# Financial Frictions and Human Capital Investments*

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**Abstract**

Does the type of financing affect college students' choice of major? Between 2001 and 2021, 22 U.S. universities implemented universal no-loan policies (UNLPs), replacing student loans with grants. I find that UNLPs increased the number of students choosing a high-paying major by 6%. The effect is strongest for students from low-income backgrounds, and it is driven by increased selection of majors associated with low initial earnings but high lifetime earnings, suggesting that financial frictions play a key role in major choice. Additional evidence on mechanisms suggest that students choose more difficult majors and are more likely to attend graduate school.

**Keywords:** Educational financing, human capital investments, college major choice

**JEL Codes:** D13, J16, J31

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Human capital investments are some of the most important decisions individuals make. This includes both the decision of whether to invest in human capital and the specific *type* of human capital to invest in. As the U.S. economy has shifted from manufacturing to services, the return to specific types of human capital has risen (Autor, 2014; Deming, 2017). One of the primary processes through which a young person invests in specific types of human capital is through their choice of college major (Hemelt et al., 2021), and there are large variations in earnings across different college majors. In fact, differences in earnings across college majors are often much greater than the average earnings gap between high school and college graduates.[1]

At the same time, investing in human capital has become increasingly expensive. Since 1980, tuition for four-year college has risen at five times the rate of inflation, and the majority of students rely on student loans to finance their investment (Dynarski et al., 2018). This rising cost presents financial barriers, especially for students from low-income families. Although the federal government offers billions of dollars of aid each year, in the form of both unsubsidized and subsidized loans—which has helped increase college attendance for students from low-income families—such students still tend to earn considerably less, later in life than students from higher-income families (Bartik and Hershbein, 2018).[2] To design better policies for allocation of student loans and financial aid, we need to ask: How do financial frictions affect the *type* of human capital investments that students make? And how do the effects vary across the income distribution?

This paper investigates how reliance on student loans affects a student's choice of college major. To answer this question, I study a natural experiment that sharply decreased the amount of student loans that were taken by undergraduate students: the implementation of "Universal No Loan Policies" (UNLPs). Between 2001 and 2021, 22 U.S. higher education institutions implemented UNLPs which lowered the need for students to take student debt by providing no-strings-attached grants. These universities include many Ivy League and elite private liberal arts institutions, such as Amherst College, University of Pennsylvania, and Yale University. Using a novel hand-collected dataset combined with a generalized difference-in-differences empirical strategy, I find that UNLPs had a large positive impact on the number of students graduating with a high-paying major. The effect is strongest for students from low-income neighborhoods and students whose parents have low credit scores, and it is driven by an increase in the number of students

[1] See, for example, Arcidiacono (2004), Hamermesh and Donald (2008), Altonji et al. (2012), Andrews et al. (2017), and Andrews and Stange (2019).

[2] Bachelor's-degree holders from low-income backgrounds start their careers earning about two-thirds as much as those from higher-income backgrounds, but this fraction declines to one-half by mid-career (Hershbein, 2016).

choosing majors associated with lower initial earnings but higher lifetime earnings. This is consistent with the idea that financial frictions play a key role in major choice. I provide additional evidence on possible economic mechanisms driving my results; in particular, I show that students benefiting from UNLPs choose more difficult majors and are more likely to attend graduate school.

For my analysis, I have combined four types of microdata to build a novel dataset that links students with their parents and tracks the students from college through their later careers. First, I hand-collect student-level data from university commencement programs to create a baseline sample of over 400,000 students, covering 32 schools and 24 graduating classes.[3] Importantly, the commencement programs list each graduating student's major, allowing me to measure how major choices within an institution shift over time; this forms the basis for my key outcome variables. Second, for each domestic student on the roster, I use United States Postal Service (USPS) records to construct a history of addresses for each student and their parents. Third, for each student and their parents, I obtain account-level credit bureau records from Experian PLC (a major credit bureau), which includes amount of student debt for each individual student and the credit score the students' parents. Fourth, for each student, I collect publicly available resumes from LinkedIn, Doximity, and personal webpages.

The resulting panel dataset allows me to identify the effects of student debt across several outcome variables while controlling for observable characteristics. It also allows me to decompose the effects of student debt according to parental socioeconomic status, as measured by parents' credit bureau records and address-level characteristics. I supplement the dataset with publicly available data from the American Community Survey (ACS), which includes the average earnings profiles associated with various choices of college major, and from the Integrated Postsecondary Education Data System (IPEDS), which includes school-year variables on tuition, enrollment and graduation rates, and average student loan take-up.

In the first part of the paper, I document the following stylized fact: Students' major choices are related to their families' socioeconomic status. On average, students from low-income neighborhoods and students from low-credit score families choose majors that are associated with low lifetime earnings. For example, students from low-income backgrounds are less likely than their high-income peers to major in Biology or Economics and more likely to major in English and Psychology. Previous studies have found that cor-

[3]For comparison, Rothstein and Rouse (2011) analyze data from just one school, comprising 9,000 students; Scott-Clayton and Zafar (2019) study the recipients of West Virginia's PROMISE scholarship (a total of 30,000 students); Bettinger et al. (2019) study recipients of California's Cal Grants (roughly 50,000 students); and Chakrabarti et al. (2022) analyze a random sample of 50,000 students.

relations between socioeconomic status and major choice are partly driven by the choice of institution, students at Ivy League universities are more likely to study economics, while students at community colleges are more likely to study psychology.[4] However, in my study, I find that the correlation holds even controlling for the school: For example, conditional on attending Yale, low-income students are less likely to choose high-paying majors than high-income students. Of course, this heterogeneity could be explained by several factors, including differences in college preparedness or differential exposure to specific types of majors and occupations. Therefore, to further understand the impact of financial frictions on major choice, I next turn to a causal framework.

In the main part of the analysis, I estimate the causal impact of student loans on college major choice. As explained above, my analysis exploits the staggered implementation of UNLPs at 22 U.S. universities between 2001 and 2021. Pre-policy, the difference between how much a household can pay for college (effective family contribution) and the cost of attendance was financed with a combination of student loans and grants. After the policy was implemented, this difference is covered entirely by no-strings-attached grants. In my baseline specification, I estimate the effect of a UNLP on students who had taken out student loans before UNLP implementation (the *treated* group) relative to students who had not taken out loans (the *control* group).[5]

My research design has several empirical advantages. First, the staggered implementation of UNLPs over 20 years alleviates the risk of contemporaneous shocks. Second, UNLPs greatly reduce student loan amounts, providing a strong first stage.[6] Third, students without student debt provide a natural control group, as they were not affected by UNLPs, but were affected by any other (potentially confounding) policy changes. Finally, the size of the sample allows me to obtain reliable estimates of how the effects of UNLPs vary across sub-samples.[7]

I document that UNLPs led to a sharp and immediate decrease in reliance on student loans. In the three years following the policy, the treated students (those with student loans before the policy) decreased their student loan holdings by $7,000. Turning to the

[4]See Bleemer et al. (2023) for an excellent review of the recent literature on college major choice and differences in earnings across majors.

[5]I thank Sandra Black and Zachary Bleemer for suggesting this specification.

[6]For example, Rothstein and Rouse (2011) find that upon implementation of a UNLP at one university, the typical amount of student loan debt at graduation fell by over $11,000, from over $15,000 before the UNLP was implemented to $4,000 afterward.

[7]For comparison, one closely related paper is Chapman (2016), which studies the impact of financial aid on earnings and reports the findings by bins of family income. However, the author does not find any statistical differences across the family income distribution. As mentioned in the paper, this is likely due to the small sample size and the fact that the income distribution in the sample differs significantly from the national average.

key outcome of interest, I find that UNLPs increased the share of students who graduated with high-paying majors. On average, this share increased by 2% in the three years following UNLP implementation; among students who were sophomores at the time of implementation (and thus had the most time available to change their majors), there is a 6% increase. Notably, these effects are for students who were already enrolled at their universities before the UNLPs were announced; thus, they are free from selection bias. The estimated elasticities suggest that for every $10,000 increase in student debt, a student is 3.7% less likely to graduate with a high-paying major.

Several pieces of evidence indicate that financial frictions are a key underlying mechanism for these effects. First, I find that the largest effects are among students from low-income neighborhoods and students whose parents have low credit scores. For example, the effect is more than twice as strong for students with parental credit scores in the bottom tercile as it is for students with parental credit scores in the top tercile.

Second, I find that UNLPs affect not only major choices sorted by average earnings, but it also major choices sorted by the earnings *trajectories*. That is, in line with recent results from Martin (2022), Huang (2023), and Leighton and Speer (2024), I find that college majors differ not only in terms of average lifetime earnings, but also in terms of earnings trajectories over the course of an individual's career. Majors associated with high lifetime earnings, such as biology and the physical sciences, often have low initial earnings, while majors associated with low lifetime earnings, such as psychology and education, often have relatively high initial earnings. I find that students' socioeconomic status affects their choices along this trade-off: students from low-income neighborhoods and students whose parents have low credit scores choose majors with higher initial earnings and lower lifetime earnings.

Furthermore, student debt levels also directly affect these choices: Following UNLP implementation, more students chose majors with lower initial earnings but higher lifetime earnings. I decompose the effect of UNLPs by different types of high-earning majors, and I find that the effect of UNLPs is driven by an increase in the share of students graduating with degrees that are associated with high earnings growth and *back-loaded* earnings, such as Biology and Physics. On the other hand, I find little to no effect on the share of students graduating with degrees that are associated with more *front-loaded* earnings, such as Business or Engineering. A quantification exercise suggests that following the policy, students choose majors associated with around $500 less in annual earnings immediately out of college, and with $700 to $2,100 more in annual earnings in their 30s and 40s.

In the last part of the paper, I provide further evidence on the economic mechanisms driving the results. I consider five additional characteristics of college majors: graduate school attendance rates, earnings variance, self-reported difficulty, grade-point average (GPA), and average weekly study time. I find that following the policy, students who were sophomores at the time of treatment are about 6% more likely to choose majors associated with high rates of graduate school attendance. Furthermore, using the resume data, I confirm that these students are more likely to have subsequently attended graduate school themselves. In relation to time-use during college, I find that UNLPs let students choose majors associated with higher self-reported difficulty levels, lower GPAs, and more study time per week. In supplemental analysis, I examine outcomes later in life. Using self-reported resumes, I find that students move into occupations that require more human capital investments (e.g. Doctors and Lawyers) that are characterized by higher slopes and move away from professions that are characterized by flatter slopes (e.g. Teachers). Finally, I find that students are more likely to take up graduate school debt, mortgage debt, and auto loans. Combined, these results indicate that in addition to changing students' major choices, UNLPs had long lasting real effects.

Taken together, I interpret these results as evidence that UNLPs dramatically lowered reliance on student loans, which in turn led students to choose higher-paying college majors. Several pieces of evidence indicate that financial frictions play a key role. First, the strongest effects are for students from low-income and low credit score families, and, second, the policies led students to choose majors with lower initial but higher lifetime wages. The results highlight that investing in human capital inherently involves an intertemporal trade-off and that the type of education financing available affects students choices along this trade-off.

The results highlight two potential implications for public policy. First, since the UNLPs were implemented for a broad sample of students at an early stage of life—before large human capital decisions had been made—the estimated elasticities are relevant to current policy proposals on free tuition, income share agreements, and payment deferral plans. Second, they speak to the current debate on how elite U.S. colleges can affect economic mobility (Chetty et al., 2023). Importantly, the results suggest that merely providing loans and *access* to college is not a panacea for inequality in future labor-market outcomes. Policymakers should consider how financial frictions and education financing options affect human capital investments both during and after college, and how the effects differ across the student population.

The findings in this paper contribute to several strands of the literature. First, my finding contribute to a longstanding literature studying determinants of college major choice, including the role of ability (Turner and Bowen, 1999; Altonji, 1993; Arcidiacono, 2004; Stinebrickner and Stinebrickner, 2008), earnings uncertainty (Nielsen and Vissing-Jorgensen, 2006; Bonin et al., 2007; Dillon, 2018; Saks and Shore, 2005), earnings expectations (Arcidiacono, 2004; Stange, 2012; Stinebrickner and Stinebrickner, 2014; Zafar, 2011; Wiswall and Zafar, 2014, 2017), and non-pecuniary benefits (Zafar, 2013; Boneva and Rauh, 2017). I contribute by analysing the effect of student loans on major choice.

Recent work in this literature highlights that major choice can lead to dramatically different labor market returns for students because average earnings vary substantially between college majors (Hershbein and Kearney, 2014; Altonji et al., 2016; Deming, 2017; Broady and Hershbein, 2020), especially as major choice is found to be tightly linked with post-college occupations.[8] While many papers focus on differences in mean earnings across majors and broad categorizations of majors (e.g., STEM vs. non-STEM, or Humanities, Social Sciences, Sciences, and Business majors), I show that this reduction in dimensionality can mask important heterogeneity. Specifically, I show that majors differ meaningfully in their average earnings *trajectories*, which relates to the effects of socioeconomic status and the use of student debt.

Within the literature studying college major choice, a set of recent and closely related papers also find that majors vary across income trajectories (Martin, 2022) and earnings variability within worker, over time (Andrews et al., 2022). Prior work has shown that on average, low-income students choose majors associated with low mean earnings, such as social work, nursing, and education (Arpita et al., 2020); I note that these majors are also associated with higher initial earnings and are more likely to be chosen by students who are incurring more student debt and financially constrained. Relatedly, Huang (2023) finds that the majors that poorer students choose majors that have flatter earnings-age profile (i.e. higher initial earnings, lower earnings growth) and lower earnings risk, and Murto (2024) finds that after an expansion in Income Driven Repayment options increased generosity, borrowers were more likely to select majors with lower initial labor market outcomes but higher wage growth, suggesting that financial frictions impact optimal human capital investments.

I contribute to this growing literature by documenting that, across majors, there is a negative correlation between initial earnings (immediately out of college) and earnings later in life. Thus, the choice of a major involves an intertemporal trade-off between initial and later earnings. Moreover, I show that financial frictions (in the form of student debt) affect major choices, especially for students from low socioeconomic families.

My findings also contribute to a large body of literature that studies the relation be-

[8]Several papers have shown the degree to which majors are concentrated within certain occupations (Black et al., 2003; Altonji et al., 2012; Ransom et al., 2014; Lemieux, 2014; Ransom, 2021).

tween student loans and labor market outcomes such as earnings (Minicozzi, 2005; Rothstein and Rouse, 2011; Di Maggio et al., 2019).[9] So far, the empirical evidence has been mixed and contradictory, and, as noted by Yannelis and Tracey (2022) in their recent review of the literature, there is significant heterogeneity in outcomes. For example, while some studies have found that lower student debt is associated with *higher* earnings, others have found it to be associated with *lower* earnings. Papers that have found that lower student debt is associated with higher earnings include Weidner (2016), Gervais and Ziebarth (2017), Di Maggio et al. (2019), Bettinger et al. (2019), and Scott-Clayton and Zafar (2019). Papers that have found it to be associated with lower earnings include Minicozzi (2005); Field (2009); Rothstein and Rouse (2011); Chapman (2016); Daniels and Smythe (2019); Denning et al. (2019), and Luo and Mongey (2019).

This wide range of results highlights a key challenge facing researchers. As described above, human capital investments often involve an intertemporal trade-off, in that individuals accept lower initial earnings in exchange for higher expected future earnings. However, the prior literature typically estimates the effect of student loans on an outcome, such as earnings, at a *single point in time*, abstracting away from the intertemporal trade-off. This means that if, for example, student debt cause students to choose different career trajectories with different earnings paths, then estimates or earnings will vary greatly depending on when they are measured. The wide range of estimates in the existing literature may also partly be due to the inherent empirical challenges associated with causal inference. For example, several papers rely on surveys with few observations (e.g. the Survey of Consumer Finances, the Baccalaureate and Beyond Longitudinal Study, and the National Longitudinal Survey of Youth), study single-year policy changes that might be confounded by contemporaneous macroeconomic shocks, or restrict their attention to low-income or low-credit-score individuals. My paper helps reconcile the apparent inconsistencies in the previous literature by considering earnings over the entire career trajectory, in relation to major choice,[10] for a large dataset spanning over two decades.

Finaly, my findings speak to a burgeoning literature on social mobility and human

[9]Other studies have examined the effect of student debt on school enrollment and drop-out rates (Bettinger, 2004; Solis, 2017; Card and Solis, 2022; Black et al., 2023), graduate school (Millet, 2003; Akers, 2013; Chakrabarti et al., 2022), family formation (Gicheva, 2016; Addo, 2014; Goodman et al., 2021), business creation (Krishnan and Wang, 2018; Krishnan and Wang, 2019), home ownership (Houle and Berger, 2015; Mezza et al., 2020; Folch and Mazzone, 2022), and non-wage job amenities (Luo and Mongey, 2019). Boutros et al. (2024) and De Silva (2024) use quantitative models to estimate the welfare effect of payment referral and income-contingent repayment plans.

[10]Many studies find that the increases in earnings associated with lower student debt are due to higher completion rates on the extensive margin of students; see, for example, Denning et al. (2019). These analyses do not consider major choice.

capital investments (Galor and Zeira, 1993; Cameron and Heckman, 2001; Bell et al., 2019; Chetty et al., 2020, 2023). Much of this literature focuses on how socioeconomic background affects college attendance. Newer to the literature is the question of how socioeconomic background affects college major choice. For example, in contemporaneous work, Leighton and Speer (2024) find that students with more educated parents choose majors associated with higher lifetime earnings. I contribute to this literature by relating students' major choices to their neighborhood incomes and parental credit scores.

The remainder of this paper is structured as follows. Section 1 gives an overview of the history of UNLPs. Section 2 presents the data used in my analysis. Section 3 provides descriptive facts relating major choice to socioeconomic background. Section 4 describes the empirical strategy of my analysis, and Section 5 presents the results. In Section 6, I explore possible mechanisms driving the effects of UNLPs on major choice. Section 7 concludes the paper.

# 1 Background

## 1.1 History of No-Loan Policies in the United States

In the late 1990s and early 2000s, many U.S. universities and colleges began offering more financial aid in order to lower financial barriers to enrollment and increase the socioeconomic diversity of the student body. Increases in financial aid have been implemented through five types of policies: student loan eliminations, student loan caps, parental contribution eliminations, tuition waivers, and Pell Grant matches. In particular, between 1998 and 2022, over 75 public and private universities adopted some type of student loan elimination policy. Panel A of Figure A3 in the appendix provides a histogram of the number of financial aid policies implemented over time. We see that the number of policies implemented rises sharply in the mid-2000s.

Under student loan elimination policies, also known as no-loan policies (NLPs), universities commit to cover each admitted student's financial need gap through scholarships, grants, or work-study programs, eliminating the need for federal or private loans.[11]

[11] In general, universities offer financial aid to students to cover the difference between the cost of attendance (COA) and the expected family contribution (EFC) (i.e., the amount that the student's family can afford to pay for college). The COA includes tuition and fees, room and board, books, supplies, transportation, loan fees, and other school-related expenses. It may also include child and dependent care costs, a student's disabilities, or study-abroad programs. The EFC is calculated from family assets, family size, and the number of dependent children enrolled in college. This information is collected on the Free Application for Federal Student Aid (FAFSA), which students must complete shortly before college enrollment to receive financial aid from the federal government (as well as from many universities). The difference be-

It is important to note that students are not required to take the full value of the loan reductions offered under an NLP. Some continue to take out loans to reduce work-study hours or parental contributions, or to permit more spending during college.

An NLP may be implemented in one of two ways: It may be targeted to students from families below a certain income threshold, or it may apply to all students, regardless of family income.[12] This paper focuses on the latter type of NLP, which I call a *universal no-loan policy* (UNLP). As shown in Figure A3, Panel B, U.S. institutions implemented UNLPs between 2001 and 2021, and these institutions included both prominent research universities (e.g., Dartmouth College, Harvard University, and Yale University) and liberal arts colleges (e.g., Amherst College, Swarthmore College, and Williams College).

As shown in Table A16 in the appendix, relative to other types of financial aid policies, NLPs materially reduce the fraction of students taking out loans. Moreover, as shown in Table A17, UNLPs have a much larger impact on the fraction of students taking out loans than do NLPs targeted to low-income students.[13]

## 1.2 Motivations for NLP Adoption

The mid-2000s increase in the adoption of NLPs at elite institutions was driven by a combination of economic and political factors. At the time, tuition was increasing at more than twice the rate of inflation each year (Choy, 2002). Elite colleges and universities were spending only 5% of their endowment income on financial aid, and families and politicians were urging them to provide more support (Ehrenberg, 2001). In 2008, Senators Max Baucus and Charles Grassley of the U.S. Senate Finance Committee requested information on endowment growth and student aid spending from 136 U.S. colleges with endowments of $500 million or more. Their goal was to put public pressure on these institutions to make college more affordable by threatening to eliminate their federal tax exemption privileges (Baucus and Grassley, 2022). Senator Grassley made the following statement:

> I have been encouraged by the recent changes that several universities have made to ensure access to higher education for low and middle-income students. We need to engage America's colleges and universities to come together to address the fact that

tween the COA and EFC, the financial need gap, is covered in part by the university through scholarships, grants, and work-study programs (insofar as the student qualifies), and in part by the student through federal and/or private student loans.

[12] See Lips (2011) for a discussion of various strategies for implementing NLPs.

[13] Note that these statistics, which come from the Integrated Postsecondary Education Data System (IPEDS) provide year-specific statistics as opposed to cohort-specific, and thus reflect the fraction of *all* undergraduate students with loans, rather than only students who enrolled after NLP implementation.

> college tuition for young Americans and their families is increasing at a faster rate than inflation. The questions we put forward in this letter will help Congress better understand how colleges use their endowments to make certain that talented young folks in Montana and across the country aren't left out of the classroom. (Baucus and Grassley, 2022)

Statements from university officials also reflected a growing awareness of the need to reduce financial barriers to college enrollment. For example, Amherst College president Anthony W. Mark said of his institution's new UNLP:

> This new initiative significantly broadens that commitment by eliminating barriers for middle-income families who want to ensure that their children receive an excellent education. Highly selective colleges like Amherst must be open and accessible to all of the most talented students. This new initiative represents a significant step, enabling us to select most broadly for future leaders while ensuring mobility based on talent. (Schmeidel, 2023)

In addition to reducing financial barriers to enrollment, news accounts also suggest that universities wanted to promote socioeconomic diversity within the student body (Rothstein and Rouse, 2011). NLPs proved to be an effective means of doing this: Rosinger et al. (2019) and Hillman (2013) find that the implementation of NLPs increased the proportion of low- and middle-income students enrolled. Since my aim is to study how UNLPs affect students' major choices and career outcomes, this fact points to a selection bias that could result if my analysis were to include students who enrolled at a university after it implemented a UNLP. I therefore restrict my analysis to students already enrolled at the time of UNLP implementation. Section 4.2 provides more details on this issue.

# 2 Data

In this section, I describe the data used in my analysis. First I outline the dataset construction process, giving details on each data source. I then describe the sample, including the sample selection filters and the summary statistics, and I analyze the representativeness of the sample and the validity of the data. Section D of the appendix provides additional details on the data sources and the merging procedure and Table A2 provides matching rates and sample comparisons between the different datasets.

## 2.1 Data Construction

In this subsection I describe each dataset used and explain how the data are collected and standardized.

### 2.1.1 List of UNLP schools

To identify colleges that have adopted UNLPs and the years in which they did so, I use a list compiled by Kantrowitz (2017) that provides financial aid policies by type and implementation date until 2016. This list is supplemented with hand-collected data for the period from 2017 to 2021. It identifies four-year institutions that have implemented a financial aid policy the type of policy implemented, the date of implementation, and whether the policy was later reversed. A complete list that combines both data from Kantrowitz (2017) and my own hand-collected data is available in Appendix Section D.

### 2.1.2 Commencement Programs

My dataset is structured around commencement programs obtained from university registrars or special collections archives. To create the dataset, I collected, digitized, and standardized commencement programs and university yearbooks from 22 universities that implemented a UNLP and 10 universities that did not implement a UNLP. Lists of the treated and control universities can be found in Table 1 and Table A1, respectively.[14] Each university produces at least one commencement program per academic year, and the commencement programs span 25 graduating cohorts from 1998 to 2022.[15] Each commencement program contains a list of students graduating with bachelor's degrees, including their full name, degree type, field of study, college major, hometown and country, and honors earned.

Figure A1 in the appendix shows an example commencement program. Section D.I of the appendix explains how majors were standardized across schools, using the Classification of Instructional Programs (CIP) from the National Center for Educational Statistics (NCES). Section E.II provides additional details on how the commencement programs were collected and digitized. Finally, Section E.V describes how I assign gender and race to student records, using a probabilistic algorithm based on first and last name.

### 2.1.3 USPS Dataset

To decompose the effects of UNLPs according to family background, I merge the roster of students from the commencement programs with postal address data from two propri-

[14] A previous version of this paper included results from a larger set of control schools, including large public schools such as University of Michigan and Indiana University. I have omitted these universities to make the control schools more closely resemble the treated schools. I thank Tyler Ransom for making this suggestion. (The results are similar using either set of control schools.)

[15] Some universities produce one commencement program per academic session (i.e., quarter or semester).

etary data vendors, Intelius and BeenVerified, to create a history of geocoded addresses for each student.[16]

The merge is based on each student's full name, approximate age, hometown (as provided by the commencement program), and dates of residence in their hometown. For each domestic student, I identify the postal address where the student lived the year before they started college. I create a *household* consisting of all individuals over the age of 18 who lived at this address in that year, classifying household members who are at least 15 years older than the student as *parents*.

### 2.1.4 Credit Bureau Dataset

For data on student loans, I obtain matched credit bureau records from Experian PLC covering the period from 2004 to 2021. These records allow me to construct one of the key variables, the amount of student loans taken on by each student during their undergraduate study. Experian provides information from each household's balance sheets, giving the yearly history of all borrowers' loans from 2004 to 2021 (both included). These include auto loans, mortgages, home equity lines of credit, student loans, and credit card balances (revolving). The records provide granular information about the main features of these loans, such as date opened, account type, credit limits, monthly scheduled payment, balance, and performance history. Versions of this dataset have been employed in other papers studying households' financial decisions. However, my proprietary version is unique in a few respects.

First, to match the borrowers (students) from the commencement programs with their credit records, Experian uses both the borrowers' names and their locations (collected from the USPS records described above). They then provide me with a matched, anonymized sample of students who attended universities that implemented a UNLP, as well as a 5% random sample of students who attended comparable universities that did not implement a UNLP.

Second, using the households identified from the USPS records, Experian matches each student's parents to their credit bureau records. This information is crucial for the analysis of heterogeneity based on family socioeconomic status. I also obtain a 1% random sample of the U.S. population, which lets me compare my sample of students to the general population.

[16]These databases contain data on U.S. individuals, including personal property records, bankruptcy records, address histories, and potential relatives. In addition, I use the LexisNexis address finder to randomly check Intelius and BeenVerified results. See Cronqvist et al. (2012) and Pool et al. (2015) for details on this type of data.

Finally, to ensure the highest possible quality of the match between commencement programs and credit bureau records, I employ three novel procedures. First, for each student and parent I provide Experian with their entire history of addresses. Two, for students that have changed their last names (e.g., women who take their husband's last name), I provide each set of names. Third, I cross-reference the USPS history of addresses with self-reported locations in the resume data to minimize falls positives. For more details on the merging procedure and match rates see Section D and Table A2.

#### 2.1.5 Supplemental Datasets

I supplement the hand-collected data with a number of publicly available datasets.

**American Community Survey:** I use demographic survey data from the 2009–2019 American Community Survey (ACS), extracted from the IPUMS 1% samples (Ruggles et al., 2017; Deming and Noray, 2018). The ACS survey includes demographic variables (e.g., race and gender), highest level of education (e.g., high school or four-year college), undergraduate field of study, occupation code, and annual earnings.

**Bureau of Labor Statistics:** I deflate earnings and neighborhood income across years using the annual Consumer Price Index provided by the Bureau of Labor Statistics (BLS).

**College Scorecard:** I obtain data on earnings by major and financial aid from the U.S. government's College Scorecard.

**Housing and Urban Development:** I use spatial crosswalks provided by the Department of Housing and Urban Development (HUD). The crosswalks link counties, ZIP Codes, and census tracts.

**Integrated Postsecondary Education Data System:** I obtain data on individuals schools from the Integrated Postsecondary Education Data System (IPEDS). These data include school-level variables on tuition, enrollment rates, graduation rates, SAT scores, class composition in terms of race and ethnicity, and average student loan take-up.

**Resume Datasets:** In supplemental analysis, I study the effect of UNLPs on later-in-life labor market outcomes and post-college human capital investments. To perform this analysis, I have obtained resumes for all the students listed in the commencement programs, sourcing them from LinkedIn, Doximity, and individuals' professional websites. LinkedIn and Doximity are employment-oriented online platforms where job seekers maintain profiles and employers post jobs.[17] Each resume contains three types of information: (1) educational history, including school name, degree, major, graduation date, and honors (such as Phi Beta Kappa membership or Latin honors); (2) job history, including firm names, position titles, years of employment, locations, and descriptions of tasks; and (3) a list of technical and soft skills and areas of expertise.

Using the start and end dates of the items in each resume, I create a yearly panel of data for each student. I focus on occupational outcomes in years 1–13 after college graduation. Each position title is assigned a 2010 Standard Occupational Classification (SOC) code using the Occupational Information Network (O*NET), a labor market database maintained by the BLS. Detailed information on the matching rate is in Section D of the appendix.

One concern with these data is that individuals may falsify their profiles. However, as discussed in Jeffers (2017), this is unlikely to be a prevalent problem, since the public availability of LinkedIn profiles means that individuals who make false claims about their schooling or employment are likely to exposed. A more salient concern is that of stale profiles. To address this issue, I restrict the sample to individuals whose recorded employment history covers every year since they graduated from college, and who have connections on LinkedIn or similar online resume sites.

## 2.2 Sample Statistics, Representativeness, and Data Validation

I now provide details on the analysis sample: I describe the filters applied, give summary statistics, analyze how representative the sample is, and analyze how closely it aligns with publicly available data on students' college majors and family backgrounds.

### 2.2.1 Sample Selection

I eliminate foreign students from the sample. This is because most foreign citizens are not eligible for federal student aid from the U.S. Department of Education. In addition, many foreign students return to their home countries after completing their degrees, which means they do not appear in the USPS or credit bureau data.

[17] Similar data have previously been used by Lucca et al. (2014), Jeffers (2017), Bernstein et al. (2018), Krishnan and Wang (2018), and Egan et al. (2019).

### 2.2.2 Summary Statistics

I classify college majors into six groups: Science, Technology, Engineering, and Mathematics (STEM) (accounting for 30% of students), Social Sciences (28%), Humanities (17%), Arts and Communication (8%), Business (6%), and Health and Education (3%). The remaining 9% of students have majors outside of these six groups. Table A3 in the appendix provides full summary statistics for each group of majors, including a complete crosswalk using both the National Center for Education Statistics (NCES) classifications and the ACS classifications. Next, I classify the 25 most common majors and list these in Table A4 in the appendix. These 25 majors account for 94% of the students in the UNLP sample. The table also provides summary statistics and the NCES and ACS crosswalks for each major.

My main outcome of interest is major choice. In the baseline specification, this is quantified by a variable indicating whether a student's chosen major is high-earning (i.e., associated with high average annual lifetime income). I classify majors as high- or low-earning based on ACS data: For each major represented in the commencement programs, I match the four-digit NCES CIP codes to the four-digit ACS Detailed Degree codes. Next, I calculate the average annual income associated with the four-digit ACS code for each major.[18] I take the median among the annual incomes for the 25 most common majors as the threshold: the 12 majors associated with incomes above the median are classified as high-earning, and the rest are low-earning. Table A4 in the appendix lists all 12 high-earning and all 13 low-earning majors. The three most popular high-earning majors are Economics, Engineering, and Biology, while the three most popular low-earning majors are Arts, Psychology, and English.

Table A5 in the appendix reports summary statistics at the level of individual students. I find that 58% of the students in the sample chose one of the 12 high-earning majors. I also infer each student's race/ethnicity and gender from their name, although the algorithms for doing so are not perfect. Students classified as White, Black, non-White Hispanic, and Asian respectively make up 68%, 5%, 5%, and 4% of the sample; 20% of the students are classified as having unknown race/ethnicity. The algorithm classifies 45% of the students as female, 44% as male, and the remaining 11% as having unknown gender.

A key question in my analysis is whether UNLPs have a differential effect depending on students' family backgrounds. I capture family background in terms of two variables. First, from the USPS data, I observe the history of addresses for each student's parents.

[18] Specifically, I adjust the ACS variable *INCWAGE* for inflation, and take the average across each *DEGFIELDD* variable weighted by the *PERWT* variable. In Section 6, I discuss an alternative approach where I residualize wages for age, race, sex, and survey-year. The results are consistent across the two methods.

I then identify the census tract in which the parents lived in the year that the student matriculated, and I merge in the median household income in this census tract. (If the parents lived in two different census tracts, e.g. if they were divorced, I use the average across the two census tracts.) From the credit bureau records, I observe the parents' credit score history and calculate their average credit score in the year that the student matriculated. Finally, I observe the student's credit history and calculate the amount of student debt they had in the year they graduated from college. Table A5 reports summary statistics for all of these variables.

### 2.2.3 Sample Representativeness

As shown in Table 1, the universities that implemented UNLPs are largely elite private universities and liberal arts colleges. Therefore, one would not expect the statistical makeup of their student bodies to be identical to that of the overall population of college students in the United States. In this subsection, I describe the differences and similarities between the student bodies at UNLP-implementing schools and those at other schools.

I begin by comparing the UNLP-implementing schools with the 10 non-implementing control schools. Panel (i) of Table A6 in the appendix reports summary statistics and covariate balance test results using IPEDS data at the school-cohort level. We see that there is virtually no statistically significant difference between the UNLP and non-UNLP (control) samples: They are similar in terms of admission rates, test scores, student body makeup, costs of attendance, and completion rates.[19] The only statistically significant differences, at the 10% level, are in the share of Asian students and the average faculty salary (the control schools have a larger share of Asian students and slightly higher faculty salaries). Taken together, I verify that the control schools—which were chosen specifically to be comparable with the UNLP-implementing schools—are indeed similar.

Next, I compare UNLP schools with all four-year institutions in the United States. Panel (ii) of Table A6 reports the corresponding statistics. We see that the UNLP schools are quite different from the average U.S. university: They have lower admission rates, higher test scores, higher attendance costs, lower student-to-faculty ratios, and higher faculty salaries.

[19] This is not surprising since the control sample also includes highly selective universities, including MIT, Duke, and Caltech.

### 2.2.4 Additional Data Validation

I conduct two additional tests to analyze how closely my dataset aligns with publicly available data on college majors and family backgrounds.

First, I compare my measure of neighborhood income, which is based on USPS records, with the measure of parental income from Chetty et al. (2020), who report the average parental income by school-cohort. I find that when I collapse my measure of median neighborhood income to the average school-cohort level, there is a 75% positive correlation with the measure used by Chetty et al. (2020). Appendix F.I provides further details, including a scatter plot of the income data at the school-cohort level (Figure A19).

Next, I validate the textual classification of the commencement programs. Specifically, I compare the the CIP classification of each major from the commencement programs with the CIP classification from the publicly available IPEDS data. While this is not a perfect apples-to-apples comparison—I observe students while the IPEDS data measures degrees conferred—it can provide a sense of whether the commencement program data systematically over- or under-classify certain majors. Across all school-year-major observations, I find a 97% correlation between the commencement program data and the IPEDS data. Also, reassuringly, there is no statistical difference across categories of majors, or between classifications before and after UNLP implementation. See Appendix F for further details.

## 3 Descriptive Facts

In this section, I provide descriptive evidence showing that students from low-credit-score families and low-income neighborhoods are less likely to graduate with a high-earning major.

First, to provide context, I examine the distributions of neighborhood-level income and household-level credit scores. Figure A4 shows the distribution of ZIP-level median household income for students at UNLP-implementing schools, relative to the distributions for a 1% random sample of the general U.S. population (Panel A) and a 5% random sample of students at the non-UNLP (control) schools (Panel B).[20] There are two takeaways from this figure. The first is that families that send their children to universities with UNLPs are wealthier than the average family in the United States. The second is that, nevertheless, students from lower-income families also attend these universities. On the other hand, Panel B confirms that there is virtually no difference between the income

[20]Because of funding restrictions, I obtained credit bureau records for only a 5% random sample of non-UNLP students.

distributions for the UNLP and control schools.

The same pattern holds for credit score distributions, as shown by the histograms in Figure A5.

### 3.1 Family Background and College Major Choice

Students from low-credit-score families and students from low-income neighborhoods are less likely to graduate with a high-earning major. This pattern holds even across students from the same university and the same graduating cohort.

Figure 1 shows the relationship between the probability of a student's choosing a high-earning major and two proxies for their parents' socioeconomic status. Panel A presents a binned scatter plot with parents' credit scores on the $x$-axis and the fraction of students with high-earning majors on the $y$-axis. Panel B shows the median household income (inflation-adjusted to 2019 dollars) for the census tract in which the parents lived at the time when the student applied for college. In each panel, the dots represent 20 equal-sized bins based on the variable on the $x$-axis, and the solid line represents a linear regression on the entire dataset. The figure shows a strong correlation between family background and major choice: Students whose parents have high credit scores and students who grew up in high-income neighborhoods are more likely to choose high-earning majors.

These raw correlations could be driven by college selection. For example, Chetty et al. (2020) find that many of the nation's elite colleges have more students with family incomes in the top 1% than in the bottom 60%. If these college also happen to offer more high-earning majors (e.g., if unconditionally, more students major in Economics at Yale, while more students major in English at a community college), then my results could be driven by income-based selection into these colleges, rather than by family background in itself.

To isolate the variation due to college selection, I perform a descriptive analysis that includes gender, race, school, and year fixed effects. The results are reported in Figure 1, Panels C and D. I find the same strong relationship between family background and major choice even when controlling for school and year. In other words, even among students who graduated from the same college at the same time, those from low-income or low-credit-score families are more likely to choose low-earning majors.[21]

[21] In the Online Appendix, in Table A7, I report the full regression coefficients both with and without school fixed effects, as well as with the full set of school-year controls. In line with the results on selection reported by Chetty et al. (2020), we see that the coefficients are larger without school fixed effects. This indicates that wealthier parents (and parents with higher credit scores) send their children to more prestigious universities, where the students, on average, chose higher-earning majors.

Interestingly, while there is a linear relationship between credit score and major choice across the entire credit score distribution, this is not the case for neighborhood income. The 5% of students who grew up in the wealthiest census tracts are less likely to choose high-earning majors than students who grew up in middle-class and upper-middle-class neighborhoods.

My findings are consistent with descriptive evidence provided by Arpita et al. (2020). They show that among first-generation college graduates, those whose parents have the highest education levels are more likely to choose higher-return majors, such as physics, while students whose parents have lower education levels choose lower-return majors such as education or social work. However, these estimates do not account for institutional differences; for example, students who attend public universities may be statistically different from students who attend private universities. My analysis controls for these differences by including estimates for students within the same university. My findings are also consistent with contemporaneous work by Leighton and Speer (2024), who find that students from high-income families choose high-earning majors.

Of course, all the standard caveats of ordinary least squares regression apply, and we cannot interpret these results as showing causation. The heterogeneity in students' major choices could be explained by several factors not related to financial frictions, such as differences in college preparedness or differential exposure to specific types of majors and occupations. For example, students whose parents are lawyers or doctors might prefer these fields of study, independent of any financial frictions they face (Xia, 2016). Therefore, I now turn to an empirical framework that allows me to estimate the causal effect.

# 4 Empirical Design

In this section I outline the empirical design for my analysis. I first describe the baseline event-study model and discuss several identifying assumptions. I then introduce an alternative intention-to-treat (ITT) model and an instrumental variable (IV) approach, which allow me to refine my inferences from the analysis.

## 4.1 Baseline Specification

The empirical strategy is to study the effect of UNLPs on major choice, comparing individuals who were treated by a UNLP to those who were not using an event study methodology, or a generalized difference-in-differences (DID) design with staggered adoption. I estimate the model:

$$E(y_{i,s,t}|\mathbf{X}_{i,s,t}) = \sum_{\tau \neq 0} \beta_\tau CohortRelativeUNLP_{s,t} \times Treated_i + \gamma_s + \gamma_t + \theta_{s,t} + X_i \qquad (1)$$

Here $y_{i,s,t}$ is the outcome of interest, such as whether individual $i$ chooses a high-earning major. The variables $\gamma_s$ and $\gamma_t$ represent school and year fixed effects, respectively, and $\theta_{s,t}$ is a vector of school-year control variable that accounts for changes (e.g., in tuition) within a school over time. The variable $X_i$ is a vector of person-level demographic characteristics. The variable $CohortRelativeUNLP_{s,t}$ represents the set of event dummies, for $\tau \in [-3, 3]$, with the effect at $\tau = 0$ (the last year before UNLP implementation) normalized to 0. Finally, $Treated_i$ is a dummy variable taking the value one if individual $i$ had a positive amount of student debt before UNLP implementation.

The coefficient of interest is $\beta_\tau$, which captures the relative probability of choosing a high-paying major for students with student debt in event-year $\tau$. In Section 5 below, I report both the individual event-year coefficients, $\beta_\tau$, as well as the pre- and post- policy effects (i.e., $\frac{1}{3}\sum_{\tau=-1}^{-3} \beta_\tau$ and $\frac{1}{3}\sum_{\tau=1}^{3} \beta_\tau$). In all specifications, I scale the coefficients by the mean of the variables at $\tau = 0$ and cluster at the school-cohort level.

The baseline specification estimates the impact of a UNLP on the students who are most likely to be affected by it—namely, those who have already taken on student debt. The effect of student debt most likely differs across types of students (something I explore later in this paper). For this reason, I refer to the baseline specification estimates as measuring the average treatment effect on the treated (ATT).

## 4.2 Identifying Assumptions

In order to interpret the estimates from the event-study specification as the causal effects of UNLPs on major choice, I rely on two key identifying assumptions: (i) no anticipatory effects, and (ii) parallel trends (Sun and Abraham, 2021; Borusyak et al., 2024). The no-anticipatory-effects assumption is that individuals do not change their behavior in anticipation of the treatment. The parallel-trends assumption is that, absent treatment, the difference in potential outcomes would be the same across individuals and in all periods, conditional on the set of controls and on school and time fixed effects.

The main threat to identification is that unobserved changes in the composition of students with and without debt can explain both the timing of UNLP implementation and changes in major choice. For example, students with debt, who were specifically interested in studying high-earnings majors may pressure and convince administrators to implement UNLPs.

Several pieces of evidence suggest that this is unlikely to be the case. First, as described in Section 1, universities largely adopted UNLPs in order to eliminate financial barriers to attendance and diversify their student bodies; there was no explicit goal of influencing students' major choices or post-graduation plans. Second, if the adoption of UNLPs had been spurred by changing preferences for certain majors, then we would expect to see changes in the numbers of students choosing those majors immediately before UNLP implementation; I observe no such changes in the data. Third, my estimates include students from 10 control schools that did not implement UNLPs, but that were otherwise similar to the treated schools. (The control schools include, for example, CalTech, Duke, MIT, and Wellesley. The full list is provided in Table A1.) Any changes in student preferences would likely have affected major choices at the control schools; reassuringly, however, the estimates that include the control schools are virtually indistinguishable from those that do not.

A second potential concern is that there could be a selection bias in the types of students who attend a given university before or after it implements a UNLP. For instance, a particular type of student might be more likely to apply to and enroll in universities with UNLPs. I address this concern by restricting the event window to include only graduation years up to $\tau = 3$. This means that I am only studying students who were already enrolled at the time of the UNLP implementation.[22]

A third potential concern is that there might be heterogeneity in the treatment effect across cohorts. A growing econometrics literature has shown that two-way fixed effects (TWFE) models provide a weighted average of each cohort-specific coefficient.[23] This weighted estimate can sometimes be biased away from the standard interpretation of a difference-in-differences estimate, especially if the treatment effect differs across cohorts. To address this concern, I also estimate the bias-correcting difference-in-differences event-study regression model recommended by Sun and Abraham (2021).[24]

[22]I find no evidence of changes in the composition of students already enrolled. In Appendix Figures A7 and A8, I plot fraction various student characteristics of those enrolled at the time of policy implementation. In Appendix Figure A8, I plot the TWFE coefficients from a regression of SAT scores and racial groups on UNLP implementation. I find that the policy had no impact on SAT scores and racial groups of those admitted prior to the policy implementation. Additionally, in Appendix Figure A9, I find that the policy had no impact on completion rates of already enrolled students.

[23]See, for example, Borusyak et al. (2024), de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021), and Sun and Abraham (2021).

[24]The problem arising from negative weights is less likely to be an issue in my setting. This issue often arises when former treated units act as control units for later treated units. My setting includes two sets of additional control students: Students without debt at UNLP schools and all students at non-implementing

## 4.3 Intention-to-Treat Specification

Next, I estimate an intention-to-treat (ITT) model, which captures the average effect of UNLPs on major choice for the entire student population, regardless of whether the policy affected their student debt or not. I estimate the following model, where the notation is the same as above in equation (1):

$$E(y_{i,s,t}|\mathbf{X}_{i,s,t}) = \sum_{\tau \neq 0} \beta_\tau CohortRelativeUNLP_{s,t} + \gamma_s + \gamma_t + \theta_{s,t} + X_i \tag{2}$$

The interpretation of $\beta_\tau$ in (2) is different from the interpretation of $\beta_\tau$ in (1). In Equation (2) (the ITT model), $\beta_\tau$ captures the average effect of UNLP for all students (even those unaffected by the policy). The ITT model has the benefit that it can be estimated on a longer event window with event dummies $\tau \in [-5, 6]$. There are two reasons for this. First, since the ITT model does not rely on observing student debt at the individual level, I can extend the event window to include $\tau = -4$ and $-5$.[25] Second, I can also extend the event window forward as well, tracking students enrolled *after* UNLP implementation, i.e. $\tau = 4$, 5, and 6.[26]

The longer event-window in the ITT model offers two important credibility checks on the ATT model. First, the longer pre-period offers a stronger test of whether UNLP implementation was driven by changes in students' preferences for certain majors. To analyze whether the timing of UNLPs is correlated with changing preferences for specific majors I do two things. I report the coefficients on the event dummies for the six years preceding the implementation of the policy ($\tau$=-5 to $\tau$=-1, where $\tau$=0 is the omitted year) and I report the average pre-period effect by reporting the coefficient and standard error for $\frac{1}{5}\sum_{\tau=-5}^{-1} \beta_\tau$. When evaluating the individual event-year coefficients, I do not find an increasing trend. And I do not find any significant negative pre-period effect. Reassuringly, while almost all specifications yield statistically insignificant estimates for the pre-period, in some specifications, the point estimate is positive, while in others it is negative.

Second, by estimating the effects for a longer post-treatment event window, including

control schools. In support of this argument, I find that the Sun and Abraham (2021) coefficients are statistically indistinguishable from the standard event-study coefficients.

[25]The first calendar year that credit bureau variables appear is 2004. Since a large number of UNLPs were implemented in 2008, I do not have statistical power to extend the event-window back to -5 for equation (1).

[26] In the ATT model, I cannot include students who enrolled after the policy. This is due to the fact that there are some students who are affected by the policy but will not take on student debt. This is because student's can choose to take debt or not. Mathematically, for these students, $E[\text{student debt}|\text{No UNLP}] > 0$ and $E[\text{student debt}|\text{UNLP}] = 0$. This group of students would be misclassified as control student since $Treated_i = 0$ for them.

for several cohorts of students who enrolled after the implementation of a UNLP, we can assess whether policies or secular trends unrelated to the UNLP might have affected major choices during that period. Specifically, a UNLP provides equal benefits to students graduating in, say, cohorts $\tau = 4$ and $\tau = 6$, so it should affect the major choices of students in these two cohorts equally. On the other hand, if other policies affecting major choices were implemented around the same time, their effects would be evident in the cohorts corresponding to their implementation, leading to changes in the coefficients even after the UNLPs had been fully implemented. I find no evidence of such changes.

### 4.4 Instrumental Variable Approach

To quantify the causal effect of each dollar of student debt on major choice, I implement an instrumental variable (IV) approach using the implementation of UNLP as an instrument for the amount of student debt. I use a standard two-stage least squares estimator. In the first stage, UNLP implementation is treated as the source of variation in student debt:

$$StudentDebt_{i,s,t} = \sum_{\tau \neq 0} \beta_\tau CohortRelativeUNLP_{s,t} + \gamma_s + \gamma_t + \theta_{s,t} + X_i \tag{3}$$

where $StudentDebt_{i,s,t}$ is the amount of student debt at graduation for individual $i$ at school $s$ in cohort $t$. In the second stage, I regress the same outcome variables used in regression 1 on the predicted value of student debt from equation (3):

$$E(y_{i,s,t}|\mathbf{X}_{i,s,t}) = \eta\ \widehat{StudentDebt}_{i,s,t} + \gamma_s + \gamma_t + \theta_{s,t} + X_i \tag{4}$$

The central identifying assumption for this strategy is that the implementation of a UNLP is a valid instrument for student debt. This requires (1) a strong first stage, (2) that the UNLP is an independent instrument, (3) that the exclusion restriction holds, and (4) that the UNLP affects student debt monotonically.[27] Granting these assumptions, the coefficient $\eta$ captures the local average treatment effect of student debt on major choice. That is, $\eta$ captures the causal effect of student debt for students whose debt was affected by a UNLP. For example, given appropriate scaling, the interpretation of $\eta$ is *the effect of $10,000 of student debt on the probability of choosing a high-earning college major*.

[27] In Appendix Section G, I discuss these four assumptions in detail.

# 5 Results

In this section, I present the empirical results. First, I show that the implementation of UNLPs had an immediate impact on student loan balances. I then turn to the main result and present the estimates on how UNLPs impacted the propensity to choose high-earning majors. I explore the heterogeneity of these results across student characteristics (e.g., family income levels) and categories of majors. Finally, I report the IV results.

## 5.1 Effects of UNLP on Student Debt

In this subsection, I present the results of UNLPs on student debt take-up using both publicly available data and the hand-collected commencement program data merged with credit bureau data. I first consider how the implementation of UNLPs impacted student loan balances.

First, I present the results using the main sample with micro-data. Figure 2 shows the coefficients from equation (2) with the dependent variable is the student loan balance in the year of graduation.[28] The sample includes all treated students with a positive amount of student debt prior to the policy. There is a sharp discontinuous drop in the amount of student loans that students have that precisely coincides with the timing of the policy. To assess the robustness of this result, I plot the regression results from both the baseline TWFE, the TWFE with control groups, and two bias-corrected models allowing for treatment heterogeneity across cohorts (Sun and Abraham, 2021 and Callaway and Sant'Anna, 2021). The results are similar across all four models.

Table 2 reports the average coefficients before and after UNLP implementation. (in the Online Appendix, I report the full set of coefficients in Table A8.) Across the four models, there is no significant change in the three years before the policy relative to the omitted year. The estimates imply that in the three years following the policy, on average, students experienced a decline of between \$7,000 and \$7,700, depending on the specification.[29]

To validate the results, I replicate the findings using publicly available data from IPEDS. Using IPEDS, I estimate a Difference-in-Differences (DID) model and an event study model. Table A16 reports the DID estimates, where the dependent variable is the

[28]The credit bureau data offers a yearly snapshot on December 31. Therefore, the student loan balance variable captures the amount of student loan debt on December 31 on the year that the student graduated.

[29]To compare these estimates with those found in the literature, I run the same specifications on the entire sample of students and expand the event years to include up to six years post- policy implementation. I find that, following the implementation of the UNLP students, on average, have \$4,000 less in student debt for those already enrolled and \$9,000 less in student debt for those enrolling after the policy. This estimate is close in magnitude to the reduction in student loans of \$11,000 that Rothstein and Rouse (2011) find.

fraction of undergraduates with student debt. It compares UNLPs to other financial aid programs, including income-specific NLPs, loan caps, parental contribution elimination, and tuition waivers (for example, some colleges implement NLPs for families whose incomes fall below a specific level). UNLPs meaningfully decrease the percentage of students taking loans; specifically, they lower it by 12.6 percentage points.

Next, to understand the dynamic effects, Figure A6 reports the results from a standard event-study regression. The plot shows the coefficients on the year relative to implementation. In line with my previous results, we see a sharp drop in the fraction of students taking out loans that precisely coincides with the timing of the policy. To assess the robustness of this result, in Figure A6, I plot the regression coefficients and 95% confidence intervals from three different regressions of the percentage of students taking a student loan on year dummies relative to the implementation of a UNLP. A standard two-way fixed effect (TWFE) model is reported in blue, a TWFE model with 10 non-implementing control schools is reported in red, and two bias-corrected models allowing for treatment heterogeneity across cohorts. Across all four specifications, in the year of UNLP implementation, there is a significant drop in the share of students who take student loans.

Taken together, these results confirm that student loan use fell dramatically following the implementation of UNLPs.

## 5.2 Effects of UNLP on Major Choice

I next consider how UNLPs affect the propensity of students to take a high-earning major. Figure 3 plots the event-time coefficients and their 95% confidence intervals from estimating equations 1 and 2, with the dependent variable being an indicator for whether a given student graduated with a high-earning major. Figure 3a plots the baseline coefficients from the TWFE. Figure 3b plots the coefficient estimates from the TWFE with added control schools. Figure 3c plots the alternative estimator proposed by Sun and Abraham (2021). I summarize the magnitude of the effects in Table 3 and report the full set of coefficients in Appendix Table A9.

On average, I find that treated students are more likely to graduate with a high-earning major following the introduction of a UNLP. In all specifications, event-year 1 ($\tau = 1$) corresponds to students who were seniors at the time of UNLP implementation, and who were therefore treated for one year; similarly, event-year 2 ($\tau = 2$) corresponds to juniors, who were treated for two years, and event-year 3 ($\tau = 3$) corresponds to sophomores, who were treated for three years. Given this variation in treatment intensity, one would expect the coefficient estimates would be increasing for each cohort (in $\tau$).

This is indeed the case: Across all specifications, there is no effect on seniors,[30], moderate for juniors, and the largest for sophomores. Juniors are 2% more likely to graduate with a high-earning major. Sophomores are 6% more likely to graduate with a high-earning major.

To validate my empirical approach, I check for pre-treatment trends across all of the specifications considered in Figure 3. I find that the pre-treatment coefficients are small in magnitude and are not statistically different from zero. Moreover, the effects of UNLPs on propensity to choose a high-earning major display similar patterns across various specifications and estimators, aligning closely with the baseline results.

The results are confirmed by the ITT estimates, which are shown in Figure 3, Panels D, E, and F (corresponding to the baseline TWFE, the TWFE with control schools included, and the alternative estimator of Sun and Abraham (2021), respectively). As previously described, the ITT specifications include the students in the cohorts $\tau = 4, 5, 6$, i.e., those who enrolled after UNLP implementation and were therefore treated for the entirety of their college careers. The ITT estimates are approximately half the size of the ATT estimates. Tables A9 and A10 in the Online Appendix report the full set of coefficients.

The ITT estimates provide additional support for the identifying assumptions. Specifically, when evaluating the effects of UNLPs on students who enrolled after UNLP implementation ($\tau = \{4, 5, 6\}$), I do not find any increasing time trend. In other words, I do not find evidence of contemporaneous changes in student preferences for major choice.

To quantify the results, Table A11 reports the IV estimates from the two-stage least squares model. I scale the coefficients by ten thousand, such that they represent the effect of a \$10,000 decrease in (predicted) loan amount. For each \$10,000 decrease in student loans, students are 4% more likely to choose a high-earning major.

## 5.3 Heterogeneity by Family Background

As noted in the introduction, most of the previous literature studies the effects of student debt for the average student. However, students' investments in human capital are often at least partially financed by their parents, and it is natural to assume that the parents' financial wealth can act as a credit constraint. I now consider how the effects of UNLPs

[30] At most universities, students generally choose their major within their first three years and would not have time to change it in response to the implementation of a UNLP in their senior year. Consequently, one should not expect a nonzero effect for seniors.

on major choice vary with socioeconomic status. I augment the baseline regression to

$$E(y_{i,s,t}|\mathbf{X}_{i,s,t}) = \sum_{\tau \neq 0} \beta_\tau CohortRelativeUNLP_{s,t} \times Treated_i \times SES_i + \gamma_s + \gamma_t + \theta_{s,t} + X_i \quad (5)$$

where $SES_i$ is a proxy for the student's socioeconomic status of student $i$ when applying to college. Specifically, $SES_i$ represents either the parents' credit score or the average income in their census tract(s), and it may be low, middle, or high (as defined by tercile).

Figure 4 plots the event-time coefficients and their 95% confidence intervals from estimating equation (5) for each tercile. Figure 4 Panels A and B show the ATT coefficients for TWFE models with $SES_i$ representing credit score and neighborhood income, respectively. Similarly, Panels C and D show the ITT coefficients for TWFE models with $SES_i$ representing credit score and neighborhood income, respectively. I summarize the magnitude of these effects in Table 4.

On average, following the implementation of a UNLP, middle- and low- credit score and neighborhood income groups see the largest relative increase in the probability of graduating with a high-earning major. Specifically, for students affected as freshmen ($\tau$ = 3), I find that low-credit score students see a relative increase of 9% while middle- and high-credit score students see an increase of 6% and 4%, respectively. The results are similar for neighborhood income, with increases of 9%, 5%, and 4%.

DID estimates confirm that the effects are statistically larger for low-SES students. Table 4 report the average coefficients from equation (5) where the SES interaction dummy takes the value one when the parents have a below-median credit score or when the parents live in a below-median income census tract. I find that students with below-median parental credit scores see a 5% increase in the likelihood of choosing a high-earning major, and students with below-median neighborhood incomes see a 3% increase.

# 6 Plausible Mechanisms

In this Section, I explore potential economic mechanisms driving the empirical results. First, I document that college majors differ not only by their average life-time earnings, but also by their earnings trajectories across the life-cycle. And I find that following UNLPs, students chose majors with lower initial earnings and higher earnings growth. Furthermore, I find that these college majors on average have more graduates who go to graduate school, have higher earnings variance, are more difficult, have higher GPAs, and require more time during college.

## 6.1 The Intertemporal Trade-off

In this Section, I analyze how labor *trajectories* vary across college major. I document that the choice of college major involves an intertemporal trade-off: Students trade off majors with higher initial earnings and lower lifetime earnings against majors with lower initial earnings and higher lifetime earnings. Interestingly, student debt affect students' choices along this trade-off. I decompose the effect of UNLP on the selection of high-earning majors, and I show that students who are required to take less debt to finance their education not only choose majors with higher lifetime earnings but they also choose majors with lower initial earnings.

### 6.1.1 Earnings Trajectories across College Majors

I begin by calculating average earnings trajectories by college majors, using data on earnings by major from the 2009–2019 ACS in two steps. First, I residualize earnings for observable characteristics of ACS survey participants, and second, I construct four variables for each major: (1) mean of wages, (2) standard deviation of wage, (3) mean of initial wages, and (4) slope evaluated at age 21.[31] See Appendix I for details on the residualization and variable construction.

Lifetime earnings trajectories vary greatly across majors. Figure A12 illustrates these differences. This figure plots a binned scatter plot residualized annual wages relative to age across the 15 most frequent college majors. I highlight four majors, Biology, Education, Physical Sciences, and Psychology with vastly different wage profiles. If a student majors in education or psychology, their earnings right out of college is high, but the wage growth is relatively low. We can compare this to a student who majors in biology and goes to medical school. This person has a low wage initially after college due to investment in human capital but the slope is much higher. Education has an average annual wage of $48,000, while Biology has an average annual wage of $87,000. The difference between Biology and Education is higher than the equivalent difference between the average student with a college degree and an individual with only a high school degree.[32] There are

[31]One important caveat when using residualizing earnings to construct ex-ante measures of earnings trajectories is that the major specific earnings profiles in the ACS are not representative of college students beliefs of what they can earn in the labor market conditional on their major choices. The question is how large the deviation between major specific *expected* earnings is from realized earnings. Patnaik et al. (2022) utilizes a sample of New York University students and elicits beliefs on earnings across the life cycle where they compare beliefs about the earnings distribution with the actual life cycle growth of earnings for respondents in the American Community Survey (ACS). They find that student's beliefs of earnings for each major qualitatively matches the ACS quite well.

[32]The average annual wage for Biology minus Education is $87,000 - $48,000 = $39,000. For College minus High School, the gap is $64,000 - $28,000 = $38,000.

similar differences when we compare cumulative earnings and if we compare majors by their discounted present value of earnings.[33] On the other hand, as Figure A12 shows, while biology and physical science majors have high lifetime earnings, they have lower initial earnings than psychology and education majors. This illustrates that students trade of higher initial earnings against higher lifetime earnings when choosing majors.

To formalize this intertemporal trade-off between initial and life-time earnings, in Figure A13, I plot the average wage residuals by major at ages 45-47 agains the average wage residuals at ages 25-27. I find that there is a negative relationship between initial earnings out of college (at ages 25 to 27) and earnings in the middle of your career (at ages 45 to 47).[34] The intertemporal trade-off means that college majors with high life-time incomes have back-loaded earnings trajectories. Using wages in either logs (Figure A14) or levels (Figure A15), I find that there is a strong positive relationship between life-time earnings and post-college wage growth. Specifically, college majors with high life-time earnings, such as Biology and the Physical Sciences have back-loaded earnings and the majority of their life-time earnings come in later part of their life-cycle. On the other, majors with low life-time earnings, such as Education and Psychology, do not experience the same wage growth post college.[35]

I find that the intertemporal trade-off is closely related to the financial and socioeconomic status of parents. Specifically, in Figure A16 and A17 I show that students who have parents with low credit scores and students who grew up in low-income neighborhoods, choose majors that are associated with high initial earnings out of college but low lifetime earnings. I find that this relationship holds both unconditionally and when controlling for demographic characteristics and school and year fixed effects.

[33] The cumulative wage gap for Biology minus Education is \$3.6m - \$2.1m = \$1.5m. For College minus High School, the gap in cumulative wages is \$2.6m - \$1.2m = \$1.4m. The present value gap for Biology minus Education is \$1.6m - \$1.0m = \$1.6m. For College minus High School, the gap in cumulative wages is \$1.1m - \$0.5m = \$0.5m. When computing the present value of wages, I use the approach of Gourinchas and Parker (2002) and use a $\beta = 0.96$.

[34] I am very grateful to Tyler Ransom for suggesting this specification. In Panel (a), I plot the top-15 majors weighted by the number of students, and in Panel (b) I list the names of each of the majors.

[35] In order to project ACS major-earnings trajectories onto my sample of students, two assumptions need to be valid. First the ordinal rankings of majors need to remain constant over time and second is that the earnings of student's in the UNLP sample need to evolve similarly to the earnings of those in the ACS. In order to test these assumptions, I use ACS data, College Scorecard data, and *model implied* earnings that are obtained from the Experian match. (Experian sells data called Income Insight which is an income estimation model using credit bureau attributes. Experian trains a model based a subsample of income data that includes, wages, salaries, tips, investment, rental & farm income, unemployment compensation, and social security benefits.) When comparing the correlation amongst data sets, I find support for both assumptions. However, it is important to note that the levels are different. Specifically, the wages in the ACS are about 30% lower than that of Colleges Scorecard or Experians Model Implied wages. This means that this quantification will be a lower bound for where wed expect the true earnings changes. For more details on the validation of these assumptions, see Appendix Section I.II.

### 6.1.2 Decomposing High Earnings By Initial Post-College Earnings

I find that the effect of UNLPs on selection of high-earning majors is entirely driven by selection into high-earning majors with relatively low initial earnings.

Specifically, I decompose the main result using the following two steps. First, I rank the 12 majors that were classified as high-earning majors by their initial earnings. Among the 12 high-earning majors, the six majors with the highest initial earnings are (in ascending order) Mathematics, Engineering, Computer Science, Finance, Business, or Public Administration. These majors are associated with *front-loaded* earnings. On the other hand, the six majors with the lowest initial earnings are (in ascending order) Biology, Chemistry, Physical Sciences, other, Physics, Political Science, or Economics. These are all majors that are associated with *back-loaded* earnings profiles.

Second, I estimate equation 1 where the dependent variable is either an indicator functions for whether a student chose either a "High lifetime & High initial wage" major or a "High lifetime & Low initial wage" major. Table A12 reports the results. I find that there is no effect in the first three years of UNLP on the share of students choosing "High lifetime & high initial wage" majors. In other words, there is no effect of UNLPs on the share of students choosing majors from the group including Engineering, Finance, and Business. On the other hand, there are large effects of UNLPs on the share of students choosing "High lifetime & Low initial wage" majors. I find across the first three years the UNLPs on average increased the share of students choosing this group of majors by 5%. And for sophomores the effect is more than an 8% increase.

Taken together, these results highlight that UNLPs increased the share of students who chose high-earning majors associated with back-loaded earnings trajectories (such as Biology and Physics) while there was no effect on high-earning majors with front-loaded earnings trajectories (such as Business or Engineering).

### 6.1.3 The Effect of UNLP on College Majors by Labor Trajectories

Next, I study more generally the effect of UNLPs on college majors by earnings trajectories. Specifically, I run the baseline regression model (equation (1)) where the dependent variable is the implied wage characteristics at different points in time.

Table A13 reports the regression coefficients. Column 1 shows the event-study coefficients for the mean, Column 2 for the slope, Column 3 for the initial earnings, and Columns 4 through 6 for the average earnings across three different decades (the 20s, 30s, and 40s, respectively). I find that after UNLP implementation, treated sophomores choose majors associated with increases of $1,700 in mean earnings, a 57 basis points increase in earnings slope, a decrease of $470 in initial earnings, a (statistically insignificant) decrease of $198 in annual earnings in the 20s, and an increase in annual earnings of $736 and $2,100 in the 30s and 40s, respectively.

These results highlight that higher student debt can simultaneously lead to both higher and lower earnings at different points in a student's career: Students with less debt are more likely to choose careers that have relatively *lower initial wages* after college and *higher earnings growth* in the long term.

My results are consistent with results reported in several recent papers studying earnings trajectories across college majors. Martin (2022) separates majors in general vs. specific, and finds that specific majors initially earn more than general majors, general majors have steeper earnings growth and the initial earnings premium of specific majors decreases substantially.[36] Huang (2023) finds, using National Longitudinal Survey of Youth 97 (NLSY97) empirically, the majors that poorer students choose majors that have flatter earnings-age profile (i.e. higher initial earnings, lower earnings growth) and lower earnings risk. Leighton and Speer (2024) find that majors like nursing, education, and business rank higher in average earnings early in the career than in mid-career, while majors like biology, history, and philosophy follow the opposite pattern.

## 6.2 Graduate School, Occupations, and Time Use in College

To further explore the economic mechanisms driving the main results, I analyze five additional characteristics that may affect students' major choices, especially in the presence of financial frictions.

### 6.2.1 Characteristics of College Majors

The first characteristic is whether college graduates with a given major is more or less likely to attend graduate school. Using the ACS data, I calculate the average residualized share of undergraduates that attend graduate school by college major. There is large variation across college majors, and the share of students attending graduate school does not align monotonically with average life-time earnings. For example, among high-earning majors, 56% of Biology majors attend graduate school, while only 23% of Business majors

[36] Martin (2022) differentiates majors using the occupational concentration of recent college graduates. Some majors including Public Health, Sociology, Economics and Journalism are general because their skills map less directly to particular jobs and graduates are widely dispersed across occupations. Other majors are specific because their skills are only productive in certain work settings. While science, technology, engineering and mathematics (STEM) majors tend to be specific, non-STEM majors including Nursing, Teacher Education and Accounting are also specific.

and 39% of Engineering majors. Among low-earning majors, only 24% of students majoring in the Arts go to graduate school, while 53% of students majoring in Philosophy or Religious Studies go to graduate school.

The second characteristic is the residualized variance of earnings, also calculated using the ACS. Among high-earning majors, graduates with a major in Biology and Economics have high earnings variance, while graduates with a major in Engineering and Computer Science have low earnings variance. Among low-earning majors, graduates with a major in Foreign Languages have high earnings variance, while graduates with a degree in Sociology have low earnings variance.

The third characteristic is self-reported difficulty of the major. Using data from Novik (2023), who has created a novel dataset from a popular website compiling hundreds of thousands of student course evaluations. Each evaluation includes a self-reported level of difficulty, that has been aggregated by major. The most difficult majors include biomedical sciences (3.62/5), chemical engineering (3.49/5), and molecular biology (3.42/5), while the least difficult include secondary education (2.38/5), elementary education (2.41/5), and human resources and personnel management (2.66/5). I merge these difficulties to my classification of majors and take averages.

For the fourth characteristic, I analyze students' major choices in terms of GPA. I use data from Rask (2010), who provides statistics on mean GPA by major. The majors associated with the highest mean GPA include education (3.36/4) and languages (3.34/4), while those with the lowest mean GPA include chemistry (2.78/4), mathematics (2.9/4), and economics (2.95/4).

Finally, using results from the 2016 National Study of Student Engagement (administered by Indiana University), I obtain the average number of hours per week that students study, as reported by undergraduate students in each major. According to this survey, the majors requiring the most study time per week include chemical engineering (20 hours) and biomedical sciences (18 hours), while those requiring the least include social work (12 hours) and management (13 hours). I average these hours by my major classifications.

#### 6.2.2 Effects of UNLP on Characteristics

To make the results comparable with the baseline results, I create a dummy variable for each major characteristic, which takes the value of one if the major has an above median value of the respective characteristic. I then estimate equation (1) using each of the dummy variables as the dependent variable.

Table A14 report the results. In Columns 1 and 2, I report the results using characteristics from the ACS. I find that following the implementation of UNLPs, the coefficients for sophomores that were treated for three years are positive and statistically significant. In other words, the majors that students change into are majors with higher graduate school optionality and with higher earnings variance.[37] Using the resume dataset, I confirm that following the UNLP, students are also more likely to report a graduate degree on their resume. (This result is reported in Table A15.)

As a supplemental exercise, I ask whether UNLPs change the types of jobs that students take. I do this to validate the results on major choice. Specifically, we would expect to find that if student's are taking more majors that have higher optionality for graduate school, are higher slope, and are riskier, that these choices are reflected in their occupation choice.[38] Figure A18 reports the results from the model (2) for nine different job types. We see that following UNLP implementation, when student debt decreases, the probability that a student will become a teacher, nurse, or physician's assistant within 7-years of graduating from college, decreases. Interestingly, all of these jobs are characterized by stable earnings and lower expected earnings growth. In contrast, the probability that a student will become a doctor, lawyer, author, artist, or entrepreneur increases. These jobs are characterized by either low initial earnings after college (many prospective doctors and lawyers effectively have negative earnings while they are in graduate school and pay tuition), or high earnings risk (artists, authors, and entrepreneurs[39]). These results are consistent with findings reported by Chakrabarti et al. (2022) who find that students with more student debt are less likely to go to graduate school.

In Table A14 Columns 3, 4, and 5, I report the results where the characteristics are majors sorted by difficulty, average GPA, and Hours. The coefficients are positive, and statistically significant at the 5%-level for GPA and Hours, and at the 10%-level for difficulty. These results suggest that by removing financial frictions, UNLPs induce students to choose majors that require more hours of work, are rated as more difficult, and are associated with lower grades. These findings are in line with recent findings from Luo and Mongey (2019) who show that financial assets affect the choice of non-wage amenities in job search.

[37] The other potential tradeoff that students make when choosing a major is a risk-return tradeoff as in Saks and Shore (2005).

[38] At this time, the credit bureau data is not merged with the resume data. Therefore, I present the estimates from the ITT model in Equation 2

[39] The increase in entrepreneurship is consistent with Krishnan and Wang (2019), who find that NLPs (both targeted and universal) increased the likelihood that graduating student founded a start-up venture.

### 6.2.3 Effect of UNLP on Credit Market Outcomes

The previous sections provide estimates on how UNLPs affected major choices along a number of characteristics. These characteristics are averages, and one obvious caveat is that I do not observe, for example, individual ex-post earnings.[40] However, from the credit bureau dataset, I do observe several credit market outcomes which are related to both income and wealth. Specifically, I test whether UNLPs increased the takeup of graduate school debt, auto loans, and mortgages.

The results are reported in Table A15. I find that following UNLP implementation, in the five years following graduation, that students are 6% more likely to take on additional student loans (which I interpret as debt for graduate school.) They are 6% more likely to take on auto loans and 4% more likely to have a mortgage (although only significant at the 10-% level). I find that they are less likely to have student debt in delinquency, but I do not find any significant results in terms of auto loans or mortgage delinquencies. Finally, I find that students are younger when they obtain graduate school debt and auto loans. Specifically, students are more likely to attend gradate school, they are more likely to own a car, and they are more likely to own real estate.

Combined, these results indicate that in addition to changing students' major choices, they also had lasting real effects.

## 7 Conclusion

This paper presents novel estimates on how the type of education financing affects students' choice of college major. The main result is that UNLPs, which replaced student loans with grants as the source of financing, increased the number of students choosing a high-paying major by 6%. The strongest effects are for students from low-income and low credit score families, and the policies led students to choose majors with lower initial but higher lifetime wages. Taken together, the results imply that investing in human capital inherently involves an intertemporal trade-off and that financial frictions affects students choices along this trade-off.

The findings in this paper point to several interesting areas for future research and potential implications for public policy. First, analyzing individual micro-data that combines students educational choices with data on their parents will help us better understand the fundamental drivers of economic mobility. For example, Huang (2023) and

[40] As discussed in previous sections, I do observe model-implied earnings from Experian, and I provide supplemental evidence using these measures in the Online Appendix.

Leighton and Speer (2024) study the role of parent income and education on students' major choices and Boar and Lashkari (2022) study the family-preferences over occupational choice and the effects on intergenerational mobility.

Second, since the UNLPs were implemented for a broad sample of students at an early stage of life—before large human capital decisions had been made—the estimated elasticities can provide the empirical underpinnings for theoretical models of the effect of student debt on human capital investments and labor supply. For example, Boutros et al. (2024) and De Silva (2024) estimate dynamic life-cycle models to quantitatively evaluate the welfare implications of payment deferral and income-contingent repayment plans.

Finally, the results in the paper speak to the current debate on admissions policies at elite universities. In recent work, Chetty et al. (2023) document that elite institutions in the U.S. amplify the persistence of privilege across generations, and they argue that these institutions could diversify the socioeconomic backgrounds of Americas leaders by admitting more students from low-income families. Importantly, the results in this paper suggest that merely providing loans and *access* to college is not a panacea for inequality in future labor-market outcomes. Policymakers and university admissions officials should consider how financial frictions and education financing options affect human capital investments both during and after college, and how the effects differ across the student population.

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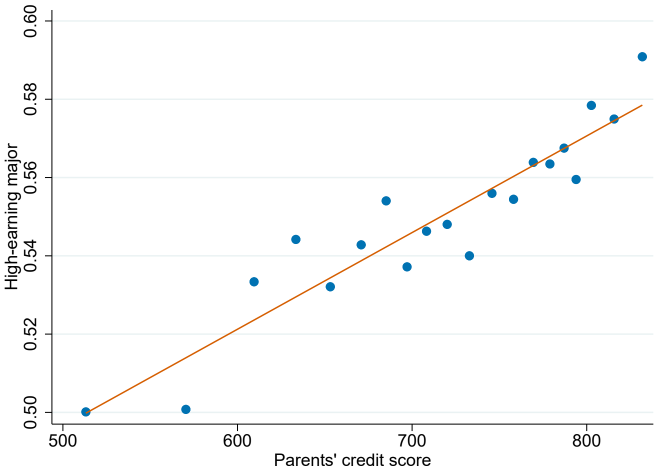
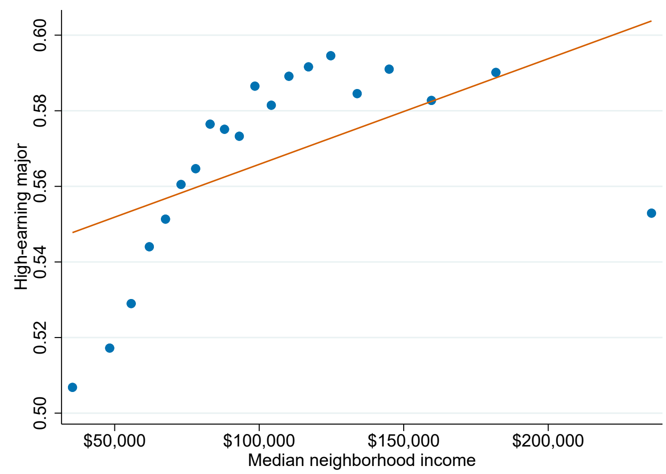
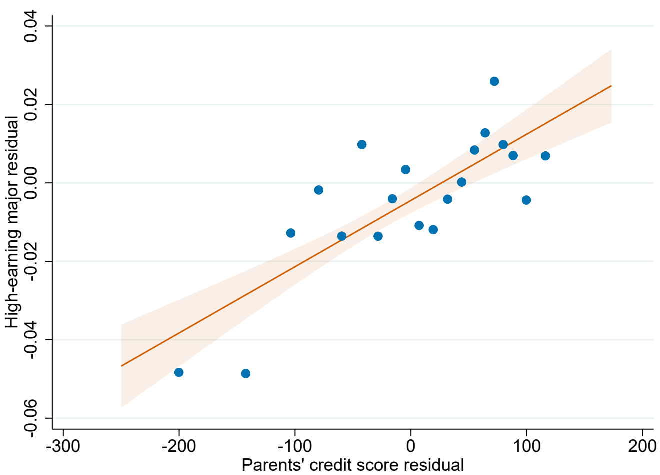
FIGURE 1: Family Background and College Major Choice



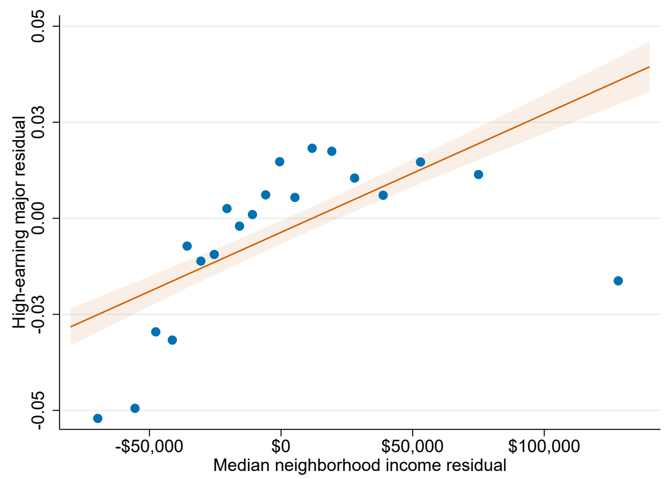

(a) Credit Score




(b) Neighborhood Income



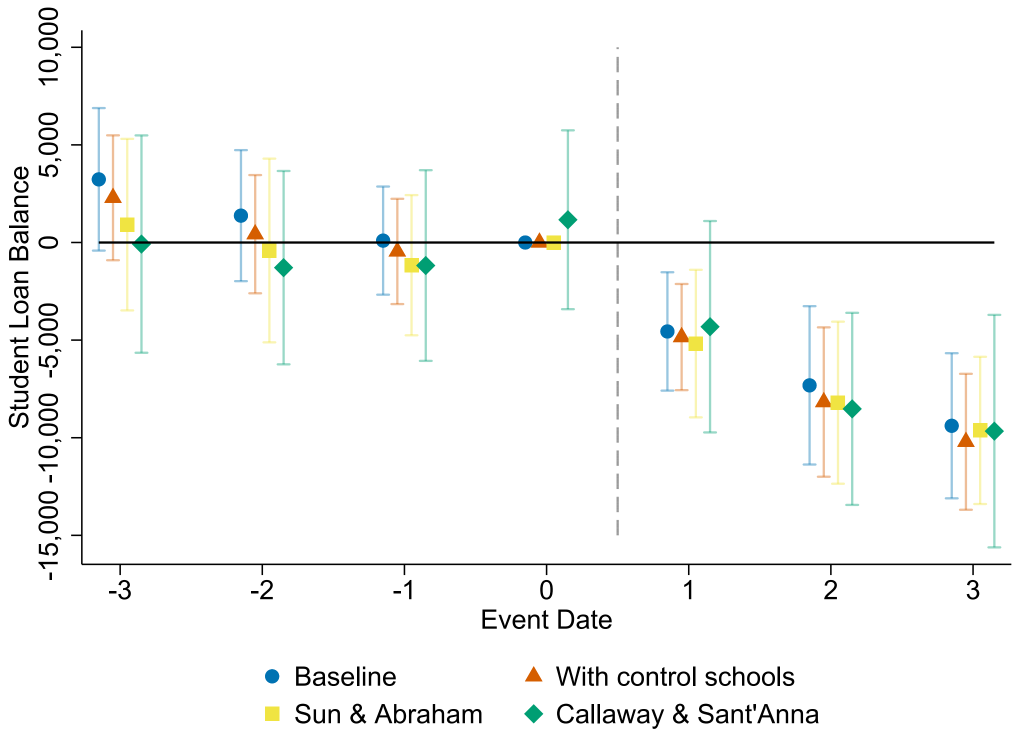

(c) Credit Score (residualized)




(d) Neighborhood Income (residualized)

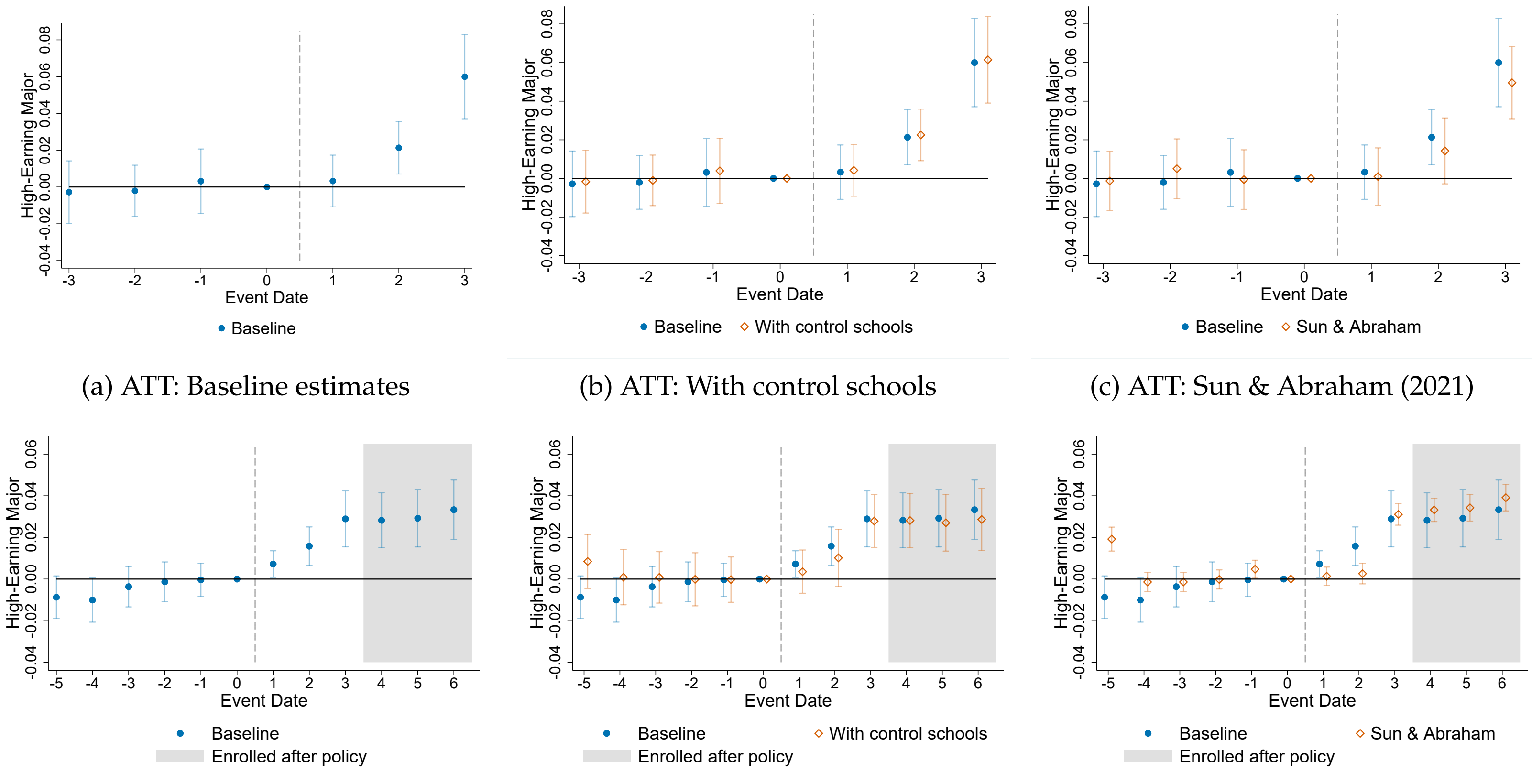
*Note:* This figure describes the relationship between family background and the type of major that the student chose in college. Panels (a) and (b) plot binned scatter plots of the raw data, and in both panels the y-axis is a dummy variable indicating the fraction of students choosing a high earning major. In Panel (a) the x-axis is the credit score of the parents, and in Panel (b) the x-axis is the median income (inflation adjusted to 2019-dollars) for the census tract where the parents live. Panels (c) and (d) plot residualized binned scatter plots controlling for race, ethnicity, gender, year, and school fixed effects. Panel (c) plot the probability of choosing a high earning major relative to credit scores controlling for the fixed effects, and Panel (d) plot the probability of choosing a high earning major relative relative to neighborhood income controlling for the fixed effects. In each panel, the dots represent 20 equal-sized bins based on the variable on the x-axis, and the solid line is a linear regression on the entire dataset. In Panels (c) and (d) the transparent bars represent the 95% confidence interval (with the standard errors are clustered at the school-year level). The data source is the Commencement Program Database merged with Credit Bureau records and USPS records.

FIGURE 2: Effect of UNLP on Student Debt




*Note:* This figure shows the regression coefficients from an event-study regression where the unit of observation is a student. The data source is hand-collected commencement programs merged with individual credit bureau records. The dependent variable is the student loan balance in the year of graduation. The main independent variable is a set of event-time dummies indicating when the student graduated relative to the implementation of the UNLP. The baseline two-way fixed effect model is reported in blue and a model with non-implementing control schools is reported in green, and two models allowing for treatment heterogeneity across cohorts, (Sun and Abraham, 2021 and Callaway and Sant'Anna, 2021) are reported in yellow and green, respectively.

FIGURE 3: Effect of UNLP on Major Choice




(a) ATT: Baseline estimates

(b) ATT: With control schools

(c) ATT: Sun & Abraham (2021)

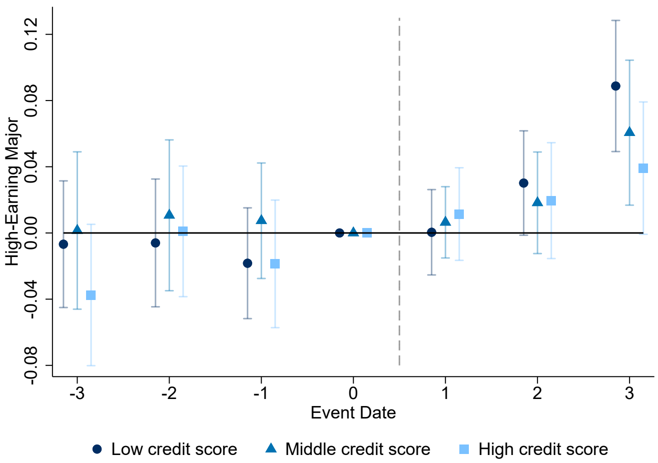
(d) ITT: Baseline estimates

(e) ITT: With control schools

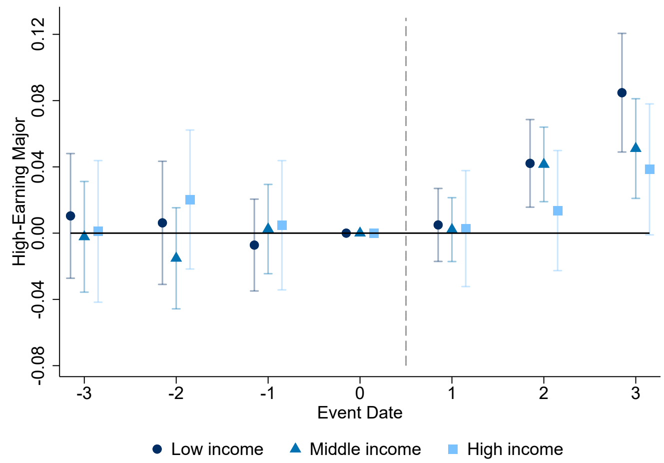
(f) ITT: Sun & Abraham (2021)

*Note:* This figure shows the regression coefficients and their 95% confidence intervals from estimating the event-study regressions where the dependent variable is a dummy variable taking the value of one if the student graduates with a high earning major. The main independent variable is a set of event-time dummies indicating when the student graduated relative to the implementation of the UNLP. Panels (a), (b), and (c) report the results from estimating equation (1), and Panels (d), (e), and (f) from equation (2). Panels (a) and (d) plot the baseline two-way fixed effect regression model. Panels (b) and (e) compare the baseline model with a model that includes 10 control schools that did not implement a UNLP. Panels (c) and (f) compares the baseline estimates with the estimators proposed by Sun and Abraham (2021). Estimates are scaled by the baseline mean at $\tau = 0$.

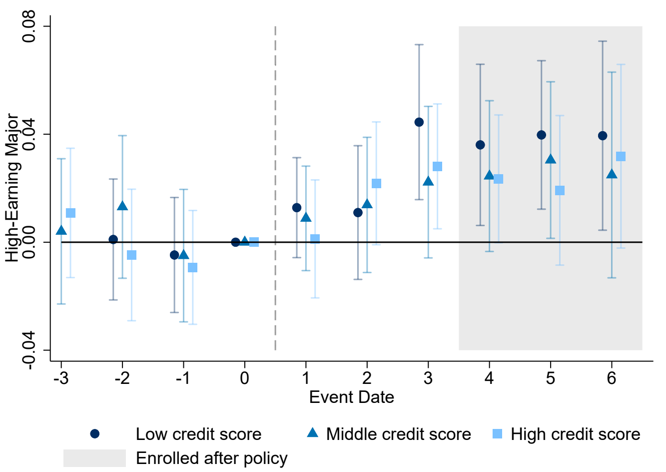
FIGURE 4: Effect of UNLP on Major Choice by Family Background



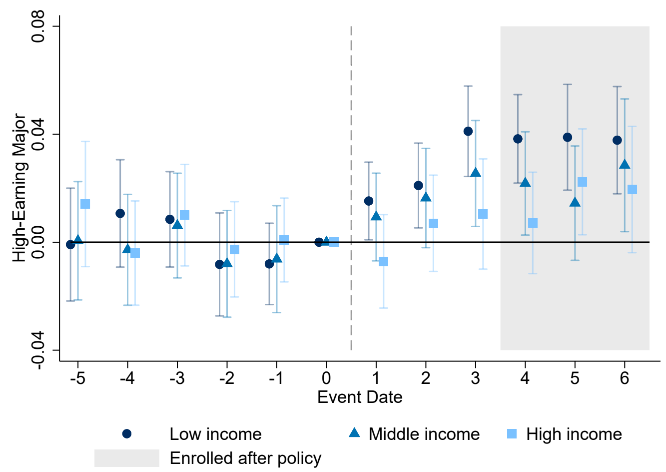

(a) ATT by Credit Score




(b) ATT by Neighborhood Income




(c) ITT by Credit Score




(d) ITT by Neighborhood Income

*Note:* This figure shows heterogeneity in the effect of UNLP by terciles of family background. Panels (a) and (b) report regression coefficients and 95% confidence intervals when estimating regression (1) and Panels (c) and (d) report coefficients and confidence intervals from equation (2). Panels (a) and (c) plot the results when splitting the sample by terciles of parents' credit score and Panels (b) and (d) plots the when splitting the sample by terciles of median neighborhood income. Estimates are scaled by the baseline mean at $\tau = 0$.

# Tables

TABLE 1: Universal No-Loan Policies

| | School | Implementation Year | Year Abandoned |
|---|---|---|---|
| 1 | Amherst College | 2008 | |
| 2 | Bowdoin College | 2008 | |
| 3 | Brown University | 2018 | |
| 4 | Claremont McKenna College | 2008 | 2014 |
| 5 | Colby College | 2008 | |
| 6 | College of the Ozarks | 2013 | |
| 7 | Columbia University | 2008 | |
| 8 | Dartmouth College | 2008 | 2011 |
| 9 | Grinnell College | 2021 | |
| 10 | Harvard University | 2008 | |
| 11 | Haverford College | 2011 | |
| 12 | Johns Hopkins University | 2018 | |
| 13 | Northwestern University | 2018 | |
| 14 | Pomona College | 2008 | |
| 15 | Princeton University | 2001 | |
| 16 | Stanford University | 2008 | |
| 17 | Swarthmore College | 2008 | |
| 18 | University of Chicago | 2018 | |
| 19 | University of Pennsylvania | 2009 | |
| 20 | Vanderbilt University | 2009 | |
| 21 | Williams College | 2008 | 2011 |
| 22 | Yale University | 2008 | |

*Note:* This table provides the list of schools that implemented universal no-loan policies, their associated year of implementation, and, when relevant their ending date.

TABLE 2: Effect of UNLP on Student Debt

| | Student Debt | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Years -3 to -1 | 1,569 | 752 | -220 | -850 |
| | (1,432) | (1,300) | (1,896) | (2,620) |
| Years 1 to 3 | -7,087*** | -7,742*** | -7,670*** | -7,500*** |
| | (1,497) | (1,374) | (1,742) | (2,670) |
| Control schools | | ✓ | ✓ | ✓ |
| Baseline mean | 43,549 | 39,589 | 39,589 | 39,589 |
| Observations | 52,157 | 55,262 | 55,262 | 56,637 |
| Estimator | OLS | OLS | Sun & Abraham | Callaway & Sant'Anna |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Note:* This table reports the average effects from estimating the event-study regression on the sample of students with student debt prior to the policy implementation. The left-hand-side variable is the amount of student debt, each column reports estimates from a separate regression, and Effect at event time $\tau = 0$ is normalized to 0. The coefficient for Years -3 to -1 is the pre-period average of the coefficients for $\tau = -3$ to $\tau = -1$, and the coefficient for Years 1 to 3 is the post-period average of the coefficients for $\tau = 1$ to $\tau = 3$. Columns (2) to (4) include the 10 non-UNLP-implementing control schools. Regressions for Columns (1) and (2) are OLS. All regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Standard errors (in parentheses) are clustered at the school-year level. The estimator for Column (3) is Sun and Abraham (2021), and the estimator for Column (4) is Callaway and Sant'Anna (2021).

TABLE 3: Effect of UNLP on Major Choice

| | High-earning major | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Years -3 to -1 | -0.001 | 0.000 | 0.001 |
| | (0.006) | (0.006) | (0.006) |
| Years 1 to 3 | 0.028*** | 0.029*** | 0.022*** |
| | (0.007) | (0.006) | (0.006) |
| Control schools | | ✓ | ✓ |
| Baseline mean | 0.56 | 0.61 | 0.61 |
| Observations | 131,049 | 139,903 | 139,903 |
| Estimator | OLS | OLS | Sun & Abraham |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports the average effects from estimating equation (1). Each column reports estimates from a separate regression. Effect at event time $\tau = 0$ is normalized to 0. The coefficient for Years -3 to -1 is the pre-period average of the coefficients for $\tau = -3$ to $\tau = -1$, and the coefficient for Years 1 to 3 is the post-period average of the coefficients for $\tau = 1$ to $\tau = 3$. Columns (2) and (3) include the 10 non-UNLP-implementing control schools. Regressions for Columns (1) and (2) are OLS, and the estimator for Column (3) is Sun and Abraham (2021). All regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Estimates are scaled by the baseline mean at $\tau = 0$. Standard errors (in parentheses) are clustered at the school-year level.

TABLE 4: Heterogeneity by Family Background

| | High-earning major | |
|---|---|---|
| | (1) | (2) |
| Years 1 to 3 | 0.005 | 0.013 |
| | (0.011) | (0.009) |
| Years 1 to 3 $\times$ Below Median Parent Credit Score | 0.049$^{***}$ | |
| | (0.007) | |
| Years 1 to 3 $\times$ Below Median Neighborhood Income | | 0.034$^{***}$ |
| | | (0.007) |
| Baseline mean | 0.61 | 0.61 |
| Observations | 139,903 | 139,903 |
| Estimator | OLS | OLS |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

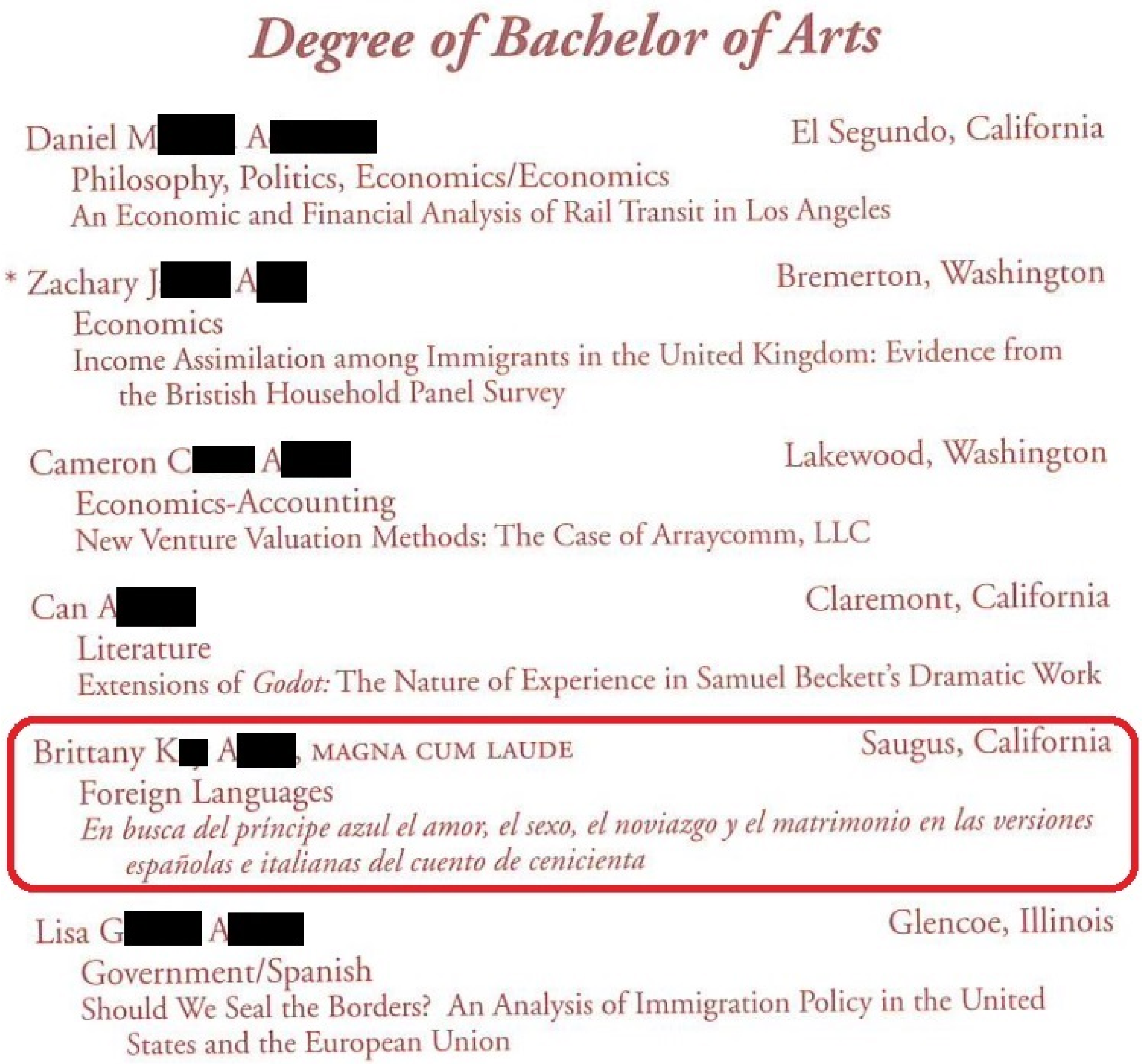
*Note:* This table reports the average effects from estimating equation (1) where the dependent variable is a dummy variable taking the value one if the student chooses a high-earning major and the event-time dummies are interacted with an interaction term indicating family background. Each column reports estimates from a separate regression. In Column (1) the interaction term is for whether the student's parents have a below median credit score, and in Column (2) the interaction term is for whether the student grew up in a census tract where the median income is below the median income (relative to other census tracts). The effect at event time $\tau = 0$ is normalized to 0. The coefficient for Years 1 to 3 is the post-period average of the coefficients for $\tau = 1$ to $\tau = 3$, and the coefficient for Years 1 to 3 interacted is the post-period average of the post-period interacted coefficients. Both regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Estimates are scaled by the baseline mean at $\tau = 0$. Standard errors (in parentheses) are clustered at the school-year level.

# Appendix for Online Publication

## Contents

# A Additional Figures

FIGURE A1: Commencement Program Example



*Degree of Bachelor of Arts*

Daniel M[redacted] A[redacted] — El Segundo, California
Philosophy, Politics, Economics/Economics
An Economic and Financial Analysis of Rail Transit in Los Angeles

* Zachary J[redacted] A[redacted] — Bremerton, Washington
Economics
Income Assimilation among Immigrants in the United Kingdom: Evidence from the Bristish Household Panel Survey

Cameron C[redacted] A[redacted] — Lakewood, Washington
Economics-Accounting
New Venture Valuation Methods: The Case of Arraycomm, LLC

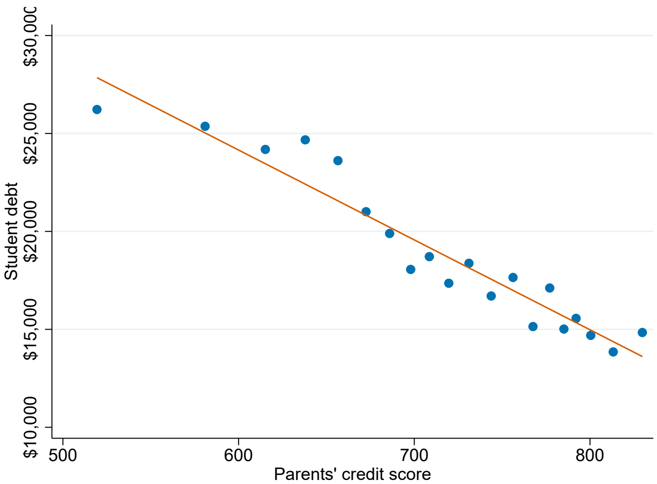
Can A[redacted] — Claremont, California
Literature
Extensions of *Godot:* The Nature of Experience in Samuel Beckett's Dramatic Work

Brittany K[redacted] A[redacted], MAGNA CUM LAUDE — Saugus, California
Foreign Languages
*En busca del príncipe azul el amor, el sexo, el noviazgo y el matrimonio en las versiones españolas e italianas del cuento de cenicienta*

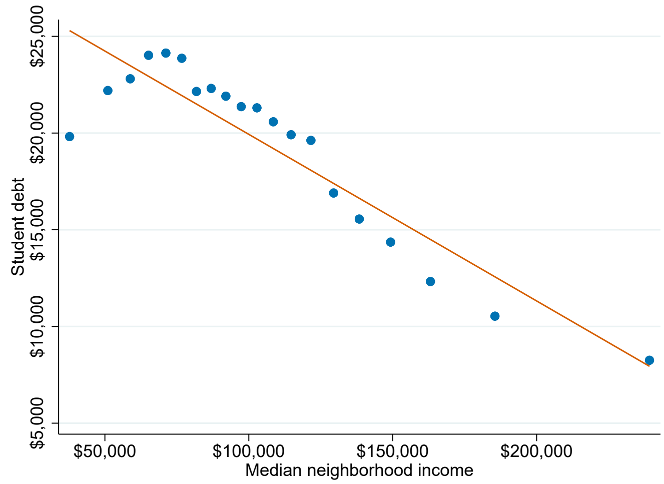
Lisa G[redacted] A[redacted] — Glencoe, Illinois
Government/Spanish
Should We Seal the Borders? An Analysis of Immigration Policy in the United States and the European Union

*Note:* This picture is an example of commencement program from the 2008 cohort of Claremont Mckenna College. The picture includes full names, major choices, hometowns and states, awards as indicated by *, and thesis titles. This digitization was procured from the Claremont Schools Special Archives and Dean's Office.

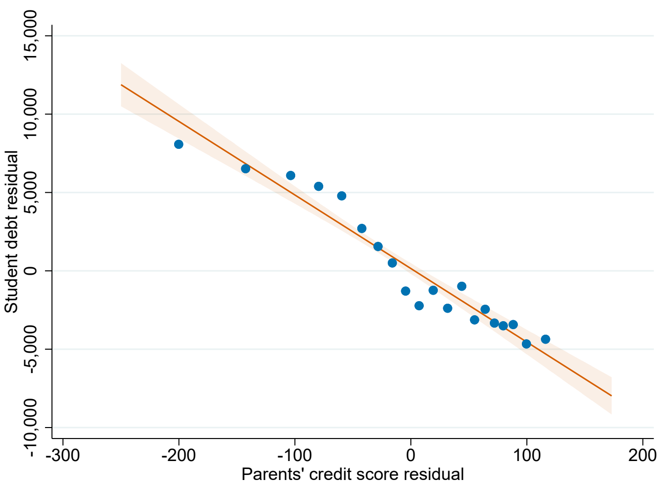
## FIGURE A2: Family Background and Student Debt



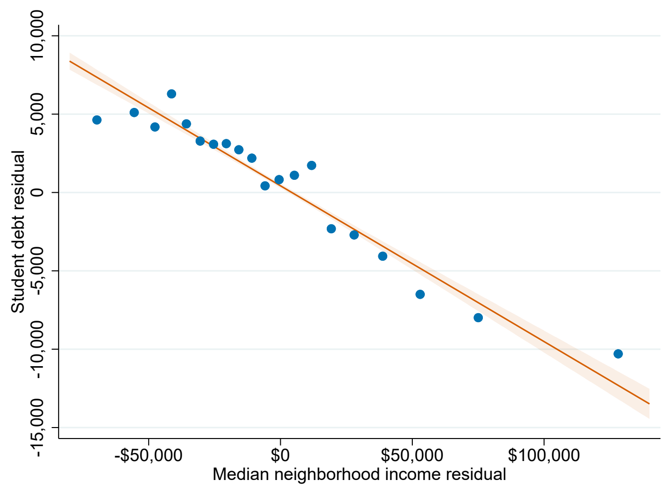

(A) Credit Score




(B) Neighborhood Income



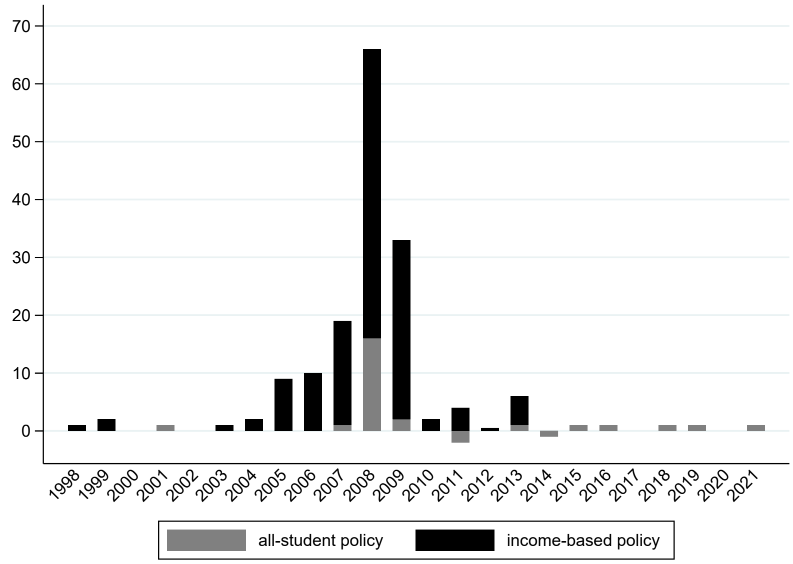

(C) Credit Score (residualized)



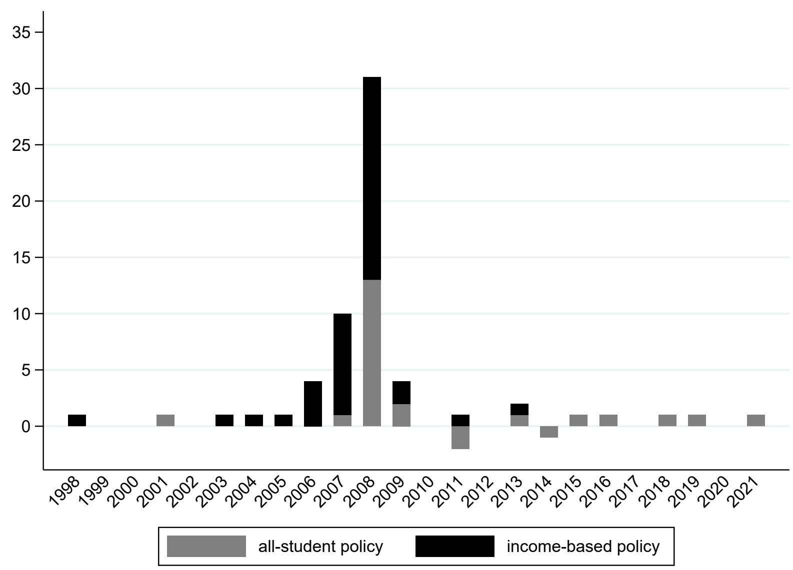

(D) Neighborhood Income (residualized)

*Note:* This figure describes the relationship between family background and the amount of student debt a student has at graduation. Panels (A) and (B) plot binned scatter plots of the raw data, and in both panels the y-axis is a dummy variable indicating the fraction of students choosing a high earning major. In Panel (A) the x-axis is the credit score of the parents, and in Panel (B) the x-axis is the median income (inflation adjusted to 2019-dollars) for the census tract where the parents live. Panels (C) and (D) plot residualized binned scatter plots controlling for race, ethnicity, gender, year, and school fixed effects. Panel (C) plot the probability of choosing a high earning major relative to credit scores controlling for the fixed effects, and Panel (D) plot the probability of choosing a high earning major relative relative to neighborhood income controlling for the fixed effects. In each panel, the dots represent 20 equal-sized bins based on the variable on the x-axis, and the solid line is a linear regression on the entire dataset. In Panels (C) and (D) the transparent bars represent the 95% confidence interval (with the standard errors are clustered at the school-year level). The data source is the Commencement Program Database merged with Credit Bureau records and USPS records.

FIGURE A3: Number of financial aid policies implemented



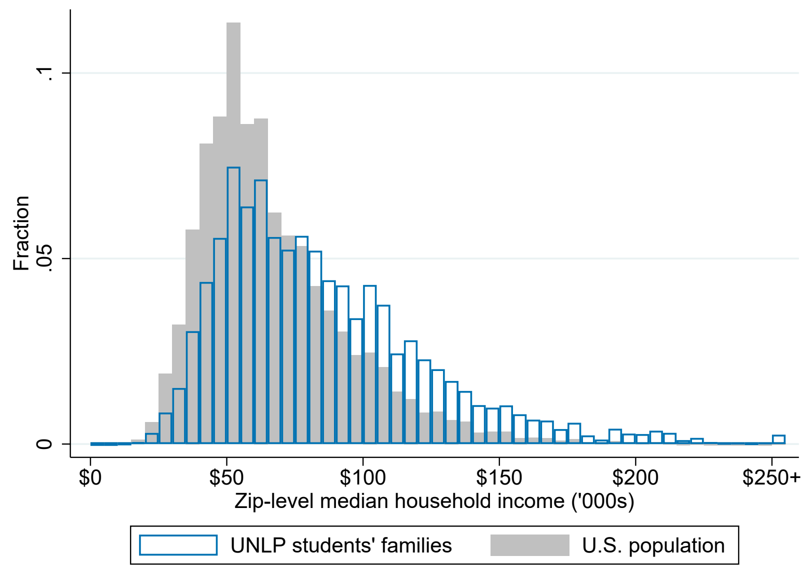

(a) Implementation of *all* financial aid policies



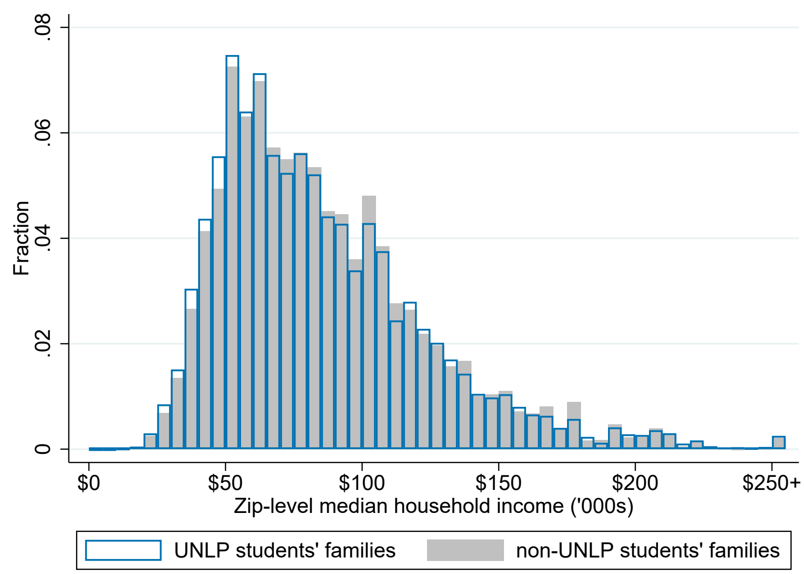

(b) Implementation of loan elimination policies (no-loan policies)

*Note:* This figure shows the number of financial aid policies that have been implemented for particular household income bands and for all students over time. Panel (a) shows the total number of universities implementing loan eliminations, loan caps, parental contribution waivers, Pell Grant matches, and tuition waivers. Panel (b) shows the total number of no-loan policies implemented over time. The black bars denote policies targeted specifically at low-income students, and the gray bars denote policies available for all students. The data source is the Integrated Postsecondary Education Data System.

FIGURE A4: ZIP-level Household Income



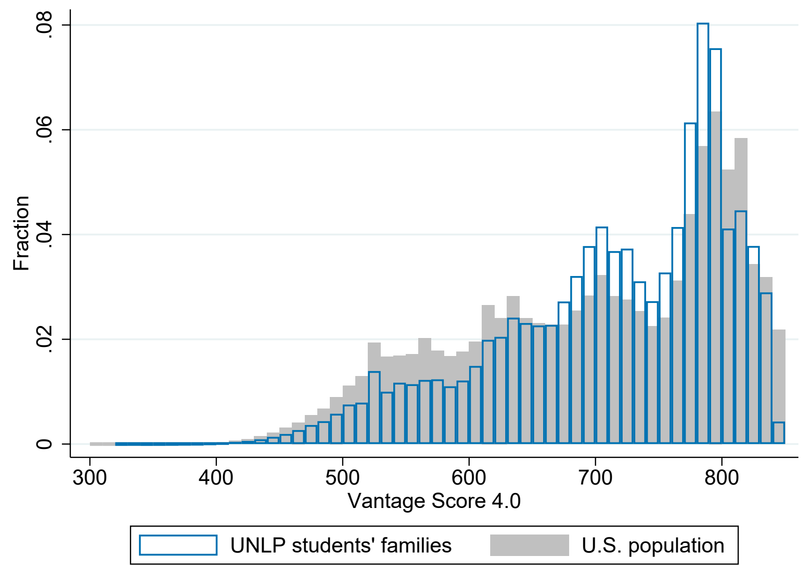

(a) UNLP families vs US population



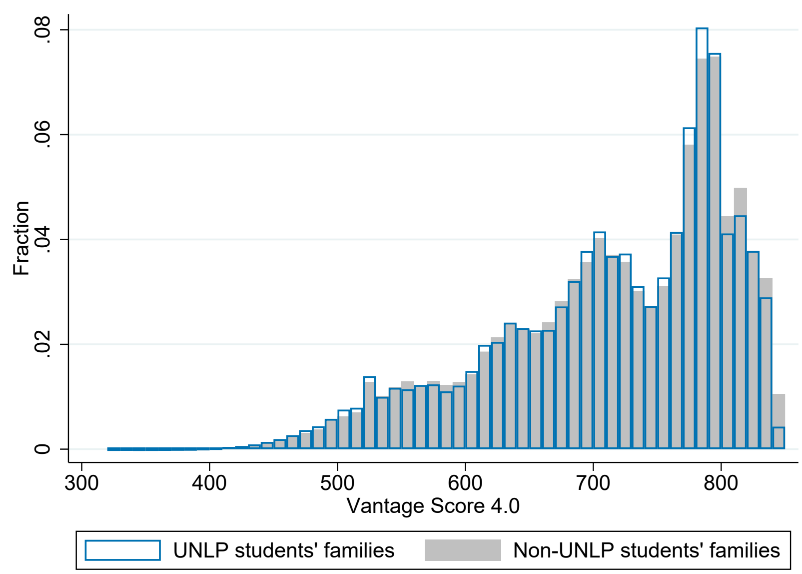

(b) UNLP families vs non-UNLP families

*Note:* The figure plots histograms of the distribution of median household income by ZIP codes. Panel (A) plots the distribution of ZIP code incomes for the UNLP students' families (in blue) relative to a 1% random sample of the U.S. population (in gray). Panel (B) plots the distribution of ZIP code incomes for the UNLP students' families (in blue) relative to a the families of students from non-UNLP control schools (in gray).

FIGURE A5: Vantage Score Distributions



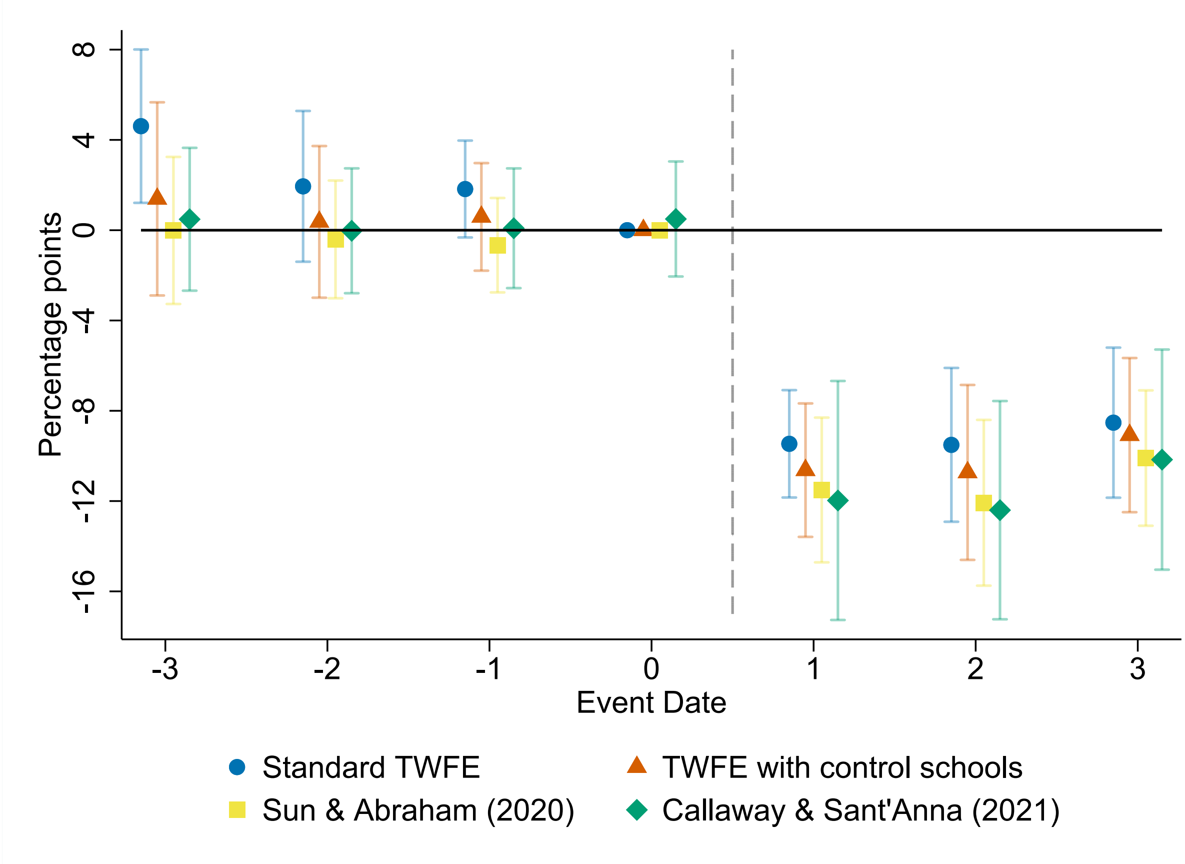

(a) UNLP families vs US population




(b) UNLP families vs non-UNLP families

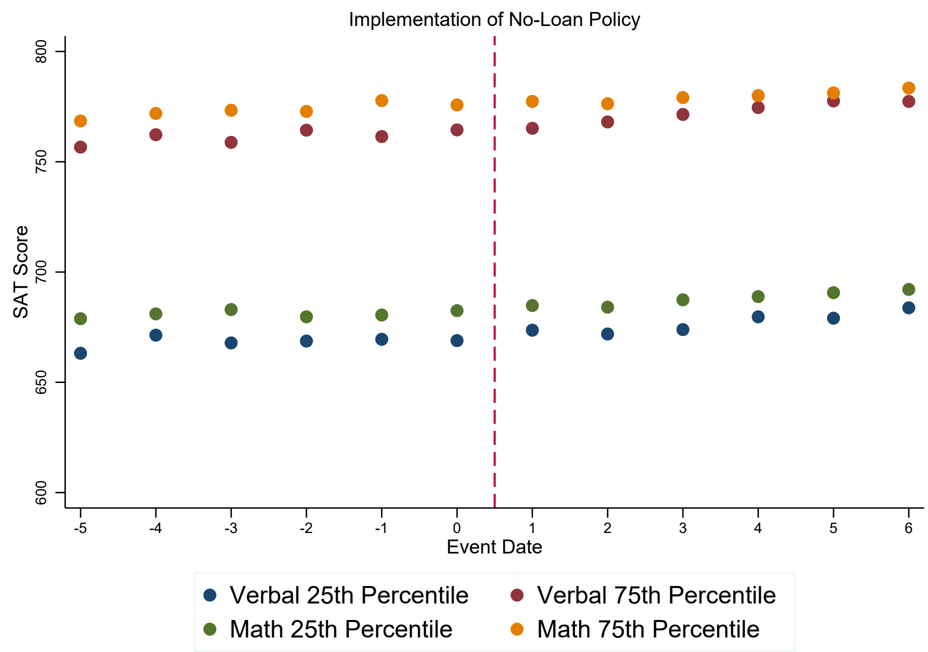
*Note:* The figure plots histograms of the distribution of Vantage Scores in the sample. Panel (A) plots the distribution of Vantage Scores for the UNLP students' families (in blue) relative to a 1% random sample of the U.S. population (in gray). Panel (B) plots the distribution of Vantage Scores for the UNLP students' families (in blue) relative to a the families of students from non-UNLP control schools (in gray).

FIGURE A6: Effect of UNLPs using publicly available data

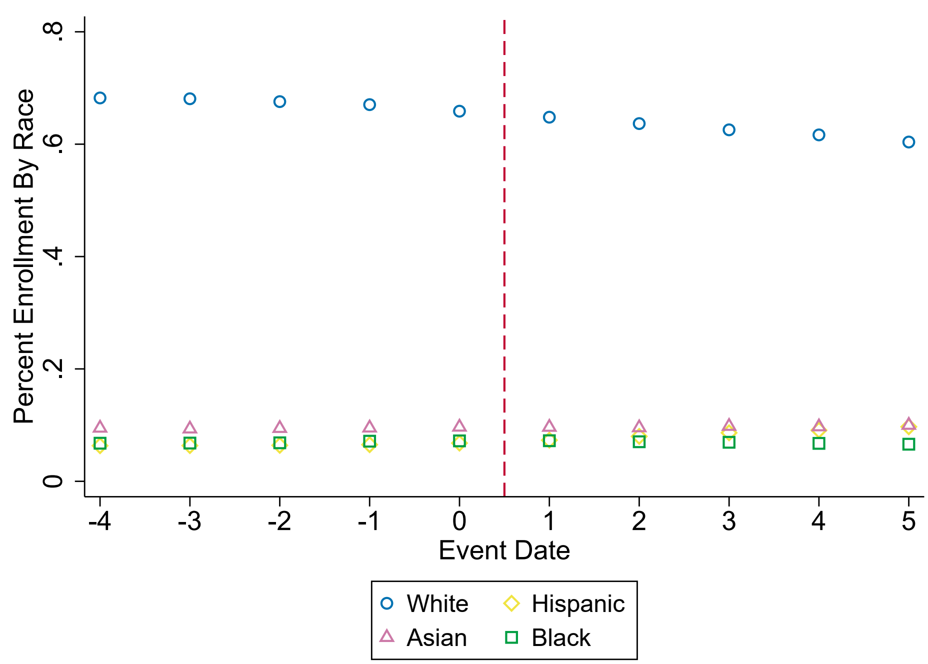



*Note:* This figure describes the effect of universal no-loan policies (UNLPs) using publicly available data. The figure shows the regression coefficients and 95% confidence intervals from two regressions of the percentage of students taking a student loan on year dummies relative to the implementation of a UNLP. A standard two-way fixed effects (TWFE) model is reported in blue, a TWFE model with non-implementing control schools is reported in red, and two models allowing for treatment heterogeneity across cohorts, (Sun and Abraham, 2021 and Callaway and Sant'Anna, 2021) are reported in yellow and green, respectively. The data source is the Integrated Postsecondary Education Data System (IPEDS).

FIGURE A7: Averages of Student Characteristics Pre- and Post- Policy



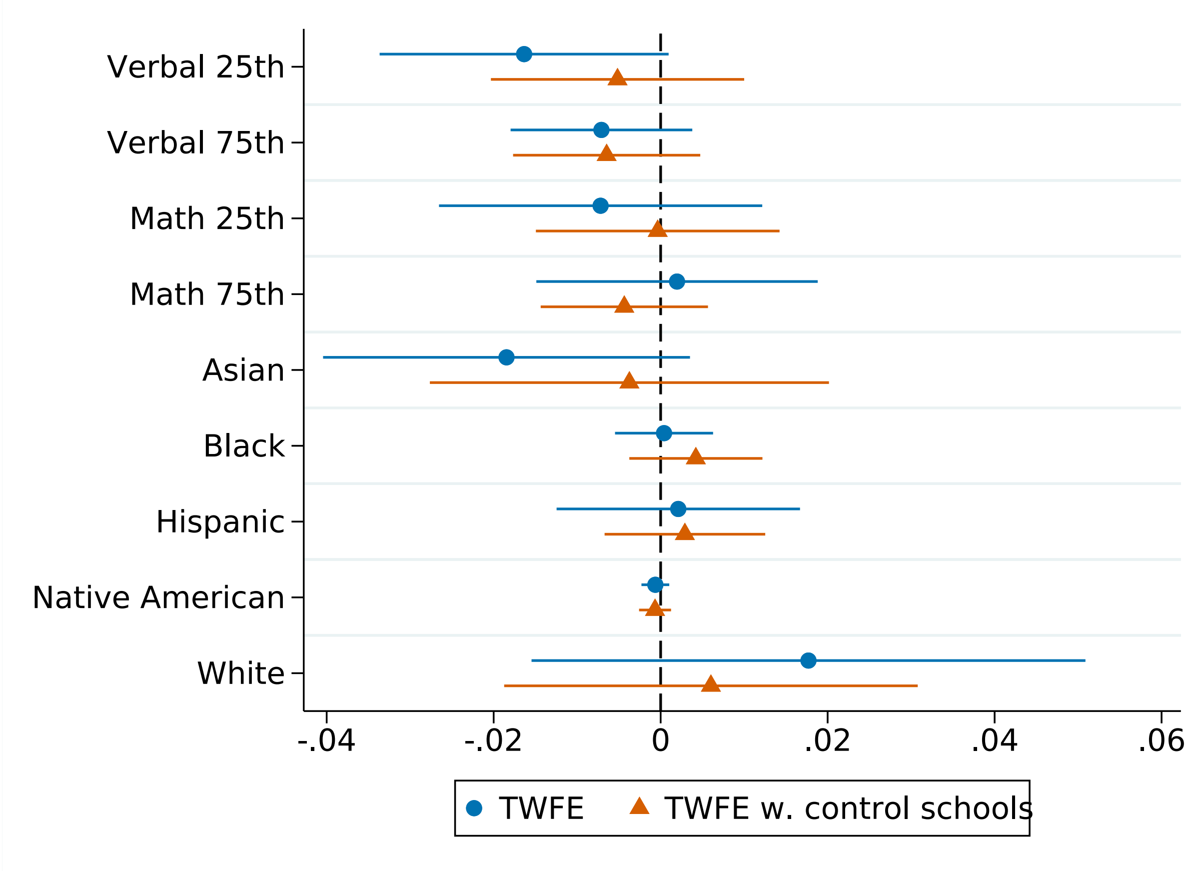

(a) SAT Scores - 25th and 75th Percentiles




(b) Racial Groups

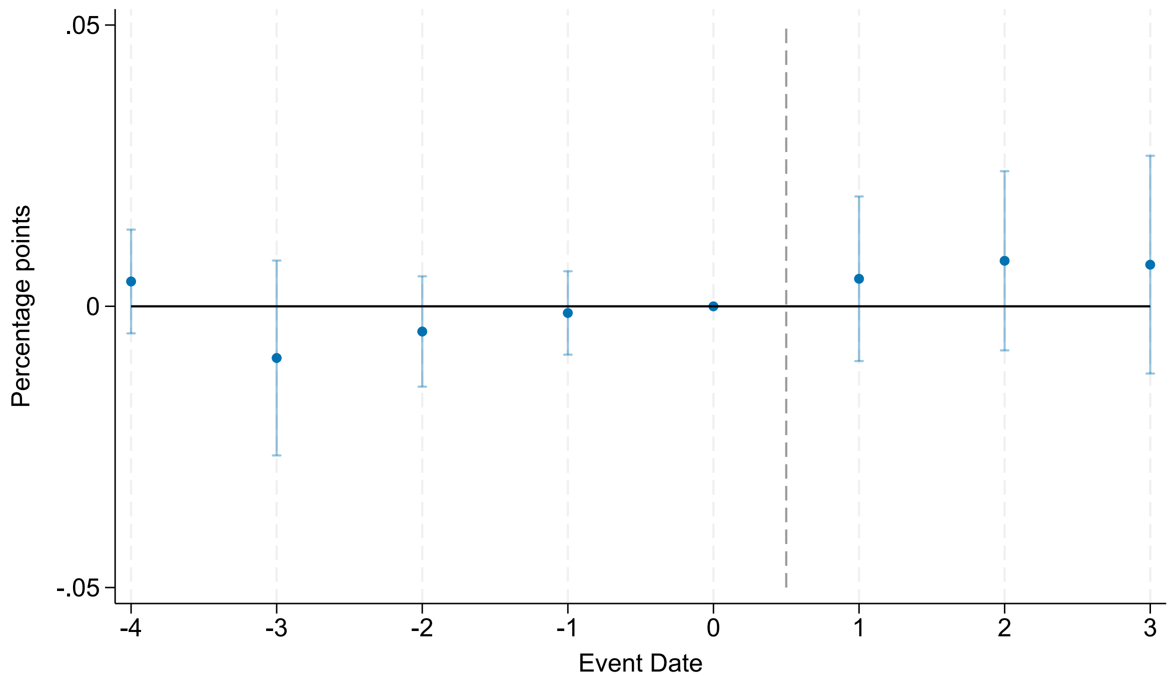
*Note:* This figure plots the average SAT scores and fraction of students by race, before- and after- implementation of UNLPs. Panel (a) plots the average SAT scores across the 25th and 75 percentiles for both Reading and Math. Panel (B) fraction of students by race.

FIGURE A8: Effect of UNLP on Student Characteristics



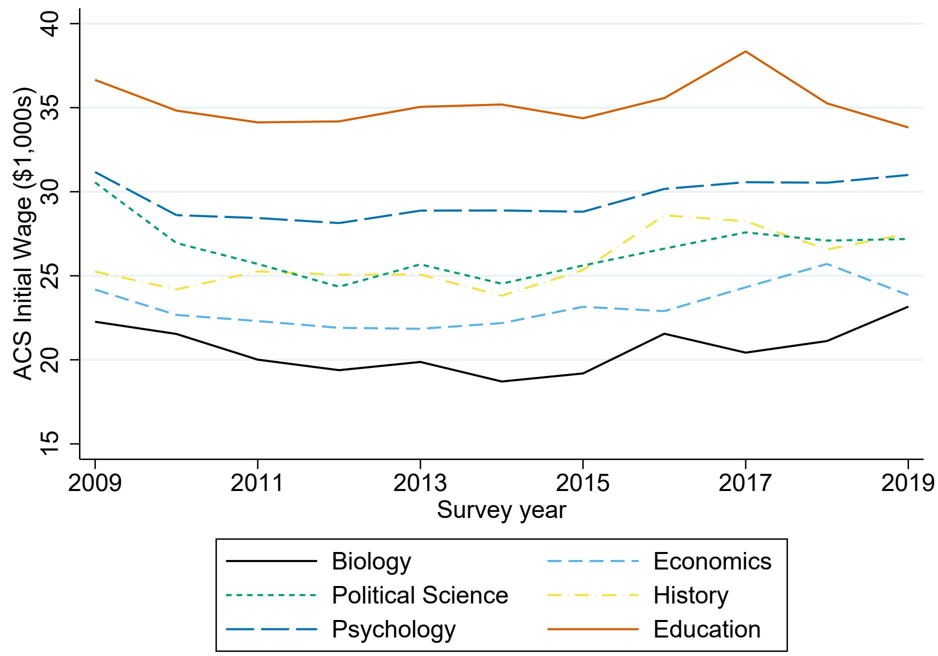

*Note:* This figure describes the effect of universal no-loan policies (UNLPs) on SAT scores and racial demographics of students already within three years of policy adoption. The figure shows the regression coefficients and 95% confidence intervals from two regressions of SAT score and fraction of undergraduate students by racial group relative to the implementation of a UNLP. A standard two-way fixed effects (TWFE) model is reported in blue and a TWFE model with non-implementing control schools is reported in red. The data source is the Integrated Postsecondary Education Data System (IPEDS).

FIGURE A9: Effect of UNLP on Completion Rates

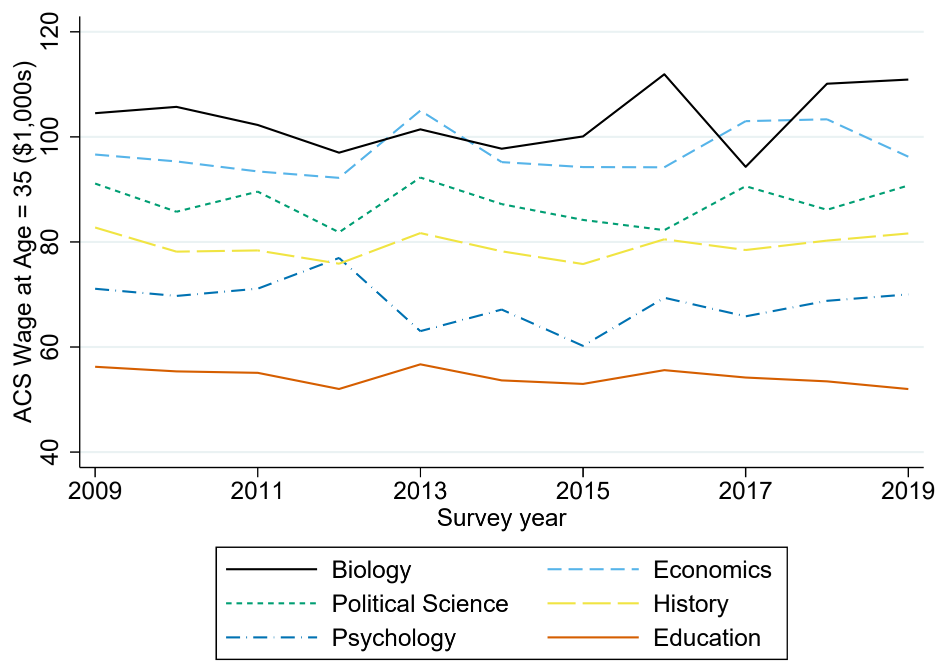



*Note:* This figure describes the effect of universal no-loan policies (UNLPs) on completion rates within three years of policy adoption. The figure shows the regression coefficients and 95% confidence intervals from a TWFE regression of completion rate relative to the implementation of a UNLP. The data source is the Integrated Postsecondary Education Data System (IPEDS).

FIGURE A10: ACS Initial Wages and Mid-Career Wages



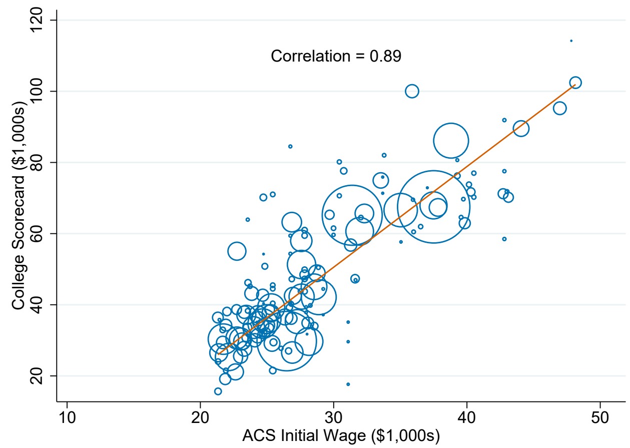

(a) ACS Initial Wage (21-24 years old)



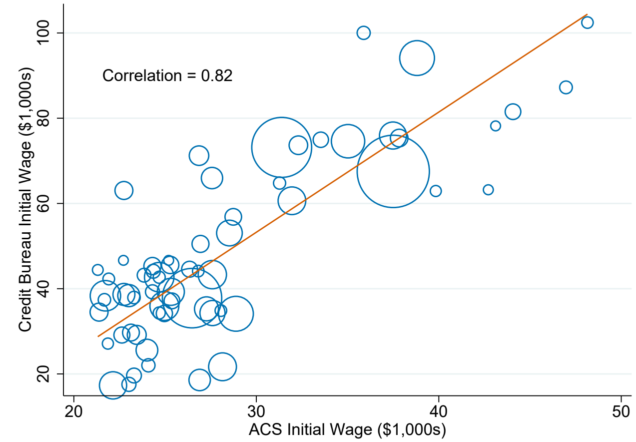

(b) ACS Mid-Career Wage (35 years old)

*Note:* This figure plots the average pre-tax wage and salary income across six common majors from the American Community Survey (ACS). Panel (a) plots the initial wages of college graduates, specifically the average wages of those with a bachelor's degree between the ages of 21 and 24. Panel (B) plots wages at age 35 of college graduates with a bachelor's degree.

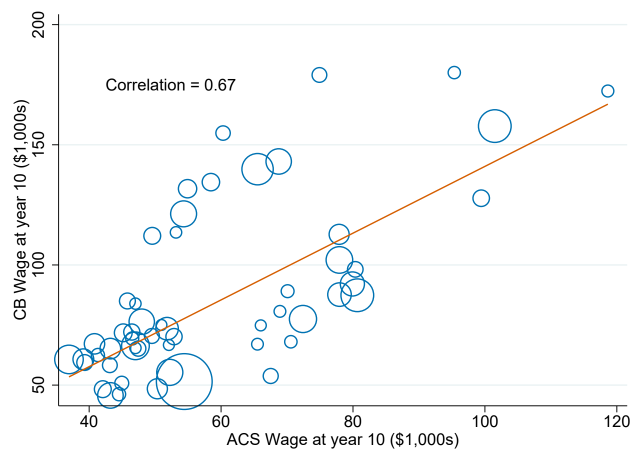
FIGURE A11: Correlation between Model-Implied Wages and ACS




(a) ACS Initial Wages vs. College Scorecard Initial Wages



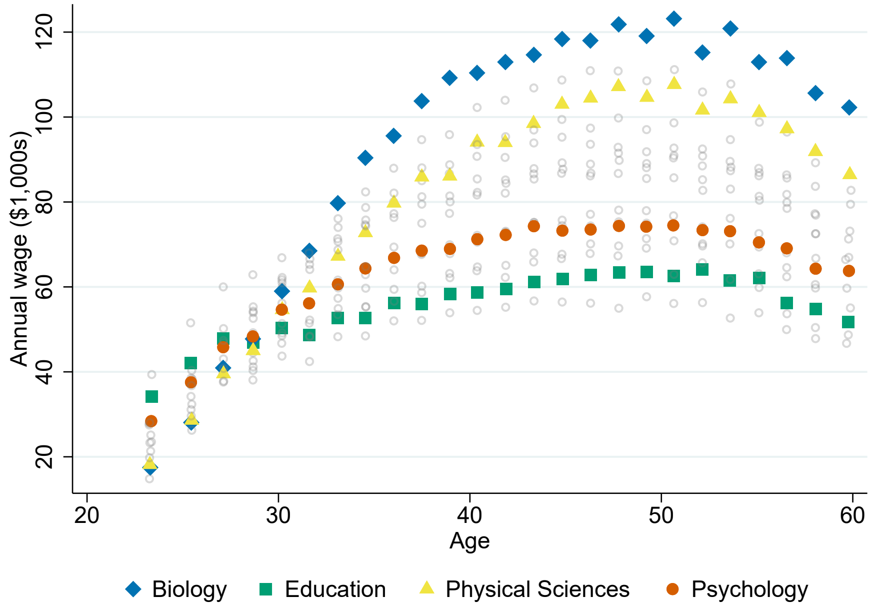

(b) ACS Initial Wages vs. Experian Initial Model-Implied Wages



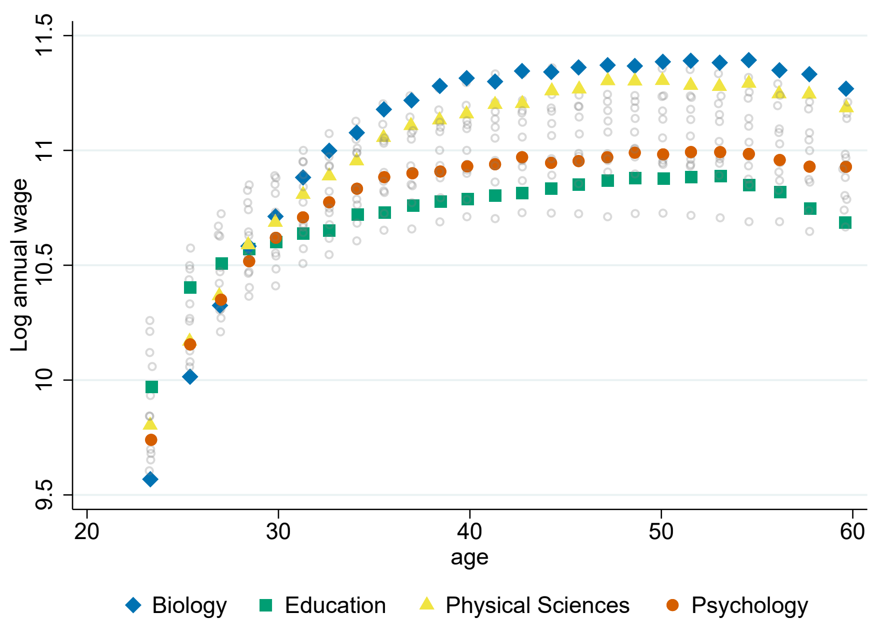

(c) ACS Year 10 Wages vs. Experian Year 10 Model-Implied Wages

*Note:* This figure plots average earnings by major across the American Community Survey (ACS), College Scorecard, and Experian's model-implied earnings data. Panel (a) plots the ACS initial wages by major (ages 21-24) against College Scorecard wages by major (within the first 3 years of graduating from college). Panel (b) plots ACS initial wages by major (ages 21-24) against Experian initial wages by major (ages 21-24). Panel (c) plots ACS year 10 wages by major against Experian year 10 wages by major.

FIGURE A12: College Majors & Life-cycle Wages



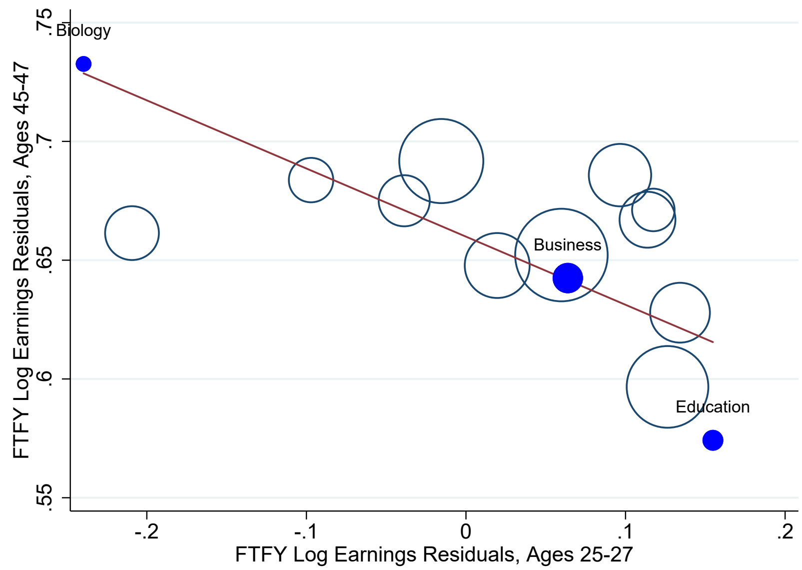

(a) Dollars



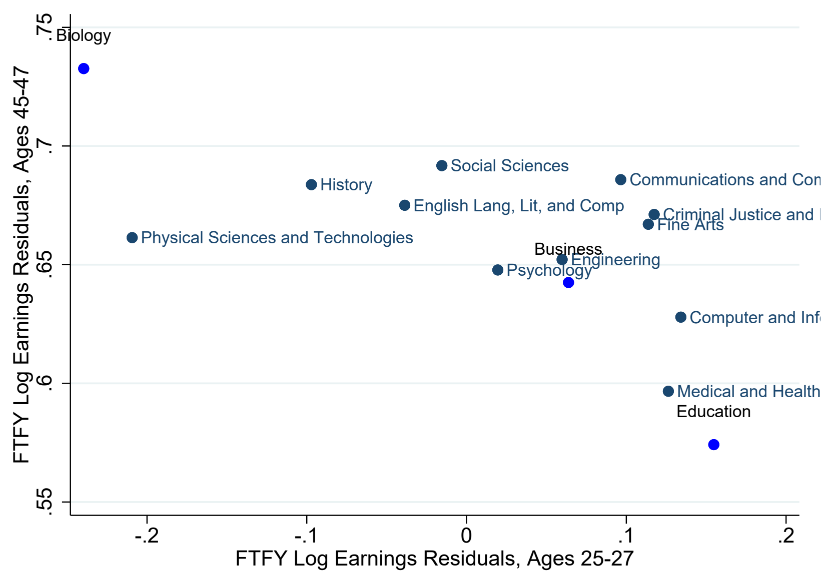

(b) Logs

*Note:* This figure describes life-cycle wages across the 15 most frequent college majors (using the 2-digit CIP classification). Panel A shows a binned scatter plot of annual wage relative to age and Panel B shows a binned scatter plot of log annual wage relative to age. In both panels, annual wages are adjusted for inflation (in 2019-dollars) and I control for race, ethnicity, gender, and year. The blue diamonds, green squares, yellow triangles, and red circles report the average wages and average log wages for students who majored in biology, education, the physical sciences, and psychology, and the gray circles represent the remaining top-15 majors. The data source is the American Community Survey (ACS).

FIGURE A13: Initial Earnings Against Later in Life Earnings



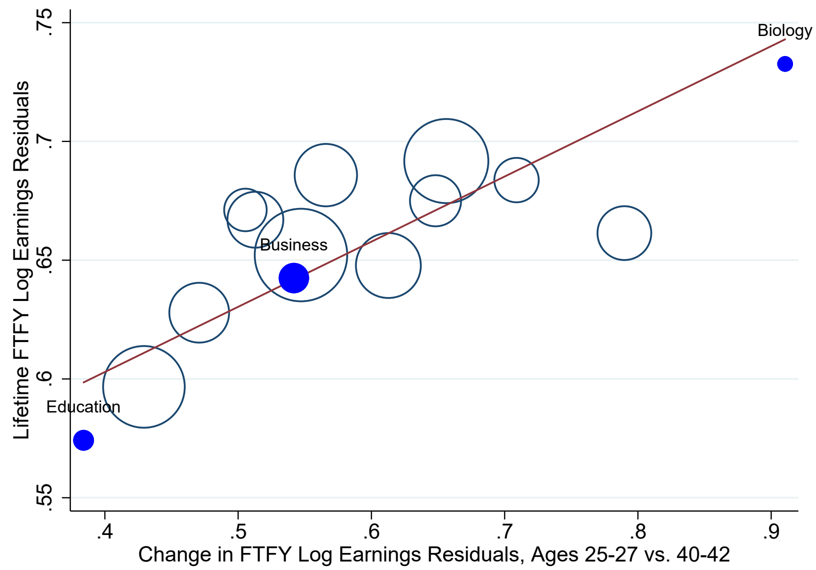

(a) Residualized Initial Earnings vs. Residualized Later in Life Earnings (Sized)



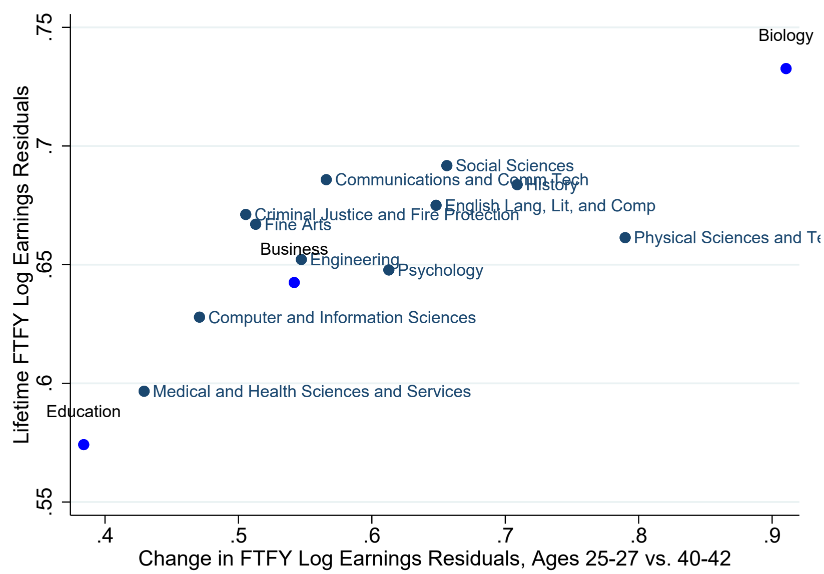

(b) Residualized Initial Earnings vs. Residualized Later in Life Earnings

*Note:* The figure plots the residualized log earnings of full-time full-year workers in the ACS. The x-axis plots the residualized average initial earnings of workers between 25-27. The y-axis plots the residualized average later-in-life log earnings of workers between 45-47. Each dot refers to a two-digit CIP major. Panel (A) plots the population-weighted dots of the top 15 majors. Panel (B) plots the top 15 majors by name.

FIGURE A14: Residualized lifetime earnings vs. avg growth



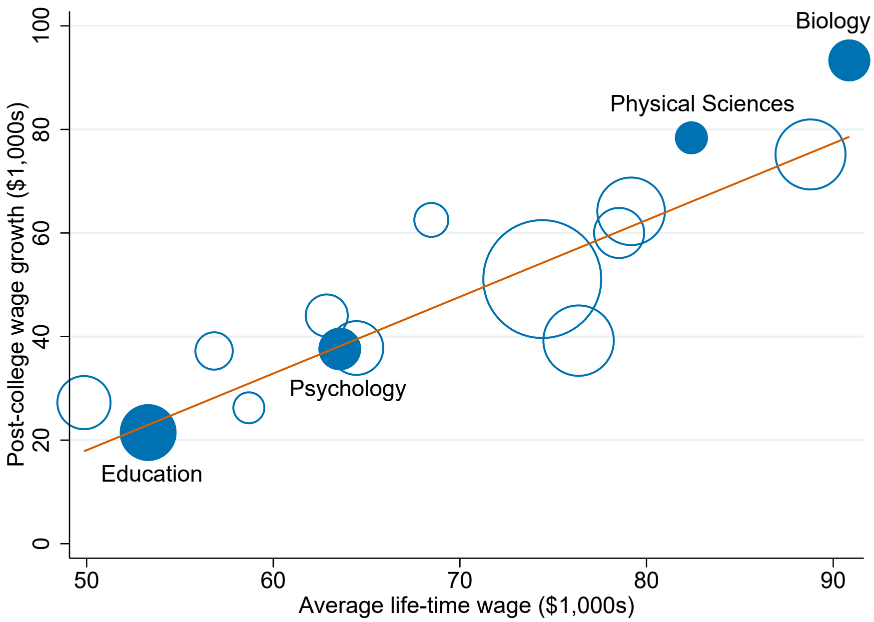

(a) Change in residualized log earnings vs. Residualized Later in Life Earnings (Sized)




(b) Change in residualized log earnings vs. Residualized Later in Life Earnings

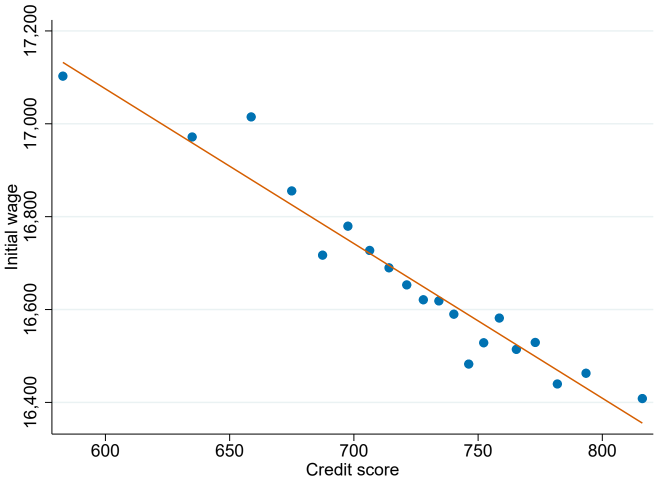
*Note:* The figure plots the residualized log earnings of full-time full-year workers in the ACS. The x-axis plots the residualized average initial earnings of workers between 25-27. The y-axis plots the residualized average later-in-life log earnings of workers between 45-47. Each dot refers to a two-digit CIP major. Panel (A) plots the population-weighted dots of the top 15 majors. Panel (B) plots the top 15 majors by name.

FIGURE A15: Post-College Dollar Growth vs Average Life-Time

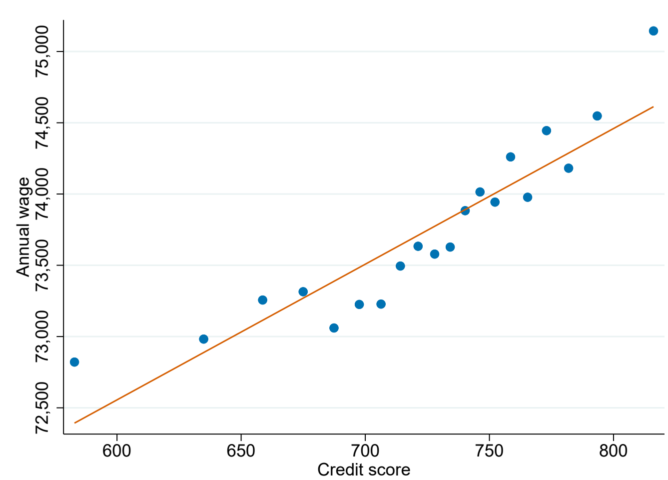



*Note:* This figure describes the relationship across college majors between life-time wage and wage growth. The x-axis plots a scatter plot of residualized average annual life-time wages relative to age (controlling for race, ethnicity, gender, and year, and adding back the sample mean of each variable back to its residuals) across the 15 most frequent college majors (using the 2-digit CIP classification). The y-axis plots the average annual wage between the ages of 45 and 47 minus the average annual wage at ages of 21 and 23. The data source is the American Community Survey (ACS) and the National Center for Education Statistics (NCES).

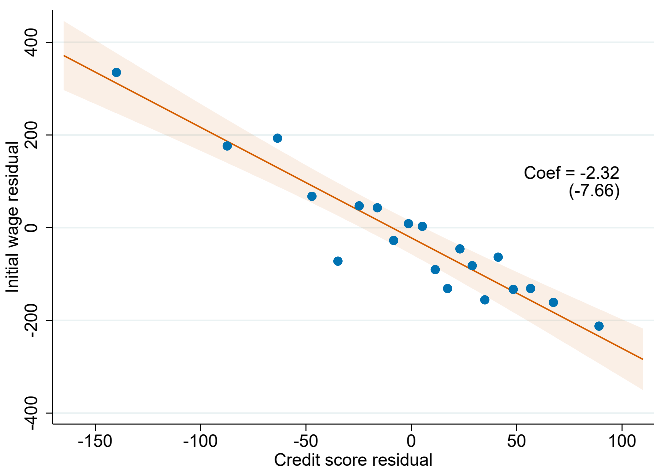
FIGURE A16: Major Choice and Parents' Credit Scores



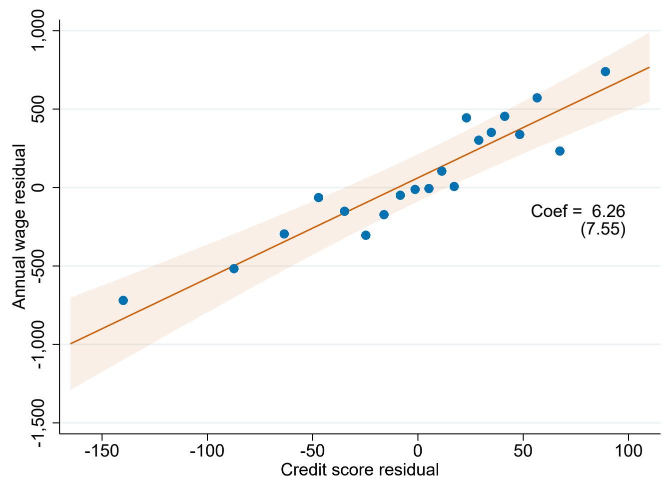

(A) Initial wage




(B) Average lifetime wage



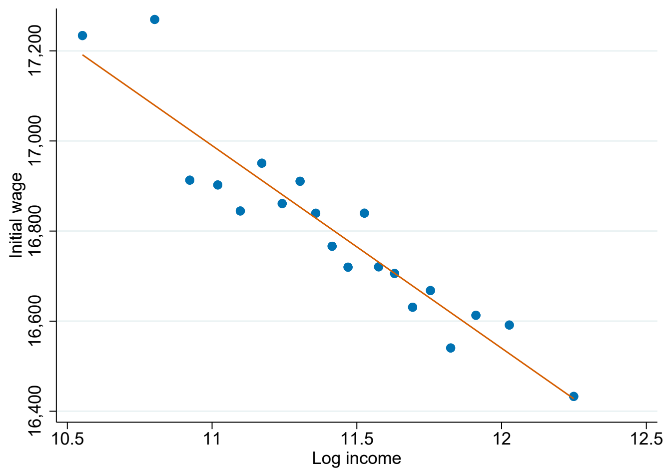

(C) Initial wage (residualized)



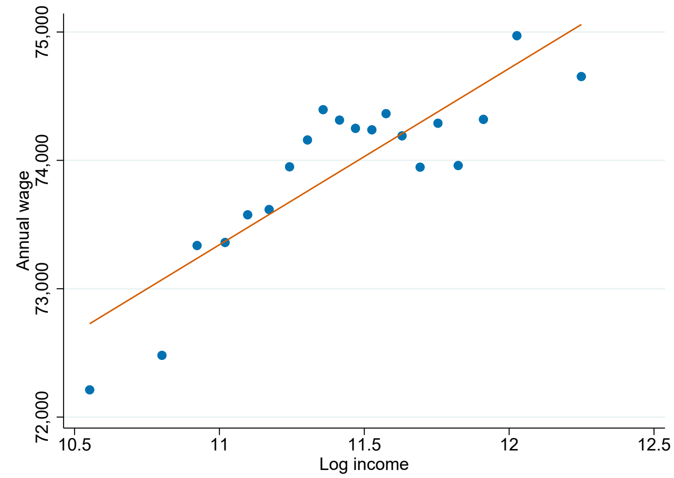

(D) Average lifetime wage (residualized)

*Note:* This figure describes the relationship between the parents' credit scores and the type of major that the student chose in college. Panels (A) and (B) plot binned scatter plots of the raw data, and both panels display the the parents' credit scores on the x-axis. In Panel (A) the y-axis plots the initial wage for the major that the student chose, and Panel (B) plots the average lifetime wage of the major that student chose on the y-axis. Panels (C) and (D) plot binned scatter plots of residualized wages and credit scores when controlling for race, ethnicity, gender, year, and school fixed effects. Panel (C) plot the initial wage relative to credit scores controlling for the fixed effects, and Panel (D) plot the average lifetime wage controlling for the fixed effects. In each panel, the dots represent 20 equal-sized bins based on the variable on the x-axis, and the solid line is a linear regression on the entire dataset. In Panels (C) and (D) the transparent bars represent the 95% confidence interval, and the coefficients are displayed (and the t statistic in parentheses with the standard errors are clustered at the school level). The data source is the American Community Survey (ACS) and the Commencement Program Database merged with Credit Bureau records.

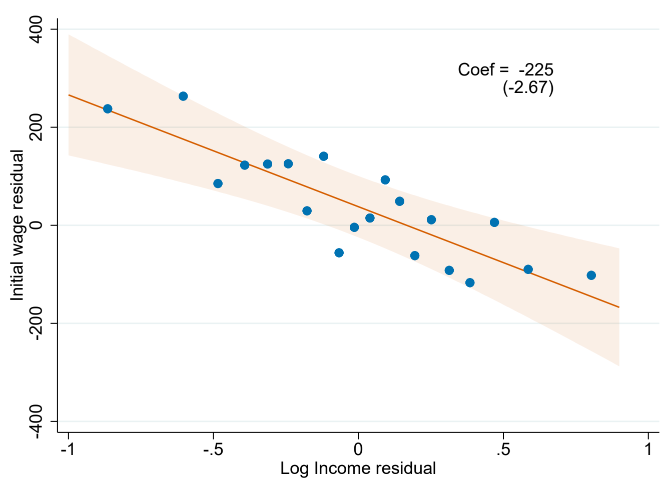
FIGURE A17: Major Choice and Parents' Income



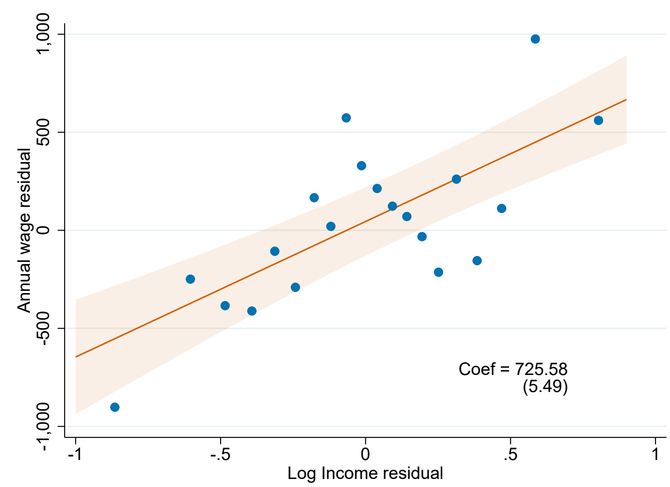

(A) Initial wage




(B) Average lifetime wage



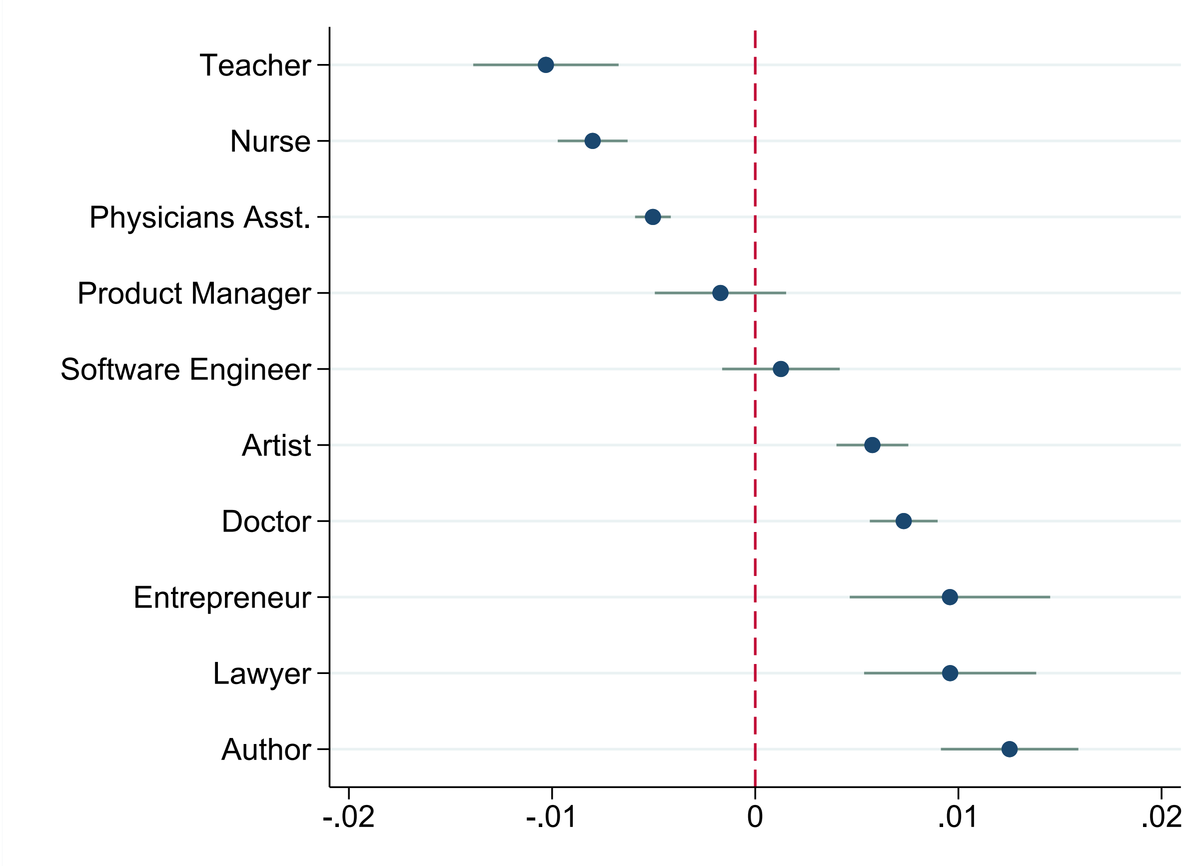

(C) Initial wage (residualized)




(D) Average lifetime wage (residualized)

*Note:* This figure describes the relationship between the neighborhood that a student grew up in and the type of major that the student chose in college. Panels (A) and (B) plot binned scatter plots of the raw data, and both panels display the natural logarithm of the median income in the ZIP code where the parents' live on the x-axis. In Panel (A) the y-axis plots the initial wage for the major that the student chose, and Panel (B) plots the average lifetime wage of the major that student chose on the y-axis. Panels (C) and (D) plot binned scatter plots of residualized wages and income when controlling for race, ethnicity, gender, year, and school fixed effects. Panel (C) plot the initial wage relative to log income controlling for the fixed effects, and Panel (D) plot the average lifetime wage controlling for the fixed effects. In each panel, the dots represent 20 equal-sized bins based on the variable on the x-axis, and the solid line is a linear regression on the entire dataset. In Panels (C) and (D) the transparent bars represent the 95% confidence interval, and the coefficients are displayed (and the t statistic in parentheses with the standard errors are clustered at the school level). The data source is the American Community Survey (ACS) and the Commencement Program Database merged with Credit Bureau records.

FIGURE A18: Occupations Post- UNLP implementation




*Note:* This figure shows a plot of the regression coefficients and 95% confidence intervals from Equation (2), with the outcome variable being a dummy variable that takes the value 1 if an individual has a particular occupation seven years after graduation. The coefficient represents the change in the fraction of students with that occupation, controlling for student characteristics, school, and cohort fixed effects, and school-time varying variables.

# B Additional Tables

TABLE A1: Control Schools

| | School |
|---|---|
| 1 | California Institute of Technology |
| 2 | Duke University |
| 3 | Kenyon College |
| 4 | Lehigh University |
| 5 | Massachusetts Institute of Technology |
| 6 | Pepperdine University |
| 7 | Rice University |
| 8 | University of Rochester |
| 9 | University of Southern California |
| 10 | Wellesley College |

*Note:* This table provides the list of the 10 non-UNLP-implementing control schools.

TABLE A2: Data waterfall for micro data

**Panel A: UNLP schools - Baseline Sample**

| | Sample restriction | N | Incremental percent | Cumulative percent relative to benchmark | Tables | Figures |
|---|---|---|---|---|---|---|
| 1 | Names in commencement programs | 382,036 | | | | |
| 2 | Not missing major (text) | 364,498 | 95.4% | | | |
| 3 | Classified majors | 355,516 | 97.5% | | | |
| **4** | **Event-window** $\tau \in \{-3, 3\}$ | **150,633** | 42.4% | **100%** | | |
| 5 | Students matched UPSPS records | 144,908 | 96.2% | 96.2% | | |
| 6 | USPS records for relatives | 138,676 | 95.7% | 92.1% | | |
| **7** | **Credit Bureau records** | **131,049** | 94.5% | **87.0%** | 3, A9 | 3, 4 |
| 7.b | Student Debt $> 0$ | 52,157 | 39.8% | | 2, A8 | 2 |
| 8 | Resume data | 112,308 | 85.7% | 74.6% | A15 | |
| 9 | Glassdoor data | 74,909 | 66.7% | 49.7% | | |

**Panel B: Comparing Specifications**

| | Event-window | Credit Bureau | Control Schools | Positive Debt | N | Tables | Figures |
|---|---|---|---|---|---|---|---|
| 1 | $\tau \in \{-3, 3\}$ | ✓ | | | 131,049 | 3, A9 | 3, 4 |
| 2 | $\tau \in \{-3, 3\}$ | ✓ | ✓ | | 139,903 | 3, 4, A9 | 3, 4 |
| 3 | $\tau \in \{-3, 3\}$ | ✓ | | ✓ | 52,157 | 2, A8 | 2 |
| 4 | $\tau \in \{-3, 3\}$ | ✓ | ✓ | ✓ | 55,262 | 2, A8 | 2 |
| 5 | $\tau \in \{-5, 6\}$ | | | | 230,070 | A10 | 3 |
| 6 | $\tau \in \{-5, 6\}$ | ✓ | ✓ | | 206,888 | A7, A11 | 1, A2 |
| 7 | $\tau \in \{-5, 6\}$ | ✓ | ✓ | ✓ | 81,721 | A11 | |
| 8 | $\tau \in \{-5, 6\}$ | | ✓ | | 404,917 | A10 | 3 |

*Note:* This table describes the data samples. Panel A reports the data waterfall table. It reports how the sample size changes for each sample restriction. "Incremental percent" reports the incremental fraction of the data that satisfies this specific sample restriction. "Cumulative percent relative to benchmark" reports the cumulative fraction of the data size relative to the benchmark sample. "Tables" and "Figures" report which tables and figures in the analysis that use each of the different samples. Panel B reports the observation counts for the different specifications used.

TABLE A3: Summary Statistics - Categories of College Majors

| | Number of students | Fraction (percent) | NCES classification | ACS classification |
|---|---|---|---|---|
| Arts and Communication | 41,284 | 7.8 | 9, 10, 50 | 1901, 1902, 1903, 1904, 2001, 6000, 6001, 6002, 6003, 6004, 6005, 6006, 6099 |
| Business | 32,512 | 6.1 | 52 | 6200, 6201, 6202, 6203, 6204, 6205, 6206, 6207, 6209, 6210, 6211, 6212, 6299 |
| Health and Education | 17,232 | 3.2 | 13, 51, 60 | 2300, 2301, 2304, 2305, 2306, 2307, 2308, 2309, 2310, 2311, 2314, 2399, 3601, 6100, 6102, 6103, 6104, 6106, 6107, 6108, 6109, 6110 |
| Humanities | 87,271 | 16.5 | 05, 16, 23, 24, 38, 39, 54 | 1501, 2601, 2602, 2603, 3301, 3302, 3401, 3402, 4801, 4901, 6402, 6403 |
| STEM | 157,711 | 29.7 | 11, 14, 26, 27, 40 | 2100, 2101, 2102, 2106, 2400, 2401, 2402, 2404, 2405, 2406, 2407, 2408, 2409, 2410, 2412, 2413, 2414, 2418, 2499, 3600, 3601, 3602, 3603, 3604, 3605, 3606, 3607, 3608, 3609, 3611, 3699, 3700, 3701, 3702, 5000, 5001, 5002, 5003, 5004, 5005, 5006, 5007, 5008 |
| Social Science | 146,128 | 27.6 | 42, 45 | 5200, 5201, 5205, 5299, 5500, 5501, 5502, 5503, 5504, 5505, 5506, 5507, 5599 |
| Other | 48,226 | 9.1 | 01, 03, 04, 12, 15, 19, 22, 28, 29, 30, 31, 36, 37, 41, 43, 44, 47, 49, 62 | 1100, 1101, 1103, 1106, 1199, 1300, 1301, 1303, 1401, 2201, 2501, 2502, 2503, 2504, 2599, 2901, 3202, 3801, 4000, 4001, 4002, 4006, 4008, 4101, 5102, 5301, 5401, 5402, 5403, 5404, 5701, 5901 |

*Note:* This table provides summary statistics by category of college major. The sample includes all schools across all years. Each college major is assigned into one of seven categories. For each category, I list the number of students with this major as their dominant major, the fraction of students, the classification codes used by the National Center for Education Statistics (NCES) for this group of majors, and the classification codes used in the American Community Survey (ACS) for this group of majors.

TABLE A4: Summary Statistics - Categories of College Majors

| | Students | Percent | High-earning | NCES classification | ACS classification |
|---|---|---|---|---|---|
| Anthropology | 6,371 | 1.20 | 0 | 45.02, 45.03 | 5502 |
| Arts | 25,957 | 1.20 | 0 | 50.01, 50.02, 50.03, 50.04, 50.05, 50.06, 50.07, 50.09, 50.10, 50.99 | 6000, 6001, 6002, 6003, 6004, 6005, 6006, 6099 |
| Biology | 52,574 | 9.91 | 1 | 26.01, 26.02, 26.03, 26.04, 26.05, 26.07, 26.08, 26.09, 26.10, 26.11, 26.12, 26.13, 26.14, 26.15, 26.99 | 3600, 3601, 3602, 3603, 3604, 3605, 3606, 3607, 3608, 3609, 3611, 3699 |
| Business | 26,398 | 4.98 | 1 | 52.01, 52.02, 52.03, 52.04, 52.05, 52.06, 52.07, 52.08, 52.09, 52.10, 52.11, 52.12, 52.13, 52.14, 52.15, 52.16, 52.17, 52.18, 52.19, 52.20, 52.21, 52.99 | 6200, 6201, 6202, 6203, 6204, 6205, 6206, 6209, 6210, 6211, 6212, 6299 |
| Chemistry | 9,056 | 1.71 | 1 | 40.05 | 5003 |
| Communication | 15,327 | 2.89 | 0 | 09.01, 09.04, 09.07, 09.09, 09.10, 09.99, 10.01, 10.02, 10.03 10.99 | 1901, 1902, 1903, 1904, 2001 |
| Computer Science | 16,694 | 3.15 | 1 | 11.01, 11.02, 11.03, 11.04, 11.05, 11.06, 11.07, 11.08, 11.09, 11.10, 11.99 | 2100, 2101, 2102, 2106 |
| Economics | 58,274 | 10.99 | 1 | 45.06 | 5501 |
| Education | 2,131 | 0.40 | 0 | 13.01, 13.02, 13.03, 13.04, 13.05, 13.06, 13.07, 13.09, 13.10, 13.11, 13.12, 13.13, 13.14, 13.15, 13.99 | 2300, 2301, 2304, 2305, 2306, 2307, 2308, 2309, 2310, 2311, 2314, 2399 |
| Engineering | 56,894 | 10.73 | 1 | 14.01, 14.02, 14.03, 14.04, 14.05, 14.06, 14.07, 14.08, 14.09, 14.10, 14.11, 14.12, 14.13, 14.14, 14.18, 14.19, 14.20, 14.21, 14.22, 14.23, 14.24, 14.25, 14.27, 14.28, 14.32, 14.33, 14.34, 14.35, 14.36, 14.37, 14.38, 14.39, 14.40, 14.41, 14.42, 14.43, 14.44, 14.45, 14.99, 15.00, 15.01, 15.02, 15.03, 15.04, 15.05, 15.06, 15.07, 15.08, 15.09, 15.10, 15.11, 15.12, 15.13, 15.14, 15.15, 15.16, 15.99 | 2400, 2401, 2402, 2404, 2405, 2406, 2407, 2408, 2409, 2410, 2412, 2413, 2414, 2418, 2499, 2501, 2502, 2503, 2504, 2599 |

TABLE A4: Summary Statistics - Categories of College Majors (cont'd)

| | Students | Percent | High-earning | NCES classification | ACS classification |
|---|---|---|---|---|---|
| English | 24,153 | 4.55 | 0 | 23.01, 23.13, 23.14, 23.99 | 3301, 3302 |
| Finance | 6,114 | 1.15 | 1 | 52.08 | 6207 |
| Foreign languages | 17,103 | 3.22 | 0 | 16.01, 16.02, 16.03, 16.04, 16.05, 16.06, 16.07, 16.08, 16.09, 16.10, 16.11, 16.12, 16.13, 16.14, 16.15, 16.16, 16.99 | 2601, 2602, 2603 |
| History | 24,122 | 4.55 | 0 | 54.01 | 6402, 6403 |
| Liberal Arts and Area Studies | 13,334 | 2.51 | 0 | 05.01, 05.02, 24.01 | 1501, 3401, 3402 |
| Mathematics | 17,302 | 3.26 | 1 | 27.01, 27.03, 27.05, 27.99 | 3700, 3701, 3702 |
| Health Professions | 15,101 | 2.85 | 0 | 51.00, 51.01, 51.02, 51.04, 51.05, 51.06, 51.07, 51.08, 51.09, 51.10, 51.11, 51.12, 51.14, 51.15, 51.17, 51.18, 51.19, 51.20, 51.21, 51.22, 51.23, 51.24, 51.25, 51.26, 51.27, 51.31, 51.32, 51.33, 51.34, 51.35, 51.36, 51.37, 51.38, 51.39, 51.99, 60.01, 60.03, 60.04, 60.05, 60.06 | 3601, 6100, 6102, 6103, 6104, 6106, 6107, 6108, 6109, 6110 |
| Philosophy and Religious Studies | 8,559 | 1.61 | 0 | 38.00, 38.01, 38.02, 38.99, 39.02, 39.03, 39.04, 39.05, 39.06, 39.07, 39.99 | 4801, 4901 |
| Physical Sciences, other | 3,800 | 0.72 | 1 | 40.01, 40.02, 40.04, 40.06, 40.10 | 5000, 5001, 5002, 5004, 5005, 5006, 5008 |
| Physics | 7,981 | 1.50 | 1 | 40.08 | 5007 |
| Political Science | 43,542 | 8.21 | 1 | 45.09, 45.10 | 5505, 5506 |
| Psychology | 25,910 | 4.89 | 0 | 42.01, 42.27, 42.28, 42.99 | 5200, 5201, 5205, 5299 |
| Public Administration | 7,942 | 1.50 | 1 | 44.00, 44.02, 44.04, 44.05, 44.07, 44.99 | 5401, 5402, 5403, 5404 |
| Social Sciences, other | 4,071 | 0.77 | 0 | 45.01, 45.12, 45.07, 45.04, 45.05, 45.99 | 5500, 5503, 5504, 5599 |
| Sociology | 7,960 | 1.50 | 0 | 45.11, 45.13, 45.14 | 5507 |

*Note:* This table provides summary statistics by individual college major for the 25 most common majors. The sample includes all schools across all years. Each college major is assigned into one of seven categories. For each category, I list the number of students with this major as their dominant major, the fraction of students, an indicator for whether this major is classified as a high-earning major, the classification codes used by the National Center for Education Statistics (NCES) for this group of majors, and the classification codes used in the American Community Survey (ACS) for this group of majors.

TABLE A5: Summary Statistics - Student-level data

| | Mean | Median | Std. dev. | 10%-tile | 90%-tile |
|---|---|---|---|---|---|
| Commencement programs | | | | | |
| High-earning major | 0.58 | 1.00 | 0.49 | 0.00 | 1.00 |
| Race/ethnicity | | | | | |
| Asian | 0.04 | 0.00 | 0.20 | 0.00 | 0.00 |
| Black | 0.05 | 0.00 | 0.22 | 0.00 | 0.00 |
| Hispanic | 0.05 | 0.00 | 0.22 | 0.00 | 0.00 |
| White | 0.68 | 1.00 | 0.47 | 0.00 | 1.00 |
| Other | 0.00 | 0.00 | 0.04 | 0.00 | 0.00 |
| Unknown race/ethnicity | 0.20 | 0.00 | 0.40 | 0.00 | 1.00 |
| Gender | | | | | |
| Female | 0.45 | 0.00 | 0.50 | 0.00 | 1.00 |
| Male | 0.44 | 0.00 | 0.50 | 0.00 | 1.00 |
| Unknown gender | 0.11 | 0.00 | 0.31 | 0.00 | 1.00 |
| USPS addresses | | | | | |
| Median neighborhood income | 104,386 | 95,338 | 48,012 | 52,207 | 167,803 |
| Credit bureau records | | | | | |
| Student debt | 18,322 | 0 | 37,655 | 0 | 54,791 |
| Parents' credit score | 713 | 725 | 84 | 593 | 808 |

*Note:* This table provides summary statistics from the datasets where the unit of observation is a student. It reports summary statistics for three data sources: the commencement programs, the USPS address data, and the credit bureau records. High-earning major is a dummy taking the value of one if the students chose one of the 12 highest earning majors, as defined in Table A4. Race, ethnicity, and gender are inferred algorithmically based on first and last name, with details provided in Appendix Section E.V. Median neighborhood income is the median income in the census tract where the student's parents lived the year that the student matriculated in college. The data source is the census bureau. Student debt is the dollar amount of student debt in the year that the student graduated from college. Parents' credit score is the average credit score of the studen's parents in the year that the student matriculated college.

TABLE A6: Summary statistics - School-year-level data

| | i: UNLP vs. non-UNLP control schools | | | | ii: UNLP vs. All U.S. schools | | | |
|---|---|---|---|---|---|---|---|---|
| | UNLP | non-UNLP | Diff. | Std. err. | UNLP | All U.S. | Diff. | Std. err. |
| Admissions rate | 0.21 | 0.26 | -0.05 | 0.04 | 0.21 | 0.73 | -0.52 | 0.04*** |
| SAT median (critical reading) | 702 | 684 | 17.71 | 14.51 | 702 | 525 | 177.39 | 13.14*** |
| SAT median (math) | 707 | 709 | -2.11 | 17.76 | 707 | 526 | 181.14 | 14.03*** |
| SAT median (writing) | 702 | 691 | 10.93 | 19.76 | 702 | 495 | 207.40 | 22.88*** |
| ACT median (cumulative) | 31 | 31 | -0.37 | 0.76 | 31 | 22 | 8.51 | 0.71*** |
| ACT median (English) | 31 | 32 | -0.88 | 0.79 | 31 | 22 | 9.19 | 0.89*** |
| ACT median (math) | 31 | 31 | -0.53 | 0.96 | 31 | 22 | 8.87 | 0.79*** |
| ACT median (writing) | 9 | 11 | -1.54 | 3.81 | 9 | 9 | -0.06 | 2.45 |
| Enrollment of undergraduate students | 4,339 | 4,603 | -264 | 1,346 | 4,339 | 2,237 | 2,101 | 1,372 |
| Share of students who are white | 0.41 | 0.40 | 0.01 | 0.05 | 0.41 | 0.44 | -0.03 | 0.09 |
| Share of students who are black | 0.07 | 0.05 | 0.02 | 0.01 | 0.07 | 0.18 | -0.11 | 0.07 |
| Share of students who are Hispanic | 0.09 | 0.09 | -0.00 | 0.01 | 0.09 | 0.15 | -0.06 | 0.07 |
| Share of students who are Asian | 0.09 | 0.16 | -0.08 | 0.04* | 0.09 | 0.03 | 0.06 | 0.02** |
| Average net price | 23,982 | 26,090 | -2,108 | 2,859 | 23,982 | 18,336 | 5,646 | 2,815** |
| Average net price for $0-$30k fam. inc. | 10,792 | 10,632 | 160 | 2,697 | 10,792 | 17,042 | -6,250 | 2,615** |
| Average net price for $30k-$48k fam. inc. | 10,940 | 10,709 | 231 | 2,655 | 10,940 | 17,944 | -7,004 | 2,688*** |
| Average net price for $48k-$75k fam. inc. | 15,093 | 15,264 | -170 | 2,706 | 15,093 | 19,923 | -4,830 | 2,705* |
| Average net price for $75k-$110k fam. inc. | 22,521 | 24,241 | -1,720 | 2,431 | 22,521 | 22,157 | 364 | 2,703 |
| Average net price for $110k+ fam. inc. | 40,940 | 41,653 | -713 | 1,882 | 40,940 | 23,809 | 17,131 | 3,205*** |
| Net tuition revenue per full-time eq. student | 17,872 | 19,824 | -1,953 | 3,475 | 17,872 | 16,207 | 1,665 | 28,204 |
| Average faculty salary | 10,183 | 11,465 | -1,282 | 715* | 10,183 | 5,225 | 4,958 | 416*** |
| Proportion of faculty that is full-time | 0.80 | 0.83 | -0.03 | 0.04 | 0.80 | 0.59 | 0.21 | 0.06*** |
| Pct. of undergraduates with a Pell Grant | 0.21 | 0.16 | 0.05 | 0.05 | 0.21 | 0.50 | -0.30 | 0.07*** |
| Completion rate | 0.90 | 0.87 | 0.02 | 0.03 | 0.90 | 0.45 | 0.44 | 0.04*** |

*Note:* This table reports summary statistics and covariate balance $t$-test for comparing universities that implemented UNLPs with the control group. The sample academic years are 1996-1997 to 2020-2021 (for non-implementing schools) and 1996-1997 until the year of implementation (for implementing schools). For each variable, I control for year fixed effects. The unit of observation is at the school-year level, and the data source is the Integrated Postsecondary Education Data System (IPEDS) and the College Scorecard. Significance level: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01.

TABLE A7: Descriptive Evidence: Family Background and Major Choice

| | High-earning major | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Parents' Credit Score | 0.025*** | 0.017*** | 0.017*** | | | |
| | (0.002) | (0.002) | (0.002) | | | |
| Log(Neighborhood Income) | | | | 4.127*** | 3.194*** | 3.229*** |
| | | | | (0.506) | (0.316) | (0.313) |
| Admissions rate | | | -14.869* | | | 1.209 |
| | | | (8.560) | | | (8.237) |
| SAT median (critical reading) | | | 0.066** | | | 0.024 |
| | | | (0.028) | | | (0.023) |
| SAT median (math) | | | -0.084** | | | 0.028 |
| | | | (0.034) | | | (0.039) |
| SAT median (writing) | | | 0.012 | | | -0.033 |
| | | | (0.039) | | | (0.032) |
| ACT median (cumulative) | | | 0.309 | | | 0.489 |
| | | | (0.585) | | | (0.564) |
| ACT median (English) | | | -0.654 | | | -1.060*** |
| | | | (0.425) | | | (0.387) |
| ACT median (math) | | | -0.121 | | | -0.746* |
| | | | (0.484) | | | (0.425) |
| ACT median (writing) | | | 0.078 | | | 0.054 |
| | | | (0.081) | | | (0.067) |
| Enrollment of undergraduate students | | | 0.000 | | | 0.000 |
| | | | (.) | | | (.) |
| Share of students who are white | | | 0.686 | | | -3.026 |
| | | | (3.327) | | | (2.969) |
| Share of students who are black | | | 43.546 | | | 24.156 |
| | | | (29.673) | | | (25.300) |
| Share of students who are Hispanic | | | 0.582 | | | 22.594 |
| | | | (27.420) | | | (17.790) |
| Share of students who are Asian | | | -8.652 | | | -7.752 |
| | | | (8.974) | | | (8.146) |
| Average net price (1,000s) | | | 0.084 | | | -0.299** |
| | | | (0.166) | | | (0.148) |
| Avg net price for \$0-\$30k fam. inc. (1,000s) | | | -0.062 | | | 0.273* |
| | | | (0.192) | | | (0.141) |
| Avg net price for \$30k-\$48k fam. inc. (1,000s) | | | 0.259 | | | -0.003 |
| | | | (0.203) | | | (0.186) |
| Avg net price for \$48k-\$75k fam. inc. (1,000s) | | | -0.426** | | | -0.203 |
| | | | (0.171) | | | (0.136) |
| Avg net price for \$75k-\$110k fam. inc. (1,000s) | | | 0.014 | | | -0.056 |
| | | | (0.121) | | | (0.108) |
| Avg net price for \$110k+ fam. inc. (1,000s) | | | -0.097 | | | -0.098 |
| | | | (0.128) | | | (0.131) |
| Average cost of attendance (1,000s) | | | 0.255 | | | 0.394* |
| | | | (0.274) | | | (0.222) |
| Net tuition revenue per full-time eq. student (1,000s) | | | 0.028 | | | 0.115 |
| | | | (0.125) | | | (0.097) |
| Average faculty salary (1,000s) | | | 0.377 | | | -0.112 |
| | | | (0.248) | | | (0.201) |
| Proportion of faculty that is full-time | | | 14.531** | | | 11.776* |
| | | | (7.245) | | | (6.372) |
| Pct. of undergraduates with a Pell Grant | | | 7.490 | | | -0.019 |
| | | | (19.877) | | | (16.886) |
| Completion rate | | | -8.324 | | | -21.147 |
| | | | (17.097) | | | (13.788) |
| School FE | | ✓ | ✓ | | ✓ | ✓ |
| Year FE | | ✓ | ✓ | | ✓ | ✓ |
| N | 206,888 | 206,888 | 206,888 | 206,888 | 206,888 | 206,888 |

Standard errors in parentheses
$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

*Note:* This table provides the regression coefficients from OLS regressions where the dependent variable is a dummy taking the value one if the student graduates with a high-earning major. Parents' Credit Score and Log(Neighborhood Income) come from the merged address and credit bureau data and is a student-level variable. The remaining variables come from the IPEDS data and vary at the school-year level. The dollar variables (net price, cost of attendance, tuition, and salary) are scaled by 1,000, such that the coefficient represents the effect of a \$1,000 increase in the variable.

TABLE A8: Effect of UNLP on Student Debt

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\tau = -3$ | 3,235* | 2,290 | 914 | -81 |
| | (1,863) | (1,632) | (2,241) | (2,841) |
| $\tau = -2$ | 1,375 | 426 | -410 | -1,289 |
| | (1,712) | (1,545) | (2,401) | (2,527) |
| $\tau = -1$ | 96 | -459 | -1,163 | -1,180 |
| | (1,414) | (1,376) | (1,832) | (2,491) |
| $\tau = 1$ | -4,556*** | -4,844*** | -5,178*** | -4,314 |
| | (1,547) | (1,387) | (1,928) | (2,762) |
| $\tau = 2$ | -7,318*** | -8,173*** | -8,206*** | -8,523*** |
| | (2,069) | (1,952) | (2,118) | (2,510) |
| $\tau = 3$ | -9,387*** | -10,209*** | -9,626*** | -9,663*** |
| | (1,896) | (1,776) | (1,924) | (3,038) |
| Control schools | | ✓ | ✓ | ✓ |
| Observations | 52,157 | 55,262 | 55,262 | 56,637 |
| Estimator | OLS | OLS | Sun & Abraham | Callaway & Sant'Anna |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports the average effects from estimating equation (2) for the baseline sample of students with a positive amount of student debt prior to the policy. Each column reports estimates from a separate regression. Effect at event time $\tau = 0$ is normalized to 0. Each column refers to a specific event-year coefficient. Columns (2) to (4) include the 10 non-UNLP-implementing control schools. Regressions for Columns (1) and (2). All regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Standard errors (in parentheses) are clustered at the school-year level. The estimator for Column (3) is Sun and Abraham (2021), and the estimator for Column (4) is Callaway and Sant'Anna (2021).

TABLE A9: Effect of UNLP on Major Choice

| | (1) | (2) | (3) |
|---|---|---|---|
| $\tau = -3$ | -0.003 | -0.002 | -0.001 |
| | (0.009) | (0.008) | (0.007) |
| $\tau = -2$ | -0.002 | -0.001 | 0.005 |
| | (0.007) | (0.007) | (0.007) |
| $\tau = -1$ | 0.003 | 0.004 | -0.001 |
| | (0.009) | (0.009) | (0.008) |
| $\tau = 1$ | 0.003 | 0.004 | 0.001 |
| | (0.007) | (0.007) | (0.007) |
| $\tau = 2$ | 0.021*** | 0.023*** | 0.014* |
| | (0.007) | (0.007) | (0.006) |
| $\tau = 3$ | 0.060*** | 0.061*** | 0.050*** |
| | (0.012) | (0.011) | (0.008) |
| Control schools | | ✓ | ✓ |
| Observations | 131,049 | 139,903 | 139,903 |
| Estimator | OLS | OLS | Sun & Abraham |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports the average effects from estimating equation (1) where the left-hand-side variable is a dummy variable taking the value one if the student chose a high-earning major. Each column reports estimates from a separate regression. Effect at event time $\tau = 0$ is normalized to 0. Each row refers to a specific event-year coefficient. Columns (2) to (4) include the 10 non-UNLP-implementing control schools. Regressions for Columns (1) and (2). All regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Standard errors (in parentheses) are clustered at the school-year level. Regression coefficients have been scaled by the baseline mean at $\tau = 0$. The estimator for Column (3) is Sun and Abraham (2021).

TABLE A10: Effect of UNLP on Major Choice (ITT)

| | High-earning major | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| $\tau = -5$ | -0.009* | 0.008 | 0.019*** |
| | (0.005) | (0.007) | (0.003) |
| $\tau = -4$ | -0.010* | 0.001 | -0.001 |
| | (0.005) | (0.007) | (0.002) |
| $\tau = -3$ | -0.004 | 0.001 | -0.001 |
| | (0.005) | (0.006) | (0.002) |
| $\tau = -2$ | -0.001 | -0.000 | -0.000 |
| | (0.005) | (0.007) | (0.002) |
| $\tau = -1$ | -0.000 | -0.000 | 0.005** |
| | (0.004) | (0.006) | (0.002) |
| $\tau = 1$ | 0.007** | 0.004 | 0.001 |
| | (0.003) | (0.005) | (0.002) |
| $\tau = 2$ | 0.016*** | 0.010 | 0.003 |
| | (0.005) | (0.007) | (0.003) |
| $\tau = 3$ | 0.029*** | 0.028*** | 0.031*** |
| | (0.007) | (0.006) | (0.003) |
| $\tau = 4$ | 0.028*** | 0.028*** | 0.033*** |
| | (0.007) | (0.007) | (0.003) |
| $\tau = 5$ | 0.029*** | 0.027*** | 0.034*** |
| | (0.007) | (0.007) | (0.003) |
| $\tau = 6$ | 0.033*** | 0.029*** | 0.039*** |
| | (0.007) | (0.008) | (0.003) |
| Control schools | | ✓ | ✓ |
| Observations | 230,070 | 404,917 | 404,917 |
| Estimator | OLS | OLS | Sun & Abraham |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports the average effects from estimating equation (2) where the left-hand-side variable is a dummy variable taking the value one if the student chose a high-earning major. Each column reports estimates from a separate regression. Effect at event time $\tau = 0$ is normalized to 0. Each row refers to a specific event-year coefficient. Columns (2) to (4) include the 10 non-UNLP-implementing control schools. Regressions for Columns (1) and (2) are OLS, and the estimator for Column (3) is Sun and Abraham (2021). All regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Regression coefficients have been scaled by the baseline mean at $\tau = 0$. Standard errors (in parentheses) are clustered at the school-year level.

TABLE A11: Effect of Student Debt on Major Choice (IV Estimates)

| | First stage | | Second stage |
|---|---|---|---|
| | Loan amount | | High-earning major |
| | (1) | (2) | (3) |
| | $ | Log | |
| Years 1 to 3 | -3,965.13*** | -0.219*** | |
| | (954.41) | (0.052) | |
| Years 4 to 6 | -8,621.37*** | -0.401*** | |
| | (1,358.72) | (0.093) | |
| Loan amount | | | -0.037*** |
| | | | (0.013) |
| Baseline mean | 18,798 | 10.57 | 0.55 |
| Observations | 206,888 | 81,721 | 206,888 |
| Estimator | OLS | OLS | OLS |
| F-stat | 121.14 | | |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports the effect of the instrumental variables model. Column (1) and (2) report the average effects from estimating equation (3) where the dependent variable is the amount of student debt in dollars and in logs, respectively. The effect at event time $\tau = 0$ is normalized to 0. The coefficient for Years 1 to 3 is the post-period average of the coefficients for $\tau = 1$ to $\tau = 3$. The coefficient for Years 4 to 6 is the average of the coefficients for $\tau = 4$ to $\tau = 6$ which are the students who enrolled after the policy was announced. Column (3) report the second stage coefficients from equation (4) using the predicted value of student debt from Column (1). The coefficient is scaled to represent the effect of an increase in student debt of $10,000. All regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Regression coefficients have been scaled by the baseline mean at $\tau = 0$ in Column (3). Standard errors (in parentheses) are clustered at the school-year level.

TABLE A12: Effect of UNLP on Major Choice

| | High-lifetime & high-initial | High-lifetime & low-initial |
|---|---|---|
| | (1) | (2) |
| Years 1 to 3 | 0.001 | $0.046^{***}$ |
| | (0.005) | (0.005) |
| Control schools | ✓ | ✓ |
| Observations | 139,903 | 139,903 |
| Estimator | OLS | OLS |

Standard errors in parentheses

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Note:* This table reports the average effects from estimating equation (1). Each column reports estimates from a separate regression. In Column (1), the dependent variable is a dummy variable taking the value of one if the major is Mathematics, Engineering, Computer Science, Finance, Business, or Public Administration. In Column (2), the dependent variable is one if the major is Biology, Chemistry, Physical Sciences, other, Physics, Political Science, or Economics. Effect at event time $\tau = 0$ is normalized to 0. The coefficient for Years 1 to 3 is the post-period average of the coefficients for $\tau = 1$ to $\tau = 3$. Both regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Estimates are scaled by the baseline mean at $\tau = 0$. Standard errors (in parentheses) are clustered at the school-year level.

TABLE A13: The Effect of UNLP on Major-Implied Earnings Trajectories

| | Mean life-time wage | Slope (bps.) | Initial wage | Year bins | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) 20s | (5) 30s | (6) 40s |
| $\tau = -3$ | 149 | 2 | -257 | -280 | 96 | -193 |
| | (256) | (16) | (172) | (197) | (192) | (386) |
| $\tau = -2$ | 130 | 13 | -10 | 15 | 354** | 52 |
| | (262) | (15) | (166) | (188) | (179) | (353) |
| $\tau = -1$ | -138 | 11 | 16 | 46 | 278* | -100 |
| | (232) | (12) | (141) | (159) | (152) | (304) |
| $\tau = 1$ | 312 | 23* | -98 | -124 | 121 | 301 |
| | (193) | (12) | (109) | (128) | (161) | (288) |
| $\tau = 2$ | 475* | 17 | -117 | -78 | 315 | 697* |
| | (250) | (15) | (149) | (165) | (199) | (371) |
| $\tau = 3$ | 1,692*** | 57*** | -470** | -198 | 736*** | 2,134*** |
| | (326) | (21) | (235) | (255) | (236) | (501) |
| Control schools | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 139,903 | 139,903 | 139,903 | 139,903 | 139,903 | 139,903 |
| Estimator | OLS | OLS | OLS | OLS | OLS | OLS |

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports regression coefficients from estimating equation (1) where the dependent variable is implied earnings by college major at different points in time. Column 1 give the coefficients for the mean life-time wage, Column 2 the coefficient for the slope, Column 3 the coefficients for the initial wage, and Columns 4, 5, and 6 the coefficients for the wage in different age decades. All regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Standard errors (in parentheses) are clustered at the school-year level. The data sources are hand-collected commencement programs and the ACS.

TABLE A14: Plausible Economic Mechanisms

| | Grad school | Std. dev. | Time allocation in College | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) Difficulty | (4) Avg. GPA | (5) Hours |
| $\tau = -3$ | 0.012 | 0.042*** | 0.004 | 0.009 | 0.009 |
| | (0.014) | (0.012) | (0.014) | (0.014) | (0.009) |
| $\tau = -2$ | -0.005 | 0.017 | 0.003 | 0.006 | 0.001 |
| | (0.013) | (0.011) | (0.013) | (0.013) | (0.007) |
| $\tau = -1$ | 0.004 | 0.006 | 0.011 | 0.002 | 0.014** |
| | (0.011) | (0.010) | (0.011) | (0.012) | (0.006) |
| $\tau = 1$ | 0.000 | 0.003 | -0.003 | -0.013 | 0.010 |
| | (0.011) | (0.011) | (0.010) | (0.010) | (0.006) |
| $\tau = 2$ | 0.013 | 0.020 | 0.004 | -0.023* | 0.013 |
| | (0.013) | (0.012) | (0.013) | (0.013) | (0.009) |
| $\tau = 3$ | 0.056*** | 0.034** | 0.036* | -0.055*** | 0.017** |
| | (0.019) | (0.017) | (0.019) | (0.021) | (0.008) |
| Control schools | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 139,903 | 139,903 | 139,903 | 139,903 | 139,903 |
| Estimator | OLS | OLS | OLS | OLS | OLS |
| Data Source | ACS | ACS | Novik (2022) | Rask (2010) | NSSE (2016) |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports regression coefficients from estimating equation (1) where the dependent variable is a dummy variable taking the value of one if the college major is above the median of a specific characteristic. In Column 1 the characteristic is the share going to graduate school. In Column 2, the characteristic is the residualized standard deviation of earnings. In Columns 3, 4, and 5 the characteristics are self-reported difficulty, the average GPA, and the average hours worked. Standard errors (in parentheses) are clustered at the school-year level. The data sources for each of the major-specific characteristics is the ACS (Columns 1 and 2), and Novik (2022), Rask (2010), and the NSSE (2016) in Columns 3, 4, and 5, respectively. Regression coefficients have been scaled by the baseline mean at $\tau = 0$.

TABLE A15: Effect of UNLP on Later-in-life Outcomes

| | Grad school | Grad school debt | | | Auto debt | | | Mortgage debt | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | Dummy | Dummy | Delinquency | Age | Dummy | Delinquency | Age | Dummy | Delinquency | Age |
| Years 1 to 3 | 0.039*** | 0.062*** | -0.032** | -0.044** | 0.056*** | -0.019 | -0.018* | 0.036* | -0.026 | 0.012 |
| | (0.014) | (0.022) | (0.016) | (0.021) | (0.023) | (0.017) | (0.010) | (0.019) | (0.019) | (0.017) |
| Control schools | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 112,308 | 85,928 | 85,928 | 85,928 | 85,928 | 85,928 | 85,928 | 85,928 | 85,928 | 85,928 |
| Estimator | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Note:* This table reports the average effects from estimating the effect of UNLP on later-in-life outcomes. Each column reports estimates from a separate regression. Column (1) reports the effect from estimating equation (2) where the dependent variable is a dummy variable taking the value one if the student has listed a graduate degree on their resume. Columns (2)–(10) report coefficient estimates from estimating equation (1). In Columns (2), (5), and (8), the dependent variable is a dummy variable taking the value one if the student within five years after graduation has taken on graduate school debt, auto debt, or mortgage debt, respectively. In Columns (3), (6), and (8), the dependent variable is a dummy indicating if the student has any debt in delinquency. In Columns (4), (7), and (10) the dependent variable is the age at which the student obtained their first loan. Effect at event time $\tau = 0$ is normalized to 0. The coefficient for Years 1 to 3 is the post-period average of the coefficients for $\tau = 1$ to $\tau = 3$. Both regressions include school fixed effects and cohort fixed effects as well as school-year control variables. Estimates are scaled by the baseline mean at $\tau = 0$. Standard errors (in parentheses) are clustered at the school-year level.

TABLE A16: Comparing Financial Aid Policies

| | *Dependent variable:* Pct. of Undergraduates with Student Debt | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Full Sample | | | | No Loan | Loan Cap | No Par. Contr. | No Tuition |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Financial Aid Program | −6.126*** | −4.293*** | −5.650*** | −0.231 | −2.591*** | 2.582*** | −1.688 | 0.099 |
| | (0.527) | (0.973) | (0.301) | (0.560) | (0.848) | (0.767) | (1.132) | (2.631) |
| Constant | 38.096*** | | | | | | | |
| | (0.350) | | | | | | | |
| Year FE | | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ |
| School FE | | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 2,887 | 2,887 | 2,887 | 2,887 | 1,296 | 641 | 360 | 378 |
| $R^2$ | 0.045 | 0.068 | 0.712 | 0.745 | 0.661 | 0.668 | 0.853 | 0.755 |
| Adjusted $R^2$ | 0.044 | 0.062 | 0.704 | 0.735 | 0.641 | 0.647 | 0.841 | 0.731 |
| *Note:* | | | | | | | | $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01 |

*Note:* This table reports regression coefficients from regressing percent of undergraduates with student debt at school s in year t on an indicator taking the value 1 if school s has implemented a policy in year t, school fixed effects, and year fixed effects. The data source is the Integrated Postsecondary Education Data System (IPEDS).

TABLE A17: Comparing No Loan Policies

| | *Dependent variable:* Pct. of Undergraduates with Student Debt | | | | | |
|---|---|---|---|---|---|---|
| | Full Sample | | | | Low Income Students | All Students |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| No Loan Program | $-7.892^{***}$ | $-6.690^{***}$ | $-7.042^{***}$ | $-2.591^{***}$ | 0.363 | $-12.606^{***}$ |
| | (0.615) | (1.153) | (0.426) | (0.848) | (1.068) | (1.341) |
| Constant | $36.569^{***}$ | | | | | |
| | (0.414) | | | | | |
| Year FE | | ✓ | | ✓ | ✓ | ✓ |
| School FE | | | ✓ | ✓ | ✓ | ✓ |
| Observations | 1,296 | 1,296 | 1,296 | 1,296 | 972 | 324 |
| $R^2$ | 0.113 | 0.145 | 0.619 | 0.661 | 0.636 | 0.756 |
| Adjusted $R^2$ | 0.112 | 0.133 | 0.603 | 0.641 | 0.611 | 0.726 |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

*Note:* This table reports regression coefficients from regressing percent of undergraduates with student debt at school s in year t on an indicator taking the value 1 if school s has implemented a NLP policy in year t, school fixed effects, and year fixed effects. Column 5 reports results where the dependent variable takes value 1 if the school implemented for only low income students. Column 6 reports results where the dependent variable takes value 1 if the school implemented for all students. The data source is the Integrated Postsecondary Education Data System (IPEDS).

TABLE A18: Mean and standard deviation of residualized wages

| | Mean | StdDev | Coef_var | Slope_lvl | Slope_log |
|---|---|---|---|---|---|
| Biology and Life Sciences | 19,728 | 97,176 | 4.93 | 7,899 | 26.38 |
| Engineering | 17,298 | 83,694 | 4.84 | 6,773 | 20.13 |
| Physical Sciences | 11,886 | 89,019 | 7.49 | 6,331 | 20.32 |
| Mathematics and Statistics | 10,414 | 85,148 | 8.18 | 6,158 | 16.98 |
| Social Sciences | 8,474 | 90,187 | 10.64 | 5,940 | 12.90 |
| Computer and Information Sciences | 7,443 | 71,722 | 9.64 | 5,615 | 18.99 |
| Medical and Health Sciences and Services | 6,523 | 57,669 | 8.84 | 3,668 | 12.69 |
| Business | 4,390 | 79,458 | 18.10 | 4,797 | 9.70 |
| Transportation Sciences and Technologies | 2,630 | 75,224 | 28.60 | 5,467 | 16.07 |
| Area, Ethnic, and Civilization Studies | 2,102 | 75,643 | 35.99 | 4,514 | 9.41 |
| History | -1,294 | 84,321 | -65.15 | 5,539 | 13.47 |
| Interdisc and Multi-Disc Studies | -2,118 | 64,046 | -30.24 | 4,282 | 10.58 |
| Law | -4,720 | 64,945 | -13.76 | 3,847 | 15.62 |
| Communications | -4,934 | 63,667 | -12.90 | 3,608 | 3.44 |
| Engineering Technologies | -4,988 | 64,569 | -12.95 | 4,743 | 19.70 |
| Psychology | -5,787 | 60,417 | -10.44 | 3,443 | 7.98 |
| Linguistics and Foreign Languages | -6,127 | 66,547 | -10.86 | 3,725 | 7.12 |
| English Lang, Lit, and Comp | -6,255 | 69,124 | -11.05 | 3,810 | 7.22 |
| Architecture | -8,198 | 65,959 | -8.05 | 3,664 | 9.38 |
| Public Affairs, Policy, and Social Work | -10,402 | 49,919 | -4.80 | 2,477 | 6.92 |
| Philosophy and Religious Studies | -10,464 | 80,478 | -7.69 | 4,991 | 11.91 |
| Liberal Arts and Humanities | -11,420 | 62,077 | -5.44 | 3,489 | 9.30 |
| Criminal Justice and Fire Protection | -11,986 | 48,567 | -4.05 | 3,805 | 14.63 |
| Physical Fitness and Leisure | -12,074 | 50,626 | -4.19 | 4,260 | 14.98 |
| Environment and Natural Resources | -12,662 | 56,124 | -4.43 | 3,699 | 9.59 |
| Family and Consumer Sciences | -12,820 | 44,407 | -3.46 | 2,104 | 0.70 |
| Agriculture | -14,551 | 61,743 | -4.24 | 3,512 | 8.29 |
| Education Administration and Teaching | -14,834 | 40,040 | -2.70 | 2,225 | 9.01 |
| Fine Arts | -18,325 | 53,725 | -2.93 | 2,704 | 3.20 |
| Theology and Religious Vocations | -34,594 | 43,975 | -1.27 | 2,341 | 6.50 |

*Note:* This table reports the average residual wage (in Column 1) and the standard deviation in residuals (in Column 2) from a Mincer regression of annual wage on age, age-squared, race, ethnicity, sex, and survey-year fixed effects. Column 3 reports the predicted slope on age from a Mincer regression of annual wage where each major interacts with age and age-squared; the slope is evaluated at age 22. The data source is the ACS.

TABLE A19: Regression coefficients for wage imputation

| | (1) Wage | (2) Log(wage) |
|---|---|---|
| Age | 8656.8 | 0.28 |
| | (94.53) | (106.51) |
| $Age^2$ | -91.4 | -0.0035 |
| | (-125.29) | (-124.15) |
| Non-Hispanic White | 13148.2 | 0.46 |
| | (53.12) | (42.54) |
| African American | -755.2 | 0.46 |
| | (-1.26) | (26.60) |
| Hispanic | 788.1 | 0.052 |
| | (3.11) | (4.96) |
| Asian | 12662.4 | -0.19 |
| | (15.79) | (-9.87) |
| Female | -32790.5 | -1.17 |
| | (-52.62) | (-94.26) |
| Survey Year FE | ✓ | ✓ |
| N | 5,628,805 | 5,628,805 |
| $R^2$ | 0.11 | 0.04 |

*Note:* This table reports regression coefficients from the Mincer regression of annual wage and log(wage) on age, age squared, race, ethnicity, gender, and survey-year fixed effects. The data source is the American Community Survey (ACS).

# C Theoretical Appendix

## C.I Conceptual Framework

In this section, I describe in three steps the theoretical framework that guides my empirical tests of the effects of student debt on career trajectories. First, I set up a standard buffer-stock consumption–savings model for the problem that a student faces when choosing a college major. I then derive the closed-form solution for the model. Finally, guided by the model, I outline the implications for the empirical tests.

### C.I.1 Model

The model is a standard buffer-stock model, where a student $i$ chooses her job $j$ and her savings $a_1$ to maximize utility from consumption subject to a per-period budget constraint and a borrowing constraint. There are two periods: $t \in \{0, 1\}$. The period $t = 0$ corresponds to the first five years of the student's career after college, and $t = 1$ corresponds to the rest of her career. The student chooses between two jobs $j \in \{0, 1\}$, where $j = 0$ represents a low-skilled job that any college graduate could fill without further investment in human capital, and $j = 1$ represents a high-skilled job that requires additional human capital investments. For tractability, I assume that utility is logarithmic, that there is no discounting, that there is student debt repayment in the first period only, and that the borrowing constraint is zero. The problem faced by the student is then

$$\max_{j, a_1} \ln(c_0) + \ln(c_1) \qquad \text{s.t.} \qquad a_1 = (y_0 - c_0 - d^i - j\theta^i) \quad \text{and} \quad a_1 \geq 0, \tag{6}$$

where $c_t$ denotes consumption, and $a_1$ denotes liquid assets that earn a per-period interest of $r$ and are subject to a borrowing constraint of 0. The term $d^i$ denotes the student debt payments; the superscript $i$ indicates that $d^i$ varies exogenously across students.

For simplicity, I assume that there is no uncertainty, and that the two jobs have the same job amenities. This means that if the student chooses the low-skilled job, $j = 0$, then she pays no investment cost in the first period, and her wage in the second period is $w_0$. If she chooses the high-skilled job, $j = 1$, then she pays an investment cost of $\theta^i$ in the first period, and her wage in the second period is $w_1$.

For tractability, I assume that $\theta^i > 0$, $w_1 > w_0$, and $(1 + r)y_0 < w_0$. This ensures that the low-skilled job has a higher wage in the first period, that the high-skilled job has a higher wage in the second period, and that the credit constraint is binding. Finally, I assume that $y_0 > d^i$ to ensure positive consumption.

We can now solve the model in closed form. The solution is characterized by the following threshold condition: The student will choose to invest $\theta^i$ to get the high-skilled job, $j = 1$, if and only if

$$\theta^i < \left( \frac{w_1 - w_0}{w_1} \right) (y_0 - d^i). \tag{7}$$

The inequality (7) states that the optimal choice of $j^*$ depends on the relative wage difference $w_1 - w_0$, the initial resources $y_0$, the investment cost $\theta^i$, and the amount of student debt $d^i$. Furthermore, we see that the probability of choosing $j = 1$ (i.e., investing in human capital and getting the high-skilled job) is increasing with respect to relative wage difference and initial resources, and decreasing with respect to investment cost and the amount of student debt:

$$j^* = j \left( \underbrace{w_1 - w_0}_{+}, \underbrace{y_0}_{+}, \underbrace{\theta^i}_{-}, \underbrace{d^i}_{-} \right). \tag{8}$$

### C.I.2 Accounting for Wage Uncertainty

This model is similar to the one above, except there is time-discounting and uncertainty over the wages in the first period. As before, there are two periods: $t \in \{0, 1\}$. The period $t = 0$ corresponds to the first five years of the student's career after college, and $t = 1$ corresponds to the rest of her career. The student chooses between two jobs $j \in \{0, 1\}$, where $j = 0$ represents a low-skilled job that any college graduate could fill without further investment in human capital, and $j = 1$ represents a high-skilled job that requires additional human capital investments.

Relative to the benchmark model, I now take into account a discount rate $\beta$, and the wages are uncertain. Specifically, the low-skilled job has a wage of $w_{0H}$ with probability $\pi_0$ and a wage of $w_{0L}$ with probability $1 - \pi_0$, where $w_{0H} > w_{0L}$. Similarly, the high-skilled job has a wage of $w_{1H}$ with probability $\pi_1$ and a wage of $w_{1L}$ with probability $1 - \pi_1$, where $w_{1H} > w_{1L}$.

The maximization problem is then

$$\max_{j, a_1} \ \ln(c_0) + \beta \mathbb{E}[\ln(c_1)] \qquad \text{s.t.} \qquad a_1 = (y_0 - c_0 - d^i - j\theta^i) \quad \text{and} \quad a_1 \geq 0. \tag{9}$$

The budget constraint can be described as the consumption choices given each choice and each state of the world:

$$
\begin{aligned}
& & c_0 &= y_0 - D^i - j\theta^i - a_1; \\
\text{if } j = 0, & \quad \text{w. prob. } \pi_0 : & c_1 &= w_{0H} + a_1(1+r); \\
& \quad \text{w. prob. } 1-\pi_0 : & c_1 &= w_{0L} + a_1(1+r); \\
\text{if } j = 1, & \quad \text{w. prob. } \pi_1 : & c_1 &= w_{1H} + a_1(1+r); \\
& \quad \text{w. prob. } 1-\pi_1 : & c_1 &= w_{1L} + a_1(1+r).
\end{aligned}
$$

We can now solve the model in closed form. The solution is characterized by the following threshold condition: The student will choose to invest $\theta^i$ to get the high-skilled job, $j = 1$, if and only if

$$\theta^i \leq (y_0 - d^i)\frac{w_{1,H}^{\beta\pi_1} w_{1,L}^{\beta(1-\pi_1)} - w_{0,H}^{\beta\pi_0} w_{0,L}^{\beta(1-\pi_0)})}{w_{1,H}^{\beta\pi_1} w_{1,L}^{\beta\pi_1}}. \tag{10}$$

The inequality (10) states that the optimal choice of $j^*$ depends on the initial resources $y_0$, the investment cost $\theta^i$, the amount of student debt $d^i$, and the relative wage difference scaled by their probabilities. Furthermore, we see that the probability of choosing $j = 1$ (i.e., investing in human capital and getting the high-skilled job) is increasing with respect to initial resources, and decreasing with respect to investment cost and the amount of student debt:

$$j^* = j\left(\underbrace{y_0}_{+}, \underbrace{\theta^i}_{-}, \underbrace{d^i}_{-}\right). \tag{11}$$

### C.I.3 Empirical Hypotheses and Discussion

From the benchmark model, I can present my three empirical hypotheses. The first two hypotheses test the average effects of student debt and family characteristics on job choices, whereas the third hypothesis tests the heterogeneous effects of student debt across family characteristics.

For the first hypothesis, we draw on the empirical relationship that high-skilled jobs have (a) higher average lifetime earnings, (b) higher variability in earnings, and (c) higher earnings growth.

We then notice that from Equations (10) and (11), higher student debt leads to a more restrictive condition for the investment choice; in other words, the derivative of $j$ with respect to $d^i$ is negative: $\frac{\partial j}{\partial d^i} < 0$. Hypothesis 1 tests this relationship in the data:

**Hypothesis 1 (H1.A):** *Higher student debt decreases the probability of choosing a job with higher average earnings.*

**Hypothesis 1 (H1.B):** *Higher student debt decreases the probability of choosing a job with more variable earnings.*

**Hypothesis 1 (H1.C):** *Higher student debt decreases the probability of choosing a job with a steep earnings path.*

For the next hypothesis, notice that from Equations (7) and (8), a higher investment cost leads to a more restrictive threshold; that is, the derivative of $j$ with respect to $\theta^i$ is negative: $\frac{\partial j}{\partial \theta^i} < 0$. Hypothesis 2 tests this relationship in the data:

**Hypothesis 2 (H2):** *Students' average job choices (e.g. whether they choose jobs with flat or steep earnings paths) differ across family characteristics such as their parents' incomes, assets, credit scores, and debt-to-income ratios.*

For the third and final hypothesis, I ask: Does the effect of student debt on job choices differ based on family characteristics? In terms of the model, I am estimating the cross-derivative, $\frac{\partial^2 j}{\partial d^i \partial \theta^i}$.

**Hypothesis 3 (H3):** *The effect of student debt on job choices varies with the demographic and financial characteristics of the student and her parents.*

Testing H3 is the main contribution of this paper. Although no consensus exists yet, there is already a nascent literature studying the effects of student debt on labor choices (H1) and the variation of labor choices across family characteristics (H2). In this project I go further, studying not just the effects of student debt for the average student, but also how these effects vary with family characteristics.

Importantly, note that H3 is two-sided. In other words, I have not taken a stand on whether the cross-derivative between student debt and family characteristics is positive, negative, or even monotonic. It is theoretically unclear whether the costs (and benefits) of investing in human capital are higher for high-income or low-income families. Recall that the choice of job $j$ occurs simultaneously with the consumption choice $c$. One extra dollar of student debt can mean either less consumption or less investment in human capital:

$$\underbrace{\frac{\partial c}{\partial d^i}}_{\text{Marginal propensity to consume (MPC)}} \qquad \underbrace{\frac{\partial j}{\partial d^i}}_{\text{Marginal propensity to invest (MPI)}}$$

There is a longstanding literature on household finance examining the marginal propensity to consume (MPC).[41] A key result in this literature is that the MPC covaries positively with credit constraints (Johnson et al., 2006; Jappelli and Pistaferri, 2014; Baker, 2018). For example, low-income households with few liquid assets or high credit card debt display higher MPCs. On the other hand, it is not obvious that the same is true for the marginal propensity to invest (MPI). For example, if human capital is a luxury good, then we might expect that a student from a wealthy family is more likely to invest in human capital when she receives more financial aid, and that a student from a low-income family might have a high MPC and choose to consume the extra financial aid rather than invest it. That is, it is theoretically ambiguous whether the MPI is increasing or decreasing with respect to household income:

$$\frac{\partial \text{MPC}}{\partial \text{household income}} < 0; \qquad \frac{\partial \text{MPI}}{\partial \text{household income}} \overset{?}{\lesseqgtr} 0.$$

In sum, the model of Subsection C.I.2 provides a theoretical framework that guides my empirical tests. The model highlights that both student debt and exogenous investment costs may lead students to choose careers with higher initial earnings and lower earnings growth. Furthermore, and critically for this project, the model highlights that the effects of student debt depend on the costs (and benefits) of investing in human capital. And these costs might vary substantially across the student population.

[41] Going back, at least, to Friedman (1957) and Hall (1978), economists have studied how changes in income affect consumer spending. Seminal theoretical work by Deaton (1991) and Carroll (1997) emphasizes the role of borrowing constraints, and a large empirical literature has identified certain spending responses to predictable changes in income (Zeldes, 1989; Parker, 1999; Souleles, 1999; Agarwal et al., 2007; Di Maggio et al., 2017; Baker and Yannelis, 2017). See Jørring (2020) for a recent review.

# D Data Collection

## D.I Description of College Commencement Programs, Yearbooks, and Degree Conferral Rosters

I hand-collect and digitize university commencement programs, yearbooks, and degree conferral rosters (which I collectively refer to as "commencement programs"). Some universities have made their commencement programs available online, at other universities I have received PDF versions from their registrar's offices, and, finally, at a few universities you can only access historical commencement programs in-person in their libraries, and in these cases, I have scanned the physical commencement programs. Once I have a PDF version for each commencement program, the digitization process involves two steps. First, I use an optical character recognition (OCR) software called ABBYY FineReader which reads and converts PDF files into spreadsheets. Then, in the second step, each commencement program is manually read and cleaned to remove any mistakes by the OCR software.

The commencement programs contains five sources of individual-level information for cohorts graduating between 1996 and 2022: (1) full name which includes first name, middle name, and last name, (2) realized major choices, (3) realized minor choices, (4) cities, states, and countries of origin, (5) awards that include phi beta kappa and cum laude awards. If the commencement programs did not contain some of the information above, I supplement by using archived yearbooks that contain overlapping information. This is also used as a method of verifying graduation dates and majors for students in the sample.

## D.II Description of LinkedIn, Doximity, and CV Data

I collect publically available CVs from LinkedIn, Doximity, and professional websites.

LinkedIn is a social media platform used primarily for professional networking. It allows job seekers to post their CVs and employers to post jobs. The platform is widely used with over 740 million members worldwide in 2021. In the United States, there are over 170 million users (Osman, 2021). It is popular among professionals with 50% of the users holding a college degree (Osman, 2021).

Doximity is the leading professional network in medicine. Doximity members include over 2 million healthcare professionals within the United States: 80% of Physicians, 50% of Nurse Practitioners and Physician Assistants, and 90% of graduating Medical Students.

Users create online public profiles that contain CV information. This contains information on all previous work experience, including job title, employer, location, job descriptions, start and end dates. Individuals can also post education and training, skills, and a personal photo. In addition, individuals can connect with other users on the platform in an online social network. I collected this data twice, first in June of 2018 and second in June of 2022. As a result, the CV information is current up to 2022.

The data is cleaned by parsing the information on the CV and reshaping the data such that a quarterly panel is created based on the start and end dates of employment. I expand the data to include observations for when someone is in nonemployment, i.e. periods where there is a gap on the CV between the start and end dates of two consecutive positions. I then collapse the dataset to the yearly level. For each year, individuals are assigned to the position in which they have spent the most time during that year. If there are ties, the position with the longer tenure takes precedence.

## D.III United States Postal Service Records

I collect a history of postal addresses for each student in my sample. I use two prominent data aggregators called Intelius and BeenVerified in order to do so.

Intelius, Inc. is a public records business that provides information services, including people and property search, background checks and reverse phone lookup. Users also have the ability to perform reverse address lookups to find people using Intelius' services and an address.

BeenVerified is a background check company that provides background checks and people search services. BeenVerified uses traditional background methods in addition to Web 2.0 and social networking to return results to a requesting user.Users enter the name and/or email address of the person they are requesting information on and are given information from public records and other privately licensed databases of public record information.

The search process is as follows. Each student's full name and approximate age, based on cohort, is fed through a search algorithm that returns one or many results. Each search result is then manually verified by a research assistant using a unique sequence of locations coming from hometown, university locations, and locations from LinkedIn. This process yields a unique match.

These data providers also use an internal algorithm to assess possible relatives. This is a based on address overlaps and last names. This allows me to construct a household of possible relatives. I cross-reference both BeenVerified and Intelius to verify a household.

## D.IV Merging Process

In this Subsection, I describe how I merge the commencement programs with the USPS data and the credit bureau records. As an intermediate step, the supplemental resume data described above is used in the merging process to increase the quality of the match.

Specifically, I first merge the commencement program data with the resumes. Here, for a pair of records to be considered a match, I require the name, university, and class year to be identical in both. Since women frequently change their last names upon marriage, in women's records, I require only the first names to match, and I verify possible name changes using USPS data. I verify all matches based on the student's field of study.

Next, I merge the commencement records with USPS records, as follows. From each student's full name (first, middle, and last) and approximate age (based on year of graduation), I identify possible matches in the USPS database. I then use the student's hometown (as given in the commencement program) and other location data from resumes to confirm a match to a specific USPS records. After obtaining the student's USPS record I create a household for them by identifying all individuals with the same last name who lived at the same postal address as the student at the same time. The time frame I consider is the entire period before the student begins college. This process is intended to capture the student's parents and likely siblings. I characterize as parents any household members who are at least 15 years older than the student.

Lastly, I provide Experian with the postal address histories obtained from the USPS data, which it uses to match a credit bureau record to each student and each parent.

# E Data Matching

## E.I Mapping Onet Codes to Occupations in CVs

This Section describes the data matching process between ONET codes and occupations in the resume data.

### E.I.1 Pre-processing

Given job title strings extracted from parsed LinkedIn webpages data, the text strings are preprocessed (1) with hard conditional rules; (2) with a constructed dictionary.

- *Hard processing*. First, the hard conditional rules rid the text strings off punctuation and delimiters such as " - " and "/", as these tokens pose no informational increment for the matching process; furthermore, for some scrapped strings in non-English languages, such as French, translation is imposed on these strings.

- *Dictionary-based processing*. Secondly, the dictionary is composed of two parts, and updated dynamically. The first part of the dictionary outlines junk tokens that conveys no information in text strings *e.g.* "senior", "sr.", etc. The second part of the dictionary characterizes tokens that tend to be ambiguous *e.g.* "advocate", "associate", etc. For the junk tokens, they are removed from the text string, and for the text strings containing indeterminate tokens, they are skipped and stored as residual samples for next step processing.

- *Dictionary update*. For each match sample that's generated *and* labeled to be a good or bad match, the sample is stored in the dictionary-generating dataset (The labeling process is detailed in later sections). Consequently, for each pre-processing call, a updated dictionary is generated by scanning all matches labeled a bad match, and identify tokens with aforementioned undesirable characteristics. This step is done by tokenizing all text strings labeled bad, computing token frequencies, and adding tokens with highest frequencies in the bad matches to the updated dictionary.

### E.I.2 Matching Algorithm

The matching process uses the modified ONET API. After pre-processing, the processed job title text strings are batched and uploaded to ONET database for the best match as determined by ONET Web Service search. However, as the returned best search are sometimes inadequate, an checking process is devised to determine whether the returned search is an adequate match for the true information as conveyed by the LinkedIn text string.

### E.I.3 Robot Checker

To train a robot checker for purpose stated above, a sample of approximately 8700 samples are selected for manual labeling with match adequacy determined by human eye to serve as training set.

- *Feature generation*. To generate features for model training, we first pull alternative job titles of the best-match ONET title from the API *e.g.* job function "Lawyers" is also known as 'Attorney', 'Attorney at Law', 'Attorney General', etc.

  $$\text{“Lawyers”} \xrightarrow{\text{is also known as}} [\text{“Attorney”}, \text{“Attorney at Law”}, \text{“Attorney General”}]$$

  according to ONET API. The intuition behind this step is that, should the LinkedIn title be a right match, then the title should be able to at some level fuzzy-match with some of these alternative titles as well. Using different fuzzy-matching functions, such as regular fuzzy ratio (*i.e.* Levenshtein distance), partial fuzzy ratio, token set ratio, token sort ratio, etc., the input argument of (LinkdeIn title, alternative title list) produces a probability vector scaled by 100 *e.g.*

  $$\texttt{fuzzy\_func(``attorney'', titleList )} \xrightarrow{outputs} [88, 61, 58, 67, 40, 45, 17, 29]$$

  where 0 is the lowest possible fuzzy score and 100 is the highest. Finally, on different score vectors generated by the fuzzy function, we apply statistical aggregate functions, such as standard deviation, mean and maximum. This process results in a total number of (Num of func)$\times$(Num of Stats) different features

  Note to author: Othre features such as ONET code first two digits to be added

- *Model: Tree-based classifier*. For the purpose of identifying whether the match by ONET is adequate or not, we employ a tree-based classifier, such as Random Forest or XGBoost library (simple decision tree is ignored as random forest ensemble tends to vastly outperform decision tree). The intuition behind this decision is easy to understand: The magnitude (high or low) of the generated statistical aggregates from different fuzzy functions must convey some information regarding the adequacy of the match, even if the aggregate cannot be easily interpreted by a human. Hence, we use decision trees to interpret these indicators, and let the trees determine pertinent threshold values for the model.

## E.II Mapping CIP codes to Majors

After collecting commencement programs with student majors, we had to standardize them. We did this by assigning Classification of Instruction Program codes (CIP) to each major from the National Center of Education Statistics in a master list. The master list was used in a mapping process to match majors and CIP codes, creating a standardized list of majors per institution. The mapping process would have holes when schools had niche or interdisciplinary majors. In this circumstance, we would manually cross-reference the major's description to the National Center for Education Statistics to best identify a CIP code. For example, Vanderbilt had a major called "INTERDISCIPLINARY: CORPORATE LEADERSHIP AND FINANCE." Looking further into Vanderbilt's course offerings, we closely aligned each interdisciplinary major to something that fell under the CIP code(s). Sometimes to best capture the interdisciplinary major, we would include two CIP codes, like business management and finance for the example above.

## E.III Matching between Commencement Programs and CV Profile

Starting from the commencement programs for classes 2004 to 2022, I collect publicly available CVs from Linkedin, Doximity, and personal pages. I matched students to their online profiles based on the following variables:

- Full name
- Undergraduate school name
- Year of graduation

I require names, undergraduate school and class year to match perfectly to be considered a match. For women, I require only the first names to match and we conduct an online verification for those that may have changed their last names due to marriage (e.g., a wedding registry webpage). This verification process also includes alternative names listed in the USPS data.

## E.IV Linking between Credit Bureau data and Hand-collected data

This project was reviewed for IRB and determined to fall under a Category 4 Exemption. This project falls under a research exemption for purposes of FERPA.

The linking procedure between the hand-collected data that I provided and Experian Credit Bureau followed the below procedure:

- First, an independent third party created a random, study-specific identifier. They then provided Experian with personally identifiable information (PII) necessary for linking and the random identifier.
- Experian queried their data and created an anonymized, individual-level dataset that contained the random identifier and credit bureau records.
- The anonymized dataset was provided to the researcher. Data was stored in encrypted format. At no point did the researcher have access to PII, isolating research process from PII. Data were only used for approved research purposes.

## E.V Assigning Gender and Ethnicity

I obtain gender and ethnicity through algorithmic assignments.

For gender, I use "genderize" which is an R function. This function predicts the gender of a first name given a year or range of years in which the person was born. The genderize functions works by the "ssa" method looks up names based from the U.S. Social Security Administration baby name data. The "ipums" method looks up names from the U.S. Census data in the Integrated Public Use Microdata Series. The "napp" method uses census microdata from Canada, Great Britain, Denmark, Iceland, Norway, and Sweden from 1801 to 1910 created by the North Atlantic Population Project. The "kantrowitz" method uses the Kantrowitz corpus of male and female names.

For ethnicity, I use "predictrace," which is an R function. The goal of predictrace is to predict the race of a surname or first name and the gender of a first name. This package uses U.S. Census data which says how many people of each race has a certain surname. For first name data, this package uses data from Tzioumis (2018). From this we can predict which race is mostly likely to have that surname or first name. The possible races are American Indian, Asian, Black, Hispanic, White, or two or more races.

## E.VI Set of High-Earning and Low-Earning Majors

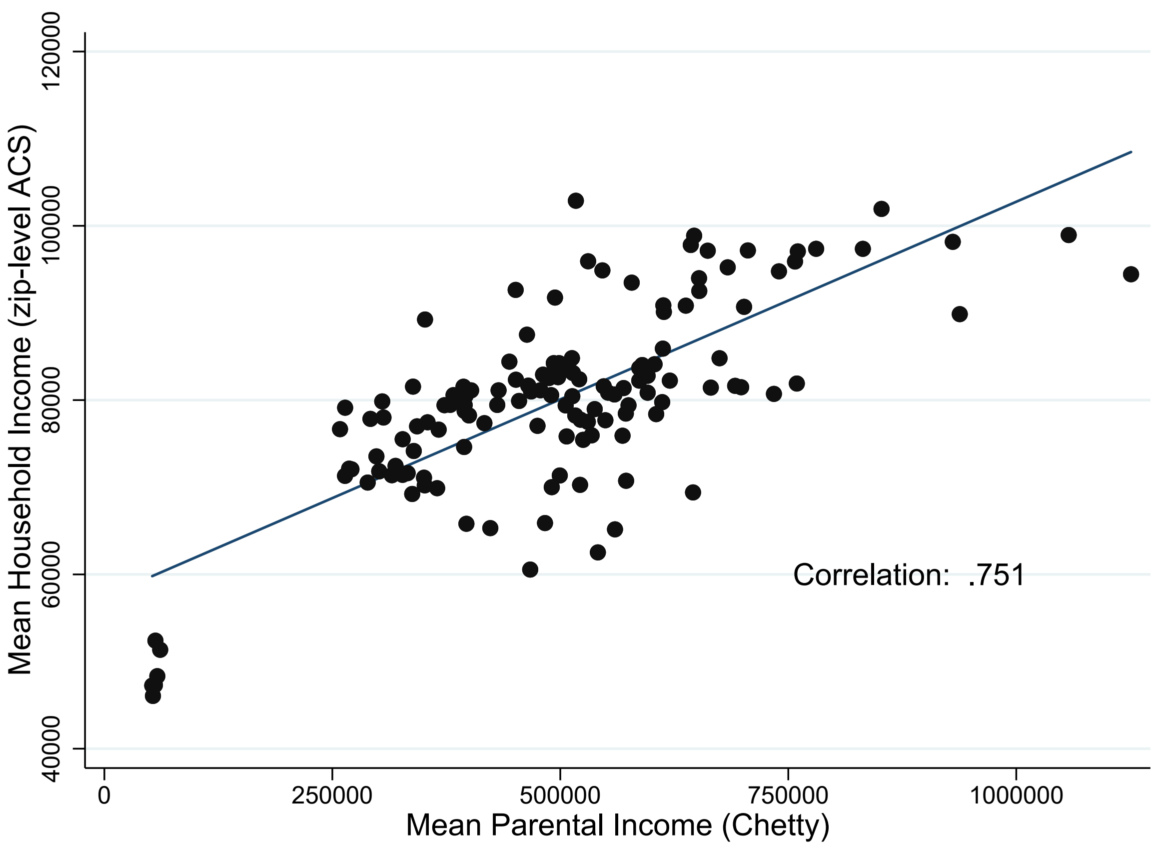
The 25 most common majors are in the UNLP sample are: Anthropology, Arts, Biology, Business, Chemistry, Communication, Computer Science, Economics, Education, Engineering, English, Finance, Foreign languages, History, Liberal Arts and Area Studies, Mathematics, Nursing and Health Professions, Philosophy and Religious Studies, Physical Sciences and other (including Astronomy, Astrophysics, Atmospheric Sciences, Meteorology, Geological, Earth Sciences, Geosciences, and Materials Science), Physics, Political Science, Psychology, Public Administration, Social Sciences and other (including Criminology, Demography, Population Studies, Geography, Cartography, and Urban Studies/Affairs), and Sociology. The 12 majors defined as "high-earning majors," are Biology, Business, Chemistry, Computer Science, Economics, Engineering, Finance, Mathematics, Physical Sciences and other (including Astronomy, Astrophysics, Atmospheric Sciences, Meteorology, Geological, Earth Sciences, Geosciences, and Materials Science), Physics, Political Science, and Public Administration. The 13 "low-earning majors" are Anthropology, Arts, Communication, Education, English, Foreign languages, History, Liberal Arts and Area Studies, Nursing and Health Professions, Philosophy and Religious Studies, Psychology, Social Sciences other (including Criminology, Demography, Population Studies, Geography, Cartography, and Urban Studies/Affairs), and Sociology.

# F Data Validation

## F.I Address Data

To validate the address data that I collect from USPS records, I compare the mean parental income by school-cohort as reported in the replication package of Chetty et al. (2020) to the school-cohorts in my sample[42]. In Figure A19, I plot the raw data between the two datasets and find that mean household income of zipcode at the school-cohort level has a positive relationship with mean parental income from Chetty et al. (2020), which is measured using IRS administrative records. Specifically, I observe that there is a 75% correlation between the two datasets.

FIGURE A19: Comparison with Opportunity Insights Parental Income




*Note:* This figure plots mean parental income at the school-cohort level, constructed using IRS data from Chetty et al. (2020) and compares it to mean zip code level household income at the school-cohort. The data source is the ACS and Table 3 from Chetty et al. (2020).

[42]Chetty et al. (2020) only reports data up until 2013.

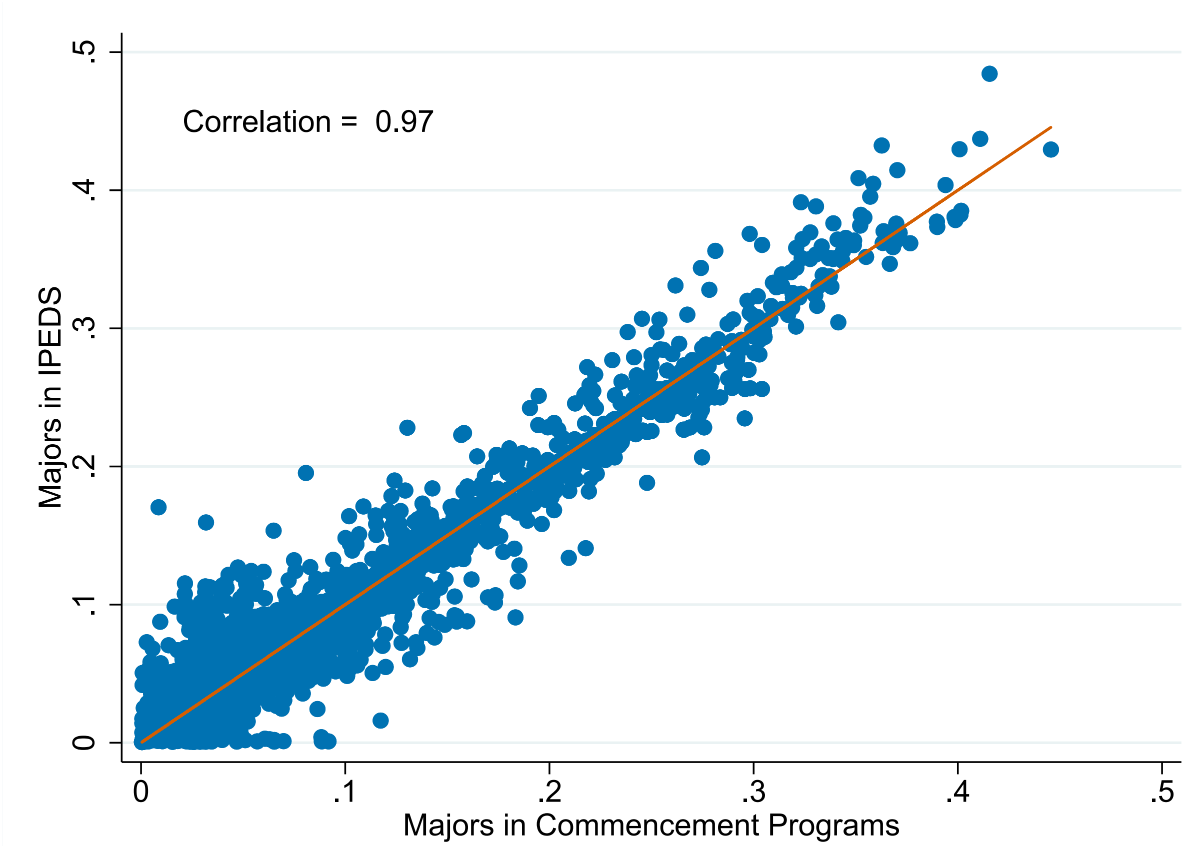
## F.II Commencement programs

To validate the hand-collected commencement programs, I compare the classification of college majors with the share of college majors reported in the IPEDS data.

In figures A20 and A21, I plot the correlation between IPEDS reported share of degrees conferred at the school-cohort-major level compared to the share of degrees conferred at the school-cohort-major level from the commencement program data. Figure A20 reports a correlation of 97% at the two-digit CIP code level and figure A21 reports a correlation of 95% at the four-digit CIP code level. This exercise shows that the classification of major text to CIP code is robust.

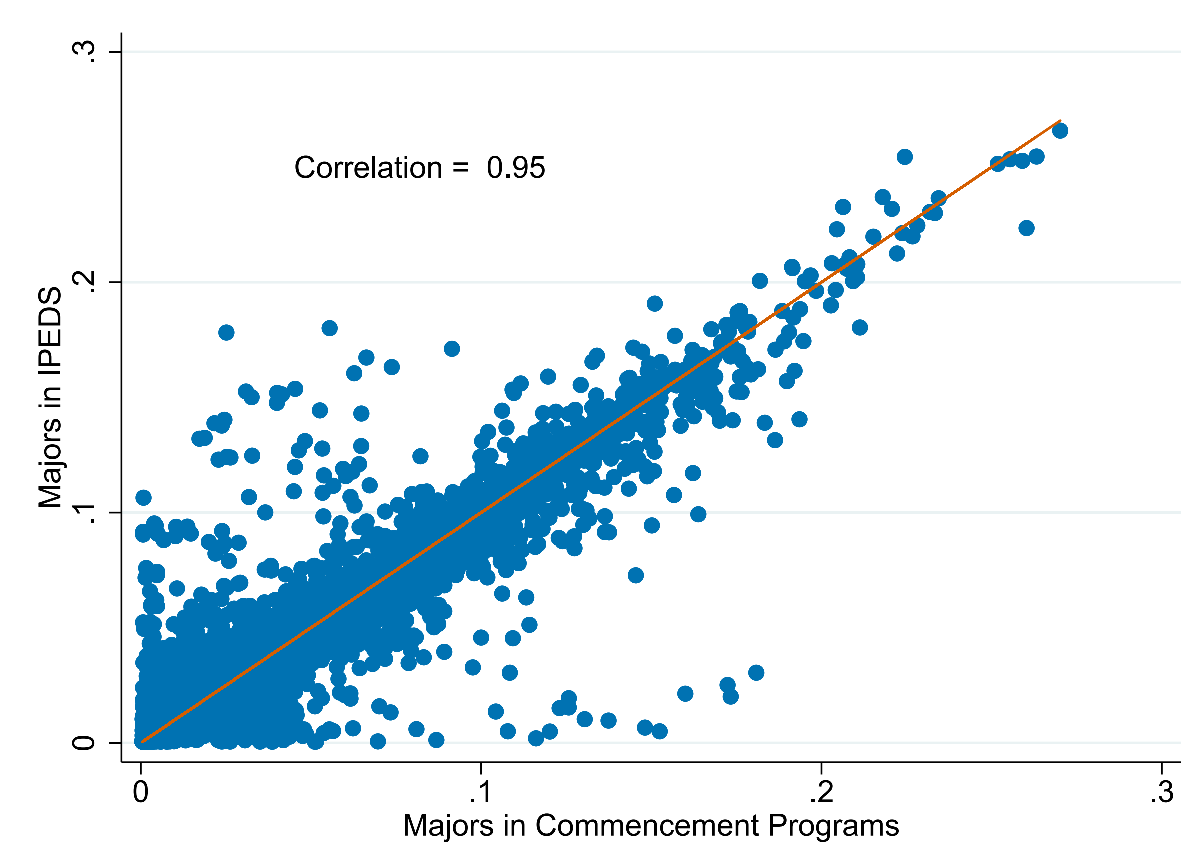
In figures A22 A23, and A24, I plot both the difference-in-means between the hand-collected commencement program data and IPEDS relative to the policy implementation. In A22, I plot the difference-in-means across all majors, in four different specifications, (1) Standard TWFE, (2) TWFE with control schools, (3) Sun & Abraham, and (4) Callaway & Sant'Anna. In order to do so, for ever major-school-cohort, I measure the share of students in IPEDS and in the commencement programs. For example, there are 8% of students who graduated with a history major in 2009. Then, I take the difference between the IPEDS share and the commencement program share, such that for every, school-major-year, I have a difference. Finally, I estimate an event study regression on these difference-in-means. Figure A22, shows that there is no discontinuous effect of the policy on the categorization of majors. In Figure A23, I plot the difference in means across broad categories of majors, Arts, Business, Education, Health Professions, Humanities, STEM, and Social Sciences. This figure shows that there are no differences in categorizations across majors. In Figure A24, I plot the difference in means across majors relative to the policy implementation. I find no discontinuous effect of the policy implementation. Specifically, there is not an increase in discrepancy between IPEDS and the commencement programs right around UNLP. In other words, the results cannot be attributed to measurement error that is correlated with UNLP.

FIGURE A20: Comparison with IPEDS - CIP 2 digit



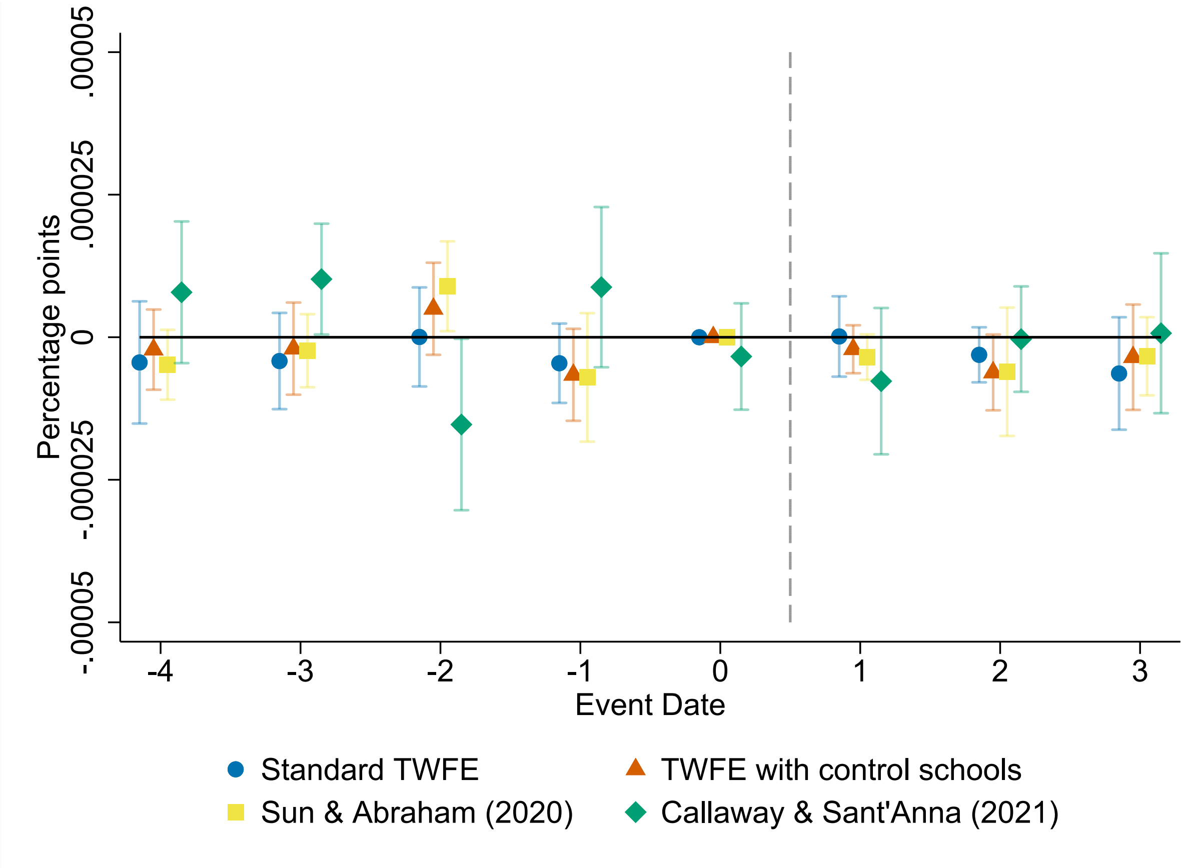

*Note:* This figure plots the fraction of 2-digit CIP majors at the school-cohort level from IPEDS against the classification of the hand-collected data. The data source is IPEDS and the hand-collected commencement program data.

FIGURE A21: Comparison with IPEDS - CIP 4 digit



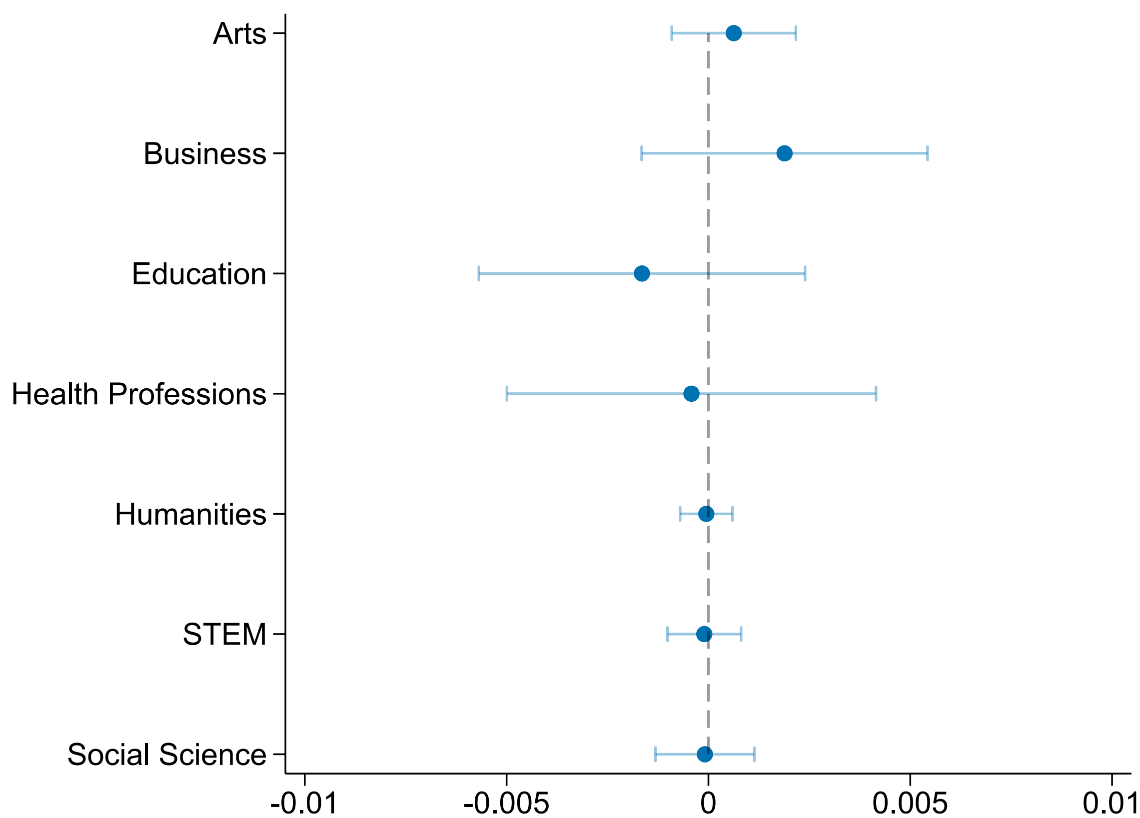

*Note:* This figure plots the fraction of 4-digit CIP majors at the school-cohort level from IPEDS against the classification of the hand-collected data. The data source is IPEDS and the hand-collected commencement program data.

FIGURE A22: Event Study - Difference in Means



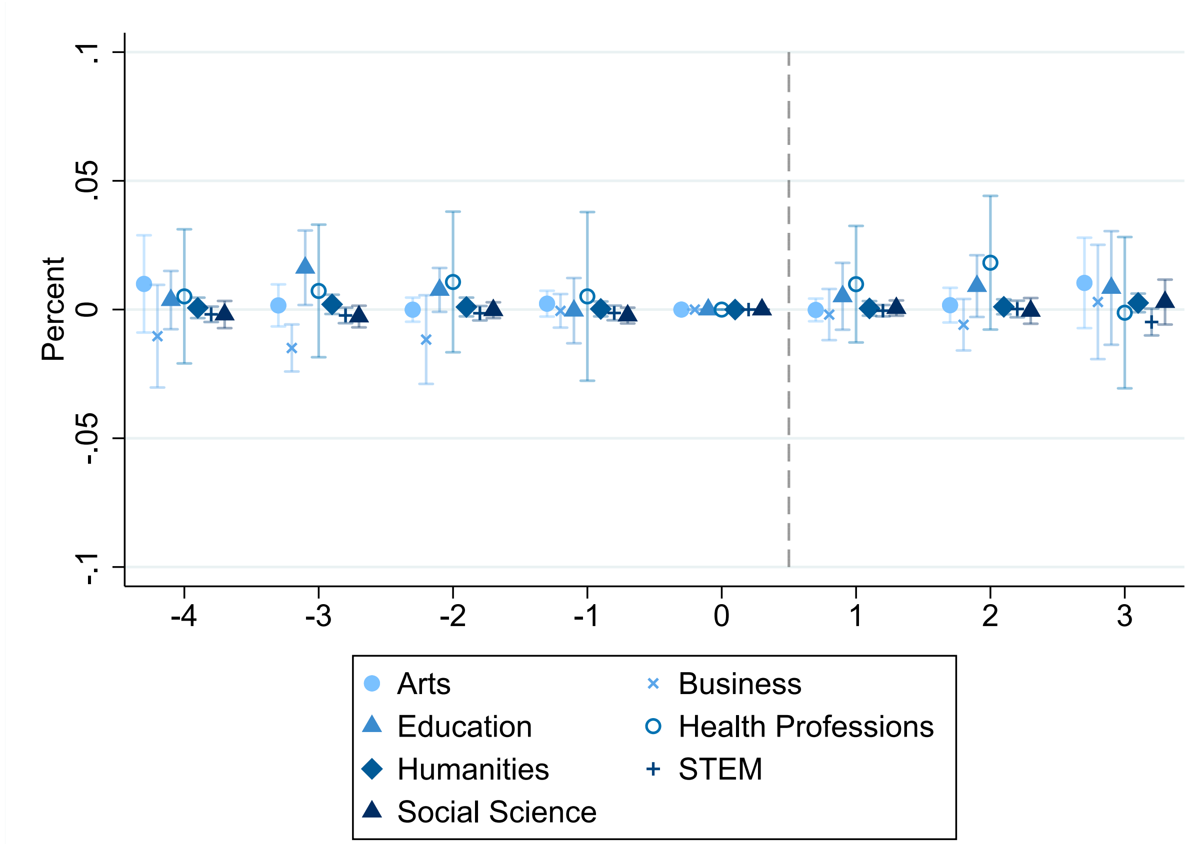

*Note:* This figure plots the difference-in-means of 2-digit majors relative to the policy implementation. The data source is IPEDS and the hand-collected commencement program data.

FIGURE A23: Difference in Means by Large Classification of Majors




*Note:* This figure plots the difference-in-means of the broad categorizations of majors. The data source is IPEDS and the hand-collected commencement program data.

FIGURE A24: Event Study - Difference in Means by Broad Major Categorizations




*Note:* This figure plots the difference-in-means of 2-digit majors relative to the policy implementation across broad categorization of majors. The data source is IPEDS and the hand-collected commencement program data.

# G Discussion of IV assumptions

In this section, I discuss the validity of the instrumental variables (IV) strategy. Specifically, I discuss and test the four central assumptions that are required to interpret the IV estimates as a causal effect of student debt.

## G.I Relevance

First, I discuss the strength of the first stage. I use both publicly available data aggregated at the school-cohort level as well as my main sample of student-level micro data to test the relevance assumption.

First, I test the assumption using publicly available data from IPEDS. Table A16 reports the DID estimates, where the dependent variable is the fraction of undergraduates with student debt, and compares UNLPs to other financial aid programs, including income-specific NLPs, loan caps, parental contribution elimination, and tuition waivers. (For example, some colleges implement NLPs for families whose incomes fall below a specific level.) I find that UNLPs meaningfully decrease the percentage of students taking loans; specifically, they lower it by 12.6 percentage points.

Next, to understand the dynamic effects, Figure A6 reports the results from a standard event-study regression. The plot shows the coefficients on the year relative to implementation. We see a sharp drop in the fraction of students taking out loans that precisely coincides with the timing of the policy. To assess the robustness of this result, in Figure A6, I plot the regression coefficients and 95% confidence intervals from three different regressions of the percentage of students taking a student loan on year dummies relative to the implementation of a UNLP. A standard two-way fixed effect (TWFE) model is reported in blue, a TWFE model with non-implementing top-50 schools as a control group is reported in red, and a bias-corrected model allowing for treatment heterogeneity across cohorts (following Sun and Abraham (2021)) is reported in green. Across all three specifications, in the year when a UNLP is implemented, there is a significant drop in the share of students who take student loans.

Second, I test the assumption using the main sample with micro-data. Figure 2 shows the coefficients from an event-study equation where the dependent variable is the student loan balance in the year of graduation.[43] We see a sharp discontinues drop in the amount of student loans that students have that precisely coincides with the timing of the policy. As above, to assess the robustness of this result, I plot the regression results

[43] The credit bureau data offers a yearly snapshot on December 31. Therefor, the student loan balance variable captures the amount of student loan debt on December 31 on the year that the student graduated.

from both a standard TWFE, a TWFE with control groups, and a bias-corrected model allowing for treatment heterogeneity across cohorts. The results are similar across all three models. Table 2 summarizes the findings and reports the average coefficients. Following the implementation of the UNLP, treated students, on average, have $7,000 less in student debt.

Taken together, these results confirm that following the implementation of UNLPs, the use of student loans fell dramatically. In other words, there is a strong first stage.

## G.II Independence

It is important to consider whether the implementation of a UNLP is an *independent* instrument for student debt: Does the first stage represent a causal estimate of the effect of the policy? There are two central arguments in favor. First, the staggered implementation of the policies makes it unlikely that contemporaneous macroeconomic shocks are driving the effects; additionally, with the inclusion of year fixed effects, I study the variation across schools only within each year. Second, as I include school fixed effects, I study the variation across cohorts only within the same school. Thus, any correlation between "selective" schools and low student debt is eliminated.

Additionally, it is worth noting that the assumption for an independent instrument are similar to those discussed above (the assumptions required to interpret the reduced form evidence as causal evidence.) Specifically, I rely on two key identifying assumptions: no anticipation of the treatment and parallel trends (Sun and Abraham (2021); Borusyak et al. (2024)). Under the no anticipatory effects assumption, I assume that units do not change their behavior in anticipation of the treatment.[44] The second identifying assumption is the parallel trends assumption, in which we assume that absent the reform, the difference in potential outcomes would be the same across all units and all periods conditional on the set of controls, unit and time fixed effects.[45]

The main threat to identification is that unobserved changes in the composition of students attending the UNLP school can explain both the timing of the UNLPs and changes in major choice. For example, with the implementation of the policy, the school may enroll students that are more interested in studying specific types of majors. In order to mitigate this issue, I focus on students that were enrolled at the time of the policy implementation. Specifically, I focus only on students that were sophomores, juniors, and seniors at the time the policy is implemented.

[44] Formally, in the notation of potential outcomes, this is equivalent to $E[Y_t - Y_t(0)|X] = 0$ for all t prior to the policy, conditional on covariates X.

[45] Formally, we assume that $E[Y_{it}(0) - Y_{it}0(0)|X]$ has to be the same across units i for all periods t, t0.

As highlighted by the recent econometric literature, the estimates may also be biased if there is heterogeneity in treatment effects between groups of units treated at different times. This bias can occur even if both the no-anticipatory effects and parallel trends assumptions hold (de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Goodman-Bacon, 2021; Borusyak et al., 2024; Callaway and Sant'Anna, 2021). Given the potential for biased estimates, I employ both the Sun and Abraham (2021) and Callaway and Sant'Anna (2021) correction methodologies, which adjusts for potential treatment heterogeneity across cohorts. I find that the results are consistent across all four models (TWFE, TWFE with control schools, Sun and Abraham (2021) and Callaway and Sant'Anna (2021)).

Importantly for the independence assumption for the two-stage least squares, I do not find any difference between the TWFE event-study results and the bias-corrected results that correct for potential treatment heterogeneity across cohorts.

## G.III Exclusion

The exclusion restriction posits that UNLPs affect outcomes only through their effects on student debt. This would not be the case if, for example, the student body changed as it became more advantageous to apply to a school after the implementation of an UNLP. I address this issue in two ways. First, I study only cohorts who were already enrolled by the time of the UNLP implementation. That is, I study only the three cohorts following the implementation of the UNLP. Second, when analyzing student characteristics, I do not find any discontinuous change in student characteristics around the time of implementation of the UNLPs.

Figure A7 show that there is no discontinuous change in student characteristics around the event date. Panel (A) show average SAT scores for students for each year relative to the implementation of an NLP. There is a slight upward trend for both the 25th and 75th percentiles in verbal and math, but importantly, no discontinuous change around the event date. Panel B show the average fraction of students who are White, Black, Hispanic, and Asian respectively. We see slight trends over time. For example, the fraction of Hispanic students is trending upwards. Importantly, we see no discontinuous change around the event date. Figure A8 plots the TWFE estimates of race and sat scores on UNLP of those that were enrolled at the time of the policy implementation. I see no statistically significant impact on any of these outcome variables.

## G.IV Monotonicity

The monotonicity assumption posits that there are no *defiers* of the treatment. That is, no students take on more student debt as a result of the UNLP. For example, the monotonicity assumption would not hold if grants for low-income students were crowded out with the implementation of universal loan elimination programs, which would leave low-income students worse off and presumably with more student debt. Given the results on the extensive margin reported in table A16, this would be a particularly important assumption to test, if I was using loan caps as an instrument for student debt (instead of UNLPs).

I test the monotonicity assumption using the credit bureau data in the following way. First, I tabulate students by family income quartile.[46] For each quartile I calculate (a) the average amount of student debt, (b) the fraction of students that have any student debt, and (c) the fraction of students that have more than \$10,000 of student debt. Next, I find that neither (a), (b), or (c) weakly decrease after the policy implementation within each quartile.

Finally, I refer to Krishnan and Wang (2018), who have also studied the monotonicity assumption. They show that there is no crowding out of other financial aid with the introduction of NLPs. They find that when schools implemented NLPs and increased financial aid grants, they did not simultaneously decrease other forms of financial aid.

[46] I have done this both using the imputed value of household income from W2 data provided by Experian, and by using ZIP codes as a proxy for income.

# H Comparison with Other Financial Aid Programs

In this Section, I compare UNLPs with other financial aid programs. Specifically, I compare UNLPs with no-loan programs that were targeted to specific income groups (targeted NLPs or TNLPs).

Since 1998, at least 85 universities have implemented new financial aid policies. Figure A3 displays the implementation of financial aid policies over time. Panel A displays the implementation of all five financial aid policies (including NLPs, student loan caps, parental contribution eliminations, tuition waivers, and Pell Grant matches), and Panel B displays the implementation of just NLPs. The black bars denote policies targeted specifically at low-income students, and the grey bars denote policies available for all students. The years refer to starting year of the policy. For example, 22 universities implemented a NLP for low-income students matriculating in 2008 (and graduating in 2012). Notably, three universities reversed their policies: Dartmouth College and Williams College in 2011 and Claremont McKenna College in 2014.

Tables A16 and A17 compares the implementation of different types of financial aid policies on student loans: (1) NLPs, (2) Loan caps, (3) No parental contribution policies, (4) Elimination of tuition. Comparing policies I find that NLPs have the largest impact on loan reduction compared to loan caps, parental contribution, or no tuition policies. NLPs could be implemented in two ways, either targeted towards a family income threshold or for all students that demonstrate financial need. In Table A17, I compare schools that implemented a targeted policy to those that implemented for all students. I find on average, there is no noticeable change on loans on average at schools that implemented a targeted program. This can be seen in Column (5).

# I Constructing ACS Earnings Trajectories

## I.I Variable Construction

In this subsection, I detail how I calculate the expected earnings trajectory for each major.

To calculate the expected earnings trajectory, I use data on earnings by occupation and major from the 2009–2019 ACS, extracted from the IPUMS 1% samples (Ruggles et al., 2017; Deming and Noray, 2018). Importantly for my study, the ACS data includes each individual's undergraduate field of study, occupation, and annual wage. I classify fields of study according to the Classification of Instructional Programs (CIP) system developed by the National Center for Education Statistics (NCES), and occupations according to the

SOC system, using the 2010 Census Bureau definitions of occupations. I include individuals who have a minimum of a bachelor's degree and are between the ages of 21 and 60, which yields a sample with 5,628,805 observations.

To quantify, in dollars, the effects of UNLPs on major choice, I ask how earnings differ across individuals with different college majors. The main variable of interest is total pre-tax wage and salary income—that is, money received as an employee—for the previous year (Ruggles et al., 2017).[47] To purge the data of variation unrelated to field of study, I regress the annual wage on age, age-squared, race, ethnicity, sex, and survey-year fixed effects:[48]

$$log(1 + wage_{it}) = \beta_1 Age_i + \beta_2 Age_i^2 + \Gamma X_i + \gamma_t + \epsilon_{it}. \tag{12}$$

Next, I calculate the difference between the predicted value (transformed to dollars) and the annual wage in dollars:

$$\varepsilon_{it} = wage_{it} - e^{\widehat{log(1+wage_{it})}} - 1. \tag{13}$$

Once I have the dollar residuals, I calculate the mean, $\mu_m$, and standard deviation, $\sigma_m$, of the residuals for each major, $m$, separately:

$$\mu_m = \frac{1}{N_m} \sum_{i=1}^{N_m} \varepsilon_{it} \mathbb{I}_{\text{major}=m}, \tag{14}$$

$$\sigma_m = \sqrt{\frac{1}{N_m} \sum_{i=1}^{N_m} (\varepsilon_{it} - \mu_m)^2 \mathbb{I}_{\text{major}=m}}. \tag{15}$$

Table A18 reports the results for each major. I find that engineering, mathematics, business, and physical sciences have high mean residuals and high standard deviation of residuals, while fine arts, English, and education have low mean residuals and low standard deviation of residuals. Column 3 reports the coefficient of variation of the residuals, which is the standard deviation scaled by the mean.

As well as differing in mean and standard deviation, earnings across majors also differ

[47] The average pre-tax wage and salary income in the sample is $61,190, the 10th percentile has zero wage and salary income, and the 90th percentile is $126,000.

[48] Table A19 in the online appendix reports the regression coefficients. To avoid having to drop zeros, I add one dollar to the annual wage before taking the natural logarithm.

in their life-cycle trajectories. For example, if a student majors in education or business, the earnings right out of college is high, but the wage growth is relatively low. On the other hand, a student who majors in biology and goes to medical school has a low wage initially after college but a much higher wage later on.

I first regress the annual wage and age on race, ethnicity, gender, and survey-year fixed effects, then generate the residuals from those regressions and add the sample mean of each variable back to its residuals.

To capture the trade-off between initial wage and later wage, I run the following regression:

$$log(1 + wage_{imt}) = \sum_m \beta_1^m \mathbb{I}_{\text{major}=m} Age_i + \sum_m \beta_2^m \mathbb{I}_{\text{major}=m} Age_i^2 + \Gamma X_i + \gamma_t + \varepsilon_{it}. \tag{16}$$

From the above regression, I recover the major-specific coefficients on age and age-squared. I then calculate the slope of wage with respect to age, evaluated at age 22. Table A18, Column 4, reports these slopes for each major. The slopes are positively correlated with the mean of the residuals. In other words, majors with high lifetime earnings, such as biology and engineering, also have steep earnings trajectories immediately after college.

## I.II Assumptions for ACS Earnings Trajectories

In order to project ACS major-earnings trajectories onto my sample of students, two assumptions need to be valid. First the ordinal rankings of majors need to remain constant over time and second is that the earnings of student's in the UNLP sample need to evolve similarly to the earnings of those in the ACS.

In Figure A10, I plot initial wages (the average of wages between ages 21-24 in 2019 dollars) and mid-career wages (age 35 in 2019 dollars) across six popular majors, Biology, Economics, Political Science, History, Psychology, and Education. In both panels, we see that wages across majors do not change rank order.

The second assumption is that wages of those in the sample are similar to those that are in the ACS. While this sample focuses on those that attend elite colleges and wages will presumably be different, the question is are majors that have high mean wages the same majors that are high mean in the ACS and do these trajectories evolve similarly. I validate these assumptions but assessing the correlation between major-specific wages in the ACS against the model-implied earnings from the credit bureau and wages from College Scorecard.

In Figure A11, I plot the correlation between wages in the sample and wages from the

ACS. In Figure A11 Panel a, I plot the ACS initial wages by majors (ages 21-24) against College Scorecard wages by major (within the first three years of graduating from college). In Panel B, I plot the ACS initial wages by major (ages 21-24) against Experian initial wages by major (ages 21-24). In Panel C, I plot the ACS year 10 wages by major against Experian 10 year wages by major. In Panel's A, I find that the correlation between the ACS initial wages and the initial wages from College Scorecard of the schools have a correlation of 89%. Similarly, in Panel B, I find that the correlation between the ACS Initial Wages and the Experian initial model-implied wages is 82% respectively. In Panel C, I find that the ACS year 10 wages have a 67% correlation with the year 10 Experian model-implied wages. While there is a high correlation, between the different data sets, it is important to note that the levels are different. Specifically, the wages in the ACS are about 30% lower than that of Colleges Scorecard or Experian's Model Implied wages. This means that this quantification will be a lower bound for where we'd expect the true earnings changes.

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