<i>Panel C: Household debt variables (measured as of age of 30)</i>				
Has Mortgage	0.2451	0.4302	0.0000	48,533
Mortgage Balance	34,867	95,185	0	48,533
Delinquent on Mortgage	0.0123	0.1105	0.0000	48,533
Has Credit Card	0.8798	0.3253	1.0000	48,533
Credit Card Balance	2,551	5,855	0	48,533
Delinquent on Credit Card	0.2235	0.4166	0.0000	48,533
Has Auto Loan	0.6193	0.4856	1.0000	48,533
Auto Loan Balance	5,120	11,019	0	48,533
Delinquent on Auto Loan	0.0622	0.2415	0.0000	48,533

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:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
<i>Tuition yrs. 1-4</i>	0.0490* (0.0256)	-0.0376*** (0.0039)	-0.0212*** (0.0045)	0.0121** (0.0054)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,175	46,175	46,175	46,175
R^2	0.15	0.15	0.36	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:	<i>Tuition yrs 1-4</i>	<i>Age entry</i>	<i>Median income</i>	<i>Public school</i>	<i>Selective</i>
	(1)	(2)	(3)	(4)	(5)
Grade 2	0.6267*** (0.0419)	-0.0579 (0.1231)	0.1028 (0.1717)	-0.3654*** (0.0375)	0.1292*** (0.0383)
Grade 3	0.4440*** (0.0314)	-0.0854 (0.1360)	0.1574 (0.1505)	-0.3424*** (0.0368)	0.1201*** (0.0366)
Grade 4	0.2428*** (0.0221)	-0.0183 (0.1139)	0.0036 (0.1619)	-0.3482*** (0.0430)	0.1268*** (0.0386)
Fixed effects:	Cohort, School	Cohort, School	Cohort, School	Cohort	Cohort
Observations	46,040	46,040	46,040	46,288	46,288
R^2	0.99	0.20	0.20	0.05	0.01
F -test	128.438	0.174	0.498	0.667	0.120
p -value	0.00	0.84	0.61	0.51	0.89

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 (3). 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:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
Tuition years 1-4	0.3491*** (0.1344)	-0.0518** (0.0244)	0.0052 (0.0272)	0.0263 (0.0286)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,040	46,040	46,040	46,040

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 2 and the first-stage corresponds to equation 3. 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, 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:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
<i>Panel A: \$800 increase</i>				
Tuition yrs. 1-4	0.3776*** (0.1397)	-0.0633** (0.0255)	0.0280 (0.0298)	0.0067 (0.0302)
Observations	45,990	45,990	45,990	45,990
<i>Panel B: \$900 increase</i>				
Tuition yrs. 1-4	0.4151*** (0.1374)	-0.0575** (0.0252)	0.0195 (0.0293)	0.0163 (0.0299)
Observations	46,015	46,015	46,015	46,015
<i>Panel C: \$1,100 increase</i>				
Tuition yrs. 1-4	0.3717*** (0.1307)	-0.0510** (0.0236)	0.0113 (0.0258)	0.0194 (0.0269)
Observations	46,038	46,038	46,038	46,038
<i>Panel D: \$1,200 increase</i>				
Tuition yrs. 1-4	0.3165** (0.1320)	-0.0486** (0.0236)	0.0127 (0.0250)	0.0305 (0.0268)
Observations	46,055	46,055	46,055	46,055
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School

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, 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: **Robustness to the timing of large tuition changes.**

Dependent variable:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
<i>Panel A: Exclude large tuition increases that took place in 2005</i>				
Tuition yrs. 1-4	0.3754** (0.1512)	-0.0510* (0.0264)	0.0158 (0.0300)	0.0171 (0.0319)
Observations	46,042	46,042	46,042	46,042
<i>Panel B: Exclude large tuition increases that took place in 2007</i>				
Tuition yrs. 1-4	0.4646*** (0.1640)	-0.0618** (0.0282)	0.0133 (0.0333)	0.0120 (0.0338)
Observations	46,068	46,068	46,068	46,068
<i>Panel C: Exclude large tuition increases that took place in 2009</i>				
Tuition yrs. 1-4	0.3522** (0.1622)	-0.0264 (0.0280)	-0.0360 (0.0353)	0.0251 (0.0357)
Observations	46,067	46,067	46,067	46,067
<i>Panel D: Exclude large tuition increases that took place in 2011</i>				
Tuition yrs. 1-4	0.2577* (0.1360)	-0.0654** (0.0268)	-0.0078 (0.0276)	0.0337 (0.0305)
Observations	46,033	46,033	46,033	46,033
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School

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 drop one year of large tuition increases in each panel. 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 7: **The effect of tuition on human capital and debt accumulation: Placebo test.**

Dependent variable:	<i>Tuition yrs 1-4</i>	<i>Debt</i>	<i>Graduate school</i>	<i>Bachelors</i>	<i>Transfers</i>
	(1)	(2)	(3)	(4)	(5)
<i>Tuition yrs. 1-4</i>		-1.8404 (4.2644)	-1.0968 (1.4961)	-0.8047 (1.2860)	0.4072 (1.1040)
Grade 2 (placebo)	-0.0357 (0.0419)				
Grade 3 (placebo)	-0.0303 (0.0326)				
Grade 4 (placebo)	-0.0190 (0.0220)				
Fixed effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,150	46,150	46,150	46,150	46,150
<i>F</i> -test	0.185				
<i>p</i> -value	0.83				

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports the estimates of a placebo test, in which the year of a false large tuition increase is the year of large tuition increase minus five. This table reports estimates of two stage least squares regressions (2SLS) where the second stage corresponds to equation 2 and the first-stage corresponds to equation 3. First stage results are reported in column 1, where the dependent variable is *Tuition yrs 1-4*, which 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). 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 8: School-year level matched sample.

Dependent variable:	<i>Tuition</i>	<i>Instruction</i>	<i>Research</i>	<i>Completions</i>	<i>Loan pct ratio</i>	<i>Admit rate</i>	<i>Student fac</i>	<i>On-time</i>	<i>Fraction non-white</i>	<i>Fraction female</i>	<i>Sat-M-25</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<i>Large change</i> $\times \delta_{-3}$	103.31 (152.593)	100.76 (282.391)	166.38 (240.273)	0.00 (0.005)	-1.32* (0.749)	-0.01 (0.008)	-0.14 (0.923)	0.02** (0.006)	0.01 (0.008)	-0.00 (0.008)	6.48*** (2.213)
<i>Large change</i> $\times \delta_{-2}$	77.01 (133.107)	274.34 (246.914)	153.53 (237.127)	0.01 (0.005)	-1.49* (0.765)	-0.00 (0.007)	-0.70 (1.375)	0.01 (0.007)	0.00 (0.007)	-0.00 (0.007)	7.13*** (2.202)
<i>Large change</i> $\times \delta_{-1}$	94.25 (135.287)	274.19 (211.732)	109.19 (243.886)	0.00 (0.005)	-1.02 (0.786)	-0.01 (0.007)	1.09 (1.074)	0.01 (0.006)	0.01 (0.007)	0.00 (0.007)	6.26*** (2.267)
<i>Large change</i> $\times \delta_0$	1,096.22*** (103.475)	329.72 (217.359)	100.97 (246.576)	0.01 (0.007)	-1.00 (0.758)	-0.01 (0.007)	-0.82 (1.582)	0.02*** (0.006)	0.01 (0.007)	-0.01 (0.007)	6.44*** (2.247)
<i>Large change</i> $\times \delta_1$	852.98*** (129.570)	347.52* (207.500)	218.59 (231.101)	-0.00 (0.007)	-1.45** (0.722)	-0.01* (0.008)	-0.28 (0.433)	0.01** (0.006)	0.01* (0.007)	0.00 (0.007)	8.35*** (2.318)
<i>Large change</i> $\times \delta_2$	753.30*** (136.046)	440.64* (259.295)	139.64 (273.597)	0.00 (0.004)	-1.34* (0.750)	-0.01 (0.008)	-0.70 (0.429)	0.02*** (0.006)	0.01 (0.007)	0.01 (0.008)	6.56*** (2.308)
<i>Large change</i> $\times \delta_3$	655.19*** (109.046)	400.49* (214.509)	142.27 (284.123)	0.00 (0.004)	-0.74 (0.731)	-0.01 (0.008)	0.28 (0.399)	0.01** (0.006)	0.01 (0.007)	0.00 (0.008)	6.69*** (2.337)
Observations	15,107	15,086	15,089	15,102	14,775	12,809	11,194	13,674	15,102	12,411	9,890
R^2	0.967	0.601	0.564	0.346	0.588	0.535	0.306	0.652	0.724	0.534	0.828

55

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of regression (4) 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, 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_k are event year dummies, centered at zero the year of a tuition increase for schools with a large change. Outcomes include *Tuition*, the nominal dollar value of in-state tuition and fees for full-time undergraduates (Sticker price); *Instruction* the average expenditures in instruction measured in units of dollars per student; *Research* the average expenditures in research measured in units of dollars per student; *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; *On-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 Math 25th percentile score among admitted students. Standard errors are clustered at the school level and are reported in parentheses. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 9: **Heterogeneous 2SLS estimates: The role of income.**

Dependent variable:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
Tuition years 1-4	0.3526*** (0.1361)	-0.0520** (0.0244)	0.0086 (0.0271)	0.0212 (0.0286)
Tuition 1-4 x Low Income	-0.0371 (0.0319)	-0.0016 (0.0048)	-0.0139** (0.0068)	0.0131** (0.0061)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,040	46,040	46,040	46,040

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 instrumented tuition variable are interacted with the indicator of low income. Section 5.2 describes two first stage regressions, one for each endogenous variable, *Tuition years 1-4* and *Tuition 1-4 x 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, 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 10: The effect of tuition on household debt and delinquencies.

Outcome variable horizon:	<i>8 years after entry</i> (1)	<i>6 years after entry</i> (2)	<i>4 years after entry</i> (3)	<i>age 30</i> (4)
<i>Panel A1: Has Mortgage</i>				
<i>Tuition yrs. 1-4</i>	-0.0417** (0.0202)	-0.0268 (0.0167)	-0.0030 (0.0148)	-0.0689** (0.0282)
<i>Panel A2: Mortgage Balance</i>				
<i>Tuition yrs. 1-4</i>	-5,989 (5,254)	-4,589 (3,777)	-1,713 (2,675)	-18,675** (7,475)
<i>Panel A3: Delinquent on Mortgage</i>				
<i>Tuition yrs. 1-4</i>	0.0004 (0.0054)	0.0024 (0.0044)	0.0005 (0.0037)	-0.0016 (0.0050)
<i>Panel B1: Has Credit Card</i>				
<i>Tuition yrs. 1-4</i>	0.0287 (0.0214)	0.0269 (0.0241)	0.0083 (0.0279)	0.0025 (0.0165)
<i>Panel B2: Credit Card Balance</i>				
<i>Tuition yrs. 1-4</i>	-264.3 (298)	-240.6 (236)	-217 (194)	-1,201*** (387)
<i>Panel B3: Delinquent on Credit Card</i>				
<i>Tuition yrs. 1-4</i>	0.0474** (0.0223)	0.0540** (0.0223)	0.0301 (0.0200)	0.0315 (0.0224)
<i>Panel C1: Has Auto Loan</i>				
<i>Tuition yrs. 1-4</i>	-0.0107 (0.0295)	0.0324 (0.0293)	0.0292 (0.0246)	-0.0254 (0.0282)
<i>Panel C2: Auto Loan Balance</i>				
<i>Tuition yrs. 1-4</i>	-1,068* (610)	170 (517)	380 (388)	284 (671)
<i>Panel C3: Delinquent on Auto Loan</i>				
<i>Tuition yrs. 1-4</i>	0.0100 (0.0111)	0.0211** (0.0105)	0.0166* (0.0090)	0.0197 (0.0125)
Observations	48,284	48,284	48,284	48,284
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School

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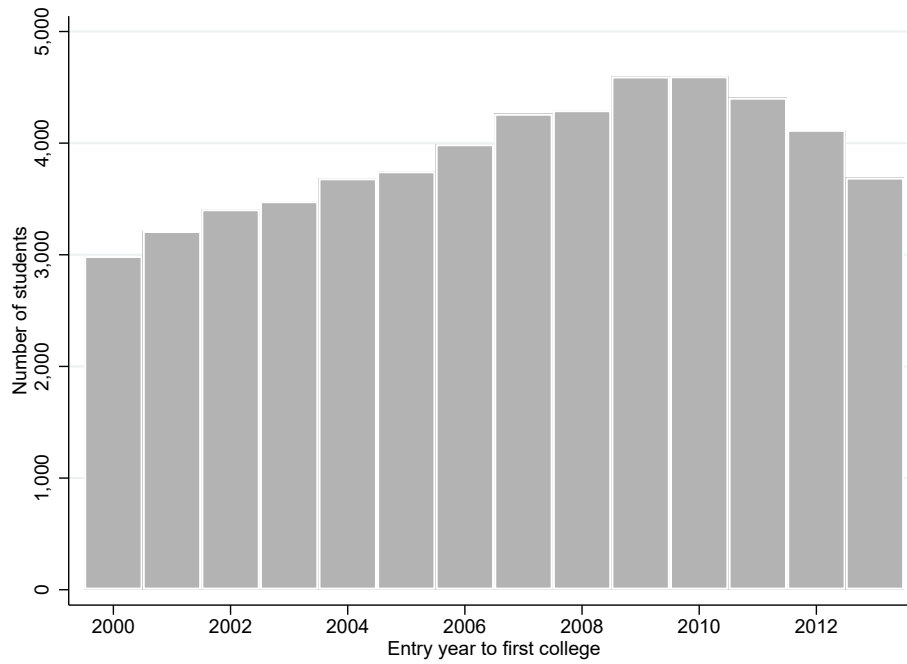
Table 10, continued..

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 2 and the first-stage corresponds to equation 3. First stage results are reported in column 1 of Table 3. Each panel reports results for an outcome variable measured across the following horizons: 8 years after entry (column 1), 6 years after entry (column 2), 4 years after entry (column 3), and at age 30 (column 4). The dependent variables in Panel A are Has Mortgage, an indicator that equals one if the student has mortgage, Mortgage Balance, the total mortgage balance, Delinquent on Mortgage, an indicator that equals one if the student is delinquent on mortgage; in Panel B, Has Credit Card, an indicator that equals one if the student has credit card debt, Credit Card Balance, the total credit card balance, Delinquent on Credit Card, an indicator that equals one if the student is delinquent on credit card debt, and in Panel C, Has Auto Loan, an indicator that equals one if the student has auto loan, Auto Loan Balance, the total auto loan balance, and Delinquent on Auto Loan, an indicator that equals one if the student is delinquent on auto loan. 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.

**Internet Appendix for the paper
“Tuition, Debt, and Human Capital”**

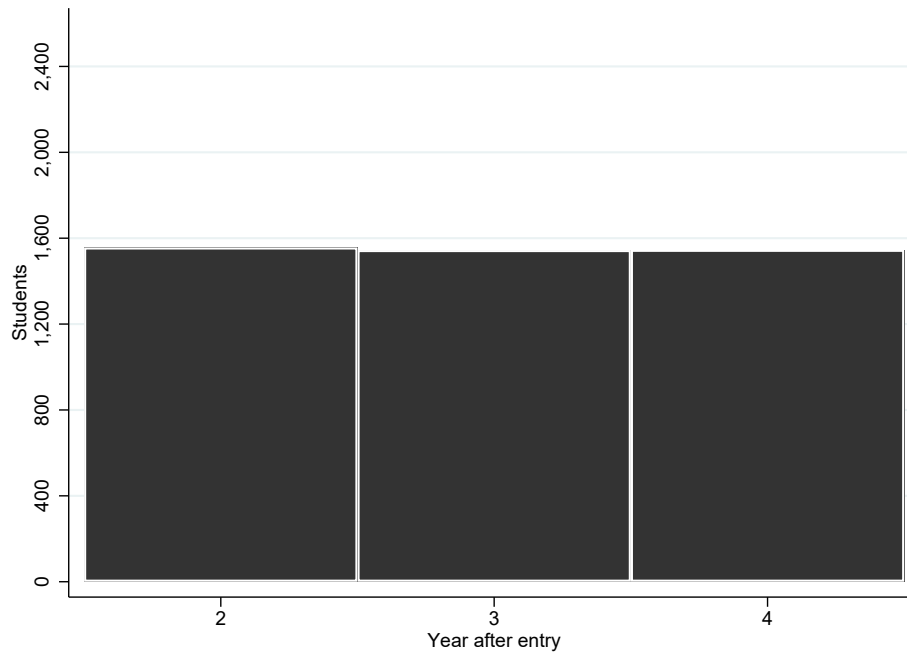
Rajashri Chakrabarti, Vyacheslav Fos, Andres Liberman, Constantine Yannelis

Figure A1: Distribution of students across entry years.



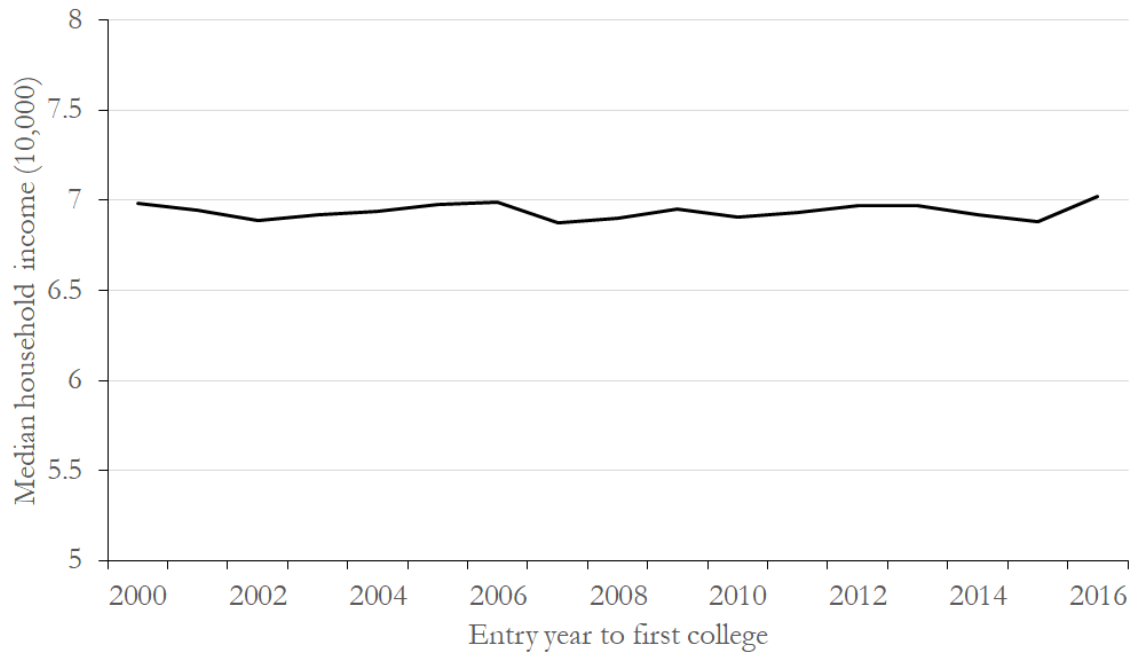
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows the number of students by cohort in the main analysis sample.

Figure A2: Distribution of students across grades.



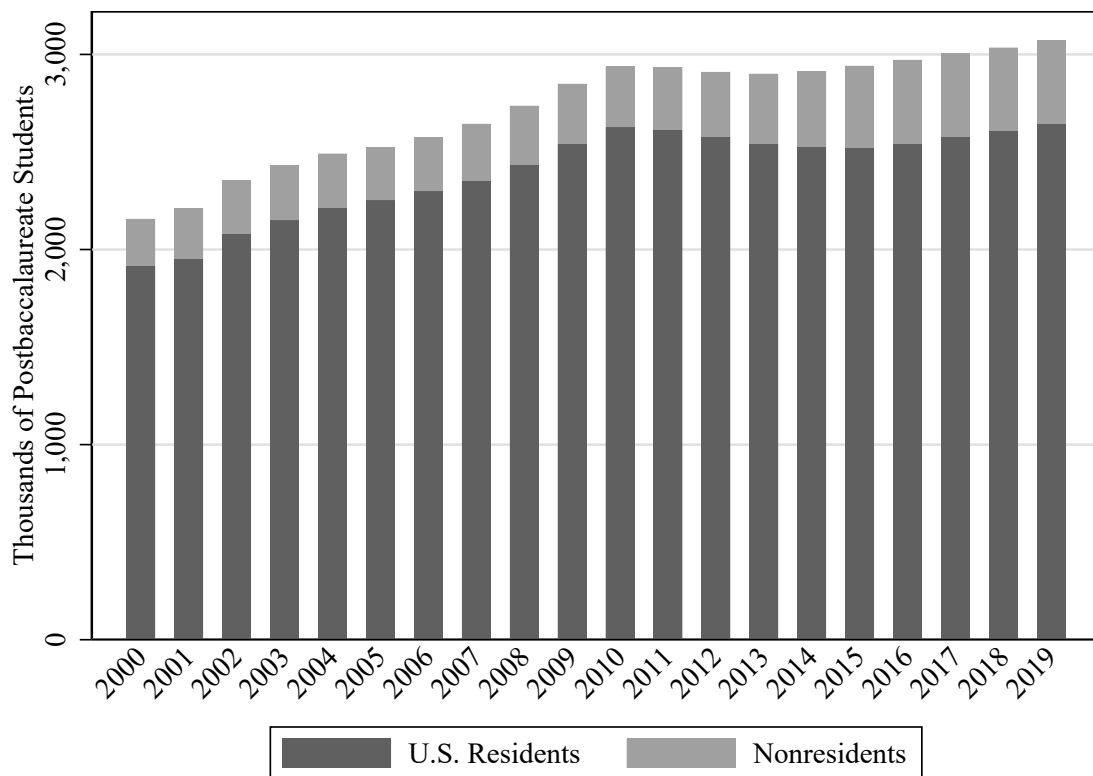
Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This figure shows the number of students in each grade in the main analysis sample.

Figure A3: Distribution of median household income across years.



Source: Internal Revenue Service (IRS). This figure shows the average (across all students) of median household income (\$10,000) across sample years.

Figure A4: Distribution of U.S. residents and nonresidents in graduate schools across years.



Source: National Center for Education Statistics. This figure shows the number of postbaccalaureate students across sample years. Dark bars indicate U.S. residents and grey bars indicate nonresidents.

Table A1: **Descriptive statistics by exposure to a large tuition increase.**

Variable	All	Exposed to a large tuition increase	Not exposed to a large tuition increase
First school is public	0.6766	0.3975	0.8526
First school is private non-profit	0.2693	0.5648	0.0830
First school is selective	0.6063	0.7316	0.5274
Age at entry	19.5349	19.3596	19.6454
Median hhld income (10,000)	6.9039	7.6032	6.4593
Bachelors	0.4965	0.5798	0.4445
Debt (10,000)	1.1989	1.4539	1.0104
Graduate school	0.1155	0.1407	0.0999
Total tuition years 1-4 after entry (10,000)	5.1601	8.4194	2.7835
Observations	58,641	22,680	35,961

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports descriptive statistics, split by whether schools are subject to a large tuition increase.

Table A2: Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax. Distribution of students across states of school.

State	Number of Students	State	Number of Students
AK	221	MT	252
AL	902	NC	1,384
AR	531	ND	249
AZ	1,989	NE	363
CA	3,719	NH	365
CO	954	NJ	797
CT	639	NM	371
DC	425	NV	577
DE	243	NY	4,111
FL	5,259	OH	2,746
GA	1,998	OK	663
HI	146	OR	472
IA	702	PA	3,325
ID	422	PR	275
IL	1,834	RI	547
IN	1,502	SC	782
KS	451	SD	235
KY	857	TN	954
LA	1,067	TX	2,875
MA	1,813	UT	991
MD	935	VA	1,363
ME	313	VI	11
MI	1,904	VT	202
MN	1,095	WA	1,386
MO	1,205	WI	1,202

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax. This table reports the number of students in each state in the sample.

Table A3: **Reduced-form regressions.**

Dependent variable:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
Grade 2	0.2384*** (0.0885)	-0.0418*** (0.0160)	-0.0021 (0.0184)	0.0268 (0.0188)
Grade 3	0.2401*** (0.0877)	-0.0234 (0.0167)	-0.0066 (0.0169)	0.0084 (0.0191)
Grade 4	0.2326*** (0.0866)	-0.0392** (0.0172)	-0.0235 (0.0176)	0.0318* (0.0192)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,040	46,040	46,040	46,040
R^2	0.16	0.16	0.37	0.12

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of reduced-form regression (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, 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 A4: **Reduced-form regressions: The role of income.**

Dependent variable:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
Grade 2	0.2334** (0.0963)	-0.0429** (0.0168)	0.0064 (0.0190)	0.0147 (0.0200)
Grade 3	0.2881*** (0.0988)	-0.0239 (0.0178)	-0.0034 (0.0177)	-0.0012 (0.0202)
Grade 4	0.2758*** (0.0925)	-0.0347* (0.0183)	-0.0054 (0.0186)	0.0309 (0.0201)
Grade 2 x Low Income	0.0227 (0.1530)	0.0062 (0.0246)	-0.0351 (0.0307)	0.0604** (0.0299)
Grade 3 x Low Income	-0.2304* (0.1275)	0.0028 (0.0240)	-0.0135 (0.0277)	0.0492 (0.0334)
Grade 4 x Low Income	-0.2117 (0.1825)	-0.0214 (0.0269)	-0.0831** (0.0358)	0.0041 (0.0333)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,040	46,040	46,040	46,040
R^2	0.16	0.16	0.37	0.12

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of reduced-form regression (5). 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 A5: OLS regressions with an indicator for students in grades 2 through 5.

Dependent variable:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
<i>Grades 2-5</i>	-0.0111 (0.1207)	-0.0138 (0.0220)	0.0098 (0.0241)	0.0001 (0.0230)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,162	46,162	46,162	46,162
R^2	0.15	0.15	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, while replacing *Tuition yrs. 1-4* with an indicator for students in grades 2 through 5 at the time of a large tuition increase. 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 A6: **The effect of tuition on human capital and debt accumulation: 2000-2010 period.**

Dependent variable:	<i>Debt</i> (1)	<i>Graduate school</i> (2)	<i>Bachelors</i> (3)	<i>Transfers</i> (4)
<i>Tuition yrs. 1-4</i>	0.2452* (0.1438)	-0.0643** (0.0283)	0.0010 (0.0275)	0.0407 (0.0306)
Fixed Effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	46,053	46,053	46,053	46,053

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 2 and the first-stage corresponds to equation 3. In this table, we use large tuition increase events during 2000-2010 and disregard large tuition increases during later period. 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 A7: **The effect of tuition on human capital and debt accumulation: \$100 threshold.**

Dependent variable:	<i>Tuition yrs 1-4</i>	<i>Debt</i>	<i>Graduate school</i>	<i>Bachelors</i>	<i>Transfers</i>
	(1)	(2)	(3)	(4)	(5)
Tuition yrs. 1-4		0.8833** (0.4095)	-0.1457* (0.0750)	0.0619 (0.0936)	0.1121 (0.0988)
Grade 2 (placebo)	0.1304*** (0.0327)				
Grade 3 (placebo)	0.0946*** (0.0230)				
Grade 4 (placebo)	0.0602*** (0.0134)				
Fixed effects:	Cohort, School	Cohort, School	Cohort, School	Cohort, School	Cohort, School
Observations	45,966	45,966	45,966	45,966	45,966
<i>F</i> -test	4.267				
<i>p</i> -value	0.01				

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 2 and the first-stage corresponds to equation 3. The threshold for a large tuition increase is \$100 (instead of \$1,000 in the main specification). First stage results are reported in column 1, where the dependent variable is *Tuition yrs 1-4*, which 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). 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 A8: School-year level matched sample: Robustness.

Dependent variable:	<i>Tuition</i>	<i>Instruction</i>	<i>Research</i>	<i>Completions</i>	<i>Loan pct ratio</i>	<i>Admit rate</i>	<i>Student fac</i>	<i>In time</i>	<i>Fraction non-white</i>	<i>Fraction female</i>	<i>Sat-M-25</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<i>Large change</i> $\times \delta_{-3}$	103.31 (152.593)	100.76 (484.658)	166.38 (361.093)	0.00 (0.004)	-1.32 (0.998)	-0.01 (0.006)	-0.14 (1.250)	0.02*** (0.005)	0.01 (0.007)	-0.00 (0.007)	6.48*** (1.840)
<i>Large change</i> $\times \delta_{-2}$	77.01 (133.107)	274.34 (374.816)	153.53 (382.727)	0.01 (0.004)	-1.49 (1.202)	-0.00 (0.007)	-0.70 (1.336)	0.01* (0.005)	0.00 (0.007)	-0.00 (0.007)	7.13*** (1.827)
<i>Large change</i> $\times \delta_{-1}$	94.25 (135.287)	274.19 (383.473)	109.19 (442.294)	0.00 (0.004)	-1.02 (0.915)	-0.01 (0.006)	1.09 (1.224)	0.01 (0.005)	0.01 (0.007)	0.00 (0.007)	6.26*** (1.934)
<i>Large change</i> $\times \delta_0$	1,096.22*** (103.475)	329.72 (380.419)	100.97 (446.725)	0.01 (0.006)	-1.00 (0.860)	-0.01 (0.006)	-0.82 (1.447)	0.02*** (0.006)	0.01 (0.007)	-0.01 (0.007)	6.44*** (1.977)
<i>Large change</i> $\times \delta_1$	852.98*** (129.570)	347.52 (295.589)	218.59 (367.059)	-0.00 (0.005)	-1.45 (0.868)	-0.01 (0.008)	-0.28 (0.400)	0.01 (0.008)	0.01* (0.007)	0.00 (0.007)	8.35*** (1.928)
<i>Large change</i> $\times \delta_2$	753.30*** (136.046)	440.64 (360.154)	139.64 (460.761)	0.00 (0.004)	-1.34 (0.980)	-0.01 (0.009)	-0.70** (0.305)	0.02*** (0.005)	0.01 (0.007)	0.01 (0.008)	6.56*** (1.972)
<i>Large change</i> $\times \delta_3$	655.19*** (109.046)	400.49 (271.105)	142.27 (469.438)	0.00 (0.004)	-0.74 (0.876)	-0.01 (0.013)	0.28 (0.282)	0.01*** (0.004)	0.01 (0.006)	0.00 (0.008)	6.69*** (1.864)
Observations	15,107	15,086	15,089	15,102	14,775	12,809	11,194	13,674	15,102	12,411	9,890
R^2	0.967	0.601	0.564	0.346	0.588	0.535	0.306	0.652	0.724	0.534	0.828

Source: National Student Clearinghouse, New York Fed Consumer Credit Panel/Equifax and Integrated Postsecondary Education Data System. This table reports estimates of regression (4) 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, 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. Outcomes include *Tuition*, the nominal dollar value of in-state tuition and fees for full-time undergraduates (Sticker price); *Instruction* the average expenditures in instruction measured in units of dollars per student; *Research* the average expenditures in research measured in units of dollars per student; *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 Math 25th percentile score among admitted students. Standard errors are double-clustered at school and year levels and are reported in parentheses. ***, **, * correspond to statistical significance at the 1, 5, and 10 percent levels, respectively.