ESSAYS ON THE ECONOMICS OF HIGHER EDUCATION

by

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To my PhD cohort.

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This dissertation includes three essays on the economics of higher education. In the first chapter, I study the effects of college transfer. Over one-third of college students in the United States transfer between institutions, yet little is known about how transferring affect students' educational and labor market outcomes. Using administrative data from Texas and a regression discontinuity design, I study the effects of a student's transferring to a four-year college from either a two-year or four-year college. To do so, I use applications and admissions data to uncover the unpublished GPA cutoffs that each institution uses in its transfer student admissions and then use these cutoffs as an instrument for transfer. In contrast to past work focused on first-time-in-college students, I find negative earnings returns for academically marginal students who transfer from two-year colleges to four-year colleges or from less-resourced four-year colleges to flagship colleges. The mechanisms include transfer students' substituting out of high-paying majors into lower-paying majors, reduced employment and labor market experience, and potential loss of support networks.

In the second chapter, joint with Minseon Park, I study how colleges' "sticker price" and institutional financial aid change during and after tuition caps and freezes using a modified event study design. While tuition regulations lower sticker prices, colleges recoup losses by lowering financial aid or rapidly increasing tuition after regulations end. At four-year colleges, regulations lower sticker price by 6.3 percentage points while simultaneously reducing aid by nearly twice as much (11.3 percentage points). At two-year colleges, while regulations lower tuition by 9.3 percentage points, the effect disappears within three years of the end of the regulation. Changes in net tuition vary widely; focusing on four-year colleges, while some students receive discounts up to 5.9 percentage points, others pay 3.8 percentage points more than they would have without these regulations. Students who receive financial aid, enter college right after the regulation is lifted, or attend colleges that are more dependent on tuition benefit less.

In the third chapter, joint with Garrett Anstreicher, I study how the scarring effects of graduating from college into a recession vary with college quality. Graduating from college into a recession is associated with earnings losses, but less is known about how these effects vary across colleges. Using restricted-use data from the National Survey of College Graduates, we study how the effects of graduating into worse economic conditions vary over college quality in the context of the Great Recession. We find that earnings losses are concentrated among graduates from relatively high-quality colleges. Key mechanisms include substitution out of the labor force and into graduate school, decreased graduate degree completion, and differences in the economic stability of fields of study between graduates of high- and low-quality colleges.

1.1 Introduction

Higher education is an important driver of social mobility in the United States. Prior work has shown that higher education leads to meaningful earnings gains, especially at well-resourced colleges.[1] Additionally, many studies find that the positive effects of attending a better-resourced college are highest for low-income students (see Lovenheim and Smith (2022) for a review of this literature). Research into the economic returns to higher education typically assume that students enroll in one institution and stay until they graduate or drop out, thereby failing to characterize a large population: students who transfer between institutions.[2]

In the United States, transfer students make up over one-third of all college students (Shapiro et al., 2018). Students who make initial college choices without full information may transfer as a way to move to a college that better matches their needs after learning that they are poorly matched with their first college. Other students, especially those under credit constraints, could use the transfer system to obtain their college degree at a lower cost by beginning at a community (two-year) college and then transferring to a four-year college. Studying transfers, especially from less-resourced to better-resourced colleges, is of particular relevance for disadvantaged populations. Low-income students, first-generation students, and students from underrepresented racial minority groups are disproportionately likely to attend community colleges or less-resourced four-year colleges, so their most accessible pathway to a well-resourced college may be through transfer. Thus, it is especially important for policy makers to understand whether the positive effects of attending a better-resourced college persist when we consider students transferring from two-year or less-resourced four-year institutions.

This paper uses administrative data from Texas and a regression discontinuity (RD) design to study the causal effect from either a two- or four-year college to a four-year college on students' degree completion and earnings. Surprisingly, I find negative earnings returns for academically marginal students who transfer from two-year colleges to four-year colleges or from less-resourced four-year colleges to flagship colleges. I investigate several mechanisms behind this result and find evidence of transfer students' substitution out of high-paying majors into lower-paying majors, reduced employment and labor market experience, and potential loss of support networks.

The primary challenge to measuring the causal effect of transfer on student outcomes is selection

[1] As discussed in Lovenheim and Smith (2022), there is a substantial amount of research on returns to college "quality" but no consensus on the definition of or best way to measure quality. In this paper, I use the term "well resourced" instead of "high quality", where institutional resources can include students, faculty, funding, and prestige. Most papers in the literature use measures of one or more inputs, such as average student test scores or expenditures per student, to proxy for college quality (Black and Smith, 2006). These inputs correlate with each other such that most colleges that are more selective or have higher average test scores are also better resourced along other dimensions. In this paper, I use whether a college is designated as a flagship institution as a proxy for its being well resourced, which aligns with most measures of quality used in the previous literature.

[2] Several notable exceptions include Andrews et al. (2014), Monaghan and Attewell (2015), and Carrell and Kurlaender (2018). I review these and other papers in the transfer literature in section 3.2.

into transfer. In general, the types of students who choose to transfer are different from students who do not transfer, such that simple comparisons of these two groups will give biased effects. The RD design addresses this issue by using a cutoff that determines (at least in part) whether students can transfer colleges, allowing me to compare students just above the cutoff to students just below under the assumption that they are similar to each other in observable and unobservable ways.[3] Despite the benefits of this empirical strategy, it is not easy to find settings in higher education where the RD can be used (especially in the U.S., where many colleges use "holistic admissions"). Even if many colleges use cutoffs in GPA to determine transfer admissions, they rarely make these cutoffs publicly available. To overcome this issue, I use methods building on Porter and Yu (2015) to estimate institution–year-specific GPA cutoffs from the application and admissions records of all transfer applicants to Texas public 4-year universities. I show that my cutoff estimation uncovers clear increases in the probability of transfer admission at certain GPA cutoffs and, intuitively, that these GPA cutoffs increase with university selectivity. I then use the detected cutoffs in an RD design to estimate the effect of a student's being narrowly granted transfer admission relative to being narrowly denied transfer admission across a variety of colleges. I explore effect heterogeneity along colleges' level of resources by separately estimating effects for flagship colleges and less-resourced institutions.

My results show that among both two-year college students who apply to transfer to four-year colleges and four-year college students who apply to transfer to nonflagship four-year colleges, those who are narrowly accepted for transfer admission are significantly more likely to earn a bachelor's degree than those narrowly denied admission. However, I surprisingly find negative earnings returns for narrowly accepted students who transfer from two-year colleges to four-year colleges or from less-resourced four-year colleges to flagship colleges. While the confidence intervals are wide, the point estimates for the average annual earnings impacts are around -$9,000 for two-year to four-year transfers and -$11,000 for four-year nonflagship to four-year flagship transfers, and they are statistically significant in most specifications. These negative impacts are not driven by transfer students spending additional time in college. In fact, they are persistent and increasing over time since transfer: the largest negative effects are 11-15 years after transfer.

To be clear, I estimate a local average treatment effect for students on the margin of transfer admission, so results should not be extrapolated to all students who transfer. Thus, the estimates are relevant for a small but policy-relevant group of students. I further facilitate interpretation of the main estimates by breaking down several pathways taken by narrowly denied students. Some students who are denied transfer admission never transfer, but others apply again in a later year and subsequently transfer. I show that the main results are a weighted average of several treatment effects (e.g., the effect of transferring relative to never transferring and the effect of transferring earlier versus later) and use a complementary analysis with a different identification strategy to shed light on treatment effect heterogeneity between the different pathways.

[3] I implement several tests to check the validity of this assumption in section 1.6 and find that students above and below the cutoff appear similar.

I also use the RD to investigate several mechanisms behind these results. First, students who transfer to flagship colleges from other four-year colleges complete degrees in lower-paying majors than their counterparts who were denied transfer admission.[4] In particular, they are less likely to major in business and are more likely to major in social sciences.[5] Second, among students enrolled in two-year colleges, those who marginally transfer to four-year colleges have lower levels of employment and labor market experience than those just below the GPA cutoff. They have fewer spells of continuous employment, suggesting that they are less attached to the labor force and/or switch between jobs more frequently, perhaps due to less stable networks. Third, I show that marginally admitted transfer students move further from their hometowns for college than those narrowly denied transfer admission, suggesting potential losses of support networks. I also explore but find no evidence for several other possible explanations: my main effects do not appear to be driven by selective out-migration from Texas, changes in industry of employment, or decreases in GPA.

My findings complement the qualitative literature that examines transfer students' experiences. This work has found that transfer students face significant challenges in meeting the academic demands of their new institution, forming social ties, and navigating complex institutional transfer processes and policies (Flaga, 2006; Packard et al., 2011; Elliott and Lakin, 2021). Difficulties navigating the transfer process may be exacerbated in Texas, where each university sets its own transfer requirements and policies and where autonomy for individual institutions is prioritized over statewide regulation (Schudde et al., 2021a; Bailey et al., 2017). Even within a university, each department sets how credits are transferred and whether they satisfy major requirements (Schudde et al., 2021b). Additionally, a lack of high-quality advising and other institutional support makes transfer students' transitions to four-year colleges difficult (Ishitani and McKitrick, 2010; Allen et al., 2014). Even institutions that have have robust support systems for students first-time-in-college (i.e., freshmen) may devote fewer resources to transfer students, because transfer students are not usually counted in graduation rates or other performance metrics that go into accountability measures and college rankings (Handel and Williams, 2012; Jenkins and Fink, 2016).[6]

The rest of this paper proceeds as follows: section 3.2 reviews related literature, section 1.3 lays out a conceptual framework to offer context to the empirical results, section 3.3 describes the data, section 1.5 details the empirical framework, section 1.6 discusses identification, section 2.5 presents the main RD results, section 1.8 elaborates on how to interpret results, section 1.9 explores

[4]See **?** and Martellini et al. (2023) for estimates of pay differentials by major in the US and global contexts, respectively.

[5]This is likely a result of restrictions on how major-specific courses are counted for transfer or on admission to the business school (transfer students may be broadly admitted to a university but not to a specific major). Past work has shown that major-specific barriers exist for non-transfer students as well: Bleemer and Mehta (2023) show that colleges limit access to high-paying and popular majors through restrictions on introductory course grades, while Stange (2015) shows that many universities charge higher tuition for these majors.

[6]My own conversations with administrators at 4-year universities in Texas revealed that attention and resources are much more focused on first-time-in-college students than transfer students (e.g., the university has a goal of a 70 percent graduation rate within 4 years, but the measurement of four-year graduation rates does not include transfer students, and thus, steps taken toward achieving this goal center on first-time students). However, many of these universities have committed more funding and implemented several new programs for transfer students in recent years that may not be captured by my estimates of longer-term effects on earlier cohorts of transfer students.

mechanisms behind the main earnings results, and section 2.7 discusses policy implications and concludes.

1.2 Literature Review

I contribute to the literature on the effects of transfer on students outcomes by (1) providing a causal estimate using a regression discontinuity design, (2) studying labor market returns as well as educational outcomes, and (3) studying heterogeneity between flagship and less-resourced colleges. Since it is difficult to find exogenous variation in transfer, previous work has studied the relationship between transfer and student outcomes by either providing descriptive evidence, assuming selection on observables, or using qualitative methods such as interviewing students or conducting focus groups. Among them, some have focused on positive relationships between transfer status and student outcomes (Hilmer, 2000; Light and Strayer, 2004) or descriptively documented how transfer student outcomes vary by type of transfer (e.g., transfer to more selective or less selective college) (Andrews et al., 2014; Jenkins and Fink, 2016). Others document difficulties that transfer students face in the adjustment process and the pattern of students' GPAs decreasing after transfer, often called "transfer shock" (Flaga, 2006; Packard et al., 2011; Ellis, 2013; Monaghan and Attewell, 2015; Lakin and Elliott, 2016; Elliott and Lakin, 2021). Bloem (2022) uses a regression discontinuity to estimate the effect of minimum transfer admission requirements on rates of transfer but does not estimate the effect of transfer on degree completion or labor market outcomes. Some studies present causal effects of various policies on transfer and degree completion (Baker, 2016; Boatman and Soliz, 2018; Shaat, 2020; Baker et al., 2023; Shi, 2023), but there is little evidence on labor market outcomes. Others take up the related question of whether there are differences in returns to starting at a two-year college (with the intention of transferring to a four-year) versus starting at a four-year directly and find negative returns to starting at a two-year college (Long and Kurlaender, 2009; Mountjoy, 2022).[7] These causal studies, along with much of the transfer literature, have focused exclusively on students transferring from two-year colleges to four-year colleges. Despite the fact that around 20 percent of students who begin at a four-year institution transfer to another four-year institution within six years[8], research on the four-year to four-year transfer pathway has been more sparse. I contribute to both strands of the literature.

My work also relates to the literature that uses regression discontinuity designs to estimate the effect of access to colleges of varying resource levels (often referred to as "quality", see footnote 1). I contribute to this literature by estimating the effect of transferring to a well-resourced college, since prior work has only considered the quality/resources of one's initial institution (Hoekstra, 2009; Cohodes and Goodman, 2014; Zimmerman, 2014; Goodman et al., 2017; **?**; Kozakowski, 2023). I also add to the literature that considers the interaction between field of study and college

[7] Some of these differences may be due to discrimination in the labor market. Zhu (2023) uses a randomized audit study to find that among fictitious bachelor's degree holding students, those with a community college listed on their resume receive fewer callbacks for accounting jobs.

[8] Author's calculations using the Beginning Postsecondary Study (U.S Department of Education, 2022).

quality/resources (Hastings et al., 2013; Arcidiacono et al., 2016; Aucejo et al., 2022; Bleemer, 2022), which has not previously considered transfer students.

Finally, this paper relates to the few papers studying college resources that explicitly consider transfer students. Two papers that estimate the labor market returns to college resources analyze transfer as a mechanism for returns to college quality/resources. Dillon and Smith (2020) find some evidence that students whose academic ability is not well-matched to the resources of their initial college may transfer to a better- or less-resourced college that is more aligned with their academic ability. Mountjoy and Hickman (2019) find that institutions that induce transfer have lower value-added in terms of bachelor's completion and earnings. Andrews and Thompson (2017) is the only study that considers students who begin elsewhere and transfer to a well-resourced college.[9] They estimate the effect of transferring to the University of Texas - Austin (UT–Austin) through the Coordinated Admissions Program (CAP), which allows students who were initially rejected from UT–Austin to transfer in after completing their first year at a UT branch campus with a specified minimum GPA. However, CAP serves a relatively narrow population of students who (1) initially apply to UT–Austin, (2) are offered CAP and decide take up the program by June 1 following their final year of high school, (3) begin the following fall at another UT branch with the intention of transferring to UT–Austin one year later, and (4) complete the other CAP course/credit requirements. My work adds to this literature by including a broader set of students who begin at any four-year college in Texas and may not make the decision to transfer until later in their college career. Additionally, I explore the effects of transferring to a broader set of universities, including those that are less resourced than UT–Austin.

1.3 Conceptual Framework

In this section, I provide a brief conceptual framework laying out factors which may impact a student's payoff to transfer to highlight that the expected impact of transfer on earnings is ambiguous. I focus on the case of a student transferring to a better-resourced college since most students in my sample apply to transfer to a better-resourced college.[10]

First, I expect a better-resourced college to have a positive effect on earnings through both its signaling value (i.e., employers will assume that graduates of well-resourced colleges will be better workers) and its effect on human capital accumulation (e.g., a college with better instructors will raise students' human capital more). This implies that, all else equal, transferring to a better-resourced college should raise earnings. Second, students accumulate more human capital at colleges to which their academic abilities are well-matched. Therefore, if a student transfers to a college for which they are better matched, the transfer will have a positive effect on earnings. Third, college graduates earn more than non-graduates, so if transferring affects a student's probability of graduating it will in turn affect her earnings. Fourth, transferring could cause a student to

[9] Andrews (2016) is a closely related short paper considering the same question.

[10] Each channel that depends on college resources could occur with opposite signs when considered a student transferring to a less-resourced college.

switch majors. There are several reasons for this major switching. First, there may major-specific admissions (i.e., a student may be admitted as a transfer student to a college but not to all majors within the college). Second, if students lose many credits in the transfer process, they may not have time to complete all requirements for more intensive majors and still graduate on time. Third, students may have been under-prepared by their sending college for the upper-level classes at the receiving college in a given major. This change in major could affect students' human capital accumulation and earnings. Finally, transferring may have a negative impact on students earnings because of the disruption to both the student's academic environment and social networks.

Students will choose to transfer only if they expect that it will positively impact the sum of their expected earnings and non-pecuniary benefits. However, students do not have full information about their human capital and how well they are matched with each college. Thus, it is possible for students to make "mistakes" due to information frictions.[11] Students with worse information will be more likely to choose transfers which have worse payoffs.

1.4 Data and Institutional Background

I use administrative data from the Education Research Center (ERC) at the University of Texas–Dallas on all Texas public high school students matched to data on all within-state postsecondary enrollment, degree completion, and earnings from 2000 to 2021.[12] In addition to including detailed student-level data on background characteristics (e.g., gender, race, free or reduced-price lunch status, high school ID, standardized test scores), these data track students through all semesters of enrollment in any four-year or public two-year college in Texas. I also observe all applications (including transfer applications) and admissions decisions for any Texas four-year public institution. Institutions do not directly report student GPA, but they do include the number of credits attempted and the number of grade points earned for each semester of enrollment for all years. Therefore, I construct student cumulative GPA at the end of each semester by dividing the total number of grade points earned by the total number of credits taken in all prior semesters. Finally, the ERC data include linkages to the Texas Workforce Commission's individual-level quarterly earnings records, which give total earnings at each job in each quarter for all Texas employees subject to the state unemployment insurance (UI) system.[13]

The ERC data allow me to identify four-year public colleges in Texas that use college GPA cutoffs in their transfer admissions decisions. As noted in Altmejd et al. (2021), many colleges use minimum SAT cutoffs in admissions decisions without making these cutoffs publicly known. Similarly, some institutions use college GPA cutoffs in their admissions decisions for transfer students. Although these cutoffs are sometimes made publicly available, often they are not. These cutoffs may be used for

[11]Note that not all students who have negative earnings returns to transfer are necessarily making mistakes, since they may knowingly accept the lower earnings in return to higher non-pecuniary benefits (e.g., transferring leads them into a lower-paying major but they enjoy the work more).

[12]Data on private college enrollment for years prior to 2003 are not available.

[13]Self-employed workers, some federal employees, independent contractors, military personnel, and workers in the informal sector are excluded from the state UI system.

Table 1.1: Summary Statistics by Sector

	2-year Students			4-year Students		
	N	Mean	SD	N	Mean	SD
Male	90,692	0.516	0.500	27,330	0.506	0.500
Math test score	77,081	0.225	0.793	23,547	0.623	0.691
Reading test score	76,984	0.264	0.657	23,524	0.535	0.518
FRPL	90,692	0.204	0.403	27,330	0.168	0.374
Nat. American	90,692	0.00292	0.0540	27,330	0.00231	0.0480
Asian	90,692	0.0546	0.227	27,330	0.117	0.322
Afr. American	90,692	0.111	0.315	27,330	0.118	0.323
Hispanic	90,692	0.283	0.450	27,330	0.244	0.429
White	90,692	0.543	0.498	27,330	0.510	0.500
Two or More Races	90,692	0.00527	0.0724	27,330	0.00809	0.0896

Notes: Summary statistics of high school characteristics of analysis sample. FRPL = free or reduced-price lunch recipient.

minimum admissions standards (students with a GPA below the cutoff are automatically rejected), for guaranteed admission (students with a GPA above the cutoff are automatically accepted), or as part of some formula or other strategy that gives a “boost" to a student’s probability of admission if she is above a certain cutoff. These thresholds can be empirically determined even when they are not published. In section 1.5, I describe my procedure for identifying these cutoffs in the data.[14]

Texas has two flagship institutions: the University of Texas–Austin and Texas A&M University. By almost any measure of college quality/resources used in the literature, these are the two top public universities in the state.[15] Thus, I use flagship status as a proxy for college resources and separately estimate results by whether students apply to transfer to a flagship or a nonflagship university.[16] [17] Table 1.1 gives summary statistics on the background characteristics for my analysis sample (described in section 1.5) broken down by students’ sector (2-year/4-year college) of enrollment at the time of transfer application. “Math test score" and “Reading test score" refer to student test scores on 10th grade state standardized tests, which have been normalized within each statewide cohort to have mean zero and a standard deviation of one.

My primary outcomes of interest are bachelor’s degree completion and earnings, both of which are observed for the period through 2021. I define degree completion relative to the year in which the student intends to transfer. For example, in the 2010–2011 academic year, the student submits

[14]I focus on GPA cutoffs rather than SAT cutoffs because most transfer applications do not require students to submit their SAT scores.

[15]Using the college quality/resource measure from Dillon and Smith (2020), which combines incoming SAT scores, applicant rejection rates, faculty salaries, and faculty–student ratio, UT–Austin is the top-ranked public university in Texas, and Texas A&M is ranked second. *US News & World Report* also ranks UT–Austin and TAMU as the first- and second-best public universities in Texas (and the second- and third-best overall behind only Rice University) (US News and World Report, 2022).

[16]My estimates for flagship universities primarily reflect UT–Austin rather than Texas A&M since I identify many more years with admissions cutoffs for UT–Austin.

[17]Although it would be interesting to study variation in effects among nonflagship universities, unfortunately, I do not have enough statistical power to do so with my empirical strategy.

an application to transfer the following year; that is, she would like to enroll in fall of the 2012–2013 academic year. Then, "bachelor's within 2 years" indicates whether she has earned a bachelor's by the end of the 2013–2014 academic year.[18]

Since earnings are reported quarterly, I create annual earnings that align with the academic year by defining an earnings year to include the third and fourth quarter of year t and the first and second quarter of year $t+1$ (e.g., the earnings year 2012–2013 includes earnings from July 1, 2012, to June 31, 2013). I define earnings relative to the intended transfer year, where the transfer year is year 0; e.g., for a student who first enrolled at the new institution in the 2012–2013 academic year, "earnings 2 years after intended transfer" gives her earnings from July 2014 to June 2015.

Since the earnings data come from Texas administrative records, they do not capture earnings for individuals working in another state or self-employed individuals.[19] Therefore, if a worker does not appear in the earnings data, she may really have zero earnings, or she may have earnings that are not observed. To account for this, I use three different measures of annual earnings. First, to fully capture any effects on the extensive margin of employment, I use an "unconditional" earnings measure, which codes earnings for quarters in which workers do not appear as zero. However, this might induce bias since they are not all true zeros, so the second measure ("conditional" earnings) averages over only nonzero quarters.[20] Finally, the third measure ("sandwich" earnings) follows Sorkin (2018) by averaging only over positive quarters that are "sandwiched" between two quarters with positive earnings levels. In addition to increasing the probability that the worker is in Texas, this measure aims to avoid counting quarters when a worker may have started or stopped working in the middle of the quarter and is meant to measure potential earnings when a worker is employed full-time.[21] For all measures, I convert earnings to real 2012 dollars using the personal consumption expenditures price index and winsorize each quarter of earnings at the 99th percentile (among the full distribution of earnings of Texas workers). I also implement robustness checks where I proxy for out-migration following Grogger (2012) and find no evidence that my main effects are driven by selection bias due to differential migration between transfer and nontransfer students.

[18] My main results are similar if I measure bachelor's completion in time since high school graduation or time since first college enrollment rather than time since intended transfer.

[19] Foote and Stange (2022) discuss issues with attrition bias in postsecondary empirical applications using state-level administrative data and find that while out-migration can substantially bias results, self-employment is not a major source of bias. Luckily, Texas has the lowest out-migration rate of any state in the U.S., making out-migration less of an issue in this setting.

[20] Mountjoy (2022) also uses the TX administrative data and uses this strategy to measure earnings.

[21] Here, "positive" earnings are defined as earnings above an annual earnings floor of $3,250 in 2011 dollars. If an individual has no "sandwiched" quarters within a calendar year, I use quarters adjacent to (either before or after) one other quarter of employment and multiply by 8. The reason for this step is because if we assume that employment duration is uniformly distributed, then, on average, the earnings for each adjacent quarter will represent one-half of a quarter's work. For details, see the online appendix of Sorkin (2018).

1.5 Empirical Strategy

Detection of Admissions Cutoffs

First, I estimate the GPA cutoffs that universities use in transfer admissions. As long as there exist cutoffs—even if the specific cutoffs are unknown—above which a student's probability of being accepted for transfer discontinuously increases, the regression discontinuity (RD) design can be used to estimate the effects of transfer. Porter and Yu (2015) propose methods to use the RD design in the case of an unknown discontinuity point and show that estimating the discontinuity point does not affect the efficiency of their treatment effect estimator, implying that the cutoffs can be treated as known in the second stage since the influence of estimation error in the cutoffs is negligible in the final results.[22] I use a variant of these methods to estimate thresholds for each year and institution from the empirical distribution of transfer applications to four-year public institutions.

These cutoffs may vary across years within a given college, so I search for thresholds separately in each institution and year from 2000 to 2019. For a given institution and year, I also separately search by whether the student applies to transfer from a two-year or four-year institution (i.e., sector) since these transfer processes are different and admissions officers may treat GPAs from two-year college differently from those from four-year universities. Since I do not know which colleges use admissions thresholds and I want to limit false positives, I search for cutoffs in each college–year–sector combination only if it contains at least 500 transfer applications. Among this set, separately for every potential GPA threshold from 1.5 to 3.8, I estimate the following local linear regression with a bandwidth of 1.0 and a uniform kernel:

$$\mathrm{Accept}_{icts} = \beta_0 + \beta_1 \mathbb{1}(\mathrm{GPA}_i \geqslant T_{cts}) + f(\mathrm{GPA}_i) + \varepsilon_{icts} \tag{1.1}$$

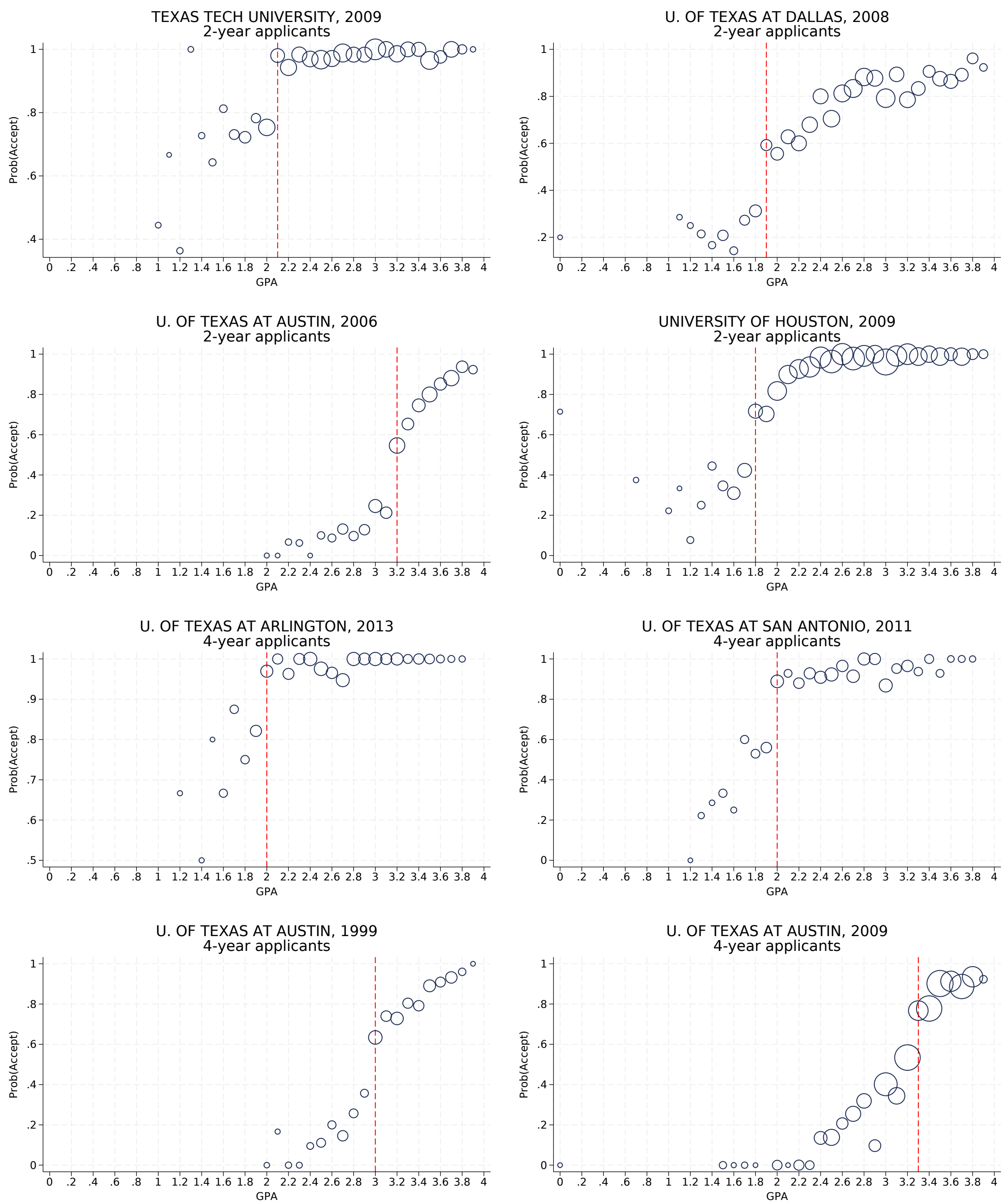
where Accept_{icts} is an indicator for application i to college c from a student in sector s in year t being accepted and T_{cts} is a potential threshold used in admissions decisions. β_1 estimates the magnitude of any potential discontinuity in application acceptance at the given threshold T_{cts}. I want to use T_{cts} as a threshold only if there is strong evidence of a jump in admissions at that point, so I keep only thresholds for which the p-value of the test that β_1 is equal to zero is less than 0.01. If there is more than one threshold with a p-value less than 0.01, I take the one with the maximum t-statistic.[23]

I identify eight colleges that use admissions cutoffs for four-year students and 24 colleges that use admissions cutoffs for two-year students, which I collectively refer to as “target" colleges. A few examples of these cutoffs identified at target colleges are illustrated in the binned scatterplots in

[22]The intuition behind this result is that estimating a discontinuity point is a nonstandard estimation problem with a different distribution than a more standard estimation of a mean. Within this distribution, it turns out that estimating a jump is easier than in other cases. Estimation of the discontinuity point has a faster convergence rate such that, in a large sample, the approximation error is negligible. See Porter and Yu (2015) for more details and formal proofs.

[23]This procedure is similar to the ones used to identify discontinuities in Altmejd et al. (2021), Brunner et al. (2021), and Andrews et al. (2017). I test the sensitivity of this procedure by considering analyses with stricter p-value thresholds (i.e., less than 0.001 and less than 0.0001) and obtain qualitatively similar results.

Figure 1.1: Examples of Identified GPA Cutoffs in Transfer Admissions



Notes: Each subfigure shows an example of an estimated discontinuity for a particular institution, year, and sector (2-year/4-year) of applicants. The subfigures are binned scatterplots of applicant acceptance rates, where each bin is 0.1 grade points. Circle sizes are proportional to the number of applications in each bin. Some bins are suppressed because of disclosure avoidance for small sample sizes. The dotted vertical line shows the identified threshold.

Figure 1.1. Each dot represents the acceptance rate of applicants with GPAs that fall within that 0.1 grade point bin. The dotted vertical line marks the identified cutoff. In each of these cases, although the probability of acceptance is generally increasing in GPA, there is a jump in this relationship that is indicative of using GPA cutoffs in admission. Table 1.2 and Table 1.3 show the summary statistics of the full set of cutoffs that I identify for each college for applicants from four-year and two-year colleges, respectively.[24] For some colleges, I do not identify a cutoff for every year, which we might observe if the cutoff was not binding in some years. It's also possible that there are some true cutoffs that I do not detect. This is not a problem for my identification strategy; excluding those cutoffs will weaken the first stage but not bias effects. Cutoffs for a given college may change from year to year depending on the applicant pool or the available seats for transfer students. Using variation within colleges and across time, I find that, among four-year transfer students, the identified cutoffs for colleges are higher in years when they receive a higher volume of applications, which lends some support that I am picking up real changes in the underlying cutoffs rather than randomness in the applications and admissions process.[25]

In this context, I estimate "fuzzy" regression discontinuities (i.e., there is a jump in the probability of being accepted for transfer at the cutoff, but the probability does not jump from 0 to 1). Intuitively, this is because not all students who pass the GPA cutoff are accepted for transfer and some students below the GPA threshold may gain transfer admission on the strength of other aspects of their application. It is important to note that GPA is not the only factor that determines whether a student is accepted for transfer admission. Students may also be judged on their transcripts, letters of recommendation, and other application materials. This is not a problem for my empirical design since fuzzy cutoffs can still be used to estimate causal effects in RD designs. It implies that crossing the threshold is a weaker instrument for transfer than if admission were determined fully by GPA, but it does not bias the estimated local average treatment effect for students on the margin of being accepted for transfer. To make my instrument stronger, I pool data across years and institutions instead of separately estimating the effects of transfer for each individual cutoff.[26] However, I keep

[24]For cutoffs that lie near 2.0, there may be a concern that I am picking up the effects of academic probation and/or failure to maintain satisfactory academic progress (SAP), which applies to students with a GPA below 2.0. The literature on the effects of falling below this threshold is mixed: while some work has found negative effects on degree completion and/or earnings (Ost et al., 2018; Bowman and Jang, 2022), many works find null effects overall (Lindo et al., 2010; Schudde and Scott-Clayton, 2016; Casey et al., 2018; Scott-Clayton and Schudde, 2020; Canaan et al., 2023). I test whether this is a concern in my setting by estimating treatment effects at two regression discontinuities at 2.0: one for my analysis sample and one for all students who apply to transfer in Texas (regardless of whether they are in my sample). Neither test shows evidence of statistically or economically significant effects on degree completion or earnings, suggesting that probation and SAP are not likely to affect my main results.

[25]Specifically, I regress colleges' identified cutoffs for four-year applicants on the number of applications (including both first-time and transfer applications) along with institution fixed effects. I find that, on average, when a college receives 10,000 more applications, its identified cutoff is approximately 0.1 grade points higher (p-value=0.005). The number of applications that an institution receives in a given year ranges from 10,000 to 55,000. I conduct a similar exercise with cutoffs for applicants from two-year colleges but do not find similar evidence of cutoffs being higher when the college receives more applications; this may be because universities prefer to set a bar and accept all two-year students who meet it rather than admit students based on the number of available seats.

[26]Since some students may apply for transfer to multiple colleges, some individuals are included in my sample more than once. However, because students are unlikely to be close to the cutoffs used by multiple target colleges, this group is small (around 4% of my sample) and results are not sensitive to dropping them.

Table 1.2: Identified Admissions Cutoffs for Transfer Applicants from Four-Year Colleges, 1999–2019

University	N years	Mean	Min	Max
Flagship				
U. of Texas at Austin	20	3.2	2.9	3.8
Texas A&M University	1	2.7	2.7	2.7
Nonflagship				
Texas State University	16	2.0	1.6	2.3
Texas Tech University	4	2.0	1.5	2.4
U. of Texas at Arlington	13	1.8	1.6	2.0
U. of Texas at San Antonio	10	2.0	1.6	2.2
University of Houston	19	1.9	1.7	2.2
University of North Texas	12	1.7	1.5	1.9
Total	**95**	**2.2**	**1.5**	**3.8**

Notes: This table presents GPA cutoffs identified as discontinuities in admissions at public four-year institutions for transfer applicants from four-year colleges with the procedure described in section 1.5. The first column (N years) represents the number of years for which a discontinuity was identified for a given institution, and the next three columns give summary statistics of those cutoffs.

applicants from two-year and four-year colleges separate in all specifications. I also estimate some specifications in which I separate out applications to flagship universities to explore heterogeneity by college resources.

Regression Discontinuity

To form this stronger instrument that pools the estimated discontinuities, I create a centered GPA by subtracting the relevant college–year–specific estimated threshold from the GPA of each student who applies to a threshold-using college.[27] I then pool the data across colleges and application years and estimate the first stage:

$$\begin{aligned} \mathit{TransferTarget}_{ict} = \alpha_0 + \alpha_1 \mathbb{1}(GPA_i \geqslant T_{ct}) + f(GPA_i) \\ + \Omega X_i + \gamma_{ct} + \kappa_{m(i,t)} + \theta_{s(i,t)} + \epsilon_{ict} \end{aligned} \tag{1.2}$$

where $\mathit{TransferTarget}_{ict}$ is an indicator that equals 1 if student i transfers to a target college c in year t and zero if student i applied to transfer to target college c but did not transfer in year t. α_1 gives the estimated difference in transfer rates between students who are just above and just below the threshold used by the target college to which they applied. I include college-by-year fixed effects γ_{ct} to ensure that comparisons are made only between individuals who applied to the same

[27] I measure the student's GPA as her cumulative GPA at the end of the fall semester the year before her anticipated transfer entry to align with transfer application deadlines. If a students applies to transfer multiple times, I use the first time she applies so that any later transfers can be considered as outcomes following the first transfer.

Table 1.3: Identified Admissions Cutoffs for Transfer Applicants from Two-Year Colleges, 1999–2019

University	N years	Mean	Min	Max
Flagships				
U. of Texas at Austin	19	3.3	2.9	3.7
Texas A&M University	16	2.5	2.3	2.8
Nonflagship				
Lamar University	7	1.7	1.5	1.8
Sam Houston State University	11	1.7	1.5	2.0
Stephen F. Austin State Univ	8	1.7	1.5	2.1
Tarleton State University	9	1.7	1.5	1.8
Texas A&M Univ-Corpus Christi	6	1.7	1.5	2.0
Texas A&M Univ-San Antonio	4	1.7	1.5	1.7
Texas A&M University-Commerce	8	1.6	1.5	1.8
Texas State University	20	1.9	1.6	2.1
Texas Tech University	8	1.8	1.5	2.1
Texas Woman's University	1	2.9	2.9	2.9
U. of Houston-Clear Lake	9	1.8	1.7	2.1
U. of Houston-Downtown	1	1.5	1.5	1.5
U. of Texas at Arlington	18	1.7	1.5	1.8
U. of Texas at Dallas	11	2.1	1.9	2.3
U. of Texas at El Paso	14	1.6	1.5	1.9
U. of Texas at San Antonio	19	1.8	1.5	2.2
U. of Texas at Tyler	12	1.7	1.5	2.0
U. of Texas-Permian Basin	1	1.5	1.5	1.5
U. of Texas-Rio Grande Valley	7	1.6	1.5	1.8
University of Houston	21	1.9	1.7	2.2
University of North Texas	10	1.7	1.5	3.1
West Texas A&M University	3	2.2	1.6	3.4
Total	**243**	**1.9**	**1.5**	**3.7**

Notes: This table presents GPA cutoffs identified as discontinuities in admissions at public four-year institutions for transfer applicants from two-year colleges using the procedure described in section 1.5. The first column (N years) represents the number of years for which a discontinuity was identified for a given institution and the next three columns give summary statistics of those cutoffs.

college in the same year. I also include a vector of student characteristics X_i (gender, race, ethnicity, free or reduced-price lunch status, high school standardized test scores in math and reading, year of high school graduation, and cumulative credits at the time of application), fixed effects for major at the time of application $\kappa_{m(i,t)}$, and sending college fixed effects $\theta_{s(i,t)}$.[28] Because the admissions thresholds may be measured with noise, I use a donut-hole specification that drops observations within 0.01 grade points of the cutoff.

I then generate reduced-form estimates of the effect of crossing a target college's GPA threshold on student outcomes using the following equation:

$$\begin{aligned} Y_{ict} = \delta_0 + \delta_1 \mathbb{1}(GPA_i \geqslant T_{ct}) + g(GPA_i) \\ + \Lambda X_i + \pi_{ct} + \nu_{m(i,t)} + \phi_{s(i,t)} + \upsilon_{ict} \end{aligned} \tag{1.3}$$

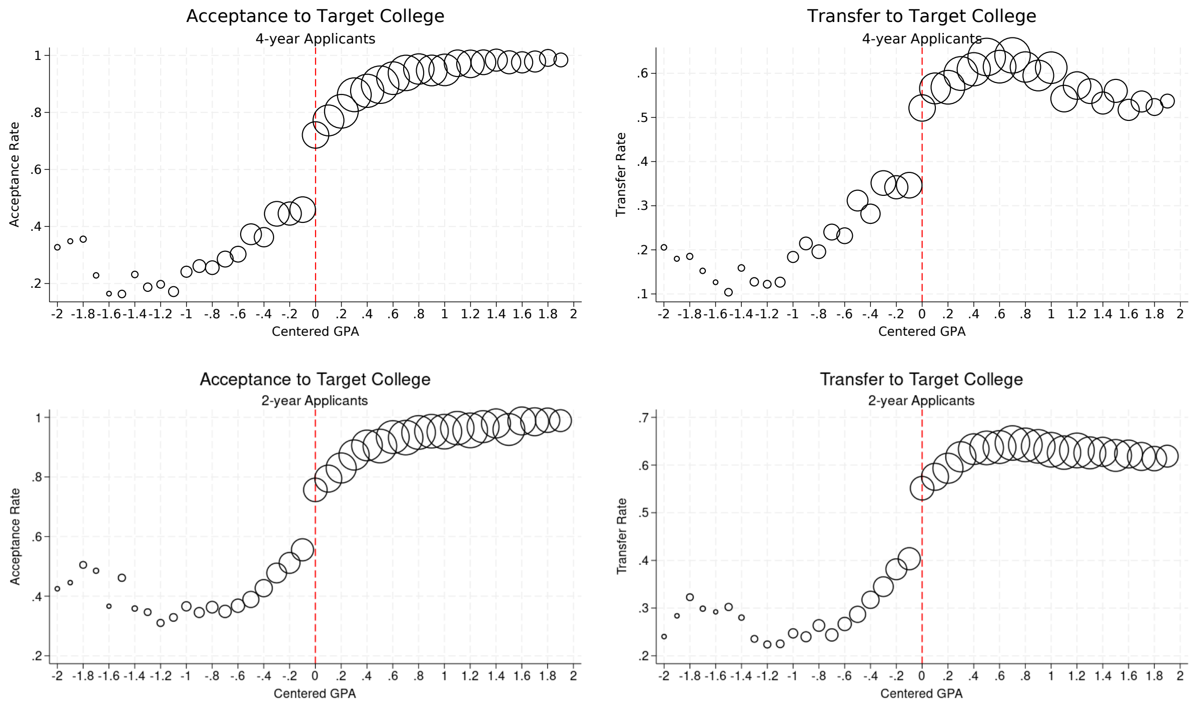
The coefficient of interest δ_1 measures the effect of a student being just above a target college's GPA cutoff on outcome Y_{ict} relative to the outcomes when she falls just below the target college's GPA cutoff. The main outcomes of interest are degree completion and earnings. Analogous to the first stage, I also include student characteristics X_i, application college-by-year fixed effects π_{ct}, sending major fixed effects $\phi_{m(i,t)}$, and sending college fixed effects $\upsilon_{s(i,t)}$.

Finally, I generate instrumental variable (IV) estimates of the effect of transferring to a target college on student outcomes using:

$$\begin{aligned} Y_{ict} = \eta_0 + \eta_1 \widehat{TransferTarget}_{ict} + h(GPA_i) \\ + \Gamma X_i + \zeta_{ct} + \mu_{m(i,t)} + \lambda_{s(i,t)} + \xi_{ict} \end{aligned} \tag{1.4}$$

where $\widehat{TransferTarget}_{ict}$ is the predicted value from Equation 1.2. The coefficient of interest, η_1, measures the effect of transferring to a target college on outcome Y_{ict} for the students who are induced to transfer by crossing the GPA threshold. In addition to estimating the pooled effect of transfer to any target college, I separately estimate effects by level of institutional resources by breaking out flagship institutions (UT–Austin and Texas A&M) from the rest of the target colleges. I refer to these two subsamples as "flagship" and "nonflagship" target institutions. One complication in interpreting the results of the IV estimates is that students who are narrowly denied transfer admission follow a variety of pathways. Thus, for students who do transfer, I do not know which pathway they would have followed otherwise. I elaborate on this and how it affects the interpretation of my results in section 1.8.

Figure 1.2: Identified Cutoffs in Transfer Admission, Pooled across Colleges and Years



Notes: Binned Scatterplots of Application Acceptance and Transfer Outcomes on Centered GPA. Centered GPA is created by subtracting the college–year-specific cutoff from each student's GPA for each application she submits. Circle sizes are proportional to the number of applications in each bin.

1.6 Identification

For me to use the GPA admission cutoffs as a valid instrument for transferring to a target college, they must be relevant and exogenous. The relevance condition holds if a student's crossing the GPA threshold of a target college increases her probability of transferring to a target college. First, I provide graphical evidence in support of this assumption in Figure 1.2, which shows binned scatterplots of transfer on centered GPA, which refers to each student's GPA recentered on the college–year-specific admissions cutoff of the target college to which she applied. The top two subfigures are for applicants from 4-year colleges and the bottom two subfigures are for applicants from two-year colleges. The outcome in the left subfigures is acceptance to a target institution. In the right subfigure, the outcome is transfer to a target institution in the year for which the student applied. The figures show that, although the admission probability is increasing in GPA across the spectrum, there is a visible jump in the probability of admission to a target college at the estimated discontinuity point, which in turn leads to a jump in the probability of transferring to that institution.

Next, I more directly show evidence of relevance by presenting first-stage results from Equa-

[28]Given that the source of data is administrative, missing data are rare. However, some students are missing ethnicity or test score data. To maintain the maximal sample size, I replace missing test scores with zero and include an indicator variable for missing test scores. The results are not sensitive to my dropping these individuals.

Table 1.4: First-Stage Results

	2-year Applicants		4-year Applicants	
	Accept	Transfer	Accept	Transfer
$\mathbb{1}(GPA_i \geqslant T_{cy})$	0.15***	0.12***	0.21***	0.14***
	(0.007)	(0.009)	(0.014)	(0.016)
F Statistic	485.9	170.1	207.2	80.0
Observations	54,194	54,194	21,626	21,626

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates of Equation 1.2 on sample of transfer applicants. Accept = application accepted to target college. Transfer = Enroll in target college in the semester for which transfer admission was applied. F Stat gives the F statistic from a test that the coefficient on the excluded instrument is equal to zero. Standard errors clustered at the application–college–year level.

tion 1.2 in Table 1.4. Through all analyses presented in the main body, I use a local linear specification with a triangular kernel, a bandwidth of 0.3 for two-year applicants and 0.4 for four-year applicants, and standard errors clustered at the application–college–year level. Appendix Tables A1 and A2 show that the results are robust across a range of these choices for my main outcomes.[29] The first column of Table 1.4 shows that two-year students who are just above the GPA cutoff are 15 percentage points more likely to be admitted for transfer to a target college than students just below the cutoff. The second column uses a different outcome based on whether the student actually transfers to the target college in the semester for which she applied. In the instrumental variables results in the rest of the paper, I use this measure as the first-stage, so the results can be interpreted as the effect of transferring to a target college on various outcomes. This specification treats students who are accepted for admission but choose not to transfer as “never-takers." The results in the second column show that, while not all accepted students transfer, there is still a sizable jump in transfer rates at the discontinuity. Among students who applied to a target college, students with GPAs just above their colleges’ cutoff are 12 percentage points more likely to transfer to that college than students just below the cutoff. The third and fourth columns show that applicants from four-year colleges who are just above their respective cutoffs are 21 percentage points more likely to be accepted and 14 percentage points more likely to transfer to a target college than four-year students below the cutoff. The “F Statistic" row gives the first-stage F statistic on the excluded instrument for these specifications and demonstrates that crossing the GPA threshold is a strong instrument for transfer acceptance and transfer to target colleges. This provides evidence that the first identifying assumption, the relevance condition, is satisfied.

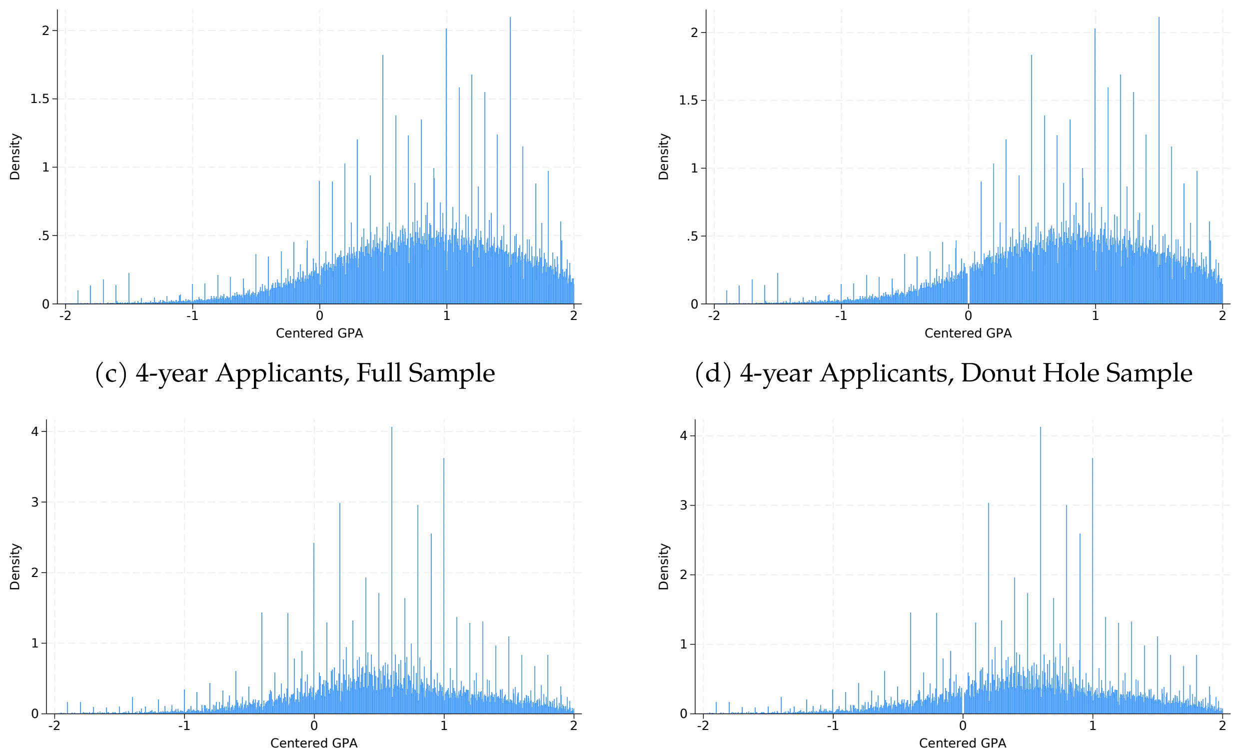
Next, I assess the second condition that must hold for the RD threshold to be a valid instrument: exogeneity. If students are able to strategically manipulate their GPAs in response to the cutoffs,

[29]The choice of bandwidth is driven by the optimal bandwidth values as calculated by Calonico et al. (2020), which fall around 0.3/0.4 for most outcomes for two-/four-year applicants.

Figure 1.3: Density of Applicant GPAs

(a) 2-year Applicants, Full Sample

(b) 2-year Applicants, Donut Hole Sample



(c) 4-year Applicants, Full Sample

(d) 4-year Applicants, Donut Hole Sample

Notes: Histograms of applicants' GPAs after centering on the relevant college–year-specific admissions cutoff. Top row shows two-year applicants, and bottom row shows four-year applicants. Both figures on the right drop all students within 0.01 grade points of the cutoff.

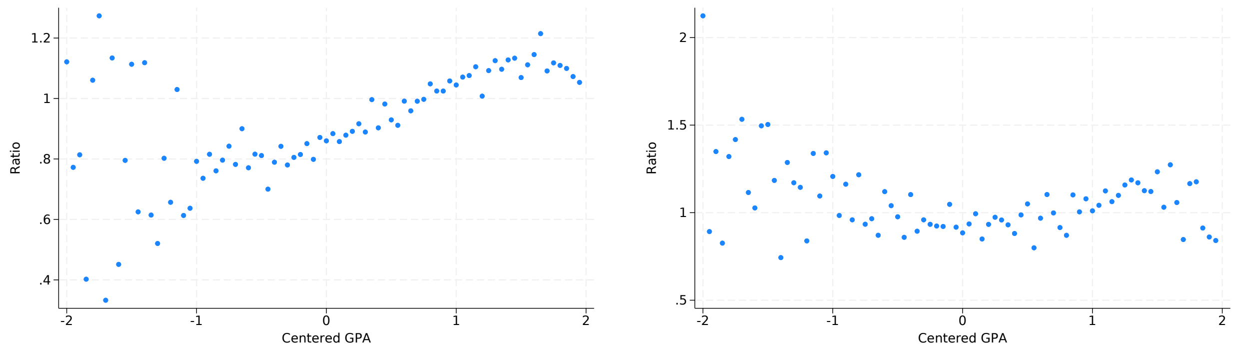
the assumption of exogeneity will fail to hold, and I will not be able to identify the causal effect of transferring. The concern is that, if students are aware of the cutoffs and able to manipulate their GPAs accordingly, then some more motivated students may increase their GPA to ensure that they are just above the cutoff. This would lead to biased results on the effect of transferring since the difference in outcomes between students just above and just below the cutoff may be more related to their difference in motivation or other unobservable characteristics than to the difference in transfer admission.[30] Given that most admissions thresholds are not publicly known, this scenario seems unlikely. Nevertheless, to investigate possible manipulation, I use two tests that are standard in the RD literature.

The first test is to look at the density of the running variable around the cutoff to see whether there is bunching on one side (McCrary, 2008; Cattaneo et al., 2020). However, even absent manipulation, using GPA as the running variable is expected to produce some lumpiness in the distribution since

[30]Another concern is that my bandwidth is large enough that there is bias. This is not an identification issue but an issue in estimation that is present to some degree in all empirical applications. I address this issue by using optimal bandwidth values as calculated by Calonico et al. (2020), using triangular weights so that observations closer to the cutoff are given more weight, and by examining the sensitivity of my results to changes in bandwidth in Appendix Tables A1 and A2.

Figure 1.4: Density Smoothness Tests

(a) 2-year Applicants (b) 4-year Applicants



Notes: Each figure shows the ratios of conditional to unconditional densities relative to the admissions cutoff. Conditional densities condition on whether students receive free or reduced-price lunch, $\Pr(\text{GPA}|\text{FRPL})/\Pr(\text{GPA})$. Ratios computed within 0.05 grade point bins.

grades are assigned in whole numbers (e.g., 3.0 corresponds to a “B" grade). Panels (a) and (c) of Figure 1.3 show that, for both two-year and four-year applicants, the distribution of GPA has a spike right at the cutoff. However, two considerations alleviate concerns about these spikes. First, the panels (b) and (d) show that, after I drop observations within 0.01 grade points of the cutoff, as I do in my main specifications, the density appears relatively smooth through the cutoff. Second, I implement an alternative test from Zimmerman (2014) that plots the ratios of unconditional densities to densities that condition on observed student characteristics that are correlated with educational and labor market outcomes:

$$\frac{f(\text{GPA}|x)}{f(\text{GPA})} \tag{1.5}$$

where $f(\text{GPA}|x)$ and $f(\text{GPA})$ are the conditional and unconditional densities of the centered GPAs, respectively. The idea is that, if the spikes in the GPA distribution come from processes unrelated to the admissions cutoffs, they should appear in both the unconditional and conditional distributions. Taking the ratio cancels these two parts out so that the ratio should appear smooth through the cutoff. In Figure 1.4, I show these ratios where the conditional density conditions on whether students received free or reduced-price lunch in high school. The left figure is for two-year applicants, and the right figure is for four-year applicants. Both ratios appear smooth through the discontinuity, consistent with the exogeneity assumption.

To further test the exogeneity assumption, I implement the second standard RD test, a balance test using composite measures of students’ predicted earnings based on their observable characteristics. To create the composite measure, I use the full population of Texas high school students who enroll in a Texas postsecondary institution[31] excluding my analysis sample and regress average annual

[31] For students who enroll in college for multiple semesters, I randomly choose one from which to pull the corresponding values on these characteristics so that each individual is counted only once.

Table 1.5: Balance Test

	2-year Applicant Predicted Earnings			4-year Applicant Predicted Earnings		
	Unconditional	Conditional	Sandwich	Unconditional	Conditional	Sandwich
$\mathbb{1}(\mathsf{GPA}_i \geqslant \mathsf{T}_{cy})$	-67.3	-60.2	-36.3	187*	188	147
	(76.9)	(96.8)	(104)	(94.6)	(123)	(138)
p-val	0.38	0.53	0.73	0.051	0.13	0.29
$\mathsf{TransferTarget}$	-585	-524	-316	1,174*	1,177	919
	(670)	(841)	(906)	(594)	(775)	(864)
p-val	0.38	0.53	0.73	0.051	0.13	0.29
Obs	54,186	54,186	54,186	22,197	22,197	22,197

Notes: *** p<0.01, ** p<0.05, * p<0.1. Reduced-form (RF) estimates of Equation 1.3 and instrumental variable (IV) estimates of Equation 1.4, where the outcome is predicted average annual earnings across unconditional, conditional, and sandwich earnings measures (see section 3.3 for descriptions of the annual earnings measures). Predicted earnings estimated on full sample of Texas high school graduates who enroll in a Texas postsecondary institution (excluding my analysis sample) with the following covariates: gender, race/ethnicity, standardized math and reading test scores, number of advanced courses taken in high school, suspensions, attendance, risk of dropping out, high school fixed effects, year of high school graduation fixed effects, college fixed effects, major fixed effects, number of cumulative semesters enrolled, and cumulative credits attempted. p-val gives the p-value of a test that the coefficient is equal to zero. Standard errors clustered at the application–college–year level.

earnings[32] on the following covariates: gender, race/ethnicity, standardized math and reading high school test scores, number of advanced courses taken in high school, suspensions, attendance, risk of dropping out, high school fixed effects, year of high school graduation fixed effects, college fixed effects, major fixed effects, number of cumulative semesters enrolled, and cumulative credits attempted. I then use the fitted values to predict earnings for my analysis sample. When matching these measures to my analysis sample, I use characteristics of the students' college experiences as measured in the semester when they submitted their transfer applications (i.e., the year before they intend to transfer).[33]

In Table 1.5, I estimate Equation 1.3 and Equation 1.4, where the outcome is predicted earnings, measured using my three different measures of earnings. If students do not manipulate their GPAs, we would expect to see these measures move smoothly through the discontinuity since these outcomes are measured using only pre-treatment characteristics. Evidence of a discontinuity may imply that the exogeneity assumption does not hold. The results show that, in most cases, the predicted earnings measures move smoothly through the discontinuity. In the case of unconditional earnings for four-year applicants, there does appear to be a small increase in predicted earnings at the discontinuity. However, this is not excessively concerning since positive estimates point toward positive selection. That is, students above the cutoff may have higher earnings potential than those below the cutoff. This selection runs against my main finding of zero to negative returns for transfer students, such that correcting for any potential bias would strengthen my results. I also

[32] I use each of the three annual earnings measured described in section 3.3.

[33] Students in my analysis sample with missing values for any of the covariates are excluded from the balance test.

estimate balance tests separately for flagship and nonflagship colleges to ensure that the exogeneity assumption holds in these subsamples and find similar results, shown in Appendix Table A3.

1.7 Main Regression Discontinuity Results

Bachelor's Degree Completion

Next, I investigate the effects of transferring on the first main outcome of interest: bachelor's degree completion. The reduced-form and instrumental variable (IV) results are shown in Table 1.6, where the top panel sample is applicants from two-year colleges and the bottom panel is applicants from four-year colleges. The first six columns measure degree completion based on time since intended transfer. Thus, "1 yr" is an indicator variable that takes a value of one if the student earns a bachelor's degree within one academic year since the semester in which she would first enroll at the target institution if she was accepted and chose to transfer.[34] The first row gives the reduced-form effect of crossing the threshold on bachelor's completion. For example, the interpretation of the third column for two-year applicants is that transfer applicants just above the GPA cutoff are 1.8 percentage points more likely than students just below the GPA cutoff to complete a bachelor's degree within three years of the semester for which they applied to transfer. These effects are also shown graphically in Figure 1.5 with binned scatterplots and local linear regression lines fit on each side of the discontinuity. However, the reduced form estimate is difficult to interpret because it applies to a mix of "compliers," whose transfer behavior would be changed by crossing the cutoff; "always takers," who would transfer even if they were just below the cutoff; and "never takers," who would not transfer even if they were just above the cutoff (Angrist et al., 1996). The second row gives the IV estimates that isolate compliers by scaling up the reduced-form estimates by the first stage.

For two-year applicants, the point estimates are positive across the board although only marginally significant in most specifications. However, the magnitude of the effect is quite stable at approximately 15 percentage points from two to six years after intended transfer. The final column gives the number of years between intended transfer and bachelor's completion for those who complete a degree. However, note that this measure does not have a clean causal interpretation since it is a selected sample of students who complete a degree. The $E[Y_0|C]$ row underneath gives the estimated base rate, i.e., the expected value of the outcome for compliers when untreated.[35] If we examine this value across years, the bachelor's completion rates for compliers who are not accepted for transfer are low within the first few years but quickly increase, even among students who apply to transfer from two-year colleges. This may seem counterintuitive since most two-year colleges do not award bachelor's degrees. However, these rates of bachelor's completion for untreated compliers are large because many students who are narrowly denied admission at a target college still end up

[34]Note that sample sizes change across years because students who applied to transfer in recent years are not observed for a long enough period to know whether they will complete a bachelor's within the longer time frames.

[35]Note that, because this value is for untreated compliers, it is estimated rather than taken directly from the data. See section B for details on the estimation of $E[Y_0|C]$.

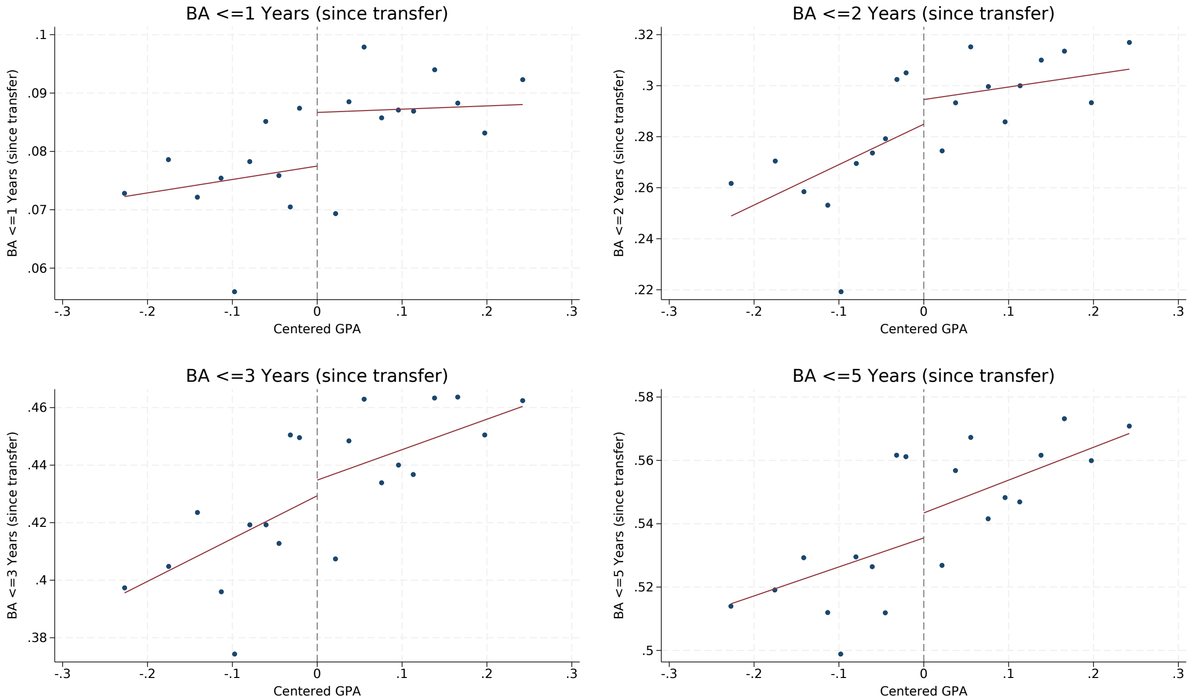
Table 1.6: Bachelor's Completion in Years Since Intended Transfer, Reduced-Form and Instrumental Variable Results

	BA within X years since intended transfer						
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs	Yrs to BA
Panel A: 2-year Applicants							
$\mathbb{1}(\mathsf{GPA}_i \geqslant \mathsf{T}_{cy})$	0.0093	0.017	0.018	0.018	0.020*	0.018	-0.034
	(0.0059)	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)	(0.061)
$\mathsf{TransferTarget}$	0.08*	0.15*	0.15*	0.15**	0.17**	0.16*	-0.31
	(0.043)	(0.076)	(0.082)	(0.078)	(0.085)	(0.088)	(0.46)
$\mathsf{E}[Y_0\|C]$	0.04	0.22	0.37	0.45	0.49	0.50	3.17
Obs	54,194	51,032	48,550	45,189	42,469	39,458	29,993
Panel B: 4-year Applicants							
$\mathbb{1}(\mathsf{GPA}_i \geqslant \mathsf{T}_{cy})$	-0.016	0.022	0.017	0.020	0.014	0.018	0.094
	(0.011)	(0.015)	(0.014)	(0.014)	(0.013)	(0.014)	(0.069)
$\mathsf{TransferTarget}$	-0.11*	0.16	0.12	0.14	0.099	0.12	0.77
	(0.066)	(0.10)	(0.089)	(0.086)	(0.086)	(0.088)	(0.52)
$\mathsf{E}[Y_0\|C]$	0.15	0.21	0.43	0.51	0.53	0.57	2.68
Obs	22,196	20,875	20,227	18,941	17,944	16,996	14,402

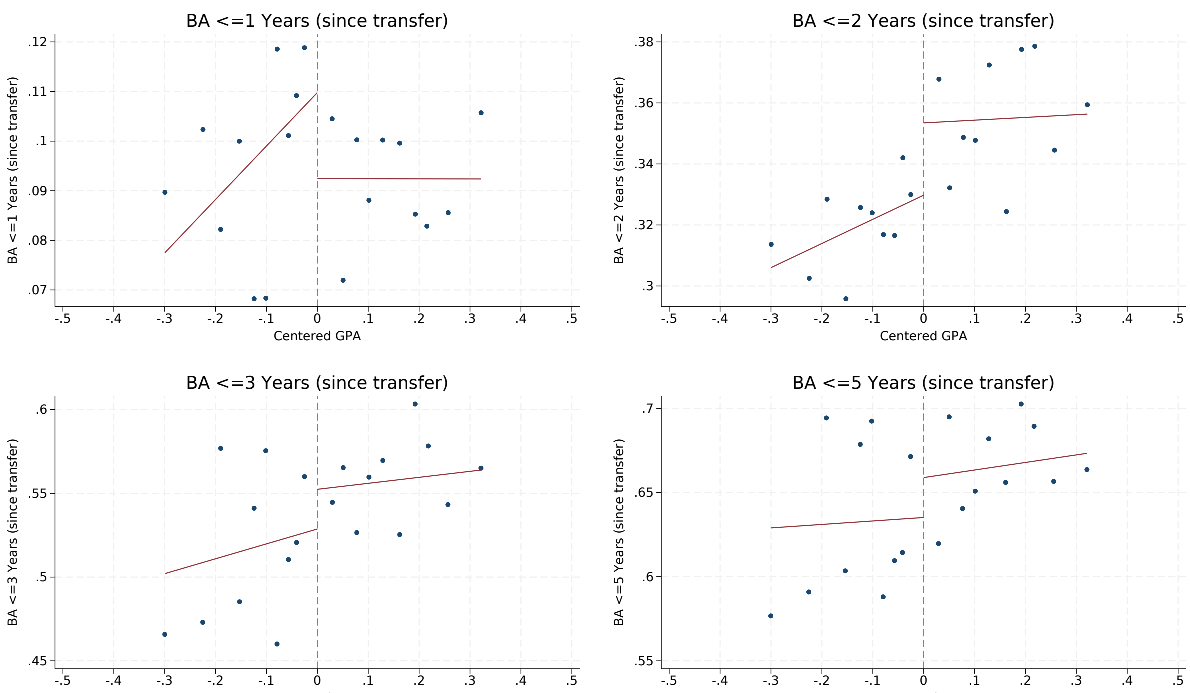
Notes: *** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(\mathsf{GPA}_i \geqslant \mathsf{T}_{cy})$ gives reduced-form estimates from equation (1.3); $\mathsf{TransferTarget}$ gives instrumental variable estimates from equation (1.4). Outcome in rows 1–6 is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 yrs indicates earning a bachelor's within 2 years of the semester for which the student applied for transfer). Yrs to BA gives the number of years between the intended transfer semester and bachelor's completion for those who completed a bachelor's. Top panel gives estimates for transfer applicants from two-year colleges and the bottom panel for applicants from four-year colleges. $\mathsf{E}[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Figure 1.5: Bachelor's Completion in Years Since Intended Transfer

(a) 2-Year Applicants



(b) 4-Year Applicants



Notes: Binned scatterplots of earnings outcomes on centered GPA with local linear regression fit on each side. Centered GPA is created by subtracting the college–year-specific cutoff from each student's GPA for each application she submits. Outcome is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 yrs indicates earning a bachelor's within 2 years of the semester for which the student applied for transfer). Top panel gives estimates for transfer applicants from two-year colleges and bottom panel for applicants from four-year colleges.

Table 1.7: 4-Year Applicants: IV bachelor's Completion in Years Since Intended Transfer, by Flagship Status

	BA within X years since intended transfer						
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs	Yrs to BA
Panel A: Flagship							
TransferTarget	-0.23**	0.13	-0.11	-0.071	-0.031	0.0072	0.61
	(0.12)	(0.19)	(0.15)	(0.14)	(0.14)	(0.15)	(0.49)
$E[Y_0\|C]$	0.23	0.34	0.78	0.86	0.86	0.85	2.48
Obs	11,037	10,305	10,305	9,753	9,363	8,880	8,432
Panel B: Nonflagship							
TransferTarget	0.021	0.16	0.29**	0.29**	0.20*	0.21*	1.34
	(0.070)	(0.11)	(0.13)	(0.13)	(0.12)	(0.13)	(1.31)
$E[Y_0\|C]$	0.05	0.10	0.08	0.16	0.20	0.28	2.93
Obs	11,160	10,571	9,923	9,190	8,583	8,118	5,973

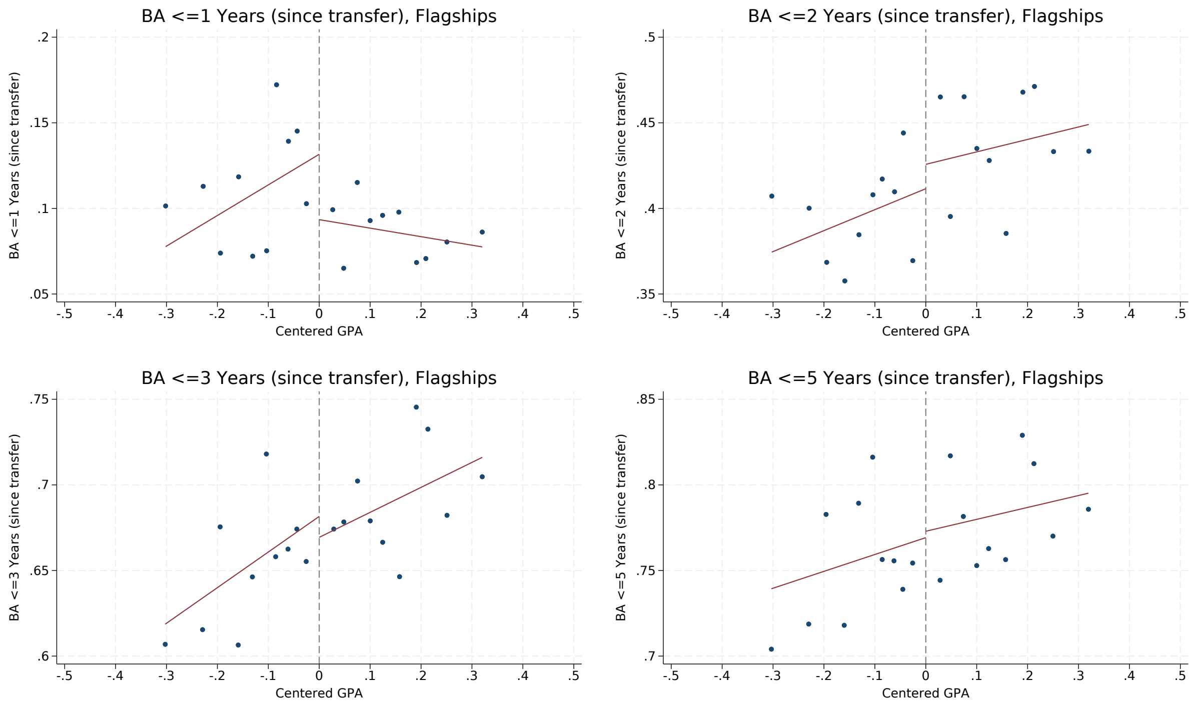
Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Outcome in rows 1–6 is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 yrs indicates earning a bachelor's within 2 years of the semester for which the student applied for transfer). Yrs to BA gives the number of years between the intended transfer semester and bachelor's completion for those who completed a bachelor's. Sample of transfer applicants from four-year college. Top panel gives estimates for transfer applicants to nonflagship colleges and bottom panel for applicants to flagship colleges. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

transferring to a four-year college eventually. I return to this issue and talk about how it affects the interpretation of the estimates in section 1.8.

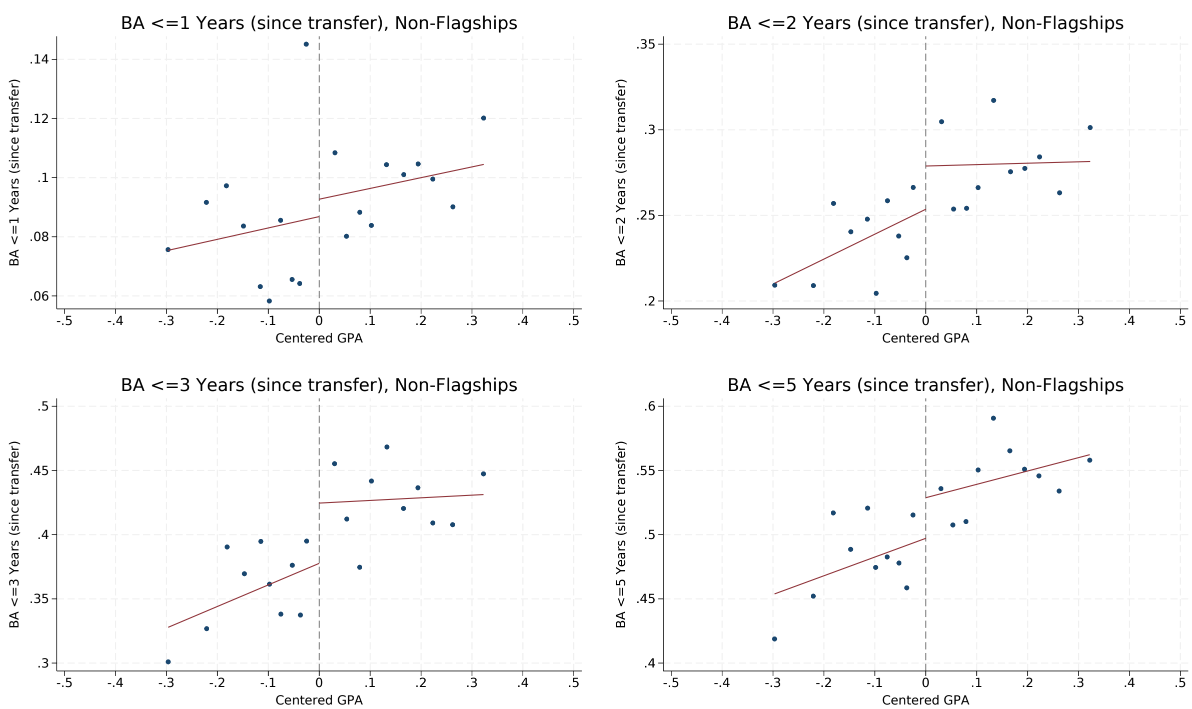
Table 1.7 focuses on four-year applicants and shows the same outcomes, but it breaks out flagship colleges from nonflagship target colleges and reveals that the average effects in panel B of Table 1.6 mask heterogeneity between these two groups. While the point estimates are positive in every column for students who transfer to nonflagship target colleges, they are mostly negative for students who transfer to flagship colleges. Focusing on flagship colleges, first note that the base completion rates are very high among this group: although only 23 percent of students have completed a bachelor's degree within one year, this figure climbs to 86 percent for completion within four years. While the estimates show short-term decreases in bachelor's completion rates for marginal transfer students, there do not appear to be long term differences in bachelor's completion rates relative to those who apply but are marginally denied admission. Moving to nonflagship colleges in panel B, the story is different. Transfer students are between 16 and 29 percentage points more likely to complete bachelor's degrees within two to six years of intended transfer. Although the statistical significance of these estimates varies over the time frames, the magnitudes are very large across the board, especially when we consider the base rates of bachelor's completion for this

Figure 1.6: Bachelor's Completion in Years Since Intended Transfer

(a) 4-Year Applicants to Flagship Colleges



(b) 4-Year Applicants to Nonflagship Colleges



Notes: Binned scatterplots of earnings outcomes on centered GPA with local linear regression fit on each side. Centered GPA is created by subtracting the college–year-specific cutoff from each student's GPA for each application she submits. Outcome is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 yrs indicates earning a bachelor's within 2 years of the semester for which the student applied for transfer). Top panel gives estimates for transfer applicants to flagship colleges from four-year colleges and bottom panel for applicants to nonflagship colleges from four-year colleges.

Table 1.8: Annual Earnings, Pooled across All Years

	Unconditional	Conditional	Sandwich
Panel A: 2-year Applicants			
$\mathbb{1}(GPA_i \geqslant T_{cy})$	-1,259**	-1,065**	-849*
	(490)	(500)	(502)
$TransferTarget$	-10,971***	-9,176**	-7,319*
	(3,835)	(3,741)	(3,754)
$E[Y_0 \mid C]$	37,206	46,123	48,667
Obs	534,472	417,026	399,979
Panel B: 4-year Applicants			
$\mathbb{1}(GPA_i \geqslant T_{cy})$	-140	-1,054	-1,115
	(727)	(817)	(851)
$TransferTarget$	-910	-6,403	-6,618
	(4,171)	(4,393)	(4,495)
$E[Y_0 \mid C]$	33,084	45,906	49,147
Obs	233,793	174,986	166,498

Notes:*** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(GPA_i \geqslant T_{cy})$ gives reduced-form estimates from equation (1.3); $TransferTarget$ gives instrumental variable estimates from equation (1.4). Observations are at person–year level. Unconditional earnings give average annual earnings over all quarters after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters, following Sorkin (2018). Top panel gives estimates for transfer applicants from two-year colleges and bottom panel for applicants from four-year colleges. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

subgroup. Three years after intended transfer, only eight percent of compliers below the threshold have earned a bachelor's, but this rate quadruples for students who transfer. The corresponding reduced form results are shown graphically in Figure 1.6. Appendix Table A4 shows an analogous table for applicants from two-year colleges, where the point estimates of the effects of transfer on bachelor's completion are positive across the board for both flagship and nonflagship colleges but very noisy.[36]

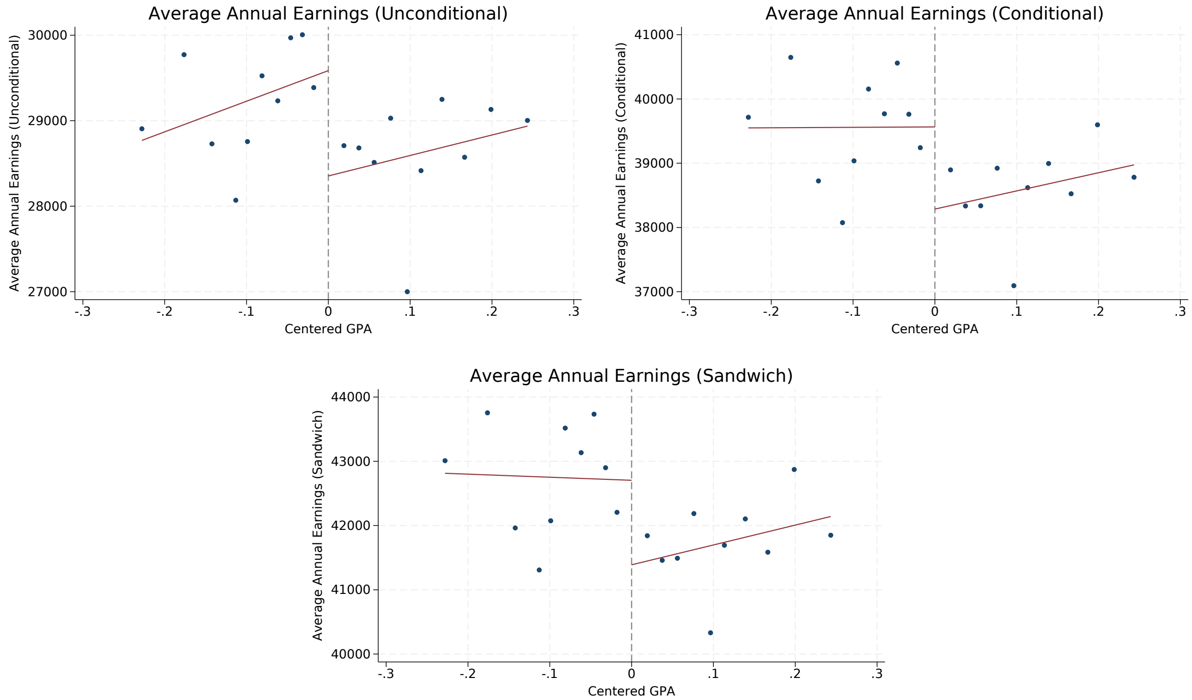
Earnings

The second main outcome of interest is earnings. My measures of earnings are annual, which means that the earnings data are at the person–year level. I present estimates from specifications that pool

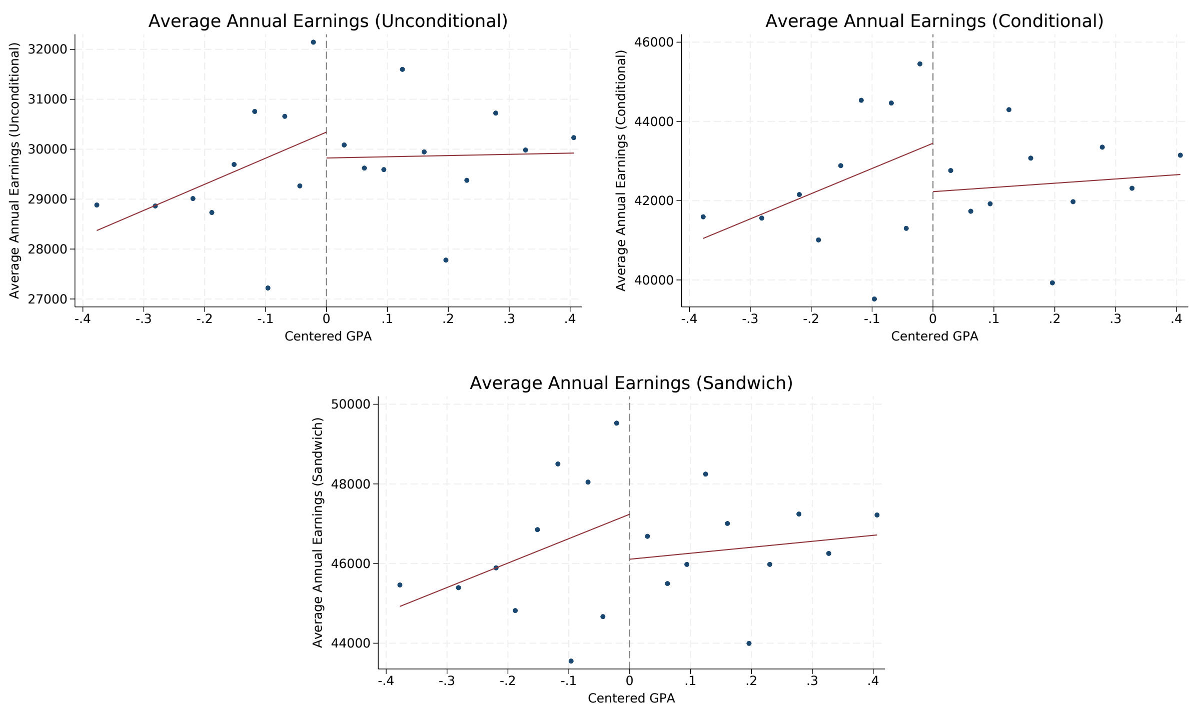
[36]The point estimates also indicate that effects may be larger at flagship colleges, but the coefficients are not statistically different from those for nonflagship schools.

Figure 1.7: Annual Earnings, Pooled across All Years

(a) 2-Year Applicants



(b) 4-Year Applicants



Notes: Binned scatterplots of earnings outcomes on centered GPA with local linear regression fit on each side. Centered GPA is created by subtracting the college–year-specific cutoff from each student's GPA for each application she submits. Unconditional earnings give average annual earnings over all quarters after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters, following Sorkin (2018). Top panel gives estimates for transfer applicants from two-year colleges and bottom panel for applicants from four-year colleges.

across the time since transfer and specifications that allow for effect heterogeneity by the time since transfer to offer a sense of the dynamics of earnings profiles over the life cycle. The first specification pools across all person–year observations, so the results can be interpreted as a weighted average of the effect of transfer on earnings over the next 1–21 years. Table 1.8 shows the results, where the top panel has estimates for the sample of transfer applicants from two-year colleges and the bottom panel for transfer applicants from four-year colleges. I present three measures of earnings: unconditional (i.e., including quarters with zero earnings), conditional (excluding quarters with zero earnings), and sandwich (including only positive quarters that are "sandwiched" between two positive quarters).[37] In each panel, the top row gives the reduced-form effect of crossing the GPA threshold on earnings, and the second row gives the IV results on the effect of transfer for compliers at the cutoff.

The top panel shows the surprising result that marginal students who transfer from two-year to four-year colleges earn substantially less than two-year college students who were marginally denied transfer admission to target colleges. These results are consistently negative across all three earnings measures, although the magnitude varies from just over $7,000 to nearly $11,000 less per year. Although they are noisy, these are large effects: a comparison with the base rates shows that they correspond to reductions in annual earnings of 15 to 30 percent. The bottom panel of Table 1.8 shows suggestive evidence of decreases in earnings for transfer from four-year colleges as well, but these estimates are not statistically significant. Figure 1.7 shows these results graphically with binned scatterplots and local linear regression lines fit on each side of the discontinuity.

Table 1.9 and Figure 1.8 shows these results broken out by flagship status for transfer applicants from four-year and reveals that any negative effects are fully driven by students who apply to transfer to flagship institutions. Although the estimates are imprecise, the magnitudes are quite large and suggest that, for students at four-year colleges, the effect of being marginally admitted to a flagship is not positive, and could likely be large and negative. Meanwhile, being admitted for transfer to nonflagship target institutions does not appear to have economically or statistically significant effects on earnings. Although the point estimate on unconditional earnings is large, it is near zero for the other two earnings measures. Appendix Table A6 shows the effects for two-year applicants broken down by flagship status, offering suggestive evidence of larger decreases for students transferring to flagship universities. However, the earnings estimates for those who transfer from two-year colleges to both flagship and nonflagship four-year colleges are negative, so I focus on the pooled results for two-year applicants since they are more precise and, in both cases, students are moving to better-resourced institutions. Conversely, for four-year applicants, I focus on those who transfer to flagship colleges since this is the negative effects are concentrated in this subgroup and since many students transferring to nonflagship schools are not moving to a better-resourced university.

We may also expect heterogeneity along a number of different demographic dimensions. For example, information frictions and the challenges of navigating the transfer system may play more

[37]See section 3.3 for details on the earnings measures and the motivation for using each.

Table 1.9: 4-year Applicants: Annual Earnings, Pooled across All Years, by Flagship Status

	Unconditional	Conditional	Sandwich
Panel A: Flagships			
$TransferTarget$	-8,199	-11,695*	-14,330*
	(5,342)	(6,870)	(7,357)
$E[Y_0\|C]$	37,184	51,946	57,007
Obs	123,410	88,765	83,814
Panel B: Nonflagship			
$TransferTarget$	6,941	-1,000	692
	(6,166)	(5,588)	(5,414)
$E[Y_0\|C]$	27,972	39,313	40,754
Obs	110,383	86,221	82,684

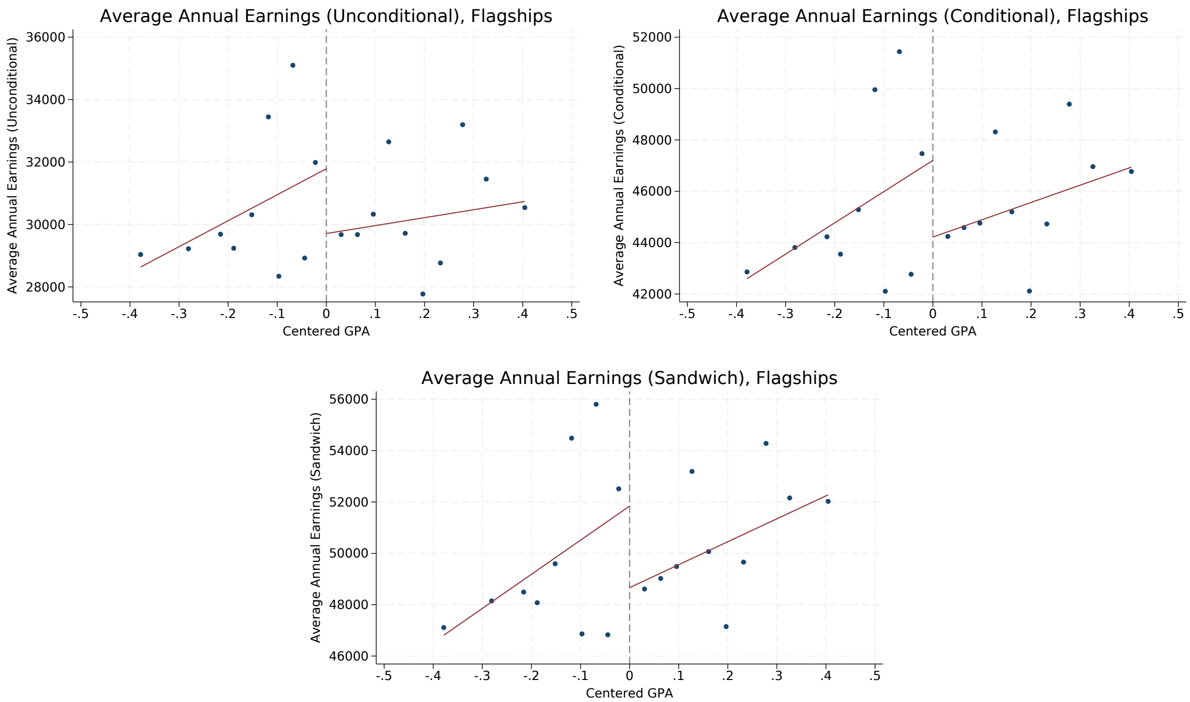
Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Unconditional earnings give average annual earnings over all quarters after the intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters, following Sorkin (2018). Both panels are limited to applicants from four-year colleges; top panel gives estimates for transfer applicants from to flagship colleges and bottom panel for applicants to nonflagship colleges. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

of a role for students of low socioeconomic status since they are less likely to have family and friends who have attended college. Men may be more likely to apply to colleges and majors for which they are academically "overmatched" (i.e., the average academic qualifications of students in the college are higher than those of the applicant) due to overconfidence (see Owen (2023) and references therein). I focus only on the results for two-year applicants broken down by gender since these are where I find the most evidence of heterogeneity. Table 1.10 shows that the negative earnings effects for two-year applicants are driven by men. This pattern aligns with the effects of bachelor's degree completion by gender in Table A5, which shows that, for applicants from two-year colleges, increases in bachelor's degree completion are concentrated among women.

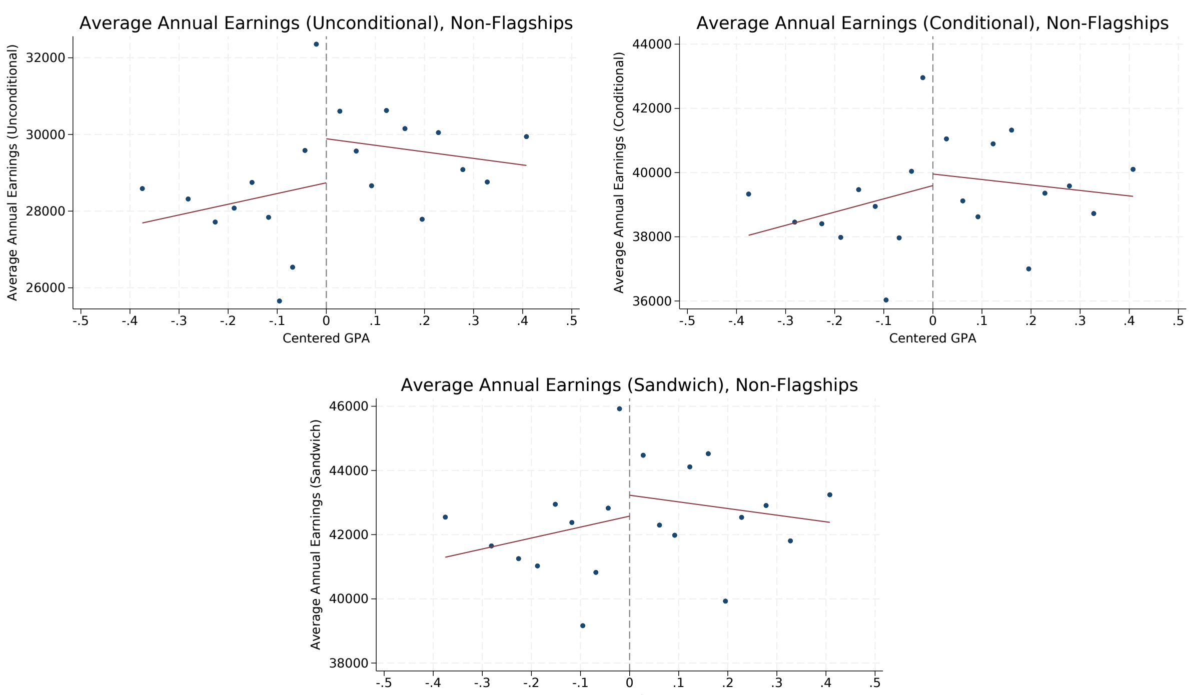
To offer a sense of how the effects change as individuals gain work experience and progress in their careers, Table 1.11 and Table 1.12 present the earnings effects separately by the time since intended transfer. To reduce variance, I estimate the effects in five-year earnings bins rather than individual years since transfer. The first bin corresponds to average annual earnings one to five years after transfer. For some individuals who complete their degree or drop out within one year of transferring, this will not include any years when they are still enrolled in college. For others, it may include some years of enrollment. I do not include the intended transfer year, as nearly all individuals are still enrolled at that time. The second bin averages earnings over six to ten years

Figure 1.8: 4-year Applicants: Annual Earnings, Pooled across All Years, by Flagship Status

(a) 4-Year Applicants to Flagship Colleges



(b) 4-Year Applicants to Nonflagship Colleges



Notes: Binned scatterplots of earnings outcomes on centered GPA with local linear regression fit on each side. Centered GPA is created by subtracting the college–year-specific cutoff from each student's GPA for each application she submits. Unconditional earnings give average annual earnings over all quarters after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters, following Sorkin (2018). Top panel gives estimates for transfer applicants from two-year colleges and bottom panel for applicants from four-year colleges.

Table 1.10: 2-year Applicants: Annual Earnings, Pooled across All Years, by Gender

	Unconditional	Conditional	Sandwich
Panel A: Women			
$\mathit{TransferTarget}$	-766.4	-3,837	-4,593
	(5,725)	(5,820)	(6,365)
$E[Y_0\|C]$	25,484	36,056	41,332
Obs	249,691	195,012	169,155
Panel B: Men			
$\mathit{TransferTarget}$	-19,073***	-12,950**	-10,953*
	(6,490)	(6,160)	(6,454)
$E[Y_0\|C]$	46,828	54,110	58,512
Obs	275,737	215,045	186,750

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Sample of transfer applicants from two-year colleges. Top panel gives estimates for women and bottom panel for men. Unconditional earnings give average annual earnings over quarters observed after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers for the estimate directly above it. Standard errors clustered at the application–college–year level in parentheses.

after transfer, giving estimates of early-career earnings effects, while the third and fourth bins show longer term results. If the negative effects of transferring are concentrated in early years in the labor market but dissipate over time, it may imply that the lifetime effect of transfer on earnings is minimal. However, Table 1.11 and Table 1.12 show that the earnings effects are persistently negative for both two-year students transferring to any four-year college and for four-year students transferring to flagship colleges. In both tables, the largest negative effects are for the bin corresponding to 11–15 years after transfer across all three earnings measures which are equivalent to over 20 percent of earnings for two-year students and approximately 30 percent for four-year students who transfer to flagship schools.

Since my earnings data come from administrative records of the state of Texas, there may be a concern that my effects are biased if transfer affects the probability of migrating out of state and out-of-state workers have systematically different earnings than those working in Texas. I address this in several ways. First, the use of the "conditional" and "sandwich" measures reduces the bias by dropping individuals who are working out of state from the sample rather than incorrectly recording them as having zero earnings. However, if students who transfer are more likely to leave the state and earn more out of Texas than students who do not transfer, there will still be selection bias in my estimates. To mitigate this concern and test whether transfer affects the probability of out-migration, I follow Grogger (2012) in using a series of continuous absences from administrative

Table 1.11: 2-year Applicants: Annual Earnings, by Number of Years Since Transfer

	Unconditional	Conditional	Sandwich
$TransferTarget$			
1-5 years	-3,962	-4,076*	-3,618
	(2,485)	(2,437)	(2,497)
$E[Y_0\|C]$	21,131	26,820	30,092
Obs	241,439	194,984	183,228
6-10 years	-12,918***	-12,754***	-10,607**
	(4,926)	(4,634)	(4,596)
$E[Y_0\|C]$	43,466	53,655	55,199
Obs	163,660	127,765	124,438
11-15 years	-23,784***	-19,737**	-16,477*
	(8,745)	(8,750)	(8,684)
$E[Y_0\|C]$	58,455	72,433	74,027
Obs	91,447	67,221	65,837
16+ years	-22,765	548.8	9,459
	(14,284)	(13,639)	(13,920)
$E[Y_0\|C]$	63,650	68,433	67,731
Obs	37,926	27,056	26,476

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Unconditional earnings give average annual earnings over quarters observed after the intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers for the estimate directly above it. Standard errors clustered at the application–college–year level in parentheses.

records to proxy for out-migration. Specifically, for individuals who transferred at least five years before the end of my data period (2021), I create an indicator variable that takes a value of one if an individual has no recorded earnings for the last five years for which their earnings could potentially be observed (i.e., no earnings from 2017 to 2021). I repeat this exercise with a window of 10 years rather than five.[38]

Table 1.13 shows that for both 2-year and 4-year applicants, there is no statistically significant effect of transferring to a target college on out-migration from the Texas workforce, implying that any bias from out-migration will be minimal. As a final test, I calculate which observable characteristics

[38]This exercise also tests for attrition due to self-employment or other jobs not included in the administrative earnings data if individuals who work in those jobs tend to stay in them rather than switching back and forth between self-employment and formal employment. Even if this is not the case, selection into self-employment is less of a concern in this setting since Foote and Stange (2022) show limited scope for bias using Texas administrative data linked to national data that include self-employment.

Table 1.12: 4-year Applicants to Flagship Colleges: Annual Earnings, by Number of Years Since Transfer

	Unconditional	Conditional	Sandwich
TransferTarget			
1-5 years	-1,541	-2,081	-3,081
	(3,758)	(4,224)	(4,798)
$E[Y_0\|C]$	14,265	19,642	23,868
Obs	50,763	37,661	33,835
6-10 years	-560.9	-2,385	-10,187
	(8,691)	(10,585)	(10,869)
$E[Y_0\|C]$	37,976	56,051	62,617
Obs	39,000	28,298	27,606
11-15 years	-18,845**	-31,412**	-31,768**
	(8,917)	(13,075)	(13,823)
$E[Y_0\|C]$	63,038	92,195	93,951
Obs	24,147	16,506	16,201
16+ years	-24,117**	-28,370*	-23,503
	(12,051)	(15,929)	(14,949)
$E[Y_0\|C]$	73,238	100,012	101,774
Obs	9,500	6,300	6,172

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Unconditional earnings give average annual earnings over quarters observed after the intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers for the estimate directly above it. Standard errors clustered at the application–college–year level in parentheses.

are most predictive of my proxies of out-migration using the full sample of Texas workers and then re-estimate my main effects after dropping the individuals who are most likely to migrate. These results, shown in Appendix Table A8, align with my main estimates, which provides additional assurance that out-migration from Texas does not drive my main effects.

1.8 Interpretation of Estimates

Decomposition of Local Average Treatment Effect

The main regression discontinuity IV estimates that I have presented identify a local average treatment effect (LATE). To interpret the effects, we need to understand both (1) which types of students identify the LATE and (2) what their counterfactual would be if they were below the GPA

Table 1.13: Out-Migration

	2-year Applicants No Earnings in Last		4-year Applicants No Earnings in Last	
	5 yrs	10 yrs	5 yrs	10 yrs
$TransferTarget$	-0.022	-0.045	0.034	-0.015
	(0.071)	(0.073)	(0.085)	(0.077)
$E[Y_0\|C]$	0.12	0.11	0.09	0.04
Obs	39,458	25,958	16,996	12,397

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Standard errors clustered at the application–college–year level in parentheses.

cutoff. More concretely, consider a standard potential outcomes framework where some individuals from a population receive a treatment D_i. Their potential outcomes are defined by $Y_i(0)$ if they do not receive the treatment and $Y_i(1)$ if they do. We observe $Y_i = Y_i(D_i) = D_i Y_i(1) + (1 - D_i)Y_i(1)$, and the object of interest is the causal effect of treatment, $Y_i(1) - Y_i(0)$. Suppose that we have a binary instrument Z_i that is independent of potential outcomes $Y_i(0)$ and $Y_i(1)$ but correlated with treatment D_i. Then, we can identify the local average treatment effect, i.e., the average treatment effect for individuals who would receive treatment if $Z_i = 1$ but not if $Z_i = 0$. This group of people, whose value of Z_i influences whether they receive treatment, are the “compliers." Some people would receive treatment regardless of their value of Z_i (“always-takers"), and some people would not receive treatment regardless of their value of Z_i (“never-takers"). We must assume that there are no “defiers," i.e., people who would receive treatment if $Z_i = 0$ but not if $Z_i = 1$, which seems innocuous in this setting.

In this context, I define the treatment to be transferring to a target college c in year t (i.e., the year in which the student applied for transfer), and the instrument is an indicator for having a GPA above T_{ct}. Thus, compliers are individuals who would transfer to target college c in year t if their GPA is above T_{ct} but would not transfer to target college c in year t if their GPA is lower than T_{ct}. Note that this is determined both by individuals’ actions and the actions of admissions officers at target colleges. First, because admissions officers consider other parts of individuals’ applications aside from their GPA (e.g., admissions essays, transcripts), some individuals with GPAs above the cutoff may not be admitted, and some with GPAs below the cutoff may be admitted anyway. Second, some individuals may choose not to transfer even if they are accepted, so they will be never-takers. Note that this assumes there is no causal effect of being admitted to a target college on students’ outcomes if they do not actually enroll there.

While the treatment of transferring to target college c in year t is well defined, the counterfactual determining $Y_i(0)$ is a bundle of possible pathways. Consider students at two-year colleges who apply but do not transfer to target college c in year t (i.e., untreated two-year students). Some of them may never transfer to any four-year college, but others may still transfer even though they are not treated, either by transferring to a nontarget college in year t or by not transferring in year t but transferring later in some year τ, where $\tau > t$ (either to a target college or a nontarget college). These different possible pathways for untreated students are observable in the data for students who do not transfer to a target college. We may be interested in the separate treatment effects for transferring to a target college c in year t relative to each of these potential counterfactual pathways, but these are not identified with only one instrument because we do not know which counterfactual pathway each treated individual would have followed had they been below the GPA cutoff.

Instead, the IV estimates are a weighted average of the effects of transferring to a target college in year t relative to the outcomes under each pathway. Specifically,

$$\hat{\eta}_1 = \Pr(Nev)\omega_{Nev} + \Pr(O_t)\omega_{O_t} + \Pr(TT_{\tau>t})\omega_{TT_{\tau>t}} + \Pr(O_{\tau>t})\omega_{O_{\tau>t}} \tag{1.6}$$

where $\hat{\eta}_1$ is the estimate of η_1 from equation (1.4). $\Pr(Nev)$ is the fraction of compliers who would never transfer to a four-year college if they were below the GPA cutoff, and ω_{Nev} is the treatment effect of transferring to a target college c in year t relative to never transferring to a four-year college. The next three terms are defined analogously, where O_t defines transferring to some other (i.e., nontarget) four-year college in year t, $TT_{\tau>t}$ defines transferring to a target college in some year τ later than t, and $O_{\tau>t}$ defines transferring to a nontarget college in some year τ later than t.

Fraction of Compliers in Each Counterfactual Pathway

Although the separate treatment effects (ωs) are not identified, the proportion of compliers who would fall into each category, $\Pr(Nev)$, $\Pr(O_t)$, $\Pr(TT_{\tau>t})$, and $\Pr(O_{\tau>t})$, is identified and can be estimated (see section B for details on the estimation). This tells us how much weight is being put on each treatment effect in the combined IV estimate. If the large majority of untreated compliers were to fall into one category, e.g., if almost all students who are rejected from a target college in year t never transfer to a four-year college, we could ignore the other categories and interpret the effects as being close to the effect of transferring to a target college relative to never transferring. However, the first row of Table 1.14 shows my estimates of the fraction of compliers who fall into each counterfactual category and reveals that only approximately one-third of untreated compliers never transfer to a four-year college. There are nontrivial shares in each of the other three categories (transfer to other college in year t, transfer to target college later, and transfer to other college later). Therefore, the IV results for the two-year applicants should be interpreted as the combination of the effect of transferring to a target college relative to never transferring, the effect of transferring to a target college relative to transferring to a nontarget college, and the effect of transferring earlier relative to later. The final two rows show the results for men and women separately and reveal that these two groups have a different mix of counterfactual pathways, which may explain the heterogeneity by gender in the effects of transferring to a target college on bachelor's completion and earnings.

Table 1.15 shows the fraction of compliers who fall into each counterfactual category for four-year

Table 1.14: 2-year Applicants: Fraction of Compliers in Each Counterfactual Category

	Never Transfer 4y	Transfer Other 4y Now	Transfer Target Later	Transfer Other 4y Later
All 2-year	0.33	0.19	0.29	0.19
Male	0.40	0.22	0.30	0.081
Female	0.24	0.16	0.27	0.31

Notes: Estimated fraction of compliers who fall into each mutually exclusive counterfactual outcome. Sample of all two-year applicants.

transfer applicants for the full sample and the subsamples broken down by flagship status. The possible counterfactuals for four-year applicants correspond to those of two-year applicants but add two categories for students who transfer from a four-year college to a two-year college either in year t or later. The second row of Table 1.15 shows that the most common counterfactual for students who apply to transfer to a flagship college is to never transfer and the second most common is to transfer to a nontarget four-year college in some year later than t. For those who apply to transfer to nonflagship schools, many students below the cutoff instead transfer to a two-year college, and very few never transfer. This tells us that the difference in results between flagship and nonflagship schools may be partly due to differences in the relevant counterfactual. The results for flagship schools will be closer to the results for transferring between four-years relative to never transferring, whereas the results for nonflagship schools are more similar to the results of transferring between four-year colleges relative to transferring from a four-year to a two-year college.

Selection on Observables Estimates of Effects Relative to Each Counterfactual

In principle, it is possible to separately identify the treatment effect relative to each counterfactual if there is enough heterogeneity in the relative first stages by observable characteristics (Caetano et al., 2023). Unfortunately, in this setting, observable characteristics are not very predictive of which pathway untreated students will take. This makes estimation of separate treatment effects as in Caetano et al. (2023) too imprecise to be useful.[39] Instead, to help interpret the RD results, I separately estimate ω_{Nev}, ω_{O_t}, $\omega_{T_{\tau>t}}$, and $\omega_{O_{\tau>t}}$ using ordinary least squares (OLS) with the sample of all college students in Texas who apply to transfer to a four-year college. In these specifications, I control for demographics, high school test scores, sending college, and all the other covariates included in Equation 1.2.[40] Since these estimates do not have the same clean identification strategy as the RD and instead rely on a "selection on observables" assumption, they are likely biased. The direction of the bias is almost certainly upward since students who are accepted for

[39]See Appendix Table A9 for the results of this exercise where I define each treatment relative to "never transfer." The standard errors are very large, such that the results are void of any meaningful information.

[40]The full list of covariates is as follows: gender, race, ethnicity, free or reduced-price lunch status, high school standardized test scores in math and reading, year of high school graduation, cumulative credits at the time of application, fixed effects for major at the time of application, and sending college fixed effects.

Table 1.15: 4-year Applicants: Fraction of Compliers in Each Counterfactual Category

	Never Transfer	Transfer Other 4y Now	Transfer Target Later	Transfer Other 4y Later	Transfer 2y Now	Transfer 2y Later
All 4-year	0.34	0.092	0.05	0.31	0.34	0.068
Nonflag	0.12	0.18	0.15	0.33	0.49	0.049
Flagships	0.51	0.009	<0.01	0.29	0.21	0.073

Notes: Estimated fraction of compliers who fall into each mutually exclusive counterfactual outcome. Sample of all four-year applicants.

transfer will be positively selected compared to observably similar students who are not accepted. Therefore, we can think of the OLS estimates as upper bounds on the true causal impacts of each treatment effect.

Table 1.16 and Table 1.17 give the results for two-year college students, where the label at the top of each column gives the counterfactual pathway of untreated students. For example, the sample in the the first column is all students who apply to transfer to a target college in year t and either (1) transfer in year t or (2) never transfer to a four-year college. Students following a different counterfactual pathway are not included. The estimate for $\mathtt{TransferTarget}$ is the average difference in earnings between students who transferred to a target college in year t and those who never transferred, with controls for my full set of covariates. $E[Y_0]$ gives the average earnings for untreated students, i.e., those who never transfer to a four-year college. Table 1.16 shows estimates that are pooled across all 1–21 years after intended transfer (analogous to those in Table 1.8), while Table 1.17 separately estimates effects by time since transfer (analogous to those in Table 1.11). These two tables give mixed evidence on the effect of transferring to a target college relative to never transferring. The estimates in Table 1.16 indicate that, on average, two-year students who transfer to a target college earn approximately $2,000 less per year than those who apply to transfer to a target college but never transfer. Since students who are accepted for transfer are likely positively selected yet the estimated effects are still negative, this lends additional evidence that the true causal effect of transferring to a target college relative to never transferring is negative. However, Table 1.17 reveals that, unlike the regression discontinuity results, the selection on observed variables estimates of transferring relative to never transferring are positive in the longer run. This discrepancy may be because the selection on observed variables estimates are biased upwards, or because the treatment effect of transferring for all students who apply to transfer is different than the treatment effect for marginally accepted students. Appendix Table A11 and Table A12 gives the OLS estimates for four-year applicants; they show persistent negative effects of transferring to a target college relative to never transferring.

The final three columns of Table 1.16 and Table 1.17 give the OLS estimates of the effect of transferring from a two-year college to a target college in year t relative to following the other three possible counterfactual pathways. The time patterns of the estimated effects relative to transferring

Table 1.16: All TX 2-year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals

	Counterfactual			
	Never Transfer 4y	Transfer Other 4y Now	Transfer Target Later	Transfer Other 4y Later
$TransferTarget$	-2,069***	386**	-134	50
	(134)	(180)	(98)	(275)
$E[Y_0]$	43,083	39,359	42,272	41,085
Obs	2,346,543	2,202,319	2,503,220	2,080,662

Notes:*** p<0.01, ** p<0.05, * p<0.1. Sample of all 2-year college students in Texas who apply to transfer to a target college. Outcome is average sandwich earnings pooled across the 1–21 years after intended transfer. Effects of transferring to target college versus the outcomes under each counterfactual listed at the top of the column, estimated by ordinary least squares with controls for all covariates. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

to a non-target college (either in year t or later) are similar to those relative to never transferring. However, the third column indicates that there may be longer-term negative returns to transferring to a target college in year t relative to waiting until later. Once again, the selection on observed variables effects are likely biased upwards because students who are accepted for transfer the first time probably have higher earnings potential than those initially denied transfer admission, so the true effects may be more negative. This implies that some students at two-year colleges may be better served by waiting until later to transfer, perhaps after they have gained more academic preparation. This is supported by evidence from the regression discontinuity design that the negative effects of transferring from a two-year college to a target college are concentrated among students with fewer credits at the time of transfer, shown in Appendix Table A10. This finding also aligns with prior research on the relationship between community college transfer timing and earnings, which shows that community college students who transfer after obtaining an associate's degree earn more, on average, than those who transfer without any degree (Belfield, 2013; Kopko and Crosta, 2016).

1.9 Mechanisms

Next, I turn to an exploration of *why* the regression discontinuity estimates of the returns to transferring to a target college are negative. Although these analyses are more speculative than the main results presented in section 2.5, they help shed light on factors that may contribute to the negative earnings effects for two-year students who transfer to four-year colleges and four-year students who transfer to flagship schools. I find evidence for the following channels: changes in field of study from high-earning to lower-earning majors, decreases in employment and experience, and changes in proximity of support networks. I do not find evidence for changes in industry of work, decreases in final GPA, or decreases in relative ranking within college based on GPA. For all of the following mechanisms analyses, I return to the IV specification as in Equation 1.4 but use

Table 1.17: All TX 2-year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals

	Counterfactual			
	Never Transfer 4y	Transfer Other 4y Now	Transfer Target Later	Transfer Other 4y Later
$TransferTarget$				
1-5 years	-5,605***	-1,455***	-605.8***	-2,248***
	(103.1)	(149.2)	(82.75)	(218.7)
$E[Y_0]$	34,154	27,990	29,021	28,343
6-10 years	364.4**	815.8***	504.0***	807.5**
	(176.1)	(230.8)	(119.7)	(320.1)
$E[Y_0]$	48,060	44,817	46,831	44,657
11-15 years	2,968***	3,210***	189.1	2,516***
	(295.6)	(383.5)	(212.2)	(485.6)
$E[Y_0]$	56,779	54,162	58,476	54,249
16+ years	4,401***	5,711***	-790.6**	3,837***
	(532.1)	(589.6)	(396.4)	(823.9)
$E[Y_0]$	63,824	60,177	68,070	62,427
Obs	2,346,543	2,202,319	2,503,220	2,080,662

Notes:*** p<0.01, ** p<0.05, * p<0.1. Sample of all 2-year college students in Texas who apply to transfer to a target college. Outcome is average sandwich earnings pooled across the 1–21 years after intended transfer. Effects of transferring to target college versus the outcomes under each counterfactual listed at the top of the column, estimated by ordinary least squares with controls for all covariates. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

alternative outcomes that may shed light on explanations for the negative earnings effects.

Field of Study

In addition to affecting degree completion rates, transfer may affect the *types* of degrees that students pursue, which can in turn affect earnings. For students transferring from a four-year college to a flagship, this appears to be an important driver of the negative earnings effects. I show this in Table 1.18, where I group students into 13 mutually exclusive categories based on the field of their bachelor's degree: general (e.g., liberal arts), sciences, engineering, health, business, education, social sciences, computer science, vocational studies, art, humanities, and others. Students who do not complete a bachelor's degree within 6 years of transfer fall into the "no degree" category. Each column is a separate regression where the outcome is an indicator variable for a student completing

Table 1.18: 4-year Applicants to Flagship Colleges: Field of Degree

	General	Science	Engineer	Health	Business	Educ	SocSci
$\mathsf{TransferTarget}$	0.12	0.11	-0.015	-0.094	-0.20**	0.013	0.20
	(0.075)	(0.16)	(0.065)	(0.094)	(0.084)	(0.009)	(0.17)
$E[Y_0\|C]$	0.01	0.05	0.09	0.06	0.20	<0.01	0.07
Obs	8,809	8,809	8,809	8,809	8,809	8,809	8,809

	CompSci	Vocational	Art	Human	Other	No Grad
$\mathsf{TransferTarget}$	-0.048	-0.035**	-0.048	0.038	-0.037	-0.004
	(0.048)	(0.016)	(0.062)	(0.15)	(0.095)	(0.12)
$E[Y_0\|C]$	0.02	0.02	0.05	0.15	0.14	0.15
Obs	8,809	8,809	8,809	8,809	8,809	8,809

Notes:*** p<0.01, ** p<0.05, * p<0.1. Sample of 4-year transfer applicants to flagship colleges. IV estimates from equation (1.4), where the outcome is an indicator variable for completing a bachelor's degree in the listed field within 6 years of transfer. Gen = general liberal arts major or undeclared. Educ = education. SocSci = social sciences. CompSci = computer science. Human = humanities. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers for the estimate directly above it. Standard errors clustered at the application–college–year level in parentheses.

her degree in the given major; the effects can be interpreted as the percentage-point change in the probability that a student will graduate with a degree in that major. Results show that among students who applied to transfer to a flagship college, those who were marginally admitted are much less likely to complete degrees in business, which is generally one of the highest-paying majors.[41] They are also less likely to major in a vocational field. Although not statistically significant, the point estimate indicates that the main field that students substitute into is social sciences.

To quantify how these changes in major might affect earnings, I use data on the earnings of all bachelor's degree holders in Texas to calculate average predicted earnings for each broad major category. Specifically, using years when individuals were the same age as those in my analysis sample, I regress earnings on fixed effects for each of these broad major categories to create a measure of average predicted earnings given the degree field.[42] I then assign these predicted earnings measures to my analysis sample based on their bachelor's degree major, where those

[41]Further investigation reveals that transfer students likely substitute out of business because they were not admitted to a business major—students can be broadly admitted to a university but not to every major. For example, in 2023, the average GPA of UT–Austin students who applied to switch their major to one in the business school and were granted admission was 3.87 (UT-Austin, 2023). I explore the timing of the major switching and find that the negative impact of transfer on holding a business major appears in the first semester after transfer, rather than when a student begins a major in business after transfer and switches later. Although these results are specific to UT–Austin, Bleemer and Mehta (2023) show that using GPA to restrict who can access business and other lucrative majors is common across many universities.

[42]To align the ages of nontransfer students with those in my analysis sample, rather than "time since transfer", I use "time since high school graduation" plus two years since the median transfer student applies to transfer two years after high school graduation.

Table 1.19: 4-year Applicants to Flagship Colleges: Predicted Annual Earnings Based on Field of Degree

	Predicted Unconditional	Predicted Conditional	Predicted Sandwich
$\mathsf{TransferTarget}$	-3,070	-3,090	-2,919
	(3,010)	(3,527)	(4,211)
$E[Y_0\|C]$	27,580	39,024	45,396
Obs	8,533	8,533	8,533

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Sample includes all individuals observed for at least 6 years following intended transfer. Predicted earnings are estimated using all Texas college graduates as described in the text. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers for the estimate directly above it. Standard errors clustered at the application–college–year level in parentheses.

without a bachelor's degree within six years of transfer are assigned to the "no BA" category. This measure will encompass the effects of transfer on both degree completion and changes in major. Table 1.19 shows the results for four-year applicants to flagship colleges across predicted versions of the same three measures of earnings presented in Table 1.9 and reveals that changes in major can account for approximately 20 to 40 percent of the total earnings effect, depending on the earnings measure used. However, the estimates are not statistically significant. Thus, while changes in major are an important mechanism, they are not the whole story. Additionally, shifts in field of study do not appear to be large drivers of the negative earnings results for students who transfer from two-year colleges; Appendix Table A13 shows that there is no clear pattern of transfer students moving from high-earning to lower-earning majors.

Employment and Experience

Transfer may additionally affect students' labor market outcomes through its effect on employment. Although employment and hours worked are not directly observed in the administrative data, I construct several measures that proxy for employment and full-time employment and present the results in Table 1.20. First, I create "Any Employment", an indicator variable that takes a value of one if an individual has any positive earnings within a given year. The second variable proxies for full-time continuous employment. Recall the sandwich earnings measure that proxies earnings under full-time employment by averaging only quarters "sandwiched" between two quarters with positive earnings. This is to avoid averaging over quarters when a worker was not working for a whole quarter because they began or ended an employment spell in the middle of the quarter. I use the presence of these quarters to proxy for frequency of continuous employment: "Continuous Employment" is an indicator variable equal to one if all four quarters in a year are sandwiched between two quarters with positive earnings. The "Quarters Worked" column gives the number of quarters with any positive earnings within the year, and "Sandwich Quarters Worked" gives the

Table 1.20: 2-year Applicants: Employment, Pooled across All Years

	Any Employment	Continuous Employment	Quarters Worked	Sandwich Quarters Worked
$TransferTarget$	-0.074	-0.13**	-0.34	-0.38*
	(0.051)	(0.057)	(0.21)	(0.22)
$E[Y_0 \vert C]$	0.85	0.60	2.97	2.66
Obs	534,472	534,472	534,472	534,472

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Observations are at person–year level. Any employment gives the probability of working at all in a given year. Continuous Employment is an indicator variable equal to one if all four quarters in a year are sandwiched between two quarters with positive earnings. Quarters Worked worked gives the number of quarters with any positive earnings within the year. Sandwich Quarters Worked gives the number of positive quarters that are "sandwiched" between two positive quarters. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers. Standard errors clustered at the application–college–year level in parentheses.

number of quarters worked that are "sandwiched" between two positive quarters. One complication with interpreting these results as effects on employment is the fact that individuals who do not appear in the earnings data may really be working outside the state of Texas. However, this concern is mitigated by the fact that I do not find evidence of transfer students being more likely to migrate out of Texas (see Table 1.13).

Table 1.20 shows that, among two-year students who apply to transfer to a target college, those who are marginally admitted work fewer quarters and have fewer years of continuous employment than those narrowly rejected. They are 13 percentage points less likely to be continuously employed each year. One may expect the negative effects of transfer on employment to be concentrated in the early years since transfer, while individuals who transfer are still enrolled in college. However, Table 1.21 shows results by time since intended transfer and reveals that the effects are driven by the later periods, well after the end of schooling for most individuals. 11-15 years after transfer, marginal transfer students are 27 percentage points less likely to be continuously employed and they have 0.8 fewer "sandwiched" quarters of work each year. These lower levels of continuous employment imply that marginal transfer students have more spells of unemployment and switch jobs more frequently than students who applied to transfer but were narrowly denied admission, perhaps because of a loss of support networks.[43] Appendix Table A14 shows that for applicants from four-year colleges who apply to transfer to flagship colleges, there is no statistically significant evidence of an effect of transfer on employment or quarters worked, although the negative point effects are sizable.[44]

Cumulative decreases in employment can lead to decreases in experience, another channel through which transfer can affect longer-term earnings. I measure experience by picking a point

[43]I explore this mechanism in section 1.9.

[44]Among four-year students who apply to nonflagship institutions, marginally being accepted for transfer increases employment and quarters worked. This explains the divergence in point estimates between the unconditional earnings and the other two earnings measures and suggests that transferring may increase labor force participation for students from this group.

Table 1.21: 2-year Applicants: Employment, by Number of Years Since Intended Transfer

	Any Employment	Continuous Employment	Quarters Worked	Sandwich Quarters Worked
$TransferTarget$				
1-5 years	-0.0373	-0.0966	-0.186	-0.211
	(0.0482)	(0.0617)	(0.208)	(0.229)
$E[Y_0\|C]$	0.83	0.55	2.89	2.51
Obs	241,439	241,439	241,439	241,439
6-10 years	-0.0603	-0.0606	-0.242	-0.231
	(0.0661)	(0.0704)	(0.262)	(0.268)
$E[Y_0\|C]$	0.84	0.60	2.97	2.68
Obs	163,660	163,660	163,660	163,660
11-15 years	-0.132	-0.273***	-0.616	-0.819**
	(0.0983)	(0.101)	(0.386)	(0.392)
$E[Y_0\|C]$	0.86	0.68	3.09	2.90
Obs	91,447	91,447	91,447	91,447
16+ years	-0.307*	-0.344***	-1.381**	-1.454***
	(0.160)	(0.123)	(0.561)	(0.533)
$E[Y_0\|C]$	0.96	0.67	3.27	3.00
Obs	37,926	37,926	37,926	37,926

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Any employment gives the probability of working at all in a given year. Continuous Employment is an indicator variable equal to one if all four quarters in a year are sandwiched between two quarters with positive earnings. Quarters Worked worked gives the number of quarters with any positive earnings within the year. Sandwich Quarters Worked gives the number of positive quarters that are "sandwiched" between two positive quarters. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers. Standard errors clustered at the application–college–year level in parentheses.

Table 1.22: 2-year Applicants: Experience 11 Years after Transfer, by Gender

	Number Years Worked	Number Quarters Worked	Number Sandwich Quarters Worked
Women	2.068	7.340	6.626
	(1.618)	(6.899)	(7.269)
$E[Y_0\|C]$	7.06	25.06	21.53
Obs	10,957	10,957	10,957
Men	-2.571**	-11.42**	-11.70**
	(1.093)	(4.744)	(5.235)
$E[Y_0\|C]$	10.63	39.54	35.50
Obs	12,220	12,220	12,220

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Observations are at person–year level. Number Years Worked gives the number of years with any positive earnings worked since transfer. Number Quarters Worked gives the number of quarters with any earnings worked since transfer, and Number Sandwich Quarters Worked gives the number of positive quarters "sandwiched" between two positive quarters worked since transfer. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers. Standard errors clustered at the application–college–year level in parentheses.

in time since intended transfer and adding up the number of years and quarters for which the individual has had positive earnings since intended transfer. In Table 1.22, I show the years of experience accumulated by 11 years after transfer.[45] Since the negative earnings effects for two-year applicants are concentrated among men, I show the effects separately for men and women. The results show that men who were marginally accepted for transfer have many fewer years of experience than men with GPAs just below the cutoff. By 11 years after transfer, they have had 2.5 fewer years with any positive earnings and over 11 fewer quarters with any earnings. The last column shows that they have also worked in fewer quarters as part of continuous employment spells. Meanwhile, the effect of transferring to a target college on experience for women is, if anything, positive, but the estimates are not statistically significant.

Loss of networks

The negative effects of transfer may be driven by students' losing access to their support networks. Qualitative literature has shown that transfer students have difficulties adjusting to their new environment and integrating socially into their new college (Flaga, 2006). While I cannot directly measure loss of networks, I shed some light on this mechanism by investigating how transfer affects students' likelihoods of attending college near their hometowns. I use students' high school location as a proxy for their hometown. I calculate the distance and travel time (driving) from each student's

[45] I choose 11 years to ensure that individuals transferred sufficiently long ago to have earnings in the 11–15 years after transfer bin, for which the negative earnings effects are the largest, but the results are not sensitive to my making other choices.

Table 1.23: 2-year Applicants: Distance and Travel Time from High School to College

	Distance (Miles)	Travel Time (min)	Within 30 min	Within 60 min
$TransferTarget$	17.6	24.5	-0.14**	-0.077
	(14.3)	(15.7)	(0.068)	(0.068)
$E[Y_0\|C]$	71.97	90.68	0.43	0.65
Obs	53,254	54,075	54,195	54,195

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Observations are at person–year level. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers. Standard errors clustered at the application–college–year level in parentheses.

high school to the last college that she attends.[46] Table 1.23 shows the results for two-year applicants. The first column gives the distance in miles "as the crow flies" (i.e., straight line distance) between students' high school and final college of attendance. The second column shows the driving time in minutes. The last two columns are indicator variables for whether each student attends a college within 30/60 minutes' driving time of her high school. Marginal transfer students do appear to attend college further from home than their peers who were narrowly denied transfer admission. They are 14 percentage points less likely to attend college within 30 minutes' driving time of their high school. Additionally, the point estimates imply that they attend college 18 miles and 25 minutes' drive further from their hometowns, but these estimates are not statistically significant. To the extent that being geographically near support networks is beneficial for students, this may contribute to the negative earnings impacts. Unfortunately, I cannot observe the geographic location of where each individual works, but since college graduates tend to work in the same local labor markets as the one in which they received their degrees (Conzelmann et al., 2022b), the effect of transfer on attending college further from home likely translates to working further from home, which could help explain the persistence of negative impacts.

GPA

Since the transfer students whom I focus on transfer to more selective colleges, it could be that they are academically unprepared and are not able to learn as much in the new college as they would have in their previous one. This loss of learning and human capital accumulation could be a driver of the negative earnings impacts later on. I investigate this channel by estimating the effects of being marginally admitted as a transfer student to a target college on subsequent GPA. In the first two columns of Table 1.24, I use final GPA as the outcome. In the first column, all transfer applicants are included regardless of whether they complete a degree. In the second column, I include only those who completed a bachelor's degree within six years of intended transfer. Neither

[46] Locations are recorded as geocoordinates, which come from the Common Core of Data (CCD) for high schools and the Integrated Postsecondary Education Data System (IPEDS). Distance is calculated "as the crow flies" with the Stata package *geodist*. Travel time is computed as the driving time in minutes with OpenRouteService.

Table 1.24: 2-year Applicants: Final Cumulative GPA and Relative Semester GPA

	Final GPA		Relative Rank from GPA			
	All	Graduates	1	2	3	4
$\mathtt{TransferTarget}$	0.041	-0.024	-0.086*	-0.036	0.038	-0.0005
	(0.06)	(0.07)	(0.05)	(0.05)	(0.05)	(0.06)
$E[Y_0\|C]$	2.31	2.55	0.42	0.37	0.34	0.36
Obs	67,172	38,733	45,496	42,682	36,911	34,445

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Sample of 2-year applicants. The first two columns use final cumulative GPA as an outcome, and the second column restricts the sample to include only bachelor's graduates. The outcomes in the final four columns is relative GPA rank in the first, second, third, and fourth semesters after intended transfer. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers. Standard errors clustered at the application–college–year level in parentheses.

estimate shows evidence of an effect of transfer on students' final GPA. In the final four columns, I investigate whether transfer students have GPAs that are low relative to those of their peers at their current college (rather than those of students at other colleges). To do so, I rank all students within a college by GPA in each semester. For this measure, I use the GPA only of classes taken in the current semester, rather than cumulative GPA. I then use the student's rank as the outcome in the regression, where a higher fraction is better ranked, e.g., where 0.75 corresponds to having a GPA that is higher than 75 percent of the GPAs of one's peers in the current college. In Table 1.24, the last four columns give the effect of being marginally admitted for transfer at a target college in the first, second, third, and fourth semesters after intended transfer. The results show that, while transfer students' relative GPAs dip in the first semester after transfer, there are no persistent effects. These results imply that changes in GPA are not large drivers of the negative earnings effect, although I note that GPA is not a perfect measure of learning. Therefore, it could be that transfer students really do learn less than they would have had they been denied transfer admission in a way that is not captured by this measure.

Industry

It is possible that transferring to a target college changes the type of industry that students work in, e.g., through connections that each college has with employers in certain industries. For each quarter of work in the administrative data, I observe the industry of employment. First, I create predicted earnings by 2-digit industry using the earnings records of all workers in Texas (not just the transfer sample), similar to how I measure predicted earnings by broad major group as described in section 1.9. I then match these predicted earnings measures to individuals' earnings in my sample earnings records in each year, based on their primary industry of work.[47] Appendix Table A15 shows the results for two-year applicants. While the point estimates are negative, they

[47]If a worker has earnings in two different industries within one year, I use the one with higher earnings.

are statistically insignificant and economically small compared to the magnitudes of the earnings decreases.

1.10 Conclusion

Over one-third of college students in the United States transfer between colleges at least once, yet little is known about the causal effects of these transfers. This paper is one of the first to provide rigorous causal evidence on the impact of transferring on educational and labor market outcomes. First, I use detailed application and admissions data from all public four-year universities in Texas to uncover the institution–year-specific GPA thresholds used in transfer admissions. I then pool data across colleges and years with cutoffs and use an RD design to estimate the effects of a student's being marginally admitted for transfer, net of the difference in student characteristics between those who do and do not transfer. My results show that, for my sample, transferring does not lead to earnings increases. Students who apply to transfer to a better-resourced college (two-year to four-year or four-year nonflagship to flagship) and are marginally admitted have large, persistent, negative earnings returns relative to students who were marginally denied transfer admission. For students who make lateral transfers between nonflagship four-year colleges, I find evidence of increases in bachelor's degree completion rates but no evidence of longer-term earnings gains.

Transfer, in principle, could be a cost-effective way for students to obtain bachelor's degrees, especially as place-based "promise" programs offering free community college grow in popularity (see Miller-Adams et al. (2022) for the growing list of states and localities that offer some form of a promise program). Widespread transfer is also a unique feature of higher education in the United States, offering more flexibility than in many other countries, where moving between colleges or even majors is heavily restricted. However, this paper offers a cautionary tale by showing that transfer can have negative impacts on marginal students' outcomes. This suggests that care must be taken in the structuring of transfer systems and the design of transfer policies.

In light of my findings, one policy response may be to change the pool of students who transfer so that they are more likely to succeed. This could be accomplished by raising the GPA cutoffs for transfer admission at these colleges or by providing more information to prospective transfer students about major-specific requirements so that they know whether they will be able to pursue their preferred major before making the decision to transfer. Another response would be to increase supports for transfer students. Prior research has shown that even marginal students who attend better-resourced colleges from the beginning of their college career see benefits (Hoekstra, 2009; Zimmerman, 2014), so we may also see benefits to transfer students if the support and programming for first-time students were extended to them. Another avenue would be to explore whether comprehensive support programs, which have proven to be effective for community colleges students (Weiss et al., 2019; Evans et al., 2020), could be extended to transfer students at four-year universities. Finally, since some of the decreases in earnings appear to be driven by substitution into lower-paying majors (especially at flagship universities), limiting barriers to lucrative majors may also help improve transfer students' earnings outcomes. In any case, future research is needed to further investigate the mechanisms behind the effects that I have uncovered and to determine which policy tools would be most effective in helping transfer students succeed.

A Supplementary Tables and Figures

Table A1: 2-Year Applicants: Sensitivity to Alternative Specifications, Earnings Pooled Across Years

	Baseline	Bandwidth						SE Clustering		Kernel	
Panel A: Unconditional											
TransferTarget	-10,971***	-9,427*	-10,872**	-10,826***	-10,994***	-10,723***	-9,923***	-10,971**	-10,971**	-12,130***	-11,071***
	(3,835)	(5,698)	(4,645)	(4,015)	(4,009)	(3,894)	(3,675)	(4,314)	(4,761)	(3,940)	(3,912)
$E[Y_0\|C]$	37,206	38,543	38,423	37,563	36,821	36,527	35,970	37,206	37,206	37,206	37,206
Obs	534,472	250,619	335,262	432,484	623,980	718,163	809,001	534,472	534,472	534,472	534,472
BW	0.3	0.15	0.2	0.25	0.35	0.4	0.45	0.3	0.3	0.3	0.3
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Uni	Epan
Clustering	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	GPA Bin	Send Coll	Appl Coll	Appl Coll
Panel B: Sandwich											
TransferTarget	-7,319*	-7,328	-7,968	-7,484	-7,792*	-8,186**	-8,382**	-7,319*	-7,319*	-8,439**	-7,262*
	(4,360)	(6,230)	(5,252)	(4,612)	(4,030)	(3,935)	(3,741)	(4,233)	(4,334)	(3,926)	(3,801)
$E[Y_0\|C]$	48,667	50,977	49,757	48,809	48,860	49,013	48960	48,667	48,667	49,175	48,667
Obs	399,979	187,918	251,263	323,954	466,721	537,810	605,905	399,979	399,979	399,979	399,979
BW	0.3	0.15	0.2	0.25	0.35	0.4	0.45	0.3	0.3	0.3	0.3
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Uni	Epan
Clustering	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	GPA Bin	Send Coll	Appl Coll	Appl Coll

Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Outcome is unconditional earnings in the top panel; sandwich earnings in the bottom panel. Tri = Tri kernel. Uni = Uniform kernel. Epan = Epanechnikov kernel. Appl coll = standard errors clustered at application–college–year level, GPA bin = standard errors clustered at GPA distance to the cutoff in 0.01 bin, Send coll = standard errors clustered at sending college-year level.

Table A2: 4-Year Applicants: Sensitivity to Alternative Specifications, Sandwich Earnings, Pooled Across Years, by Flagship Status

	Baseline	Bandwidth						SE Clustering		Kernel	
Panel A: Flagships											
TransferTarget	-14,330*	-12,990	-12,951	-13,535*	-14,909**	-15,222**	-15,418**	-14,330*	-14,330**	-19,422***	-14,561**
	(7,357)	(9,147)	(8,035)	(7,744)	(6,625)	(6,433)	(6,371)	(7,623)	(6,970)	(6,622)	(7,382)
$E[Y_0 \mid C]$	57,007	56,271	56,161	56,568	56,046	55,506	54,868	57,007	57,007	57,007	57,007
Obs	83,814	52,457	63,093	73,374	95,436	102,085	111,345	83,814	83,814	83,814	83,814
BW	0.4	0.25	0.3	0.35	0.45	0.5	0.55	0.4	0.4	0.4	0.4
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Uni	Epan
Clustering	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	GPA Bin	Send Coll	Appl Coll	Appl Coll
Panel B: Nonflagship											
TransferTarget	692	-3,632	-1,644	-196	1,023	1,235	1,380	692	692	2,183	1,558
	(5,414)	(8,001)	(6,908)	(6,058)	(4,984)	(4,834)	(4,766)	(4,159)	(5,909)	(4,954)	(5,213)
$E[Y_0 \mid C]$	40,754	42,340	42,083	41,098	40,550	40,450	40,183	40,754	40,754	40,754	40,754
Obs	82,684	49,565	61,058	71,502	93,241	103,482	116,128	82,684	82,684	82,684	82,684
BW	0.4	0.25	0.3	0.35	0.45	0.5	0.55	0.4	0.4	0.4	0.4
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Uni	Epan
Clustering	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	Appl Coll	GPA Bin	Send Coll	Appl Coll	Appl Coll

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Outcome is sandwich earnings , which averages only over quarters that are "sandwiched" between two nonzero quarters. Tri = Tri kernel. Uni = Uniform kernel. Epan = Epanechnikov kernel. Appl coll = standard errors clustered at application–college–year level, GPA bin = standard errors clustered at GPA distance to the cutoff in 0.01 bin, Send coll = standard errors clustered at sending college-year level.

Table A3: Balance Tests, by Flagship Status

	2-year Applicants			4-year Applicants		
	Unconditional	Conditional	Sandwich	Unconditional	Conditional	Sandwich
Nonflagship	-633	-493	-270	1,221	1,502	1,256
	(783)	(1,010)	(1,099)	(1,110)	(1,455)	(1,583)
p-val	0.42	0.63	0.81	0.28	0.31	0.43
	40,460	40,460	40,460	11,037	11,037	11,037
Flagship	-498	-634	-453	1,128*	829	518
	(1,319)	(1,532)	(1,595)	(568)	(725)	(871)
p-val	0.71	0.68	0.78	0.061	0.27	0.56
Obs	13,726	13,726	13,726	11,160	11,160	11,160

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Instrumental variables estimates of Equation 1.4 where the outcome is predicted average annual earnings across unconditional, conditional, and sandwich earnings measures (see text for details). Predicted earnings estimated on full sample of Texas high school graduates who enroll in a Texas postsecondary institution with the following covariates: gender, race/ethnicity, standardized math and reading test scores, number of advanced courses taken in high school, suspensions, attendance, risk of dropping out, high school fixed effects, year of high school graduation fixed effects, college fixed effects, major fixed effects, number of cumulative semesters enrolled, and cumulative credits attempted. p-val gives the p-value of a test that the coefficient is equal to zero. Standard errors clustered at the application–college–year level.

Table A4: 2-Year Applicants: bachelor's Completion in Years since Intended Transfer, by Flagship Status

	BA within X years since intended transfer						
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs	Yrs to BA
Panel B: Flagship							
$TransferTarget$	0.037	0.28*	0.21	0.2	0.24	0.30*	-0.43
	(0.092)	(0.16)	(0.16)	(0.13)	(0.16)	(0.17)	(0.45)
$E[Y_0\|C]$	0.03	0.34	0.62	0.75	0.78	0.74	2.83
Obs	14,095	13,117	12,801	11,942	11,461	10,734	10,319
Panel A: Nonflagship							
$TransferTarget$	0.089*	0.095	0.14	0.15	0.17*	0.12	-0.25
	(0.048)	(0.087)	(0.096)	(0.095)	(0.10)	(0.11)	(0.77)
$E[Y_0\|C]$	0.04	0.19	0.27	0.33	0.36	0.40	3.34
Obs	41,844	39,581	37,338	34,711	32,400	30,017	20,641

Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Outcome in rows 1-6 is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 yrs indicates earnings a bachelor's within 2 years since the semester for which the student applied for transfer). Yrs to BA gives the number of years between intended transfer semester and bachelor's completion for those who completed a bachelor's. Sample of transfer applicants from two-year college. Top panel gives estimates for transfer applicants to flagship colleges and bottom panel for applicants to nonflagship colleges. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Table A5: 2-Year Applicants: Bachelor's Completion in Years since Intended Transfer, by Sex

	BA within X years since intended transfer						
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs	Yrs to BA
Panel A: Women							
$TransferTarget$	0.096	0.23*	0.22	0.31**	0.31**	0.27*	-0.13
	(0.085)	(0.13)	(0.15)	(0.14)	(0.14)	(0.14)	(0.82)
$E[Y_0\|C]$	0.02	0.22	0.37	0.41	0.44	0.51	3.04
Obs	26,027	24,436	23,215	21,536	20,181	18,707	14,922
Panel B: Men							
$TransferTarget$	0.073	0.075	0.095	0.012	0.033	0.046	-0.39
	(0.059)	(0.11)	(0.11)	(0.11)	(0.12)	(0.13)	(0.80)
$E[Y_0\|C]$	0.05	0.21	0.32	0.43	0.47	0.44	3.04
Obs	28,166	26,595	25,334	23,652	22,287	20,750	15,070

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Outcome in rows 1-6 is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 yrs indicates earnings a bachelor's within 2 years since the semester for which the student applied for transfer). Yrs to BA gives the number of years between intended transfer semester and bachelor's completion for those who completed a bachelor's. Sample of transfer applicants from two-year college. Top panel gives estimates for women; bottom for men. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Table A6: 2-Year Applicants: Annual Earnings, Pooled Across All Years, by Flagship Status

	Unconditional	Conditional	Sandwich
Panel A: Flagships			
$TransferTarget$	-18,977**	-15,545*	-13,426
	(8,672)	(9,059)	(9,105)
$E[Y_0\|C]$	43,415	52,892	55,719
Obs	151,669	114,962	109,829
Panel B: Nonflagship			
$TransferTarget$	-7,184*	-6,486*	-4,666
	(3,984)	(3,789)	(3,821)
$E[Y_0\|C]$	34,014	42,824	45,079
Obs	382,803	302,064	290,150

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Sample of transfer applicants from two-year college. Top panel gives estimates for transfer applicants to flagship colleges and bottom panel for applicants to nonflagship colleges. Unconditional earnings give average annual earnings over all quarters after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings averages only over nonzero quarters. Sandwich earnings averages only over positive quarters that are "sandwiched" between two positive quarters following Sorkin (2018). $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Table A7: 4-Year Applicants to Nonflagship: Annual Earnings, By Years since Transfer

	Unconditional	Conditional	Sandwich
1-5 years	4,199	962	2,671
	(4,182)	(3,845)	(3,858)
$E[Y_0 \mid C]$	20,376	24,500	25,718
Obs	49,427	40,386	37,966
6-10 years	13,088*	5,475	6,514
	(7,955)	(7,246)	(7,068)
$E[Y_0 \mid C]$	31,337	42,881	44,143
Obs	34,030	26,646	25,966
11-15 years	1,124	-11,023	-7,239
	(12,834)	(12,613)	(12,185)
$E[Y_0 \mid C]$	39,896	68,738	71,691
Obs	19,888	14,500	14,172
16+ years	22,154	-23,389	-22,157
	(23,066)	(31,152)	(29,428)
$E[Y_0 \mid C]$	26,789	74,722	76,668
Obs	7,038	4,689	4,580

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Sample of transfer applicants from four-year colleges to nonflagship colleges. Unconditional earnings give average annual earnings over quarters observed after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings averages only over nonzero quarters. Sandwich earnings averages only over positive quarters that are "sandwiched" between two positive quarters. $E[Y_0 \mid C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Table A8: Annual Earnings, Pooled Across All Years, Individuals Unlikely To Migrate

	Unconditional	Conditional	Sandwich
Panel A: 2-Year Applicants			
$TransferTarget$	-11,120**	-9,358**	-7,428*
	(4,424)	(4,446)	(4,427)
$E[Y_0\|C]$	37,724	46,560	49,040
Obs	515,979	403,261	387,404
Panel B: 4-Year Applicants to Flagships			
$TransferTarget$	-8,700	-12,704*	-15,477*
	(5,780)	(7,284)	(7,639)
$E[Y_0\|C]$	39,220	54,309	59,347
Obs	117,050	84,552	80,134

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Sample of individuals with less than 50 percent predicted probability of migrating out of Texas. Observations are at person–year level. Unconditional earnings give average annual earnings over all quarters after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings averages only over nonzero quarters. Sandwich earnings averages only over positive quarters that are "sandwiched" between two positive quarters following Sorkin (2018). Top panel gives estimates for transfer applicants from two-year colleges and bottom panel for applicants from four-year colleges to flagship schools. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Table A9: 2-Year Applicants: Estimation of Multi-valued Treatment Effects

	No Controls	FEs only	Full control set
$\mathsf{TransferTarget}$	49,278	55,348	30,907
	(92,063)	(91,436)	(46,342)
$\mathsf{TransferOther4y}$	40,535	55,840	34,123
	(89,600)	(126,454)	(70,494)
$\mathsf{TransferTargetLater}$	65,586	69,025	47,561
	(56,972)	(69,717)	(49,600)
$\mathsf{TransferOther4yLater}$	112,049	124,315	74,876
	(264,110)	(216,828)	(108,899)
Obs	417,026	417,026	417,026

Notes: Estimates of separately identified treatment effects relative to "Never Transfer" using methods from Caetano et al. (2023), where I use predicted probabilities of each treatment estimated from the full set of observable characteristics in Equation 1.2. First column does not include any additional controls in the regression discontinuity; second column includes only application college-year fixed effects; third column includes all covariates as in Equation 1.4.

Table A10: 2-year Applicants: Annual Earnings, Pooled across All Years, by Amount of Credits

	Unconditional	Conditional	Sandwich
Panel A: Less Credits			
$TransferTarget$	-21,198***	-19,577***	-18,285**
	(6,458)	(6,964)	(7,111)
$E[Y_0\|C]$	39,475	49,506	52,683
Obs	279,149	215,354	205,749
Panel B: More Credits			
$TransferTarget$	-2,230	-1,631	555
	(5,808)	(5,230)	(5,209)
$E[Y_0\|C]$	36,182	44,141	46,122
Obs	255,323	201,672	194,230

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. IV estimates from equation (1.4). Observations are at person–year level. Sample of transfer applicants from two-year colleges. Top panel shows applicants with less than the median number cumulative credits at the time of application; bottom shows applicants with more than the median number of cumulative credits at the time of application. Unconditional earnings give average annual earnings over quarters observed after intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded as zero earnings. Conditional earnings average only over nonzero quarters. Sandwich earnings average only over positive quarters that are "sandwiched" between two positive quarters. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers for the estimate directly above it. Standard errors clustered at the application–college–year level in parentheses.

Table A11: All TX 4-Year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals

	Counterfactual					
	Never Transfer	Transfer Other 4y Now	Transfer Target Later	Transfer Other 4y Later	Transfer 2y Now	Transfer 2y Later
$Transfer Target$	-3,930***	296	2,077***	2,275***	113	1,654***
	(232)	(227)	(292)	(462)	(436)	(490)
$E[Y_0]$	48,007	38,863	39,309	37,876	36,599	36,522
Obs	506,750	476,152	373,292	339,184	343,795	329,653

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sample of all 4-year college students in Texas who apply to transfer to a target college. Outcome is average "sandwich" earnings pooled across 1-21 years after intended transfer. Effects of transferring to target college versus each counterfactual listed at the top of the column using ordinary least squares, controlling for all covariates. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table A12: All TX 4-Year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals, by Years Since Intended Transfer

	Counterfactual					
	Never Transfer	Transfer Other 4y Now	Transfer Target Later	Transfer Other 4y Later	Transfer 2y Now	Transfer 2y Later
$TransferTarget$						
1-5 Yrs	-4,985***	-4,522***	-3,032***	1,200***	-2,110***	-3,628***
	(212.8)	(341.5)	(198.0)	(210.7)	(371.0)	(401.5)
$E[Y_0]$	34,840	26,386	28,430	24,885	26,644	26,758
6-10 Yrs	-3,502***	2,429***	1,679***	2,968***	4,045***	4,757***
	(296.8)	(531.1)	(274.8)	(362.6)	(575.0)	(639.6)
$E[Y_0]$	57,632	44,466	47,430	47,170	44,632	44,042
11-15 Yrs	-2,836***	6,177***	5,055***	2,814***	7,594***	8,815***
	(474.9)	(853.5)	(470.5)	(613.4)	(862.7)	(1,102)
$E[Y_0]$	71,084	55,608	58,641	61,936	56,393	55,058
16+ Yrs	-2,703***	8,625***	7,751***	1,000	9,972***	10,051***
	(836.7)	(1,480)	(800.0)	(1,128)	(1,452)	(1,765)
$E[Y_0]$	78,394	63,471	65,643	73,166	65,760	65,621
Obs	483,365	327,049	453,863	354,824	322,521	313,408

Notes:*** p<0.01, ** p<0.05, * p<0.1. Sample of all 4-year college students in Texas who apply to transfer to a target college. Outcomes is average "sandwich" earnings, estimated separately by bins of years since intended transfer. Effects of transferring to target college versus each counterfactual listed at the top of the column using ordinary least squares, controlling for all covariates. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table A13: Predicted Annual Earnings Based on Field of Degree

	Predicted Unconditional	Predicted Conditional	Predicted Sandwich
Panel A: 2-Year Applicants			
TransferTarget	1,080	425.4	396
	(1,806)	(1,779)	(1,979)
$E[Y_0\|C]$	23,087	34,936	40,723
Obs	31,790	31,790	31,790
Panel B: 4-Year Applicants to Nonflagship			
TransferTarget	2,270	1,751	1,734
	(2,122)	(1,980)	(2,091)
$E[Y_0\|C]$	19,157	30,614	35,761
Obs	7,795	7,795	7,795

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Sample includes all individuals observed for at least 6 years following intended transfer. Top panel includes all 2-year applicants; bottom includes 4-year applicants to nonflagship colleges. Predicted earnings are estimated using all Texas college graduates as described in the text. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Table A14: 4-year Applicants: Employment, Pooled across All Years, by Flagship Status

	Any Employment	Continuous Employment	Quarters Worked	Sandwich Quarters Worked
Panel A: Flagships				
TransferTarget	-0.092	-0.020	-0.22	-0.17
	(0.074)	(0.063)	(0.27)	(0.26)
$E[Y_0\|C]$	0.81	0.52	2.71	2.39
Obs	123,410	123,410	123,410	123,410
Panel B: Nonflagship				
TransferTarget	0.19**	0.13	0.62*	0.53*
	(0.088)	(0.078)	(0.33)	(0.32)
$E[Y_0\|C]$	0.78	0.57	2.82	2.58
Obs	110,383	110,383	110,383	110,383

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Observations are at person–year level. Any employment gives the probability of working at all in a given year. Continuous employment Quarters worked gives the number of quarters with any positive earnings within the year. Sandwich quarters gives the number of positive quarters that are "sandwiched" between two positive quarters. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers. Standard errors clustered at the application–college–year level in parentheses.

Table A15: 2-Year Applicants: Predicted Earnings by Industry, Pooled Across Years

	Predicted Unconditional	Predicted Conditional	Predicted Sandwich
$TransferTarget$	-974	-980	-897
	(1,377)	(1,505)	(1,534)
Obs	417,717	417,717	417,717

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (1.4). Sample of 2-year applicants. Predicted earnings are estimated using all Texas workers as described in the text. Standard errors clustered at the application–college–year level in parentheses.

B Estimation of Counterfactual Probabilities for Compliers

This section explains how to estimate the fraction of untreated compliers who will follow each counterfactual pathway. I use $NeverTransfer_{ict}$ as an example, but note that the same procedure can be followed to estimate the value of any untreated outcome for compliers, $E[Y_0|C]$.

Consider one possible counterfactual pathway, $NeverTransfer_{ict}$, where student i never transfers to any college in year t or any year $\tau > t$. For each individual in the data, I observe this outcome, but our interest is the expected value of $NeverTransfer_{ict}$ for *compliers*. Precisely which individuals are compliers is not observed, but I estimate the fraction of compliers, always-takers, and never-takers from the first stage. Consider the expected value of transferring to a target college in year t given GPA and all other control variables and fixed effects from Equation 1.2, collectively referred to as $\mathbb{X}$,

$$E(TransferTarget_{ict}|GPA_i, \mathbb{X}_i) = \sigma_0 + \sigma_1 \mathbb{1}(GPA_i \geqslant T_{ct}) + m(GPA_i) + u_{ict} \tag{1.7}$$

The fraction of always-takers is given by σ_0, the fraction of compliers is given by σ_1, and the fraction of never-takers is given by $1 - \sigma_0 - \sigma_1$. Now consider the expected value of $NeverTransfer_{ict}$ times an indicator for being *not* treated, residualized against all controls $\mathbb{X}$,

$$E[(1 - D_i)NeverTransfer_{ict}|GPA, \mathbb{X}] = \psi_0 + \psi_1 \mathbb{1}(GPA_i \geqslant T_{ct}) + n(GPA_i) + \omega_{ict} \tag{1.8}$$

Let $C = \mathbb{1}$(Complier), $AT = \mathbb{1}$(Always-taker), and $NT = \mathbb{1}$(Never-taker). Because the expected value is multiplied by an indicator for not being treated, where treatment is defined as transferring to a target college in year t, this expected value is zero for always-takers. Since compliers are only treated when they are above the GPA cutoff, $E[(1 - D_i)|C]$ is equal to zero when $GPA_i \geqslant T_{ct}$ and equal to one when $GPA_i < T_{ct}$. $E[(1 - D_i)|NT]$ is equal to one on both sides of the cutoff. This implies that my estimate of $\dot{\beta}_1$, which estimates the size of the discontinuity in Equation 1.8, is given by,

$$\begin{aligned}\psi_1 =& Pr(NT)E(NeverTransfer_{ict}|Z = 1, NT) - Pr(NT)E(NeverTransfer_{ict}|Z = 0, NT) \\ &- Pr(C)E(NeverTransfer_{ict}|Z = 0, C)\end{aligned} \tag{1.9}$$

By definition, never-takers will not transfer regardless of whether their GPA is above or below the cutoff, so $E(NeverTransfer_{ict}|Z = 1, NT) = E(NeverTransfer_{ict}|Z = 0, NT)$. Thus, $\psi_1 = -Pr(C)E(NeverTransfer_{ict}|Z = 0, C)$. Since $Pr(C) = \sigma_1$, $E(NeverTransfer_{ict}|Z = 0, C) = -\psi_1/\sigma_1$.

2.1 Introduction

In the face of concerns about college affordability, tuition freezes and caps are becoming an increasingly popular policy tool for state governments to regulate public colleges. They are a rare set of policies that often receive bipartisan support. Both parties frame freezes and caps as beneficial for state residents, who will be enabled to affordably obtain a college education.

A tuition freeze or cap occurs when a state government sets limits on the amount that public colleges are allowed to raise listed tuition (i.e. "sticker price") from year to year. Typically, a "freeze" occurs when colleges are banned from raising nominal tuition at all. However, states will frequently impose limits on the percent that colleges are allowed to increase tuition (e.g. 3 percent/year), rather than fully freezing tuition. From 1990 to 2013, seventeen states implemented a tuition freeze or cap at least once, affecting 2-3 percent of institutions and 7-8 percent of students each year (**?**). These tuition regulations typically only affect the in-state undergraduate tuition level.

Under an effectively enforced tuition regulation, colleges should not be able to increase listed tuition by a large amount. However, at the same time, they may search for other ways to compensate for their tuition losses. Such responses, in turn, can yield different results from what the state government intended by imposing a tuition regulation. Previous studies have found that colleges adjust various margins in response to different financial shocks (**???????**), and which margin(s) colleges use under a tuition regulation is *a priori* ambiguous; they could decrease financial aid, hike up tuition once the regulation is lifted, or adjust other margins such as the composition of students by residency. Notably, depending on which margin universities adjust, tuition regulations could have different distributional implications.

Despite the prevalence of these policies and the *a priori* ambiguity in their effects, there has been little empirical evidence about the consequences of these tuition regulations. These effects are of direct interest to policy-makers considering these regulations, as well as to students and their families who may be subject to them.

Using a modified event study framework, we begin by estimating the effect of tuition freezes and caps on listed tuition to assess whether the regulations have any "bite". We find large heterogeneity in their effectiveness over time; tuition regulations have had large and statistically significant effects that have kept listed tuition from increasing in 2013 and earlier, but they have had no detectable effects from 2014 to 2019. We show that this is driven by a slowdown of tuition increases in recent years; in 2013 and earlier, institutions that were not under tuition regulations raised tuition by 6.3 percent, while the annual increase for these non-regulated institutions was 3.1 percent post 2014. This implies that colleges under tuition regulations are facing meaningful losses in tuition revenue in 2013 and earlier, but not in 2014 and later. Therefore, we expect to see colleges adjusting other margins such as institutional aid only in the earlier period, so we focus our analysis of outcomes other than listed tuition to the years before 2013.

Focusing on the earlier period, our primary finding is that although tuition caps and freezes reduce increases in “sticker price" tuition, they simultaneously induce universities to reduce increases in institutional financial aid, sometimes by a greater degree. This leads to an unintended consequence that when institutional aid is need-based, net benefit from a tuition regulation can be concentrated among richer students who do not receive institutional aid rather than needy students who do receive institutional aid. Dynamic changes in listed tuition and institutional aid over time have additional distributional impacts across cohorts, with some cohorts paying relatively higher tuition. Putting our results from the two periods together, our findings show that either these tuition regulations do not obtain their first-order goal of lowering listed tuition, or when they do, they simultaneously result in unintended distributional effects.

Specifically, we estimate that for four-year institutions, across all years a regulation is in place, the average yearly effect of a tuition regulation on listed tuition is -6.3 percentage points. To be precise, this means that listed tuition is 6.3 percentage points lower than it would have been in the absence of a regulation, and does not necessarily mean that tuition falls from year to year. All following effects should be interpreted in the same way. The corresponding impact on institutional aid is -11.3 percentage points. Two years after the end of the cap/freeze, listed tuition is 7.3 percentage points lower than it would have been if the regulation had never been in place while institutional financial aid is 19.5 percentage points lower. At two-year institutions, where the role of institutional aid is limited, colleges instead respond by rapidly increasing tuition once the cap/freeze has been lifted. During the regulation the impact on listed tuition is -9.3 percentage points; three years later it is only -4.8 percentage points and not statistically different from zero.

We probe for further heterogeneity in the four-year sector by estimating differential impacts by institution type. We find that institutions that are more dependent on tuition revenue lower financial aid more, and more quickly increase listed tuition after the regulation has been lifted. Similarly, we find that non-research universities adjust institutional aid more than research universities do.[1] These results imply that colleges with less monetary resources apart from tuition make larger adjustments to other margins in response to tuition regulations.

These responses from colleges imply that tuition caps and freezes have differential impacts on various groups of students. To give a sense of how this heterogeneity affects students moving through their education during and after a tuition regulation, we use our estimates to simulate the difference in net tuition paid from students' points of view. We consider students who vary in terms of 1) whether they receive institutional aid, 2) which type of institution they enroll in, and 3) when they first enroll with respect to the timing of the regulation. Our results imply that states that implement a uniform regulation on all colleges within the state may be creating inequalities in the way the regulation is felt by various students. Depending on the type of student we consider, our estimates range from a student receiving a 5.9 percent discount to having to pay 3.8 percent more over four years of college due to the regulation.

[1]A university is *More Dependent* if its fraction of total revenue from tuition and fees is greater than the median among public institutions. Research universities are defined as doctoral institutions with a Carnegie classification of high or very high research activity.

While we focus on the effects on tuition and net tuition, we also extensively investigate effects on other outcomes such as room and board charges, instructional expenditure and the composition of students by residency and academic preparedness. We do not find any of them to be as important as adjustments in institutional aid, although we do find suggestive evidence that instruction-related expenditures per student are 3.3 percentage points lower under tuition regulations. These null results on other margins could be attributed to the fact that colleges are restricted in the changes they can make. For example, universities can not pool revenue from different sources when some part of the revenue is earmarked to pay for certain expenses by their budgeting practice or outside entities (**??**). In 2010, 21% and 38% of total revenue of four-year and two-year institutions was restricted to be used for certain expenses.[2] Such restrictions can reduce incentives for increasing room and board prices, for instance, when universities can not shift the revenue to expenses sourced by tuition revenue.

Our paper fits into a literature investigating how colleges respond to financial shocks. Previous papers have studied implications of changes in state funding (**???**) or federal funding (**??**). Among various outcomes, changes in listed tuition are often found to be the main channel through which universities adjust to financial shocks. For example, **?** finds that decreases in state funding are partially passed on to students through increases in tuition. He finds that on average between 1987 and 2014, students bear 25.7 percent of the financial burden from state funding changes. Similarly, **?** find that in response to the expansion of federal Pell grants during the Great Recession, public colleges raised tuition to fully capture the increase.

We study a different type of shock on colleges' revenue: tuition regulations. Tuition has been becoming a increasing share of universities' revenue due to steady decreases in state funding in recent decades. Standing in contrast to other shocks, universities cannot adjust listed tuition to recoup the loss from the shock, by design of the regulation. We find institutional aid to be the most important margin that institutions adjust. While changes in tuition mostly yield distributional consequences from one cohort to another, changes in institutional aid can further result in an unequal distribution of benefits within each cohort.

Several papers have documented that universities often adjust institutional aid to capture additional revenue. **?** find that institutional aid decreases during recessions. **?** shows that institutional aid is crowded out by federal Pell grants, with universities giving less institutional aid to students with higher Pell grants. In contrast to these papers that study a targeted policy (Pell grants) and a non-policy shock, we study a policy that is seemingly universal, at least among in-state undergraduate students. However, we show that even though tuition regulations are applied equally to all students paying in-state tuition, they can have different impacts across students because of institutions' responses of decreasing institutional aid.

Our paper also aligns with the small set of papers that focus on the tuition regulations specifically. **??** study impacts of the Illinois 2004 "Truth in Tuition" law, which requires flat tuition rates for 4 years for each cohort of students. They find that colleges increase tuition before cohorts enter in

[2]Source: IPEDS

anticipation of not being able to increase it later. We use policy variation from all states in the US over the longer period (1990-2019) and find no anticipatory behavior but a statistically significant and economically meaningful response of changes in institutional aid in the first two decades. Relatedly, **?** exploit tuition freezes and caps in their analysis of whether increasing expenditures or lowering tuition is more effective in increasing enrollment and graduation at public colleges. They find a strong "first stage" effect of tuition caps/freezes on listed tuition; our results support this while adding the finding that the decrease in listed tuition is accompanied by decreases in institutional aid. This may be key to explaining the **?** finding that lower tuition (instrumented with tuition freezes) does not have a strong effect on total enrollment or graduation rates. We also add to this literature by examining heterogeneity in the type of regulation (i.e., cap or freeze and length of regulation) and university characteristics. Finally, we illustrate how this heterogeneity affects different types of students based on their timing of entry into college, the type of institution they attend, and whether they receive institutional financial aid.

The rest of the paper proceeds as follows: section 2.2 describes the institutional background and the data sets for our analyses, section 2.3 lays out a conceptual framework to frame empirical results, section 2.4 describes our empirical strategy and identification, section 2.5 presents results, section 2.6 illustrates the impact of a tuition regulation on a representative student by putting estimates together, and section 2.7 concludes.

2.2 Institutional Background and Data

The setting for our study is higher education institutions in the United States. Our primary analysis will be from 1990 to 2013, although we will show some specifications with more recent years (through 2019). We are interested in legislative tuition regulations and do not consider tuition freezes/caps initiated by colleges.[3] These tuition regulations almost exclusively affect only in-state undergraduate tuition; colleges are not regulated on how to set graduate tuition or out-of-state undergraduate tuition. Students fees are often regulated together with tuition, but financial aid is rarely regulated.[4]

These regulations are often put forth by politicians in an aim to make college more affordable for state residents. They are typically enacted as a part of the state higher education budget. This budget goes through multiple rounds of revisions. In addition to the general uncertainty of whether budget requests will be fully funded (which depends in part on tax revenues), there is uncertainty whether the tuition regulation will be enacted at the end of the budget process. There have been cases where either the upper house or the lower house of a state legislature proposes a bill for a tuition regulation but it does not pass the other house or the governor.[5] The duration a tuition

[3]For example, see **?**.

[4]We found only one instance of tuition regulation packaged with institutional aid regulation (Rhode Island 2013-14 HB 7133, 2014-15 HB 5900).

[5]E.g., Georgia 2016-17 HR 1326, Georgia 2018-19 SR 215, Tennessee 2014–16 HB 2179/SB 1683, Texas 2017-19 SB 19, Virginia 2018-19 HB 351).

regulation is often aligned with the duration of the budget bills because of this process; budget bills are sometimes done less frequently than annually such that a multi-year tuition regulation might be put into place. Tuition regulations could be extended to another fiscal year term when they are re-authorized along with the new budget bill, otherwise lifted. The uncertainly embedded in the budget approval process implies that it would be hard for an individual university to predict an upcoming tuition regulation.[6]

In this study, we will combine data sets from various sources. The main data is the Integrated Postsecondary Education Data System (IPEDS). IPEDS is a survey of colleges, universities and vocational institutions conducted annually by the U.S. Department of Education. All colleges that receive Title IV federal funding are required to report their data to IPEDS, so it is a universe of public colleges in the United States and a near universe of private colleges (aside from some for-profit institutions). IPEDS collects information on tuition and enrollment by student residency (i.e. in-state/out-of-state) status. IPEDS also collects detailed information on institutional finances and student financial aid, including revenues and expenditures by source.[7]

Our second data set is tuition regulations by state, detailing in which states and years tuition regulations were imposed. This data set, which we take from **?**, distinguishes between tuition freezes and caps, and records the specific limits for tuition caps. In secondary analysis, we augment this data set by hand-collecting tuition regulations from 2014 to 2019 from state legislation. We collect this legislation through a combination of Lexis-Nexis searches of legislation and news articles, communication with state boards of education and legislatures, and verification using legislative records from state websites. We also double-check the data set from **?**, making a few adjustments where we find discrepancies between their data and legislative records.

For our primary time period of focus, 1990-2013, 17 states imposed formal price regulations on public institutions at least once. For these 17 states between 1993 and 2013, 26.7 percent (109 out of 408) of state by year observations were under tuition regulations. In around half of these cases, institutions were under tuition freezes. The rest were tuition caps, with the exception of one case where institutions were mandated to cut tuition (Virginia, 2000). The caps ranged from three percent to 10 percent limits on increases in tuition. While some states imposed uniform price regulations on all public institutions, others differentiated by sector (see Table A8 and Table A9). Table A23 shows the full array of when and where freezes and caps were in place.[8]

Sometimes these regulations lasted for only one year, but they were often extended for multiple continuous years. When counting a regulation continued over multiple years as one regulation, 40% of regulations were lifted after one year (See Figure A1 for the whole distribution). Finally,

[6]Our informal conversation with government relations officials at public universities indicate that tuition regulations are imposed with very little warning.

[7]We supplement our data with IPEDS finance data constructed and published by the Urban Institute (**?**). While the Delta Cost Project is well known to aggregate multiple institutions within some public university systems into a single administrative unit (**?**), the Urban Institute data leave that decision to the data user by reporting raw finance data and parent-child relationship among institutions (i.e., branches of a university system). In our analysis of state appropriations (presented in appendix Table A19), we do not aggregate parent-child observations.

[8]Note that no regulations are in place in 1990 (our first year of data) so that we are starting with all "control" institutions.

Table A1 presents summary statistics of variables of interest by institution type (private/public, 4-year/2-year), with the first two columns showing statistics of institutions under tuition freezes or caps.

Our final two data sources consist of state level economic and political variables. First, we proxy for states' economic environments with unemployment rates from annual county level labor force data (**?**). Second, we construct a variable indicating the majority party of each state's lower and upper legislative houses based on election data collected by **?**. This data covers each individual candidate who ran for state legislative office, with general election returns between 1990 and 2015, which we aggregate to the state by year level.

2.3 Conceptual Framework

Although we do not explicitly model a colleges' objective function, here we provide a general conceptual framework to provide context to our empirical results. Previous literature has shown that public universities do not necessarily act as profit-maximizing firms and that student characteristics (e.g. academic ability or socioeconomic status) can compose a main part of their objective function (**???**). Public universities maximize their objective function subject to a budget constraint. Our study focuses on responses to a change in the universities' ability to choose a key part of that budget constraint, namely, tuition. Diminishing state appropriations have made tuition revenue an increasingly important revenue source over the past 30 years.

In a given year, a college may optimally decide to increase listed tuition for several possible reasons. They may want to generate more revenue that can be used to increase quality (e.g. increase instructional expenditures). Alternatively, they may want to increase listed tuition while simultaneously increasing targeted financial aid so that they can enroll more students from the groups they care more about (e.g. high-ability students or low-income students). A tuition cap or freeze may force a college to deviate from its optimal tuition level. Still subject to a budget constraint, universities may seek to increase other revenue sources to recoup losses in tuition revenue. Part of these losses could be offset by more generous state funding. In our analysis, we see that being subject to a tuition regulation is associated with a 6 percentage point increase in state appropriations, which could be a result of negotiations between universities and state governments. But given that the tuition revenue is nearly one-third of total revenue on average, this might not be enough. Some universities could have other means such as donations, their endowments, or other university-run businesses, while other universities need to meet their budget constraint solely by decreasing expenditures.

Given this, we should expect to see adjustments along other margins such as changes in institutional financial aid or instructional expenditures. Which margin(s) will a university adjust? The answer could depend on many factors such as what other components construct the university's objective function, what other margins a state government regulates (e.g. number of out-of-state students), and the university's degree of market power in the higher education market. We neither explicitly model a college's objective function/the higher education market nor collect all information on other regulations. However, we interpret our results considering these factors, and furthermore our empirical results can shed light on universities' behavior.

Depending on which margin(s) universities adjust, how evenly impacts are distributed across students will vary. Some margins, such as changes in required student fees, could be expected to affect all students relatively evenly. Other margins may disproportionately affect certain groups of students. In the case of institutional aid, it is clear that students who receive institutional financial aid will be hurt more than students who pay "sticker price". In other cases such as instruction-related expenditures, the equity effect is more ambiguous and hinges on the relationship between universities' expenditures and it's heterogeneous effect on students.

2.4 Empirical Strategy

We use a modified event study framework to estimate the effects of tuition regulations on the dynamics of institutions' "sticker price" tuition and institutional financial aid. Together these two determine net price, which is more relevant than sticker price alone. Not all students receive institutional financial aid, so for some their change in net price will be equal to the change in sticker price tuition. However, for students who receive institutional aid, their change in net price will depend both on changes in sticker price tuition and on changes in institutional aid. Thus, if universities adjust financial aid in response to tuition regulations, they can have distributional impacts across students depending on whether they receive aid. We are also interested in the dynamics of how tuition and aid change during and after the regulations because these changes could differentially impact students depending on the timing of their college entry. For our benchmark specification, we estimate

$$
\begin{aligned}
y_{it} = & \sum_{k=-3, k\neq -1}^{3} 1(\mathrm{TuitReg}_{t-k})_{it}\beta_k + \beta_4 \sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it} \\
& + \beta_{-4} \sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it} + \gamma_t + \phi_i + t\rho_{c(i)} + \beta_X X_{s(i)t} + u_{its} \qquad (2.1)
\end{aligned}
$$

where $1(\mathrm{TuitReg}_{t-k})_{it}$ is an indicator equal to 1 if institution i is under a tuition cap or freeze in year $t-k$ Observations more than 4 years before or after a tuition regulation are captured by $\sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it}$ and $\sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it}$, respectively.[9] γ_t is a calendar time fixed effect, ϕ_i is an institution fixed effect, $t\rho_c$ is a public/private-specific linear time trend, and X_{st} is a vector

[9]In other words, we impose a constant coefficient for all periods 4 or more years before (after) the tuition regulation to deal with differential timing of tuition regulations; the only regulations for which we observe many pre- or post-periods are those with regulations at the tail ends of the data. **?** use a similar strategy under a research design where an institution is treated at most once. Under such design, one can replace summations with the following dummy variables $1(\mathrm{TuitReg}_{t+k}, k \geqslant 4)_{it}$ and $1(\mathrm{TuitReg}_{t+k}, k \leqslant -4)_{it}$.

of time-varying state-year level controls. The control vector includes the state unemployment rate (along with its lead and lag) and the majority political party in each state's legislative lower and upper houses. Standard errors are clustered at the state level. We estimate Equation 2.1 separately for 2-year and 4-year institutions.

Our setup differs from a canonical event study set-up in two ways. First, an institution can be treated multiple times if a state has two (or more) separate tuition regulations during our time period, as opposed to being treated at most once as in a typical event study. To this end, we follow a strategy proposed in **?** which assigns a unique set of relative time indicators for each treatment. Under this strategy, the independent variables $1(\mathtt{TuitReg}_{t-k})_{it}, -3 \leqslant k \leqslant 3$ take values of at most 1 given that no two different tuition regulations could happen k years before a given year t when k is within three years of t. In contrast, $\sum_{k=4}^{\infty} 1(\mathtt{TuitReg}_{t-k})_{it}$ takes values greater than one if, for instance, there was a tuition regulation 4 years before a given year t and another regulation 7 years before t. The same holds for $\sum_{k=4}^{\infty} 1(\mathtt{TuitReg}_{t+k})_{it}$.[10]

Second, a tuition cap or freeze can last continuously for several years, with the length varying by state and enactment year. Figure A1 shows the distribution of the length of tuition regulations in our data. Whenever a tuition regulation lasts for more than one year, we impose a constant coefficient across all years in which the regulation was in place. Thus, the β_0 can be interpreted as the average yearly effect of the regulation across all years it was in place. This also implies that we can interpret the first lead as the year before the tuition regulation starts, and the first lag as the first year after the tuition regulation ends.[11]

While we expect β_k to be the weighted sum of treatment effects across tuition regulations at different timings, recent studies have shown that with heterogeneous treatment effects, the estimates from a two-way fixed effect (TWFE) regression might not be capturing this (**??**). While this is a concern in our setting, newly proposed estimators that allow for staggered adoption either assume that treatment is an absorbing state (**??**) or abstract away from the dynamics (**?**). **?** extend their previous work to capture dynamic effects when the research design is non-staggered, albeit with some limitations; the proposed estimator captures the effect of switching into treatment k periods ago, averaging different trajectories of treatment histories afterwards. Given these limitations, we use the two-way fixed effect design in Equation 2.1 as our baseline specification. However, we estimate our treatment effect using the estimator from **?**, but focus only on the instantaneous effects to avoid averaging different treatment histories. The results are presented in appendix Table A16,

[10] Therefore, β_4 is identified not only by the difference between treated and untreated units 4 and more periods after a tuition regulation but also by the linearity assumption on $\sum_{k=4}^{\infty} 1(\mathtt{TuitReg}_{t-k})_{it}$. In other words, the baseline specification assumes that the difference between a never-treated and a once-treated unit 4 or more periods after is same as the difference between the once-treated and a twice-treated unit after 4 or more periods. The same argument is applied to β_{-4}. To investigate if this linearity assumption matters, we run a variation of Equation 2.1 where we replace $\sum_{k=4}^{\infty} 1(\mathtt{TuitReg}_{t-k})_{it}$ with a set of dummy variables $1(\sum_{k=4}^{\infty} 1(\mathtt{TuitReg}_{t-k}) = N)$. Our coefficients of interest, the β_ks, $k = -3, -2, 0, 1, 2, 3$, are very robust with the modification.

[11] Note that if all tuition regulations had the same length, we could follow a more conventional event study specification by separately estimating a coefficient for each year of the regulation. This is infeasible due to large variation in the length of tuition regulations. However, because we are interested in dynamic effects of the regulation over the period in which it is in place, we estimate additional specifications that explicitly incorporate coefficients to capture variation in the length of regulations (see equations Equation 2.4 and Equation 2.5 below).

and are closely aligned with the estimates from our baseline specification.

We include a public/private-specific linear time trend rather than a state-specific trend in our main specification for two reasons. First, the inclusion of the public/private-specific trend helps us meet the parallel trends assumption while we see a positive pre-trend in the state-specific trend specification. Moreover, there could be spillover effects on private colleges located in the same state; private colleges could set their tuition or aid taking those of their competitors into account. For instance, **?** study how colleges set listed prices and institutional aid in an equilibrium setting. We will show some evidence of spillover effects in section 2.5. We also present sensitivity analyses where we instead use sector-year fixed effects or a state-specific linear time trend in appendix Table A17.

The coefficients of interest are the β_ks with $k = 0, 1, 2, 3$.[12] When the outcome variable is listed tuition, β_0 measures how effectively the tuition regulation was enforced whereas the β_ks with $k = 1, 2, 3$ capture how colleges adjust tuition after the regulation has ended. With other outcomes (e.g. institutional aid), the β_ks with $k = 0, 1, 2, 3$ show how colleges adjust other unregulated margins during and after tuition controls.

With our normalization which omits $1(\mathrm{TuitReg}_{t-1})$ in Equation 2.1, β_k captures the additional difference in y_{it} between treated and untreated units k periods after[13] the tuition cap or freeze is imposed, beyond the difference in the -1 period (which has been normalized to zero). In equation form,

$$\begin{aligned}\beta_k = {} & E(y_{it-k}|R=1,\tilde{X}) - E(y_{it-k}|R=0,\tilde{X}) \\ & - \left(E(y_{it-k-1}|R=1,\tilde{X}) - E(y_{it-k-1}|R=0,\tilde{X})\right) \qquad (2.2)\end{aligned}$$

where $R = 1$ is a university with a tuition regulation k periods before $(1(\mathrm{TuitReg}_{t-k})_{it} = 1)$ and $R = 0$ is a university without a tuition regulation. In addition, $\tilde{X}$ represents the collection of $\gamma_t, \phi_i, t\rho_c$ and X_{st} from Equation 2.1. We can interpret β_k, $k \geqslant 0$ as a causal effect of a tuition cap or freeze only when the parallel trends assumption holds, i.e., the mean change in the unobserved part of treated observations over time is equal to that of untreated observations after conditioning on $\tilde{X}$.

To bolster the case for a causal interpretation, we do three things. First, we investigate coefficients β_k, $k < 0$, in the years prior to the tuition regulation. It's possible that the state government could use the regulation as a punishment for colleges that have been increasing tuition rapidly. On the contrary, they could take advantage of colleges that are already slowing down tuition increases by advertising the tuition regulation to voters without having any meaningful impact on tuition setting. However, in these cases, we should see this behavior in the years leading up to the tuition

[12]We do not focus on β_{4+} since its interpretation is unclear due to the aggregation of periods and differing amounts of observations at the tail ends of the time period studied.

[13]In the case where $k < 0$, this can be interpreted as $-k$ periods before the treatment. For example, $k = -2$ implies it is two years before the treatment.

regulation. We do not see evidence of this, as the values of β_k are not statistically different from zero when $k < 0$. If anything, we see a small pre-trend upward in the years leading up to the regulation for both listed tuition and financial aid, so adjusting for this would strengthen our main results.

Second, we control for several key variables in Equation 2.1. Institution fixed effects capture any non-time-varying differences between treated and untreated units. Our public/private-specific linear time trend captures a linear approximation of time-varying differences between private and public schools. The calendar time fixed effect captures the national-level time trend. Our inclusion of state-level unemployment rates, and their leads and lags assuage concerns about the Great Recession or other state-varying macroeconomic trends affecting results.[14] Finally, we include indicators for the majority political party to capture state-varying differences in political factors that may affect both tuition prices and the probability of a state imposing a freeze/cap.

Third, we implement robustness checks with different comparison groups. First, we have a specification that only includes institutions that have been under a tuition cap or freeze at least once during the time frame studied. In this analysis, we leverage only variation in the timing of cap/freeze, exploiting the fact that different states imposed tuition regulations at different times (**?**). Second, we implement a matching procedure where we match treated institutions to untreated institutions with similar tuition levels and trends in the years prior to the regulation.

Conceptually, we are thinking of the results we see as colleges' response to a tuition cap or freeze being imposed on them. However, there are cases where we want to be cautious with this interpretation. First, we might be picking up other policies imposed on colleges that happen at the same time as the tuition regulation. Specifically, states imposing tuition caps/freezes often simultaneously give more generous funding to colleges as compensation. Our analysis show that institutions have 6 percentage points higher state appropriations during a tuition regulation (this effect is not statistically significant for four-year institutions but significant at a 5 percent level for two-year institutions. For more detail, see appendix Figure A9). In this case, our coefficient would capture the combined effect of the cap/freeze and the state funding. Thus we implement a sensitivity check where we control for state funding, and our findings of the effect of tuition regulations on tuition and aid are robust (see appendix Table A19).[15]

Moreover, state governments may be aware of changes in the unobservable u_{it} and use it to make a decision of whether to impose a tuition regulation. Previous work has shown that state governments adjust appropriations based on temporary financial shocks to colleges (**??**). It is also possible that the state government and colleges could be jointly deciding whether to have a tuition regulation. In this case, our estimates would simply show what happens during and after a tuition

[14] We use labor force data by county from Local Area Unemployment Statistics (LAUS) announced annually by Bureau of Labor Statistics (BLS). We control for the average unemployment rate by state, aggregated from counties within each state weighted by the size of labor force population.

[15] We do not control for state funding in our main specification because state appropriations could be determined as an outcome of the negotiation between colleges and the state after a tuition cap/freeze is imposed. In this case, colleges with different unobservable characteristics such as their bargaining power could select into different levels of increases in state funding. (This is a "bad control" discussed in **?** in detail. **?** also uses a sparse set of time-varying controls for the same reason in a similar context to ours.). However, results from our robustness check show that this might not be a concerning issue in our context.

regulation. Notably, our interpretation of effects on students (and how effects vary with student characteristics) remain the same.

In addition to the benchmark specification in Equation 2.1, we run two other specifications. First, we explore heterogeneity in whether schools experience a freeze or a cap (and in the size of the cap). Specifically, we estimate

$$\begin{aligned} y_{it} = &\sum_{k=-3,k\neq-1}^{3} 1(\mathrm{TuitReg}_{t-k})_{it}\beta_k + \sum_{k=0}^{3}(\mathrm{TuitCap}_{t-k})_{it}\alpha_k \\ &+ \beta_4 \sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it} + \beta_{-4}\sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it} + \phi_i + \gamma_t + t\rho_c + X_{st} + u_{its} \end{aligned} \tag{2.3}$$

which is the same as our benchmark specification except in the second term. $(\mathrm{TuitCap}_{t-k})$ represents the size of the cap and is coded from 0 to 1; for a 3 percent cap, $(\mathrm{TuitCap}_{t-k}) = 0.03$. When tuition is frozen, $(\mathrm{TuitCap}_{t-k})$ takes a value of 0. With this specification, β_k represents the effect of tuition being completely frozen. The effect of tuition cap is $\beta_k + \alpha_k \times (\mathrm{TuitCap}_{t-k})$.[16]

We also run regression models that consider the variation in the length of tuition regulations.

$$\begin{aligned} y_{it} = &\sum_{k=-3,k\neq-1}^{3} 1(\mathrm{TuitReg}_{t-k})_{it}\beta_k + 1(\mathrm{FirstYrofTuitReg}_t)_{it}\alpha_F + 1(\mathrm{LastYrofTuitReg}_t)_{it}\alpha_L \\ &+ \beta_4 \sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it} + \beta_{-4}\sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it} + \phi_i + \gamma_t + t\rho_c + X_{st} + u_{its} \end{aligned} \tag{2.4}$$

$$\begin{aligned} y_{it} = &\sum_{k=-3,k\neq-1}^{3} 1(\mathrm{TuitReg}_{t-k})_{it}\beta_k + (T_{it} - 1)\alpha_A \\ &+ \beta_4 \sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it} + \beta_{-4}\sum_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it} + \phi_i + \gamma_t + t\rho_c + X_{st} + u_{its} \end{aligned} \tag{2.5}$$

Equation 2.4 additionally includes indicators for the first and last year of the cap/freeze. $1(\mathrm{FirstYrofTuitReg}_t)_{it}$ 1 if the institution is under the first year of tuition cap/freeze. $1(\mathrm{LastYrofTuitReg}_t)_{it}$ is defined similarly. In this specification, β_0 gives the average effect for all years other than the first and last year in which the regulation is in place. The value of the outcome variable in the first/last year of tuition regulation is equal to $\beta_0 + \alpha_F$, $\beta_0 + \alpha_L$, respectively.[17]

[16]We do not include a tuition cap coefficient for the endpoint coefficients since their interpretations are unclear due to differing amounts of observations at the tail ends of the time period studied.

[17]If a tuition cap/freeze lasts only one year, both the first and last year dummy variables are switched on. If it lasts for

Equation 2.5 allows each additional year of a tuition regulation to have a linear effect on tuition and fees. T_{it} represents the number of consecutive tuition regulations up to year t. Thus, β_0 represents the effect of having a tuition regulation in place for exactly one year. The effect of having a tuition regulation for 5 years continuously is given by $\beta_0 + \alpha_A \times (5-1)$.

2.5 Results

Effects on Listed Tuition during Regulations

We begin by investigating the first-order effects of tuition freezes and caps on listed tuition while they are in place. Since this is the outcome being directly targeted by the policy, we consider this outcome to be a measure of whether the tuition regulation has "bite". Although our main outcomes of interest will be how colleges adjust unregulated margins during and after tuition regulations, we would only expect to see these adjustments when the regulation has some bite. To test this, we estimate the full specification in Equation 2.3 on the log of in-state undergraduate listed tuition and fees but present only our estimates of β_0 and α_0 to focus on the contemporaneous effect while the regulation is in place.[18] Table A2 shows the results and reveals that tuition regulations only had bite in the earlier time period. The first row shows that across all institutions, having a tuition freeze has an approximately -11.2 percentage point impact[19] on tuition in the years 2013 and earlier, but no impact in the years 2014 and later. This pattern persists when separately estimating effects for four-year and two-year institutions. The second row shows the effects of an institution having a tuition cap rather than a freeze. In the earlier period for all institutions, each additional percentage point in the cap reduces the effect by around 1 percentage point. For example, the effect of a 5 percent tuition cap is -11.2 + 0.05(100.1) = -6.2 percentage points.

Table A3 illuminates one of the driving forces by comparing the average annual increase in tuition between treated and untreated institutions over the two time periods. In 2013 and earlier, institutions under tuition regulations raised tuition by 2.4 percent each year on average while institutions not under tuition regulations raised tuition by 6.3 percent. Since 2014, treated institutions have behaved similarly as before, raising tuition by 2.5 percent each year. However, institutions that were not regulated only raised tuition by 3.1 percent, less than one percentage point above the treated group. Because institutions that were not forced to keep tuition levels down were not raising tuition much,

two years, the first year is switched on for the first year and the last year for the second year.

[18]We do not yet want to consider impacts on tuition in the years after the regulation since these could be capturing the response of colleges to tuition regulations once they regain control of tuition setting.

[19]This interpretation comes from the following calculation. Note that we use log of tuition. $\beta_0 = -0.112$ means $E(\log\frac{P_t}{P_{t-1}}|1(TuitReg_t)_{it} = 1) - E(\log\frac{P_t}{P_{t-1}}|1(TuitReg_t)_{it} = 0) = 0.112$. Using the approximation that $\log(1+x) \approx x$ when x is small, we have $E(\frac{\Delta P_t}{P_{t-1}}|1(TuitReg_t)_{it} = 1) - E(\frac{\Delta P_t}{P_{t-1}}|1(TuitReg_t)_{it} = 0) = -0.112$, where $\Delta P_t = P_t - P_{t-1}$.

the tuition freezes and (even more so) caps had essentially no bite.[20][21]

Our primary interest is on the downstream effects of these regulations. That is, how institutions respond by changing margins they still control, such as institutional aid. When the tuition regulation is not so effective in lowering listed tuition, colleges do not have adjust to make up for the loss from the regulation, so we would not expect to find effects on other margins. Therefore, in the rest of out analysis we focus on the time period of 1990-2013 to understand how universities respond to tuition regulations when they have some bite.

Dynamics of Listed Tuition and Institutional Aid

Figure A2 shows results of having a tuition regulation (either cap or freeze) by estimating Equation 2.1 for two outcomes: log of in-state undergraduate tuition and fees[22], and log of institutional financial aid for first-time undergraduates students. The solid lines represent coefficient estimates and the dotted lines represent 95 percent confidence intervals. Focusing first on four-year colleges in panel (a), we see that neither in-state tuition nor institutional aid statistically differs from zero in most years prior to the tuition regulation. This evidence supports our parallel trends assumption, which requires that there are no effects of having a tuition regulation in the future, because at this point, neither group has experienced treatment yet. If anything, both tuition and aid are slightly increasing in the years prior to tuition regulation so adjusting for this trend would make decreases in the years following tuition regulation larger.

Next, we are interested in the coefficient at period 0, which gives the effect of a tuition regulation on tuition and fees while the regulation is in place. As expected, we see a statistically significant negative effect (-6.3 percentage points).[23] One year after the regulation has been lifted, we still see a negative effect on tuition of 8.5 percentage points, which is slightly larger than the effect during the cap/freeze. This is due to the fact that the coefficient at period 0 captures the average effect over multiple years of tuition regulations.

To further understand the dynamics of tuition regulations that last for more than one year, Figure A4 illustrates the results from Equation 2.4. In this plot, “First Year" gives the effect of the tuition regulation on in-state tuition and fees in the first year that the regulation is in place, “Last Year" gives this same effect in the final year the regulation is in place, and “Middle Years" give the

[20]These results are not sensitive to the specific year we choose to cut the data within the years between 2009 and 2014. We decide to use 2013 as a cutoff for our main results since this is where we switch from using **?** data to our own hand-collected data, and although we tried to follow their methods there may be some differences in collection procedures.

[21]Although it goes beyond the scope of this study to understand the causes behind the slowdown of tuition increase in recent years, one conjecture is that there has been increasing attention on the price of higher education, which often results in negative media coverage or political discussion on tuition “hikes”.

[22]Results using tuition levels rather than the log of tuition are similar and can be found in Table A12. We use the sum of tuition and fees because this variable is available for the entire time period we study whereas tuition alone is not available until 2000.

[23]This does not match our estimate from Table A2 because here we use the specification without heterogeneity between freezes and caps for ease of interpretation in the figures. Results incorporating this heterogeneity are discussed below and can be found in Table A4 and Table A10.

average effect for all years other than the first and last year in which the regulation is in place. The figure shows that as tuition regulations last longer, their cumulative impact on the amount that tuition and fees deviates from its trend becomes larger, with a -2.2 percentage point estimate in the first year and a -11.6 percentage point estimate in the final year for four-year colleges. The easiest way to think about this is in the context of a three-year regulation, where tuition steady falls further from the trend in each of the years. If, instead, it was a four-year regulation, the “Middle Years" would represent the average of the second and third year, and so on with longer regulations. These differences are statistically significant: we can reject a null of a constant treatment effect between the first year, middle years, and last year with a p-value less than 0.001. In a similar vein, columns 2 and 4 in appendix Table A11 present results from Equation 2.5. The effect of having a tuition regulation in place for exactly one year is -2.3 percentage points. Having another consecutive year of regulation lowers tuition by an additional 9.9 percentage points. These results support the conclusion that the cumulative effect of tuition regulations increases as the regulation lasts longer.

Continuing to focus on the years after the regulation is lifted, both Figure A2 and Figure A4 show that tuition remains lower than it would have been in the absence of the regulation for three years after the end of the cap/freeze, with some evidence of small increases as institutions “catch up" to where they would have been without the regulation.[24] The absence of a faster catch-up may be related to state variation in the degree of autonomy that institutions have to set tuition rates, as noted by **?**. All of the coefficient and standard error estimates for Figure A2a and Figure A4a can be found in Table A4 and Table A11, respectively.[25]

Panel (b) of Figure A2 and Figure A4 show these patterns for two-year colleges. The patterns in both figures are similar to those of four-year institutions, although the magnitudes are bigger: the effect on tuition is -8.2 percentage points on average during the regulation and -18.7 percentage points in the last year of the regulation.[26] Despite the larger negative effects of the tuition regulation on tuition during the cap/freeze, we see a much stronger “catch up" effect for two-year institutions.[27] By the third year after the freeze/cap ends, there is no statistically significant difference between actual tuition and counterfactual tuition in a world where the college did not experience any cap or freeze. We suspect that two year colleges exhibit a stronger “catch-up" effect than four-year colleges because two-year colleges have less room to adjust along the institutional aid margin, given that initial levels of institutional aid at two-year colleges are very low, as presented in Table A1. All of the coefficient and standard error estimates for Figure A2b and Figure A4b can be found in appendix Table A10 and Table A11, respectively.

The line with triangle marks in Figure A2 shows the effect on institutional financial aid during and

[24]A joint test of equality between the coefficients 1, 2, and 3 years after the regulation can be rejected with a p-value of less than 0.001.

[25]In Table A11, the effect of the first year of the tuition regulation is $1(\mathsf{TuitReg}_t) + 1(\mathsf{FirstYRofTuitReg}_t)$, while the effect of the last year is $1(\mathsf{TuitReg}_t) + 1(\mathsf{LastYRofTuitReg}_t)$.

[26]Similar to 4-year schools, we can also reject a test of constant treatment effects during the regulation with a p-value of less than 0.001.

[27]We can reject a constant treatment effect among the last year of the regulation and the first, second, and third year after the regulation with a p-values less than 0.001.

after the tuition regulation. Institutional aid includes all grants given by the university to students, and does not include loans or any financial aid that the student receives from the government or any other source outside the institution. Colleges decrease institutional aid by a greater proportion than tuition, which suggests that they use institutional financial aid as a way to recoup some of the tuition losses from the tuition regulation. The pattern of institutional aid in the years after the regulation follows a similar path to that of tuition, although always of lower magnitude. The difference in the effect on institutional aid and the effect on tuition is statistically significant at the 5 percent level in every year following the regulation and at the 10 percent level during the regulation.[28] As a result of college's response of decreasing institutional aid, we indeed find that the average net tuition does not decrease significantly neither during nor following the tuition regulation (Figure A3). Because institutional aid is unlikely to be a large factor at two-year colleges, we do not include estimates for institutional aid in panel (b).[29]

There are two other possible explanations worth mentioning for the negative effect on institutional aid. First, students are spending relatively less on tuition, so they should need a smaller amount of aid to cover their costs. Relatedly, it could be that institutional aid decreases mechanically following the decrease in tuition if the amount of the aid is tied with the amount of tuition (e.g. aid is X percent of tuition). However, we see that the magnitude of the effect on institutional aid is not only bigger during the tuition cap/freeze, it falls further after the regulation is lifted.

Second, tuition regulation could change the composition of students that institutions enroll. This could make the new student body different in terms of income or academic preparedness, which could explain a change the amount of aid. However, Figure A8 shows that federal Pell grants and state grants to students were not affected by tuition caps/freezes. Given that Pell grants are need-based, this suggests regulations didn't lead to a big change in the student composition by income. Like institutional aid, state aid is awarded by both need and merit. We do not see a clear effect of tuition regulations on state aid either.[30] Further, appendix Table A22 shows there is no effect of tuition regulations on first-time students' SAT scores, giving more direct evidence that colleges' student composition by academic preparedness did not change. These results support our interpretation that the negative effect on institutional aid is at least in part an effort by institutions to make up for lost tuition revenues.

Table A5 illustrates the dynamics of tuition revenue in response to tuition regulations. During a regulation, both gross and net tuition are lower than they would have been in the absence of the regulation. However, this negative effect is over two million dollars larger for gross tuition revenue (-4.7 million dollars, statistically significant at 10%) than for net tuition revenue (-2.7 million dollars, not statistically significant). This adds to our evidence that colleges decrease institutional aid to make up for tuition losses. After the regulation is lifted, the effects on both gross and net tuition

[28] Results come from a GMM setup with conditions derived from two event study regressions, one with tuition as the outcome variable and the other with institutional aid. The p-values on the difference of the two effects 1, 2, and 3 years after the regulation are 0.028, 0.006, and 0.020, respectively. The p-value is 0.055 for the difference of the effects during the regulation.

[29] However, estimates can be found in appendix Table A10.

[30] These results also show that the decrease in institutional aid was not offset by any increases of state aid or Pell grants.

revenue are no longer statistically significant (although still sizeable).[31]

To give a sense of the impacts of tuition regulations in dollar terms, we present results with the outcome variable as levels of tuition and fees (as opposed to logs) in Table A12. Column (1) shows that a tuition regulation has a -268 dollar effect on in-state tuition and fees each year during the regulation. Column (3) shows that colleges are almost completely compensating for this loss with institutional aid: the effect on aid is -212 dollars each year. Institutional aid continues to lag behind where it would have been in the absence of a cap/freeze in the years after the cap/freeze has ended, even more than tuition in some years.

In addition to representing the information conveyed in the figures described above, columns 2 and 4 of Table A4 and Table A10 present estimates from Equation 2.3 where we differentiate tuition caps and freezes. Focusing first on four-year colleges in Table A4, we see that the effect of a 5 percent tuition cap is -9.4 + 0.05(96.7) = -4.6 percentage points. When tuition is frozen, $(\mathtt{TuitCap}_{t-k})$ takes a value of 0, so the coefficient of -0.094 indicates that the effect of tuition being completely frozen on in-state tuition and fees is -9.4 percentage points for each year that is it frozen. This specification shows the intuitive result that institutions under caps experience smaller negative effects on tuition than institutions under freezes during and after the regulation. Three years after the end of the regulation, the tuition at colleges that had a freeze are still 9.4 percentage points behind where they would have been without the freeze. Meanwhile, those with a 5 percent cap are only 5 percentage points behind. The patterns for institutional aid at four-year colleges, as well as tuition at two-year colleges shown in appendix Table A10, are similar.

Heterogeneity

Next, we investigate heterogeneity in four-year colleges' responses to tuition freezes. First, we look into whether colleges' dependency on tuition affects how they respond to tuition regulations. Following a strategy of measuring state appropriations dependency from **?**, we categorize institutions into more or less dependent on tuition based on the fraction of their total revenue that is sourced from tuition and fees in the initial year of our data, i.e. 1991. If this fraction is greater than the median fraction for all public institutions, the institution is classified as *More Dependent* whereas institutions with a fraction less than the median are classified as *Less Dependent*.

Figure A5 shows the results. Focusing first on in-state tuition (grey lines with circle markers), we see that institutions that are more dependent on tuition seem to increase tuition faster in the years following the end of the regulation, presumably because they do not have as many other sources of revenue to pull from when they take a loss from the tuition regulation. Similarly, institutions that are more dependent on tuition decrease their institutional aid more during and following the

[31]Given that revenue is tuition times the number of students, we check if there is an effect of tuition regulations on the total number of enrolled students but find no evidence of this. The coefficient of $1(\mathtt{TuitReg}_t)_i t$ is -23 with robust standard error 165.55 when we regress a measure of full time equivalent students on dummies of tuition regulations and control variables.

tuition regulation. These results support our interpretation of the decrease in institutional aid in our main results as being due to colleges adjusting to make up for tuition revenue losses.

Next, we break down institutions into three broad categories from the Carnegie classification system, using a modification of the classification from **?**. *Research* universities are doctoral-granting universities with high or very high research activity. The *Non-Research* group includes masters-granting universities and doctoral-granting universities with low research activity. All other 4+ year degree granting institutions fall into the *Other* category.

Figure A6 reveals that although the coefficients on tuition during the time of the regulation were of a similar size, there are differences in the tuition-setting behavior of colleges in the years following the cap or freeze. The *Non-Research* and *Other* groups seem to "catch up" a little more quickly while the *Research* universities' tuition remains well below where it would have been in the absence of the regulation. This may be because *Research* universities have more resources and do not need to raise tuition as rapidly to make up for the losses incurred by the regulation.

More strikingly, there is a discrepancy among the way these groups of colleges adjust their institutional aid. *Research* universities seem to reduce institutional aid in proportion to the reduction in tuition during the regulation and in the first year following, but then increase it slightly in the next two years. *Non-Research* universities do not adjust much during the regulation but reduce institutional aid in a proportion greater than tuition in the years following the cap or freeze. Finally, *Other* institutions have a sharp decline in institutional aid offered during the regulation that remains below the reductions in tuition for several years after the end of the regulation.

A possible mechanism for the heterogeneous responses by Carnegie classification come from the fact that the classification is a proxy of the university's available resources as well as stature and selectivity. *Non-Research* and *Other* universities are more dependent on tuition revenue than *Research* universities. While 32% of total revenue is sourced by tuition in *Research* universities on average, 59% and 54% of revenue is for *Non-research* and *Other* universities, respectively. Moreover, selective universities could leverage the higher demand from students to find ways to compensate their losses from tuition regulations. For example, **??** find that facing a steady decrease of state funding, *Research* universities admit more out-of-state and foreign students who pay higher tuition than in-state students. Of course, our heterogeneity results by Carnegie classification should be taken with caution due to the large standard errors associated with the coefficients on institutional aid.

Robustness

In this section, we perform five analyses to ensure the robustness of out results. First, we implement a matching procedure to ensure that treated and comparison units are balanced on their tuition levels and trends before the regulation is put into place. Matching results can be found in the first two columns of Table A15. We implement 1-1 matching of institutions by year based on the Mahalanobis distance of the level of in-state undergraduate tuition and the annual rate of increase in in-state undergraduate tuition for the years one, two, and three years before the tuition cap/freeze.[32] The main conclusions from our baseline analysis remain.

Second, we include a specification that only includes institutions in states that were treated at some point during the time period we study. This is motivated by a potential concern that there may be some unobserved differences between the time trends of states that are subject to tuition regulations and states that never experience a tuition regulation. This version leverages only variation in the timing of the tuition regulations, rather than both the timing and existence of tuition regulations. Columns 3 and 4 of Table A15 restricts the sample to "ever treated" institutions. Although estimates are nosier than our main results, the signs and magnitudes of estimates are very similar.

Third, we limit the sample to only observations where we observe both tuition and aid, which changes the sample dramatically since institutional aid data does not become available until 2001. This helps us ensure that the relative magnitude of "sticker price" and institutional aid is not driven by differences in estimating samples. The final column of Table A15 shows results for in-state tuition when only including observations that are in our estimating sample for institutional aid. Our results are robust and if anything indicate a greater gap between the change in in-state tuition and institutional aid.

Fourth, motivated by the recent literature showing pitfalls of TWFE estimators (**??**), we estimate the effect of tuition controls on the listed tuition and aid using the estimator proposed in **?**. This estimator captures the effect of the first time switching into treatment k periods ago, averaging the effect of different trajectories between $t-k-1$ and the observation year t. To ease interpretation, we present the effect of an institution's first tuition regulation on its listed tuition and institutional aid during the first year that it is under the regulation in Table A16.[33] The estimator uses not-yet treated observations up to the year t as the comparison group. The estimates support the main story from the our baseline TWFE specification; 1) the magnitude of the effect on aid is greater than that on listed tuition (Tuition: -0.035 vs. Aid: -0.11), 2) universities that are *More Dependent* on tuition revenue adjust aid more (*More Dependent*: -0.206 vs. *Less Dependent*: -0.019), and 3) there is large heterogeneity in treatment effects by Carnegie classification.

Finally, there may be some concern that our estimates are picking up not only the effects of tuition regulations, but the combined effect of tuition regulations and changes in state and local funding. To address this, we investigate the relationship between state and local funding and tuition caps/freezes. Although we find that during a tuition regulation, institutions receive 6 percentage points more in state appropriations (not statistically significant for four-year colleges), if we control for state and local funding in our main specification, the coefficients of interest do not change. Appendix Figure A9 shows the estimated effect of a tuition regulation on state funding. Appendix Table A19 shows estimates of the effects of tuition regulations on tuition and institutional aid after

[32] We use the user-written Stata command *kmatch* (**?**).

[33] Note that this captures a different effect than our baseline specification which presents the average yearly effect across all years in which the tuition regulation was in place. It is more similar to our estimated effect of the first year of a regulation from Equation 2.4.

controlling for state and local funding. Columns 1 and 2 give effects for four-year institutions, while column 3 shows results for two-year institutions.

Effects on Expenditure

In addition to adjusting revenue (i.e. net tuition) in response to financial shocks, colleges may also adjust expenditures. Here we focus on instructional expenditures since these are the most likely to affect the quality of students' education. Table A6 presents the effects of caps and freezes on per-student instructional expenditures. We see a negative effect of 3.3 percentage points during a cap/freeze. This aligns with results from **?** which show that universities decrease expenditures per student when the size of a cohort is large. Additionally, results show large heterogeneity by institutional characteristics. Colleges that are *More Dependent* on tuition decrease instructional expenditure by 5.0 percentage points per year during a regulation, relative to what they would have spent in the absence of the freeze. Effects are also magnified for the Carnegie *Others* group of colleges during and after the regulation.

By further decomposing instructional expenditures into subcategories, we find that the negative effect on per-student instructional expenditures is mainly driven by universities' tightening fringe benefits for instructional staff. We see a negative effect of 4.5 percentage points on the log of total benefits for instructional staff per student. Meanwhile, we do not find evidence that universities downsize instructional staff or decrease the baseline salary, both of which may be less adjustable in the short-run than fringe benefits. Analysis of results are presented in Table A18. In addition, during the period of analysis, the average amount of fringe benefits is equivalent to 25% of the average salary ($15,544 and $58,657, respectively, 2011 CPI adjusted.) These results imply that tuition regulations do not only affect tuition - they could have meaningful impacts on instructors which could in turn affect the quality of education.

Spillover Effects on Private Schools

We also investigate if tuition regulations have spillover effects on private colleges located within the same state. Tuition regulations do not apply to private colleges, but they may respond to tuition regulations since they are competing for students with the regulated public institutions.[34] In Table A7, we compare private institutions whose competing public institutions are under tuition caps/freezes to private institutions whose competitors are not regulated. Thus, $1(\mathtt{TuitReg}_t)_{it}$ is equal to one if a public university in the same state is under a tuition cap or freeze at time t.

Our results suggest a spillover effect of tuition regulations on private colleges' tuition and aid. Private colleges do not adjust the level of tuition during a tuition cap/freeze, but there are some

[34]Previous papers have studied how colleges set tuition and aid in an equilibrium framework (**??**). **?** consider a setting where private colleges set financial aid strategically, predicting that a student would get the same aid offer from all private colleges when her academic preparedness is common knowledge among colleges. Although our setting studies private institutions' responses to decisions of public institutions while they focus on competition among private institutions, our results are in line with their prediction.

negative effects in the post-tuition-regulation period. Meanwhile, they decrease institutional aid by 5 percentage points during tuition regulations, with a lingering effect after the regulation is lifted in a similar pattern to our main analysis. Notably, the magnitude of the coefficients are around one-third to half of the magnitude of the effects on public institutions shown in Table A4. Columns (3)-(8) of Table A7 present spillover effects by Carnegie classification. Negative effects on tuition and aid are largely driven by *Other* institutions rather than *Research* or *Non-Research* universities. This aligns with our main heterogeneity analysis in section 2.5 showing the strongest responses from public *Other* institutions and is intuitive given that private institutions are likely to compete with public institutions of similar characteristics such as selectivity or resource availability.

Other Outcomes

Student Fees, Room and Board Charges If tuition regulations do not include limits on additional student fees, we may expect to see an increase in fees during and after the regulation. However, appendix Table A20 shows that fees are not affected very much, aside from some suggestive evidence that two-year colleges increase fees in the first and second year after a regulation ends. It could be that effects are dampened by some states that also limit student fees in their tuition regulations (e.g. North Carolina, Ohio, Virginia). Appendix Table A20 also shows the effect of tuition regulations on room and board, another potential margin that colleges could adjust to make up for lost tuition revenue. However, we do not find any evidence of this behavior.

Out-of-state Student Tuition and Enrollment Appendix Table A21 illustrates the effect of tuition caps and freezes on out-of-state tuition and the composition of enrolled students by state residency. We restrict our sample to 4-year institutions given that 2-year institutions enroll few out-of-state students. We do not see a clear pattern of effects of tuition regulations on these outcome variables. Notably, colleges do not hike up out-of-state tuition to compensate for losses from freezing in-state tuition. Our lack of significant changes in out-of-state tuition may be related to colleges not having market power in the out-of-state student market, making them essentially price-takers. **?** also find that public institutions use out-of-state students to increase institutional quality, not to increase revenue.

Completion Rate We may expect the decrease in the expenditure per student and aid to impact completion rates (**?????**). However, we do not find any strong evidence that tuition regulations impact completion rates. It could be because we can not separately identify completion rates of low-income students, who are known to benefit the most from generous financial aid (**?**).[35] Column (1) in appendix Table A22 presents these results.

[35]IPEDS provides separate graduation rates for Pell grants recipients, but only beginning in 2016. We do have access to completion rates by race and gender for a longer period of time, but we do not find any meaningful patterns of tuition regulation effects on these completion rate, either.

2.6 Representative Student's Change in Tuition Paid

So far, we have shown that tuition regulations have meaningful impacts on in-state tuition and institutional financial aid and that these impacts vary over time and across different types of colleges. However, it is difficult to see a clear picture of the overall impact that one of these regulations might have on a student moving through their education around the time of one of these regulations. In this section, we summarize the effects that tuition regulations have on several "representative" students that differ in the types of university they are attending as well as whether they receive institutional financial aid. We also incorporate differences in the dynamics of tuition and financial aid during and after a cap or freeze by presenting estimates for two types of students who start their education at different times. First, we consider a student who begins their four-year education in the first year of a tuition regulation. For simplicity, we assume that the tuition regulation lasts 3 years, which is the median length of tuition regulations in our data. Next, we consider a student who begins their four-year education in the first year after a tuition regulation has ended.

We use our estimates from appendix Table A11 to calculate the effect on each representative student's tuition in each year of their four-year education. This specification captures the dynamics of negative impacts on tuition increasing as the regulation lasts longer.[36] We use the average percent of tuition covered by institutional aid at four-year public institutions as a baseline for the portion of tuition that is affected by changes in institutional aid. This average is unconditional on receipt of institutional aid, so our results can be interpreted as the average effect across students who do and do not receive institutional aid. For each subgroup, we compute this average within institutions of that subgroup.[37] To make the tuition and aid estimates comparable, we restrict the sample to observations that have non-missing values for both tuition and institutional aid.

Figure A7 presents our results. The top panel represents students starting their education in the first year of a regulation and the bottom panel represents students starting in the first year after a regulation has ended. The first column shows average effects; the second and third columns show heterogeneity in effects across types of institutions outlined in section 2.5. Tuition estimates give the percentage point change in tuition paid by the representative student, aid estimates the percentage point change in tuition paid due to changes in institutional aid received, and total estimates combine these two effects. Note that positive values for the aid column do not imply that aid is increasing, they show that the decrease in aid leads to students paying more tuition.

Focusing on the upper left panel, the top line shows that the representative student starting their education in the first year of the regulation gets a 4.3 percent discount on their tuition over the four years they are enrolled. However, the second line shows that students must pay 2.9 percent more in

[36]For the student who starts their education in the first year the regulation is imposed, we use $1(\mathsf{TuitReg_t})_{it} + (\mathsf{FirstYrofTuitReg_t})_{it}$ for their first year, $1(\mathsf{TuitReg_t})_{it}$ for their second year, $1(\mathsf{TuitReg_t})_{it} + 1(\mathsf{LastYrofTuitReg_t})_{it}$ for their third year, and $1(\mathsf{TuitReg_{t-1}})_{it}$ for their fourth and final year. For the student starting right after the regulation has been lifted, we use $1(\mathsf{TuitReg_{t-k}})_{it}$ for their kth year.

[37]The average percent of tuition covered by aid is 23.5 percent overall, 20.6 percent for institutions more dependent on tuition, 27.5 percent for institutions less dependent on tuition, 32.0 percent for research universities, 21.2 percent for non-research universities, and 17.5 percent for other institutions.

tuition due to their decrease in institutional aid. The bottom line shows the combination of these two effects, which reveals that they get a 1.4 percent discount overall. These separate estimates emphasize the importance of considering financial aid when thinking about how beneficial tuition regulations are to students, since without considering changes in institutional aid we would have concluded that the average discount was around triple the true discount. Students who do not receive any institutional financial aid experience the full tuition discount shown in the top row, highlighting the differences in benefits from the tuition regulation between students depending on their institutional aid receipt.

The middle panel splits these effects into institutions that are more or less dependent on tuition revenue. Finally, the right panel shows responses by broad Carnegie classification. Benefits to students vary greatly across types of institutions and their timing of entering college. We estimate that a student who starts their education in the first year of the regulation at an institution that is *Less Dependent* on tuition will receive an overall 3.9 percent discount, but a student who starts after the regulation at a *More Dependent* on tuition institution will end up paying 2.5 percent more than they would have in the absence of the regulation. Appendix Figure A10 shows the corresponding figure where changes in tuition and institutional aid are measured in dollars rather than percent. Results are qualitatively similar for average and tuition dependency panels, but change for the Carnegie classification panel due to differences in tuition levels between subgroups.

To illustrate how the effects of the tuition regulation vary with the timing of student entry, we further break down the yearly effects. We focus on the subgroup of colleges that are *More Dependent* on tuition, since this is where the timing of student entry leads to the most dramatic differences in total tuition paid. As shown in Figure A7, students who enter in the first year of the regulation receive a 0.5 percent discount, while those who enter after the regulation ends have to pay 2.5 percent more. Figure A11 shows that this is driven by the deep discount in the final year of the tuition regulation, which occurs in students' junior (third) year if they started with the regulation. Meanwhile, students who start after the regulation have to pay more in the last three years of their education than they would have in the absence of the regulation. This aligns with our results presented in Figure A4, which show that more tuition-dependent colleges begin to raise tuition while keeping institutional aid low in the years after the regulation.

The only margins that we consider in this analysis are changes in in-state tuition and institutional aid, abstracting away from other things that may be affected. First, we do not capture any changes in application or enrollment behavior induced by the tuition regulation. Second, we assume all students complete their university education in four years, which excludes any student who drops out or takes more than four years. In addition, we don't consider any changes in educational quality resulting from the regulations. We suspect that change in institutional quality would decrease the benefits students receive from tuition caps and regulations due to the decreases in per-student instructional expenditure discussed in section 2.5 and shown in Table A6. We do not consider these changes in benefit calculations for simplicity, but without considering them, our results may be overstating the benefits of tuition regulations for students.

2.7 Conclusion

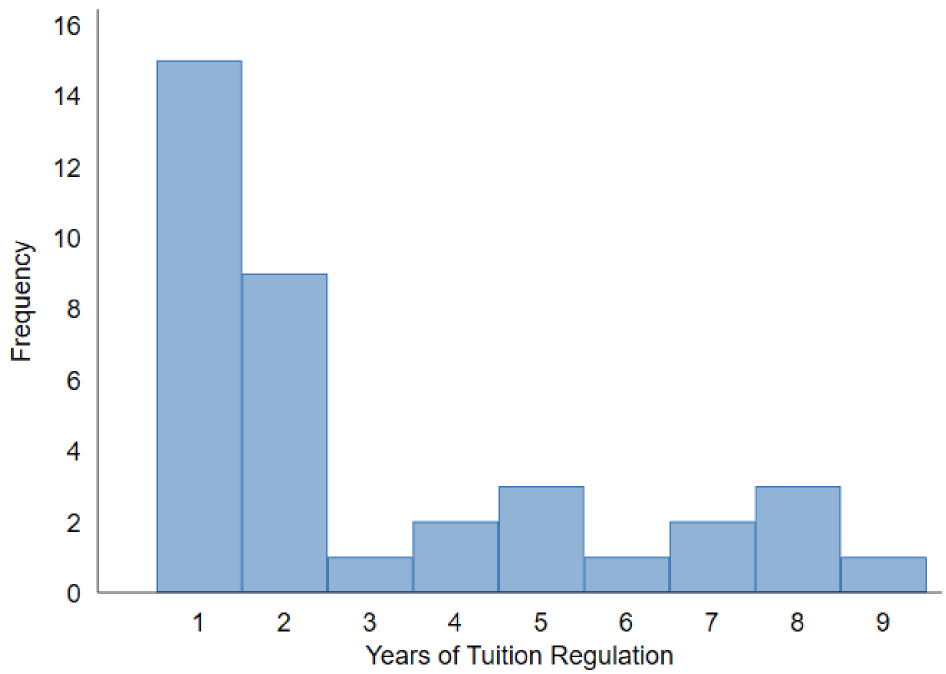
This paper has explored the effects of a popular policy tool for targeting college affordability - tuition caps and freezes. We find significant heterogeneity in the effectiveness of caps and freezes over time, with the policies only having bite in the earlier period we study (1990-2013). However, we find that when the policies have bite and tuition falls during a cap or freeze relative to where it would have been without regulations, the effects on tuition alone do not accurately reflect actual discounts for students. This is because colleges decrease their institutional financial aid when facing a tuition cap or freeze by a proportion that is almost double the decrease in tuition. Even in the years following the lifting of the regulation, institutional aid lags behind where it would have been without a regulation.

Effects of tuition regulations are not felt equally across all students. In particular, students who do not receive institutional financial aid will see much greater benefits from tuition caps and freezes than students who rely on aid. Unfortunately our institution-level data does not allow us to investigate which students see decreases in their institutional aid around the time of a tuition cap/freeze. However, we can get a sense of who is likely to be most hurt from looking at the characteristics of students who receive institutional financial aid in another data source, the National Postsecondary Student Aid Study (NPSAS).[38] Students attending four-year public colleges are more likely to receive institutional aid if they are low-income. 27 percent of students from the bottom quartile get institutional aid, as opposed to 16 percent from the top income quartile. 34 percent of students receiving Pell grants also get institutional aid, whereas only 18 percent of non-Pell-eligible students get institutional aid. This suggests that the benefit of tuition regulations may be smallest for those most in need. Further, heterogeneity analysis reveals that research institutions and institutions that do not rely heavily on tuition revenue are largely shielded from these effects, creating more inequality in how the regulations are felt by students who attend different types of colleges.

These are important impacts for policy-makers to understand. First, we have shown that tuition freezes and caps are not always effective in lowering tuition, as they have not had much bite in recent years. However, when they do have impacts on tuition, we have shown that universities respond by decreasing financial aid which disproportionately impacts students who are supported by institutional aid. This implies that tuition regulations are ineffective at best and can be harmful to needy students at worst. In the future, if policy-makers implement tuition regulations, they should be aware of these responses and consider pairing freezes/caps with policies that address the distributional consequences. Tuition freezes and caps could be accompanied with increases in financial aid or additional regulations that freeze or prohibit decreases in institutional aid.

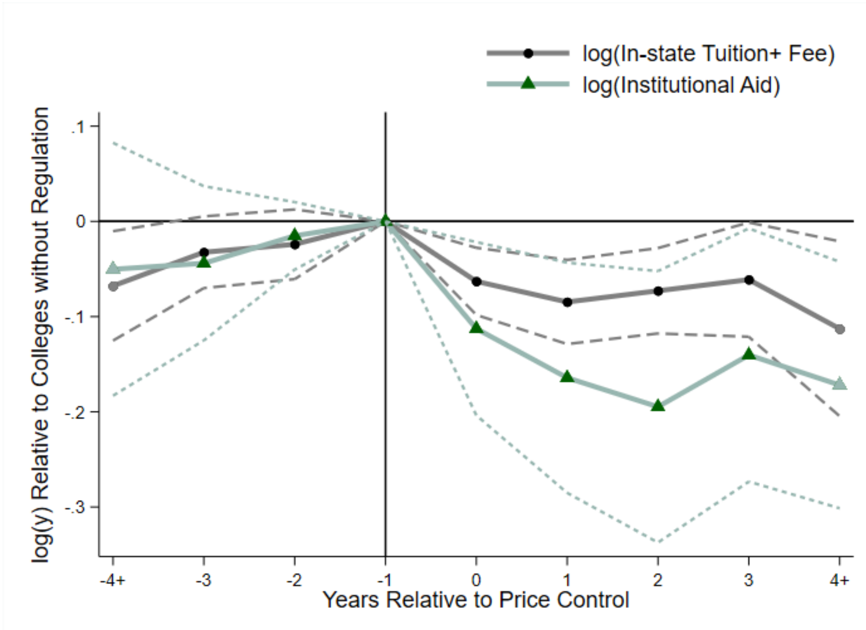
[38] Ideally we would use NPSAS data directly in our analysis, but it is only conducted every 4 years thus is not able to capture dynamics of regulations that are potentially changing every year.

Figure A1: Distribution of Length of Tuition Regulations

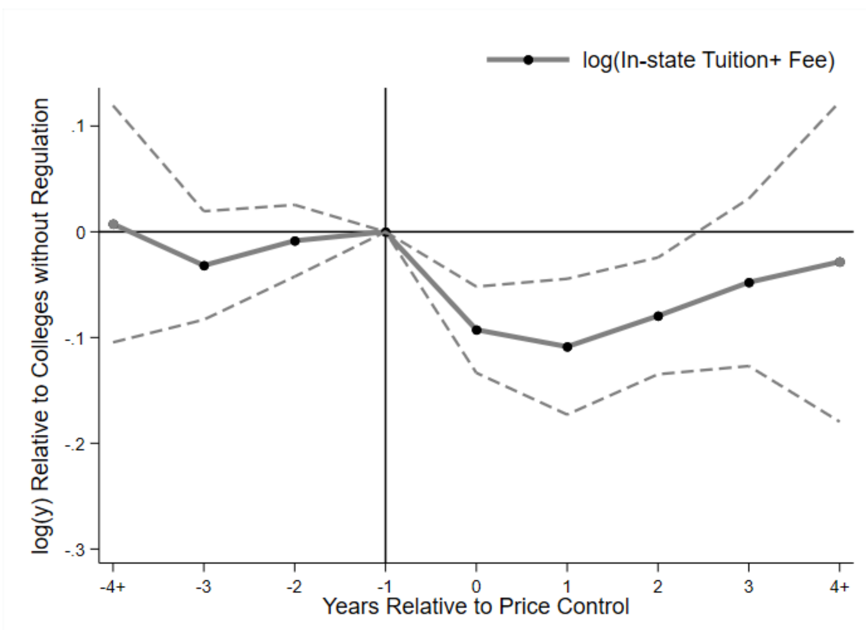


Notes: A regulation continued over multiple years is counted as one regulation. Each individual regulation represents a continuous state-level freeze or cap lasting for the specified number of years.

Figure A2: Effect of Tuition Regulation on Tuition and Aid



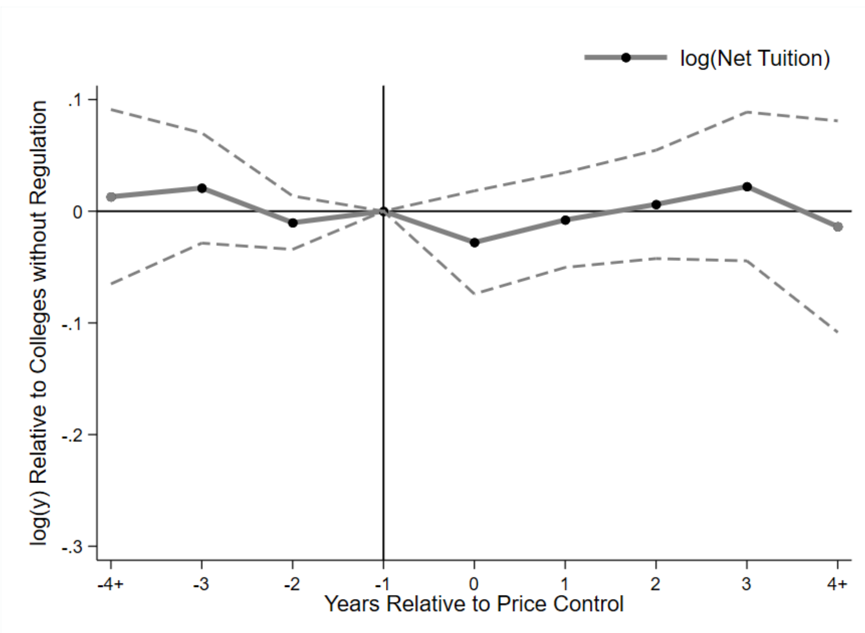
(a) 4-yr Institution



(b) 2-yr Institution

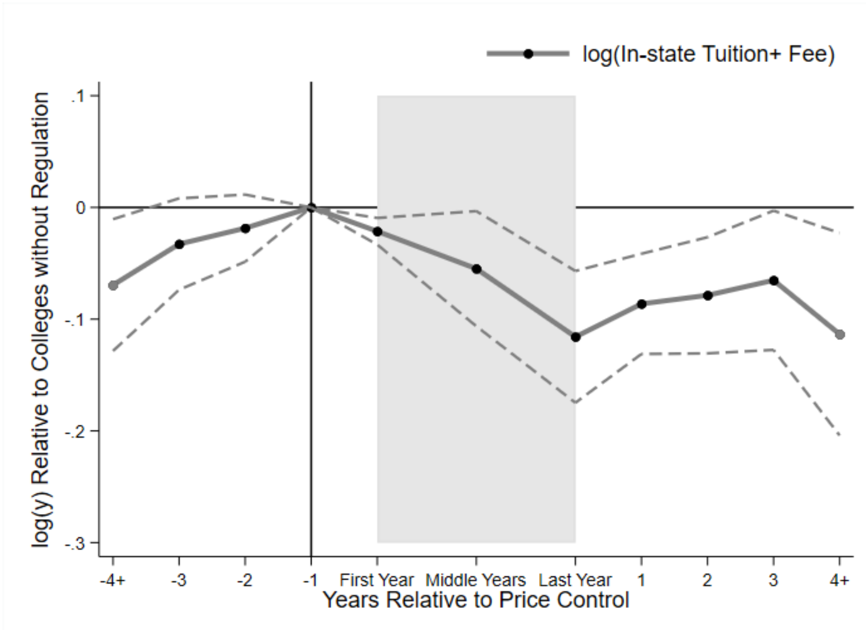
Notes: -4+ means 4 or more years before the tuition regulation is introduced, and 4+ is 4 or more years after the tuition regulation is lifted. The values of coefficients in the top panel are presented in Table A4; the bottom panel in Table A10. Confidence interval at 95% level.

Figure A3: Effect of Tuition Regulation on Net Tuition

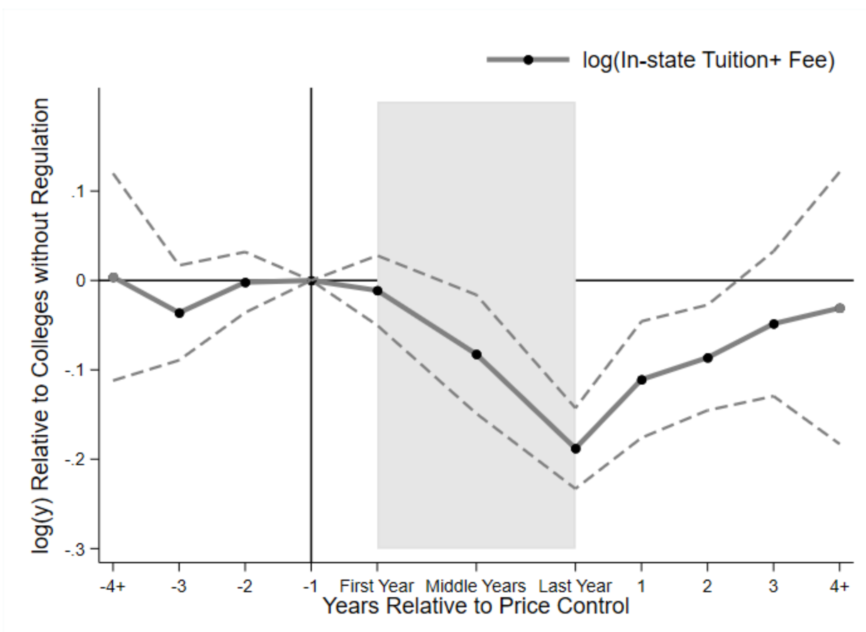


Notes: -4+ means 4 or more years before the tuition regulation is introduced, and 4+ is 4 or more years after the tuition regulation is lifted. Authors calculated the net tuition by subtracting the average institutional aid from tuition. Confidence interval at 95% level.

Figure A4: Effect of Tuition Regulation on Tuition, First and Last Year of Regulation



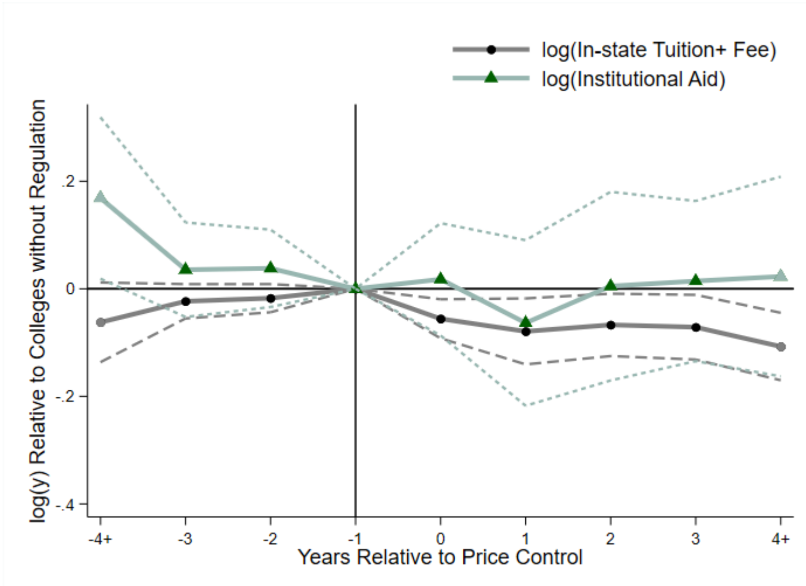
(a) 4-yr Colleges



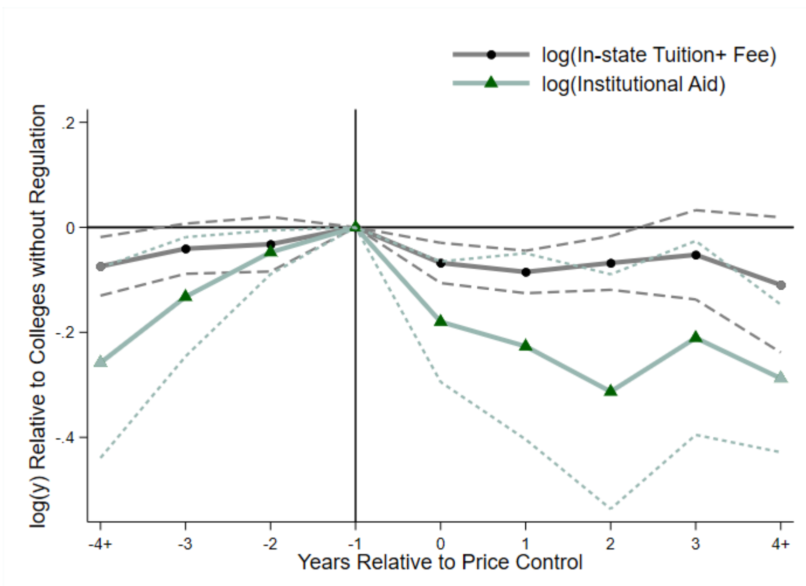
(b) 2-yr Colleges

Notes: -4+ means 4 or more years before the tuition regulation is introduced, and 4+ is 4 or more years after the tuition regulation is lifted. The values of coefficients are presented in Table A11. Confidence interval at 95% level.

Figure A5: Effect of Tuition Regulation on Tuition and Aid: by Tuition Revenue Dependency



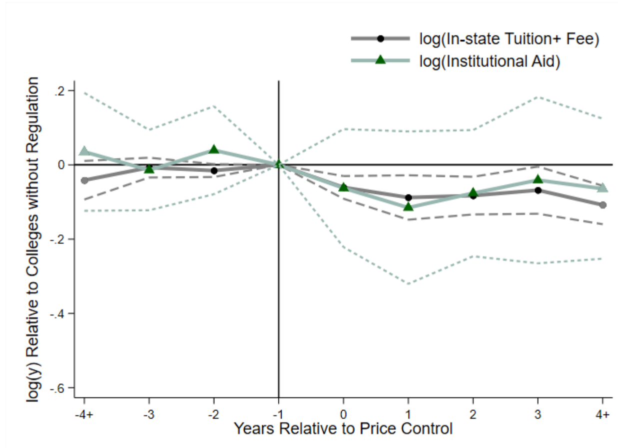
(a) *Less Dependent*



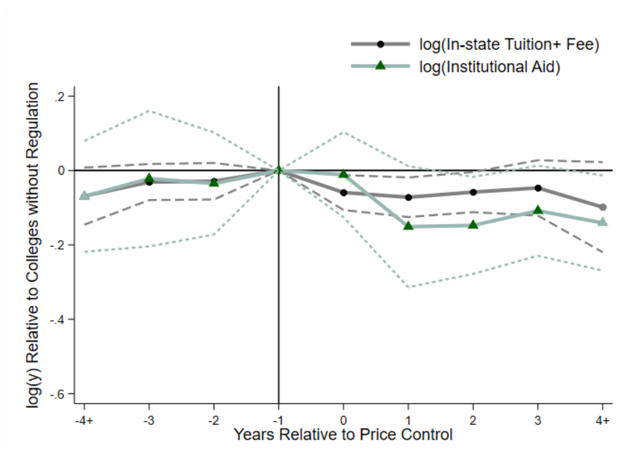
(b) *More Dependent*

Notes: Sample of 4+ year degree granting institutions. -4+ means 4 or more years before the tuition regulation is introduced, and 4+ is 4 or more years after the tuition regulation is lifted. The values of coefficients are presented in Table A13. We classify an institution into *More Dependent* if the ratio of gross tuition revenue to total revenue is above the median of the institutions in the same sector (pubic and private separately) in 1991; *Less Dependent* if below the median. Confidence interval at 95% level.

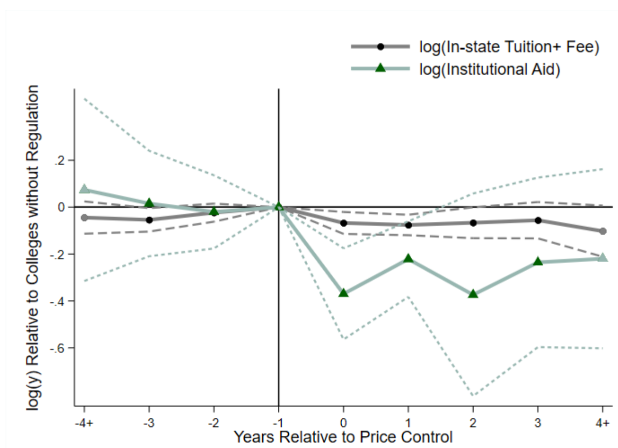
Figure A6: Effect of Tuition Regulation on Tuition and Aid: by Carnegie Classification



(a) *Research*



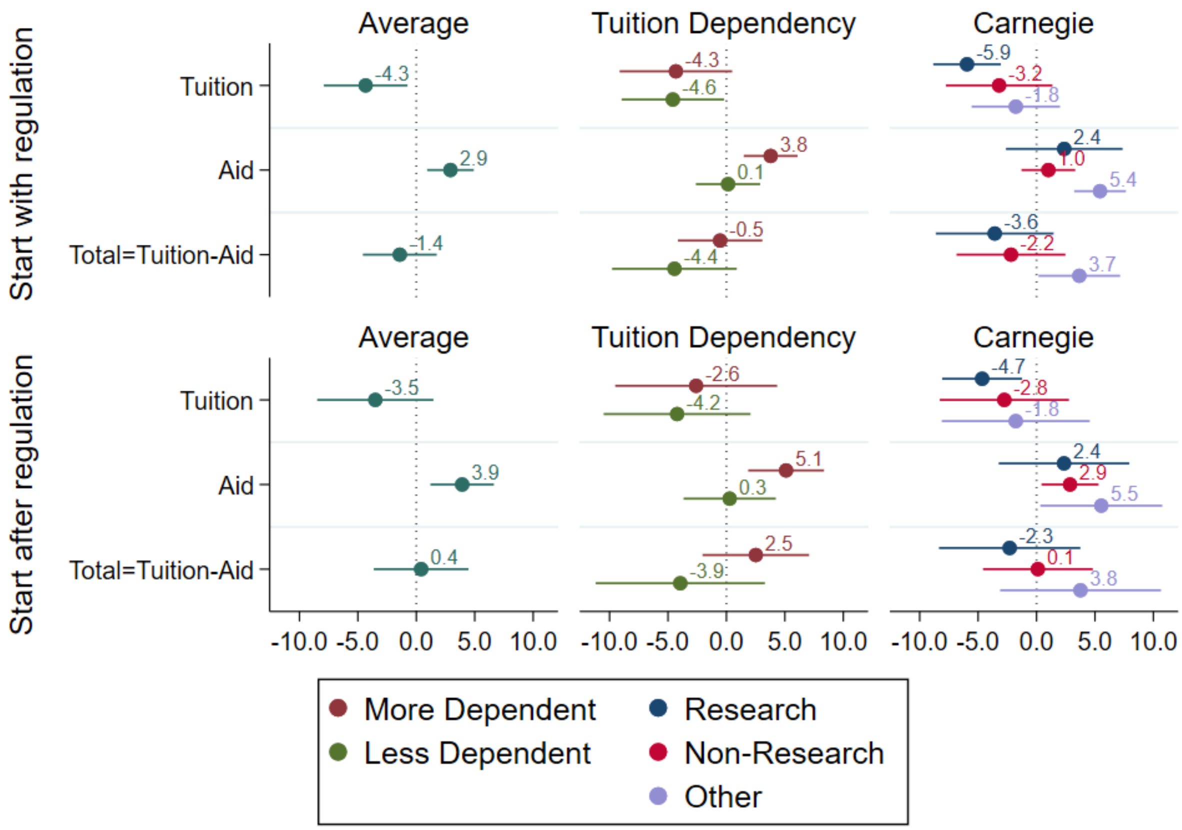
(b) *Non-research*



(c) *Others*

Notes: Sample of 4+ year degree granting institutions. -4+ means 4 or more years before the tuition regulation is introduced, and 4+ is 4 or more years after the tuition regulation is lifted. The values of coefficients are presented in Table A14. *Research* sample is of doctoral universities with high or very high research activity (Carnegie classification). *Non-Research* is sample of master's universities or Doctoral universities with low research activity. *Others* include all other 4+ year degree granting institutions. Confidence interval at 95% level.

Figure A7: Percent Change in Net Tuition Paid for Representative Students



Notes: Sample of 4+ year degree granting institutions. Tuition gives the percentage point change in net tuition paid for an average student at each type of university based on out estimates of change in listed tuition only. Aid gives the percentage point change in tuition paid due to changes in institutional aid. It is constructed by multiplying our estimates of the percent change in institutional aid with the (unconditional) percent of tuition covered by aid in each subgroup before any tuition regulations are imposed. Total combines these two effects to give the overall percentage point change in net tuition paid by a student who receives the average institutional aid, including those who receive no institutional aid. All calculations assume that the tuition regulation lasts 3 years and students attend college for 4 years. The top row gives the effect on a student whose first year of education is the first year of the regulation; the bottom row gives the effect on a student whose first year of education is the first year after the end of the regulation. Subgroups are defined as in the text. Confidence intervals at the 95% level.

Table A1: Summary Statistics by Type of Institution

Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Treated		Public 4-year		Private 4-year		Public 2-year		Private 2-year	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
In-state Tuition										
$, 2016 referenced	5,166.55	3,361.55	5,531.35	2,690.30	18,850.56	9,482.26	2,765.61	1,756.91	9,343.28	5,675.13
% annual growth	0.001	0.068	0.044	0.078	0.033	0.096	0.041	0.157	0.028	0.14
Out-of-state Tuition										
$, 2016 referenced	11,815.97	6,094.78	13,563.50	5,599.00	18,866.66	9,469.98	6,265.22	3,155.81	9,490.43	5,737.69
% annual growth	0.006	0.094	0.036	0.109	0.032	0.096	0.027	0.193	0.027	0.146
Average Institutional aid										
$, 2016 referenced	935.239	1,283.43	1,279.78	1,213.97	7,814.53	5,616.63	256.167	410.341	1,313.53	2,443.63
% annual growth	0.111	0.985	0.09	0.689	0.073	0.635	0.082	1.043	0.091	1.139
% Revenue Souced with Tuition	0.336	0.188	0.287	0.144	0.639	1.29	0.223	0.132	0.669	3.123
Carnegie Classification										
Others	0.32	0.466	0.247	0.431	0.603	0.489	-	-	-	-
Non-research	0.333	0.471	0.452	0.498	0.339	0.473	-	-	-	-
Research	0.347	0.476	0.301	0.459	0.058	0.234	-	-	-	-
N of Obs	2,636		13,856		29,025		23,908		4,683	
N of Aid Obs	2,012		8,761		17,903		14,440		1,842	

Notes: 1. The unit of observation is Year × Institution. 2. Variables in dollar amount are adjusted using Consumer Price Index (CPI). Deflator of 2016 is normalized to be 100. 3. Tuition is the sum of undergraduate tuition and fee. 4. *Research* sample is of doctoral universities with high or very high research activity (Carnegie classification). *Non-Research* is sample of master's universities or Doctoral universities with low research activity. *Others* include all other 4+ year degree granting institutions. 5. % Revenue soured with tuition is the fraction of gross tuition revenue out of total revenue.

Table A2: Effect of Tuition Regulation on Tuition: Time Periods Before 2013 and After 2014

	(1)	(2)	(3)	(4)	(5)	(6)
	All		4-year Institution		2-year Institution	
	Pre-2013	Post-2014	Pre-2013	Post-2014	Pre-2013	Post-2014
$1(\mathtt{TuitReg_t})_{it}$	-0.112***	-0.001	-0.075***	-0.021	-0.143***	0.010
	(0.035)	(0.013)	(0.025)	(0.013)	(0.050)	(0.020)
$\mathtt{TuitCap}_{it}$	1.007**	-0.128	0.977***	-0.007	0.822	-0.196
	(0.436)	(0.214)	(0.302)	(0.239)	(0.673)	(0.250)
Observations	70,845	14,556	41,360	9,776	29,485	4,780
R-squared	0.798	0.397	0.857	0.440	0.715	0.352
Two-way FEs	yes	yes	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes	yes	yes
State level control	yes	yes	yes	yes	yes	yes

Notes: 1. Pre-2013 includes 2013 and years before. Post-2014 includes 2014 and years after. 2. The outcome variables are the log of in-state undergraduate tuition and fees combined in all columns. 3. Two-way fixed effects include institution fixed effects and year fixed effects. 4. A private/public specific time trend is included. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

Table A3: Annual Tuition Increase Rate Before and After 2013

	Under Tuition Regulation			Not Under Regulation		
	N	mean	sd	N	mean	sd
Before 2013	2,664	0.024	0.075	69,239	0.063	0.139
After 2014	2,019	0.025	0.048	14,724	0.031	0.070

Notes: 1. Pre-2013 includes 2013 and years before. Post-2014 includes 2014 and years after. 2. 4-year and 2-year institution pooled.

Table A4: Effect of Tuition Regulation on Tuition and Aid: 4-year Institution

	(1)	(2)	(3)	(4)
Dep. Variable	log(In-state Tuition)		log(Institutional Aid)	
$\Sigma_{k=4}^{\infty}1(\mathrm{TuitReg}_{t+k})_{it}$	-0.068	-0.063	-0.050	-0.044
	(0.029)	(0.028)	(0.066)	(0.061)
$1(\mathrm{TuitReg}_{t+3})_{it}$	-0.032	-0.032	-0.044	-0.034
	(0.019)	(0.019)	(0.040)	(0.041)
$1(\mathrm{TuitReg}_{t+2})_{it}$	-0.024	-0.023	-0.015	-0.016
	(0.018)	(0.018)	(0.018)	(0.020)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.063	-0.094	-0.113	-0.101
	(0.018)	(0.020)	(0.045)	(0.046)
$1(\mathrm{TuitReg}_{t-1})_{it}$	-0.085	-0.115	-0.164	-0.201
	(0.022)	(0.031)	(0.060)	(0.070)
$1(\mathrm{TuitReg}_{t-2})_{it}$	-0.073	-0.100	-0.195	-0.280
	(0.022)	(0.028)	(0.071)	(0.091)
$1(\mathrm{TuitReg}_{t-3})_{it}$	-0.061	-0.094	-0.140	-0.186
	(0.030)	(0.031)	(0.066)	(0.088)
$\Sigma_{k=4}^{\infty}1(\mathrm{TuitReg}_{t-k})_{it}$	-0.113	-0.110	-0.172	-0.162
	(0.046)	(0.044)	(0.064)	(0.060)
$\mathrm{TuitCap}_{it}$		0.967		-0.199
		(0.322)		(0.373)
$\mathrm{TuitCap}_{it-1}$		1.024		1.397
		(0.528)		(1.064)
$\mathrm{TuitCap}_{it-2}$		0.831		2.837
		(0.478)		(1.606)
$\mathrm{TuitCap}_{it-3}$		0.871		1.434
		(0.337)		(1.848)
Observations	41,410	41,410	26,239	26,239
R-squared	0.856	0.857	0.293	0.293
Two-way FEs	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. The outcome variables are the log of in-state undergraduate tuition and fees combined in columns (1)-(2), and the log of average institutional aid for first-time undergraduates in column (3)-(4). 2. Two-way fixed effects include institution fixed effects and year fixed effects. 3. A private/public specific time trend is included. 4. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 5. Standard errors clustered at the state level are in parentheses.

Table A5: Effect of Tuition Regulation on Tuition Revenue

Dep. Variable	(1) Gross	(2) Net	(3) log(Gross)	(4) log(Net)
$\Sigma_{k=4}^{\infty}1(\text{TuitReg}_{t+k})_{it}$	-6.928	-5.126	0.048	0.015
	(8.920)	(6.716)	(0.045)	(0.059)
$1(\text{TuitReg}_{t+3})_{it}$	-2.367	-1.099	0.046	0.075
	(3.035)	(2.156)	(0.024)	(0.038)
$1(\text{TuitReg}_{t+2})_{it}$	-2.630	-1.262	0.024	0.060
	(1.525)	(1.009)	(0.015)	(0.032)
$1(\text{TuitReg}_{t})_{it}$	-4.720	-2.675	-0.035	-0.023
	(2.615)	(2.027)	(0.028)	(0.049)
$1(\text{TuitReg}_{t-1})_{it}$	-3.287	-3.461	-0.013	-0.012
	(4.854)	(2.897)	(0.030)	(0.054)
$1(\text{TuitReg}_{t-2})_{it}$	-3.721	-3.828	0.009	0.033
	(4.973)	(3.039)	(0.030)	(0.047)
$1(\text{TuitReg}_{t-3})_{it}$	-2.955	-3.727	0.019	0.031
	(5.603)	(3.371)	(0.029)	(0.046)
$\Sigma_{k=4}^{\infty}1(\text{TuitReg}_{t-k})_{it}$	-3.220	-3.511	-0.021	-0.019
	(7.740)	(4.555)	(0.033)	(0.051)
Observations	31,944	32,050	31,943	32,048
R-squared	0.248	0.229	0.604	0.430
Two-way FEs	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. 2. The outcome variables are gross tuition revenue (in millions) in column (1), net tuition revenue (in million) in column (2), and the log of gross/net tuition revenue in column (3) and (4), respectively. 3. Two-way fixed effects include institution fixed effects and year fixed effects. 4. A private/public specific time trend is included. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

Table A6: Effect of Tuition Regulation on Per-Student Instruction-Related Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)
		Tuition Dependency		Carnegie Classification		
Sample	All	Less Dep.	More Dep.	Other	Non-research	Research
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it}$	-0.028	-0.068	-0.005	-0.070	-0.032	0.010
	(0.029)	(0.030)	(0.027)	(0.039)	(0.031)	(0.018)
$1(\mathrm{TuitReg}_{t+3})_{it}$	-0.021	-0.035	-0.018	-0.047	-0.023	0.004
	(0.017)	(0.020)	(0.011)	(0.034)	(0.011)	(0.011)
$1(\mathrm{TuitReg}_{t+2})_{it}$	0.000	-0.013	0.001	-0.027	-0.004	0.006
	(0.014)	(0.019)	(0.011)	(0.028)	(0.012)	(0.007)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.033	-0.021	-0.050	-0.054	-0.033	-0.008
	(0.017)	(0.019)	(0.018)	(0.028)	(0.021)	(0.009)
$1(\mathrm{TuitReg}_{t-1})_{it}$	-0.022	-0.040	-0.027	-0.074	-0.010	-0.019
	(0.017)	(0.018)	(0.024)	(0.031)	(0.020)	(0.015)
$1(\mathrm{TuitReg}_{t-2})_{it}$	-0.027	-0.042	-0.029	-0.087	-0.013	-0.008
	(0.019)	(0.016)	(0.029)	(0.031)	(0.023)	(0.017)
$1(\mathrm{TuitReg}_{t-3})_{it}$	-0.024	-0.046	-0.025	-0.080	-0.010	-0.014
	(0.024)	(0.020)	(0.027)	(0.029)	(0.024)	(0.014)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it}$	-0.020	-0.044	-0.022	-0.055	-0.018	-0.023
	(0.026)	(0.026)	(0.018)	(0.030)	(0.021)	(0.014)
Observations	44,694	20,347	20,485	19,392	15,443	5,463
R-squared	0.492	0.535	0.679	0.450	0.743	0.627
Two-way FEs	yes	yes	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes	yes	yes
State level control	yes	yes	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. We classify an institution into *More Dependent* if the ratio of gross tuition revenue to total revenue is above the median of the institutions in the same sector (pubic and private separately) in 1991; *Less Dependent* if below the median. Research sample is of doctoral universities with high or very high research activity (Carnegie classification). Non-Research is sample of master's universities or Doctoral universities with low research activity. Others include all other 4+ year degree granting institutions. 2. The outcome variable is log of per-student Instruction-related Expenditure in all columns. 3. Two-way fixed effects include institution fixed effects and year fixed effects. 4. A private/public specific time trend is included. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

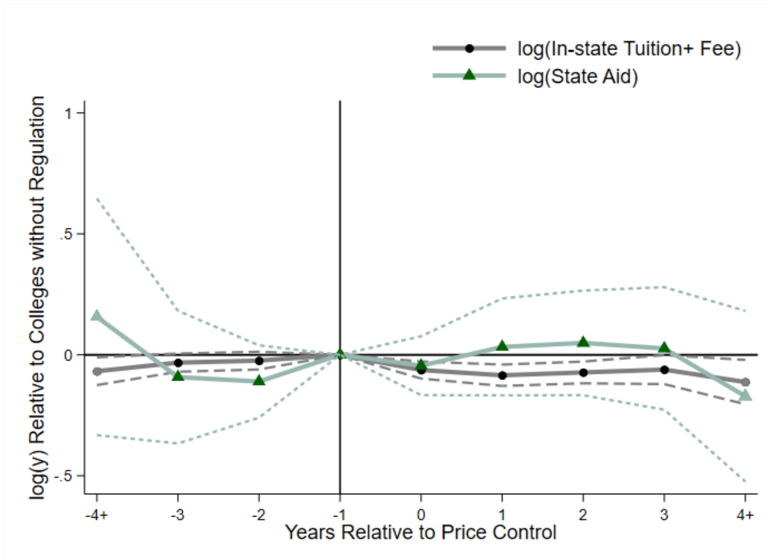
Table A7: Spillover Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Carnegie Classification			
Sample	All		Other		Non-research		Research	
Dep. Variable	log(Tuition)	log(Aid)	log(Tuition)	log(Aid)	log(Tuition)	log(Aid)	log(Tuition)	log(Aid)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it}$	-0.016	-0.051	-0.027	0.013	-0.004	-0.102	0.001	0.104
	(0.013)	(0.032)	(0.022)	(0.044)	(0.011)	(0.042)	(0.013)	(0.072)
$1(\mathrm{TuitReg}_{t+3})_{it}$	-0.012	-0.070	-0.018	-0.050	-0.010	-0.062	0.002	-0.045
	(0.010)	(0.040)	(0.014)	(0.062)	(0.011)	(0.044)	(0.006)	(0.095)
$1(\mathrm{TuitReg}_{t+2})_{it}$	-0.003	-0.021	-0.011	-0.010	0.002	-0.008	0.003	0.052
	(0.004)	(0.025)	(0.007)	(0.041)	(0.003)	(0.033)	(0.004)	(0.054)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.004	-0.059	-0.002	-0.065	-0.001	0.001	-0.008	-0.068
	(0.005)	(0.021)	(0.006)	(0.028)	(0.005)	(0.028)	(0.005)	(0.070)
$1(\mathrm{TuitReg}_{t-1})_{it}$	-0.008	-0.088	-0.012	-0.086	-0.000	-0.029	-0.014	-0.007
	(0.008)	(0.032)	(0.008)	(0.047)	(0.010)	(0.042)	(0.009)	(0.036)
$1(\mathrm{TuitReg}_{t-2})_{it}$	-0.018	-0.099	-0.018	-0.107	-0.010	-0.023	-0.011	-0.011
	(0.009)	(0.031)	(0.009)	(0.037)	(0.012)	(0.047)	(0.009)	(0.032)
$1(\mathrm{TuitReg}_{t-3})_{it}$	-0.023	-0.107	-0.032	-0.108	-0.012	-0.020	-0.011	-0.023
	(0.010)	(0.031)	(0.013)	(0.054)	(0.013)	(0.048)	(0.009)	(0.075)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it}$	-0.027	-0.107	-0.054	-0.116	0.004	-0.036	-0.008	0.062
	(0.012)	(0.038)	(0.020)	(0.066)	(0.012)	(0.053)	(0.012)	(0.044)
Observations	30,798	18,160	14,054	8,735	10,409	6,650	1,742	1,141
R-squared	0.820	0.278	0.787	0.253	0.928	0.369	0.970	0.482
Two-way FEs	yes	yes	yes	yes	yes	yes	yes	yes
State level control	yes	yes	yes	yes	yes	yes	yes	yes

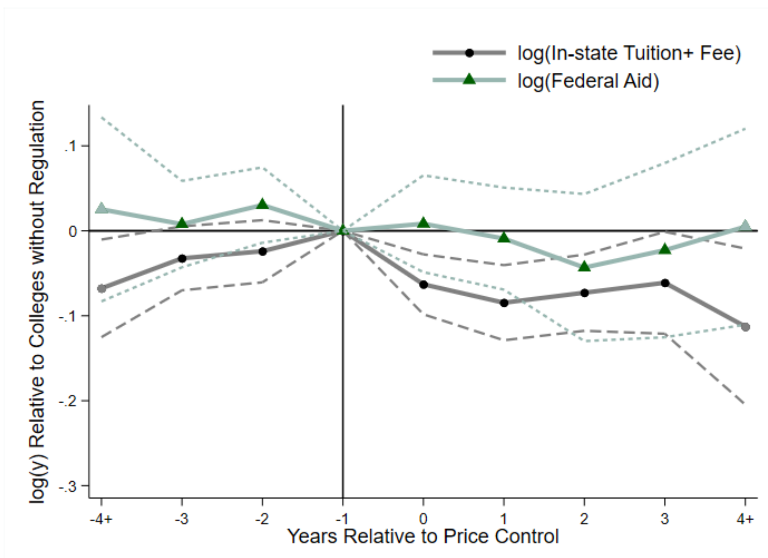
Notes: 1. Sample of 4+ year degree granting private institutions. $1(\mathrm{TuitReg}_{t-k})_{it}$ equals to one if public institutions of the same state as i are under tuition regulation in $t-k$. 2. The outcome variables are the log of in-state tuition and fees combined in odd-numbered columns, and log of average institutional aid for first-time undergraduates in even-numbered columns. 3. Two-way fixed effects include institution fixed effects and year fixed effects. 4. State level controls include lag, lead and the current year of state-level unemployment rate. 5. Standard errors clustered at the state level are in parentheses.

A Supplementary Tables and Figures

Figure A8: Effect of Tuition Regulation on Other Sources of Aid



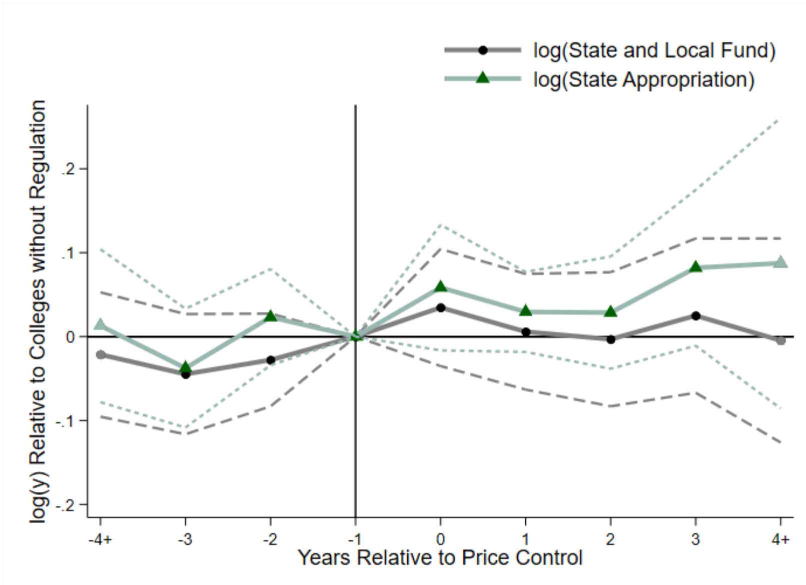
(a) State Aid



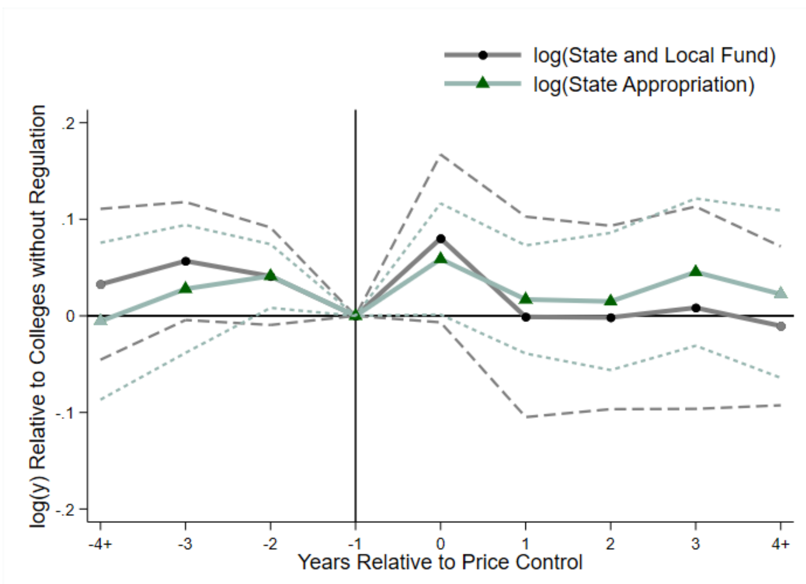
(b) Pell Grant

Notes: Sample of 4+ year degree granting institutions. -4+ means 4 or more years before the tuition regulation is introduced, and 4+ is 4 or more years after the tuition regulation is lifted. Confidence interval at 95% level.

Figure A9: State Funding Before and After Tuition Regulation



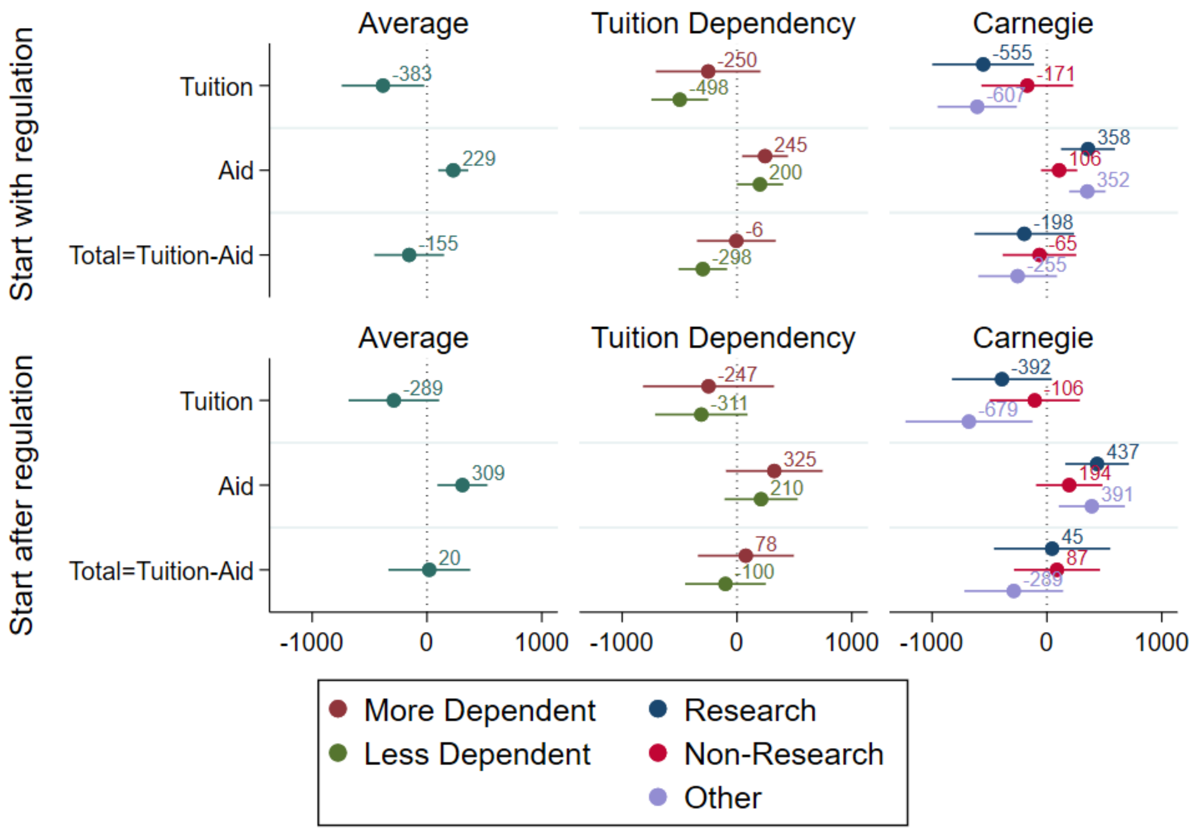
(a) 4-year Institution



(b) 2-year Institution

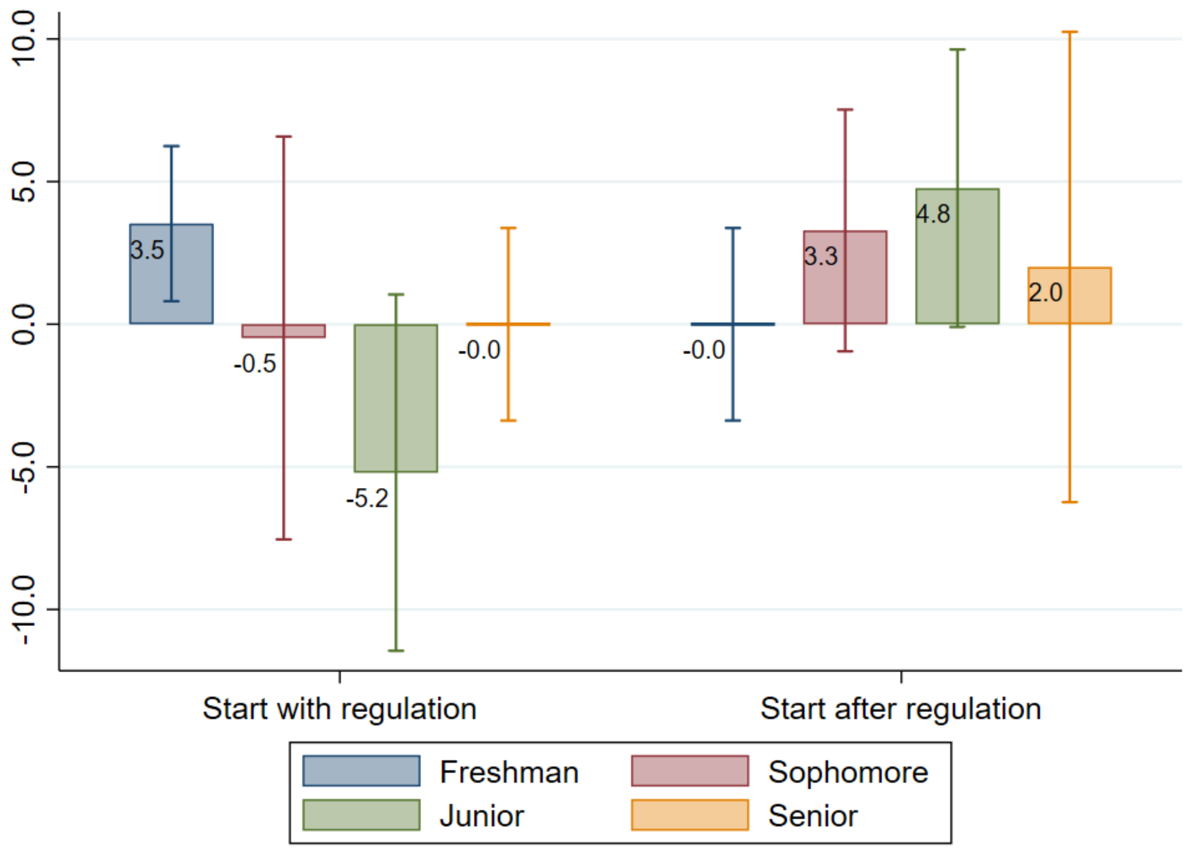
Notes: -4+ means 4 or more years before the tuition regulation is introduced, and 4+ is 4 or more years after the tuition regulation is lifted. log(State and Local Fund) is a total sum of appropriation and grants from either State or local government. log(State Appropriation) only captures the appropriation from State. Confidence interval at 95% level.

Figure A10: Dollar Change in Net Tuition Paid by Representative Students



Notes: Sample of 4+ year degree granting institutions. Tuition gives the dollar amount change in tuition paid for an average student at each type of university based on out estimates of change in listed tuition only. Aid gives the dollar amount change in tuition paid due to changes in institutional aid. Total combines these two effects to give the overall dollar amount change in tuition paid by a student who receives the average institutional aid, including those who receive no institutional aid. All calculations assume that the tuition regulation lasts 3 years and students attend college for 4 years. The top row gives the effect on a student whose first year of education is the first year of the regulation; bottom row gives the effect on a student whose first year of education is the first year after the end of the regulation. Subgroups are defined as in the text. Confidence intervals at the 95% level.

Figure A11: Percent Change in Tuition Paid for Representative Students by at *More Dependent* Colleges, by Cohort



Notes: Sample of 4+ year degree granting institutions. We classify an institution into *More Dependent* if the ratio of gross tuition revenue to total revenue is above the median of the institutions in the same sector (pubic and private separately) in 1991. Each year plots the total percentage point change paid in tuition incorporating changes in listed tuition and institutional aid. All calculations assume that the tuition regulation lasts 3 years and students attend college for 4 years. Left side shows results for a student whose first year of education is the first year of the regulation; right side gives results for a student whose first year of education is the first year after the end of the regulation. Confidence intervals at the 95% level.

Table A8: Distribution of Tuition Regulations

Cap	Freq.	Percent	Notes
-0.2 (mandated cut)	1	0.92	Virginia, 2000
0 (tuition freeze)	55	50.46	
0.03	8	7.34	
0.035	6	5.5	
0.04	7	6.42	
0.055	2	1.83	
0.06	12	11.01	
0.065	1	0.92	
0.07	2	1.83	
0.08	4	3.67	
0.09	1	0.92	
0.1	10	9.17	
Total	109	100	

Note: Unit of observation if year by state.

Table A9: Type of Affected Institutions

	By State		By Year X State	
Scope	Freq.	Percent	Freq.	Percent
All public institutions	6	35.29	44	40.36
4-year public institutions	7	41.18	35	32.11
2-year public institutions	3	17.65	16	14.68
CUNY (except 2003) and Cornell	1	5.88	14	12.84
Total	17	100	109	100

Notes: Oklahoma imposed a tuition regulation on all public institutions except for Oklahoma Technology Centers. For simplicity, it is counted as in the category “All public institutions".

Table A10: Effect of Tuition Regulation on Tuition and Aid: 2-year Institutions

	(1)	(2)	(3)	(4)
Dep. Variable	log(In-state Tuition)		log(Institutional Aid)	
$\Sigma_{k=4}^{\infty}1(TuitReg_{t+k})_{it}$	0.007	0.004	-0.054	-0.057
	(0.056)	(0.056)	(0.084)	(0.089)
$1(TuitReg_{t+3})_{it}$	-0.032	-0.039	-0.094	-0.100
	(0.026)	(0.018)	(0.108)	(0.106)
$1(TuitReg_{t+2})_{it}$	-0.008	-0.008	-0.138	-0.154
	(0.017)	(0.016)	(0.132)	(0.140)
$1(TuitReg_{t})_{it}$	-0.093	-0.104	0.033	0.036
	(0.020)	(0.024)	(0.096)	(0.096)
$1(TuitReg_{t-1})_{it}$	-0.109	-0.130	0.087	-0.014
	(0.032)	(0.043)	(0.112)	(0.153)
$1(TuitReg_{t-2})_{it}$	-0.080	-0.086	0.089	-0.020
	(0.027)	(0.035)	(0.128)	(0.158)
$1(TuitReg_{t-3})_{it}$	-0.048	-0.056	0.233	0.176
	(0.039)	(0.036)	(0.118)	(0.133)
$\Sigma_{k=4}^{\infty}1(TuitReg_{t-k})_{it}$	-0.028	-0.035	0.159	0.175
	(0.075)	(0.081)	(0.178)	(0.165)
$TuitCap_{it}$		0.749		2.069
		(0.671)		(0.774)
$TuitCap_{it-1}$		1.235		5.900
		(1.455)		(2.359)
$TuitCap_{it-2}$		0.288		5.964
		(1.358)		(3.420)
$TuitCap_{it-3}$		0.382		3.530
		(1.347)		(2.789)
Observations	29,486	29,486	15,045	15,045
R-squared	0.715	0.715	0.173	0.174
Two-way FEs	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. The outcome variables are the log of in-state undergraduate tuition and fees combined in columns (1)-(2), and the log of average institutional aid for first-time undergraduates in column (3)-(4). 2. Two-way fixed effects include institution fixed effects and year fixed effects. 3. A private/public specific time trend is included. 4. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 5. Standard errors clustered at the state level are in parentheses.

Table A11: Effect of Tuition Regulation on Tuition: Dynamics During Regulation

	(1)	(2)	(3)	(4)
	4-year Institution		2-year Institution	
$\Sigma_{k=4}^{\infty} 1(TuitReg_{t+k})_{it}$	-0.069	-0.062	0.004	0.020
	(0.029)	(0.030)	(0.058)	(0.059)
$1(TuitReg_{t+3})_{it}$	-0.033	-0.028	-0.036	-0.026
	(0.020)	(0.018)	(0.026)	(0.026)
$1(TuitReg_{t+2})_{it}$	-0.019	-0.022	-0.002	-0.007
	(0.015)	(0.016)	(0.017)	(0.016)
$1(FirstYrofTuitReg_{t})_{it}$	0.033		0.071	
	(0.025)		(0.025)	
$1(TuitReg_{t})_{it}$	-0.055	-0.023	-0.082	-0.029
	(0.026)	(0.009)	(0.033)	(0.029)
$NofConsecutiveYears - 1_{it}$		-0.019		-0.029
		(0.007)		(0.008)
$1(LastYrofTuitReg_{t})_{it}$	-0.061		-0.105	
	(0.012)		(0.033)	
$1(TuitReg_{t-1})_{it}$	-0.086	-0.085	-0.111	-0.108
	(0.022)	(0.023)	(0.032)	(0.033)
$1(TuitReg_{t-2})_{it}$	-0.079	-0.079	-0.086	-0.086
	(0.026)	(0.025)	(0.029)	(0.028)
$1(TuitReg_{t-3})_{it}$	-0.065	-0.066	-0.048	-0.049
	(0.031)	(0.033)	(0.040)	(0.040)
$\Sigma_{k=4}^{\infty} 1(TuitReg_{t-k})_{it}$	-0.113	-0.108	-0.031	-0.026
	(0.045)	(0.045)	(0.076)	(0.074)
Observations	41,410	41,410	29,486	29,486
R-squared	0.857	0.857	0.715	0.716
Two-way FEs	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions in columns (1)-(2), and 2+ but less than 4 year degree granting institutions in columns (3)-(4). 2. The outcome variable is the log of in-state undergraduate tuition and fees combined in columns (1)-(4). 3. Two-way fixed effects include institution fixed effects and year fixed effects. 4. A private/public specific time trend is included. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

Table A12: Effect of Tuition Regulation on Tuition and Aid: Dollar Amount

	(1)	(2)	(3)	(4)
Dep. Variable	In-state Tuition($)		Institutional Aid($)	
$\Sigma_{k=4}^{\infty} 1(\text{TuitReg}_{t+k})_{it}$	-465.3	-417.6	-380.7	-367.0
	(252.8)	(234.6)	(145.5)	(133.7)
$1(\text{TuitReg}_{t+3})_{it}$	-83.4	-56.3	-153.7	-119.2
	(121.5)	(126.0)	(75.7)	(71.7)
$1(\text{TuitReg}_{t+2})_{it}$	-118.4	-125.0	-64.3	-47.6
	(88.6)	(99.6)	(58.1)	(55.9)
$1(\text{TuitReg}_{t})_{it}$	-268.3	-326.0	-212.2	-243.4
	(121.8)	(98.3)	(63.3)	(52.5)
$1(\text{TuitReg}_{t-1})_{it}$	-243.7	-520.1	-292.0	-341.6
	(137.8)	(126.9)	(108.8)	(102.4)
$1(\text{TuitReg}_{t-2})_{it}$	-162.2	-510.6	-278.4	-439.8
	(162.8)	(184.9)	(139.5)	(91.5)
$1(\text{TuitReg}_{t-3})_{it}$	-129.0	-488.9	-221.1	-412.6
	(175.6)	(193.6)	(142.1)	(93.5)
$\Sigma_{k=4}^{\infty} 1(\text{TuitReg}_{t-k})_{it}$	-558.6	-518.7	-436.4	-419.8
	(218.8)	(198.7)	(99.0)	(100.9)
TuitCap_{it}		2,217.4		1,158.0
		(2,976.6)		(797.8)
TuitCap_{it-1}		10,135.8		1,509.2
		(4,982.4)		(1,290.7)
TuitCap_{it-2}		11,970.1		5,071.7
		(4,392.0)		(1,763.8)
TuitCap_{it-3}		11,059.2		5,610.4
		(3,642.0)		(1,906.6)
Observations	41,539	41,539	26,446	26,446
R-squared	0.819	0.819	0.612	0.612
Two-way FEs	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. 2. The outcome variables are in-state tuition and fees combined in columns (1)-(2), and the mean institutional aid in column (3)-(4). 3. Two-way fixed effects include institution fixed effects and year fixed effects. 4. A private/public specific time trend is included. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

Table A13: Effect of Tuition Regulation on Tuition and Aid: by Tuition Revenue Dependency

Dep. Variable	log(In-state Tuition)		log(Institutional Aid)	
Sample	*Less Dep.*	*More Dep.*	*Less Dep.*	*More Dep.*
	(1)	(2)	(3)	(4)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it}$	-0.062	-0.074	0.169	-0.258
	(0.037)	(0.028)	(0.075)	(0.090)
$1(\mathrm{TuitReg}_{t+3})_{it}$	-0.023	-0.041	0.036	-0.132
	(0.016)	(0.024)	(0.044)	(0.056)
$1(\mathrm{TuitReg}_{t+2})_{it}$	-0.017	-0.032	0.038	-0.047
	(0.013)	(0.026)	(0.036)	(0.021)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.056	-0.068	0.018	-0.180
	(0.018)	(0.019)	(0.052)	(0.057)
$1(\mathrm{TuitReg}_{t-1})_{it}$	-0.079	-0.085	-0.063	-0.227
	(0.031)	(0.020)	(0.077)	(0.088)
$1(\mathrm{TuitReg}_{t-2})_{it}$	-0.067	-0.068	0.005	-0.313
	(0.029)	(0.025)	(0.087)	(0.111)
$1(\mathrm{TuitReg}_{t-3})_{it}$	-0.071	-0.052	0.014	-0.210
	(0.030)	(0.042)	(0.074)	(0.092)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it}$	-0.107	-0.110	0.023	-0.288
	(0.031)	(0.064)	(0.092)	(0.070)
Observations	17,476	19,513	11,268	12,921
R-squared	0.844	0.920	0.296	0.333
Two-way FEs	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. We classify an institution into *More Dependent* if the ratio of gross tuition revenue to total revenue is above the median of the institutions in the same sector (pubic and private separately) in 1991; *Less Dependent* if below the median. 2. The outcome variables are the log of in-state undergraduate tuition and fees combined in columns (1)-(2) and the log of average institutional aid for first-time undergraduates in columns (3)-(4). 4. Two-way fixed effects include institution fixed effects and year fixed effects. 4. Column (1),(4) controls for quadratic sector-specific time trend, column (2),(5) state-specific linear time trend, and (3),(6) both sector- and state-specific linear time trend. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

Table A14: Effect of Tuition Regulation on Tuition and Aid: by Carnegie Classification

	(1)	(2)	(3)	(4)	(5)	(6)
	log(In-state Tuition)			log(Institutional Aid)		
	Other	*Non-research*	*Research*	*Other*	*Non-research*	*Research*
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it}$	-0.044	-0.069	-0.041	0.074	-0.069	0.035
	(0.034)	(0.038)	(0.026)	(0.193)	(0.074)	(0.079)
$1(\mathrm{TuitReg}_{t+3})_{it}$	-0.054	-0.031	-0.007	0.015	-0.022	-0.014
	(0.025)	(0.024)	(0.013)	(0.112)	(0.091)	(0.054)
$1(\mathrm{TuitReg}_{t+2})_{it}$	-0.023	-0.029	-0.015	-0.020	-0.035	0.039
	(0.019)	(0.024)	(0.009)	(0.078)	(0.068)	(0.059)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.067	-0.059	-0.061	-0.370	-0.011	-0.063
	(0.023)	(0.023)	(0.015)	(0.097)	(0.057)	(0.079)
$1(\mathrm{TuitReg}_{t-1})_{it}$	-0.076	-0.072	-0.088	-0.222	-0.151	-0.115
	(0.022)	(0.026)	(0.030)	(0.080)	(0.081)	(0.102)
$1(\mathrm{TuitReg}_{t-2})_{it}$	-0.067	-0.058	-0.083	-0.374	-0.148	-0.076
	(0.033)	(0.027)	(0.025)	(0.215)	(0.065)	(0.085)
$1(\mathrm{TuitReg}_{t-3})_{it}$	-0.056	-0.047	-0.068	-0.235	-0.108	-0.041
	(0.039)	(0.037)	(0.032)	(0.180)	(0.060)	(0.111)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it}$	-0.102	-0.098	-0.108	-0.220	-0.141	-0.064
	(0.054)	(0.060)	(0.026)	(0.190)	(0.064)	(0.094)
Observations	16,988	15,434	5,444	10,688	10,272	3,716
R-squared	0.806	0.928	0.939	0.254	0.330	0.448
Two-way FEs	yes	yes	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes	yes	yes
State level control	yes	yes	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. *Research* sample is of doctoral universities with high or very high research activity (Carnegie classification). *Non-Research* is sample of master's universities or Doctoral universities with low research activity. *Others* include all other 4+ year degree granting institutions. 2. The outcome variables are the log of in-state undergraduate tuition and fees combined in columns (1)-(3) and the log of average institutional aid for first-time undergraduates in columns (4)-(6). 4. Two-way fixed effects include institution fixed effects and year fixed effects. 4. Column (1),(4) controls for quadratic sector-specific time trend, column (2),(5) state-specific linear time trend, and (3),(6) both sector- and state-specific linear time trend. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

Table A15: Effect of Tuition Regulation on Tuition and Aid: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
Sample	Matching		Ever Treated		Aid Sample
Dep. Variable	log(In-state Tuition)	log(Institutional Aid)	log(In-state Tuition)	log(Institutional Aid)	log(In-state Tuition)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it}$	-0.072	-0.103	-0.096	-0.172	-0.011
	(0.025)	(0.079)	(0.026)	(0.103)	(0.028)
$1(\mathrm{TuitReg}_{t+3})_{it}$	-0.026	-0.099	-0.037	-0.090	0.003
	(0.016)	(0.046)	(0.013)	(0.047)	(0.014)
$1(\mathrm{TuitReg}_{t+2})_{it}$	-0.024	-0.020	-0.031	-0.061	-0.010
	(0.016)	(0.033)	(0.015)	(0.035)	(0.009)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.046	-0.110	-0.041	-0.091	-0.044
	(0.013)	(0.043)	(0.015)	(0.043)	(0.019)
$1(\mathrm{TuitReg}_{t-1})_{it}$	-0.069	-0.173	-0.054	-0.157	-0.047
	(0.017)	(0.067)	(0.020)	(0.067)	(0.017)
$1(\mathrm{TuitReg}_{t-2})_{it}$	-0.064	-0.192	-0.051	-0.172	-0.032
	(0.019)	(0.075)	(0.021)	(0.078)	(0.023)
$1(\mathrm{TuitReg}_{t-3})_{it}$	-0.063	-0.135	-0.054	-0.105	-0.008
	(0.028)	(0.067)	(0.029)	(0.077)	(0.026)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it}$	-0.109	-0.200	-0.090	-0.123	-0.045
	(0.046)	(0.060)	(0.048)	(0.084)	(0.038)
Observations	5,947	3,851	4,138	2,785	25,517
R-squared	0.928	0.311	0.936	0.297	0.860
Two-way FEs	yes	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes	yes
State level control	yes	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. Treated and comparison observations are 1-1 matched in column (1) and (2) based on the Mahalanobis distance in the annual tuition increase rate and the level of tuition from one to three years before regulation. Column (3) and (4) only include ever treated observations. Column (5) includes observations with non-missing institutional aid. 3. The outcome variables are log of in-state undergraduate tuition and fees combined in columns (1), (3), (5), and log of average institutional aid for first-time undergraduates in column (2)-(4). 4. Two-way fixed effects include institution fixed effects and year fixed effects. 5. A private/public specific time trend is included. 6. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 7. Standard errors clustered at the state level are in parentheses.

Table A16: Effect of Tuition Regulation on Tuition and Aid: DiD estimator from **?**

	(1)	(2)	(3)	(4)	(5)	(6)
		Tuition Dependency		Carnegie Classificiation		
	All	Less Dep.	More Dep.	Other	Non-research	Research
			Panel A: log(In-state Tuition)			
$1(\mathrm{TuitReg}_t)_{it}$	-0.035	-0.04	-0.025	-0.026	-0.038	-0.036
	(0.006)	(0.014)	(0.004)	(0.009)	(0.014)	(0.005)
N	5350	2698	2329	880	2240	1580
			Panel B: log(Institutional AId)			
$1(\mathrm{TuitReg}_t)_{it}$	-0.11	-0.019	-0.206	-0.32	-0.088	0.025
	(0.049)	(0.088)	(0075)	(0.100)	(0.067)	-0.131
N	3629	1761	1595	517	1507	1111

Notes: 1. Sample of 4+ year degree granting institutions. We classify an institution into *More Dependent* if the ratio of gross tuition revenue to total revenue is above the median of the institutions in the same sector (pubic and private separately) in 1991; *Less Dependent* if below the median. Research sample is of doctoral universities with high or very high research activity (Carnegie classification). Non-Research is sample of master's universities or Doctoral universities with low research activity. Others include all other 4+ year degree granting institutions. 2. The outcome variables are log of in-state undergraduate tuition and fees combined in panel A, and log of average institutional aid for first-time undergraduates in panel B. 3. DiD estimators proposed in **?** are calculated using the Stata package *did_multiplegt*. We compare the observations that is the first year of the first tuition control to not yet treated observations. 4. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 5. Standard errors clustered at the state level are in parentheses. Standard errors are calculated from bootstrapping with 50 set of samples.

Table A17: Effect of Tuition Regulation on Tuition and Aid: Different Time Trend

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	log(In-state Tuition)			log(Institutional Aid)		
	Sector-Year FE	State	State, Sector	Sector-Year FE	State	State, Sector
$\Sigma_{k=4}^{\infty}1(\mathrm{TuitReg}_{t+k})_{it}$	-0.069	-0.100	-0.056	-0.049	-0.120	-0.037
	(0.028)	(0.025)	(0.027)	(0.069)	(0.064)	(0.069)
$1(\mathrm{TuitReg}_{t+3})_{it}$	-0.024	-0.038	-0.025	-0.042	-0.071	-0.041
	(0.016)	(0.016)	(0.017)	(0.042)	(0.041)	(0.044)
$1(\mathrm{TuitReg}_{t+2})_{it}$	-0.023	-0.030	-0.024	-0.002	-0.029	-0.017
	(0.017)	(0.018)	(0.018)	(0.025)	(0.017)	(0.018)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.057	-0.040	-0.059	-0.111	-0.060	-0.101
	(0.013)	(0.016)	(0.014)	(0.047)	(0.043)	(0.047)
$1(\mathrm{TuitReg}_{t-1})_{it}$	-0.076	-0.059	-0.081	-0.154	-0.106	-0.156
	(0.017)	(0.017)	(0.019)	(0.064)	(0.055)	(0.063)
$1(\mathrm{TuitReg}_{t-2})_{it}$	-0.075	-0.047	-0.069	-0.180	-0.129	-0.185
	(0.018)	(0.020)	(0.019)	(0.076)	(0.068)	(0.077)
$1(\mathrm{TuitReg}_{t-3})_{it}$	-0.071	-0.024	-0.048	-0.124	-0.075	-0.137
	(0.026)	(0.027)	(0.025)	(0.069)	(0.066)	(0.075)
$\Sigma_{k=4}^{\infty}1(\mathrm{TuitReg}_{t-k})_{it}$	-0.124	-0.049	-0.082	-0.168	-0.073	-0.158
	(0.041)	(0.044)	(0.039)	(0.066)	(0.062)	(0.077)
Observations	41,410	41,410	41,410	26,239	26,239	26,239
R-squared	0.857	0.863	0.866	0.293	0.300	0.302
Two-way FEs	yes	yes	yes	yes	yes	yes
State level control	yes	yes	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. 2. The outcome variables are the log of in-state undergraduate tuition and fees combined in columns (1)-(3) and the log of average institutional aid for first-time undergraduates in columns (4)-(6). 3. Two-way fixed effects include institution fixed effects and year fixed effects. 4. Columns (1),(4) include sector-year fixed effects instead of a sector-specific linear time trend, columns (2),(5) a state-specific linear time trend, and (3),(6) both sector- and state-specific linear time trend. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

Table A18: Effect of Tuition Regulation on Instructional Staff Salary, Benefit, and Size

	(1)	(2)	(3)	(4)	(5)
Dep. Variable	log(Benefit Per Student)			log(Salary Per Student)	N Per Student
	Tuition Dependency				
Sample	All	Less Dep.	More Dep.	All	All
$\Sigma_{k=4}^{\infty}1(\text{TuitReg}_{t+k})_{it}$	-0.025	-0.058	0.004	-0.095	-0.043
	(0.040)	(0.064)	(0.052)	(0.034)	(0.016)
$1(\text{TuitReg}_{t+3})_{it}$	-0.024	-0.018	-0.018	-0.059	-0.011
	(0.026)	(0.030)	(0.050)	(0.050)	(0.008)
$1(\text{TuitReg}_{t+2})_{it}$	-0.024	-0.021	-0.017	-0.038	-0.022
	(0.020)	(0.026)	(0.033)	(0.042)	(0.005)
$1(\text{TuitReg}_{t})_{it}$	-0.045	-0.027	-0.057	-0.019	-0.002
	(0.020)	(0.022)	(0.021)	(0.029)	(0.010)
$1(\text{TuitReg}_{t-1})_{it}$	-0.034	-0.026	0.004	-0.026	-0.024
	(0.033)	(0.037)	(0.035)	(0.039)	(0.014)
$1(\text{TuitReg}_{t-2})_{it}$	-0.034	0.002	-0.019	-0.041	-0.028
	(0.034)	(0.043)	(0.033)	(0.045)	(0.018)
$1(\text{TuitReg}_{t-3})_{it}$	-0.031	-0.018	-0.031	-0.021	-0.013
	(0.028)	(0.044)	(0.031)	(0.042)	(0.026)
$\Sigma_{k=4}^{\infty}1(\text{TuitReg}_{t-k})_{it}$	-0.060	-0.104	-0.032	-0.058	-0.014
	(0.051)	(0.122)	(0.030)	(0.041)	(0.020)
Constant	-95.971	-97.221	-98.071	-79.551	-0.769
	(7.416)	(9.495)	(8.299)	(3.017)	(2.325)
Observations	42,604	19,496	19,746	38,527	42,138
R-squared	0.400	0.432	0.578	0.254	0.008
Two-way FEs	yes	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes	yes
State level control	yes	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. 2. The outcome variable is the log of total instructional staff benefits per-student in columns (1)-(3), the log of total instructional staff salaries per-student in column (4), and the number of instructional staff per-student in column (5). 3. We classify an institution into *More Dependent* if the ratio of gross tuition revenue to total revenue is above the median of the institutions in the same sector (pubic and private separately) in 1991; *Less Dependent* if below the median. 4. Two-way fixed effects include institution fixed effects and year fixed effects. 5. A private/public specific time trend is included. 6. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 7. Standard errors clustered at the state level are in parentheses.

Table A19: Effect of Tuition Regulation on Tuition and Aid: Control for State Funding

Dep. Variable	(1) log(In-state Tuition)	(2) log(Institutional Aid)	(3) log(In-state Tuition)
$\Sigma_{k=4}^{\infty} 1(TuitReg_{t+k})_{it}$	-0.071	-0.052	-0.035
	(0.031)	(0.073)	(0.054)
$1(TuitReg_{t+3})_{it}$	-0.032	-0.041	-0.039
	(0.019)	(0.041)	(0.026)
$1(TuitReg_{t+2})_{it}$	-0.025	-0.005	-0.025
	(0.019)	(0.021)	(0.015)
$1(TuitReg_{t})_{it}$	-0.064	-0.114	-0.089
	(0.016)	(0.046)	(0.020)
$1(TuitReg_{t-1})_{it}$	-0.081	-0.153	-0.095
	(0.022)	(0.062)	(0.035)
$1(TuitReg_{t-2})_{it}$	-0.068	-0.171	-0.053
	(0.024)	(0.080)	(0.022)
$1(TuitReg_{t-3})_{it}$	-0.073	-0.154	-0.074
	(0.030)	(0.068)	(0.047)
$\Sigma_{k=4}^{\infty} 1(TuitReg_{t-k})_{it}$	-0.123	-0.177	-0.090
	(0.050)	(0.063)	(0.070)
$log(StateLocalFund)_{t}$	-0.001	-0.003	-0.009
	(0.002)	(0.008)	(0.005)
$log(StateLocalFund)_{t-1}$	0.000	0.008	-0.015
	(0.002)	(0.010)	(0.007)
$log(StateLocalFund)_{t+1}$	0.002	0.013	0.004
	(0.002)	(0.011)	(0.011)
Observations	24,938	15,787	20,215
R-squared	0.894	0.295	0.750
Two-way FEs	yes	yes	yes
Sector specific trend	yes	yes	yes
State level control	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions in columns (1)-(2), and 2+ but less than 4 year degree granting institutions in columns (3). 2. The outcome variable is the log of in-state undergraduate tuition and fees combined in columns (1), (3) and the log of average institutional aid for first-time undergraduates in column (2). 3. log(State Local Fund) is a total sum of appropriation and grants from either State or local government. 4. Two-way fixed effects include institution fixed effects and year fixed effects. 5. A private/public specific time trend is included. 6. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 7. Standard errors clustered at the state level are in parentheses.

Table A20: Effect of Tuition Regulation on Other Charges

	(1)	(2)	(3)	(4)
	4-year Institution		2-year Institution	
Dep. Variable	Fee	log(room and board)	Fee	log(room and board)
$\Sigma_{k=4}^{\infty} 1(TuitReg_{t+k})_{it}$	-9.928	0.002	-32.521	-0.028
	(107.154)	(0.010)	(33.505)	(0.016)
$1(TuitReg_{t+3})_{it}$	2.817	0.000	-26.917	-0.063
	(46.923)	(0.005)	(19.901)	(0.062)
$1(TuitReg_{t+2})_{it}$	8.987	0.002	12.988	-0.037
	(31.422)	(0.003)	(10.185)	(0.031)
$1(TuitReg_{t})_{it}$	-32.644	-0.016	41.516	-0.008
	(69.297)	(0.007)	(41.036)	(0.014)
$1(TuitReg_{t-1})_{it}$	77.262	-0.012	52.511	0.028
	(102.909)	(0.011)	(27.217)	(0.018)
$1(TuitReg_{t-2})_{it}$	77.306	-0.005	56.821	0.009
	(118.445)	(0.008)	(32.155)	(0.016)
$1(TuitReg_{t-3})_{it}$	92.980	-0.010	51.676	0.039
	(111.926)	(0.012)	(34.774)	(0.034)
$\Sigma_{k=4}^{\infty} 1(TuitReg_{t-k})_{it}$	84.222	-0.008	-39.367	0.113
	(112.792)	(0.013)	(34.892)	(0.026)
Observations	26,548	33,937	17,031	5,039
R-squared	0.173	0.863	0.158	0.709
Two-way FEs	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. The outcome variables are the undergrad in-state Fee in columns (1), (3) and log of room and board charged in columns (2), (4). 2. Two-way fixed effects include institution fixed effects and year fixed effects. 3. A private/public specific time trend is included. 4. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 5. Standard errors clustered at the state level are in parentheses.

Table A21: Effect of Tuition Regulation on Out-of-state Students

Dep. Variable	(1) log(Out-of-state Tuition)	(2) % In-state Freshmen	(3) N In-state Freshmen
$\Sigma_{k=4}^{\infty}1(\text{TuitReg}_{t+k})_{it}$	-0.065	0.017	50.978
	(0.031)	(0.006)	(32.781)
$1(\text{TuitReg}_{t+3})_{it}$	-0.026	0.010	19.226
	(0.018)	(0.006)	(22.256)
$1(\text{TuitReg}_{t+2})_{it}$	-0.011	0.010	11.905
	(0.014)	(0.006)	(20.087)
$1(\text{TuitReg}_{t})_{it}$	-0.045	0.009	6.238
	(0.020)	(0.006)	(23.378)
$1(\text{TuitReg}_{t-1})_{it}$	0.010	0.014	32.203
	(0.027)	(0.007)	(28.278)
$1(\text{TuitReg}_{t-2})_{it}$	0.015	0.013	50.068
	(0.033)	(0.009)	(40.397)
$1(\text{TuitReg}_{t-3})_{it}$	0.004	0.009	62.738
	(0.030)	(0.009)	(47.208)
$\Sigma_{k=4}^{\infty}1(\text{TuitReg}_{t-k})_{it}$	-0.078	0.006	18.413
	(0.031)	(0.008)	(46.116)
Observations	41,410	8,147	8,147
R-squared	0.838	0.008	0.104
Two-way FEs	yes	yes	yes
Sector specific trend	yes	yes	yes
State level control	yes	yes	yes

Notes: 1. The outcome variables are the log of undergrad out-of-state tuition and fee combined in column (1), percentage/the number of students in fall cohort who paying in-state tuition rates in column (2) and (3), respectively. 2. Two-way fixed effects include institution fixed effects and year fixed effects. 3. A private/public specific time trend is included. 4. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 5. Standard errors clustered at the state level are in parentheses.

Table A22: Effect of Tuition Regulation on Graduation Rate and SAT Score

	(1)	(2)	(3)	(4)
Dep. Variable	150% time grad. rate	SAT 75	SAT 25	% submitting SAT scores
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t+k})_{it}$	0.006	-0.601	0.657	0.297
	(0.005)	(3.108)	(3.468)	(1.307)
$1(\mathrm{TuitReg}_{t+3})_{it}$	0.003	-1.293	2.626	0.432
	(0.003)	(2.025)	(2.856)	(0.711)
$1(\mathrm{TuitReg}_{t+2})_{it}$	0.002	-0.974	-0.064	0.778
	(0.002)	(1.715)	(2.330)	(0.680)
$1(\mathrm{TuitReg}_{t})_{it}$	-0.002	0.591	-1.337	0.339
	(0.003)	(1.870)	(2.400)	(0.680)
$1(\mathrm{TuitReg}_{t-1})_{it}$	0.003	0.553	-1.194	-4.278
	(0.005)	(2.091)	(3.099)	(2.795)
$1(\mathrm{TuitReg}_{t-2})_{it}$	0.003	0.812	-0.041	-0.810
	(0.006)	(2.747)	(4.184)	(1.086)
$1(\mathrm{TuitReg}_{t-3})_{it}$	0.008	2.703	0.602	-1.460
	(0.005)	(3.501)	(4.840)	(1.209)
$\Sigma_{k=4}^{\infty} 1(\mathrm{TuitReg}_{t-k})_{it}$	0.013	3.972	3.485	-2.988
	(0.006)	(3.929)	(5.414)	(1.334)
Observations	36,666	15,438	15,441	17,047
R-squared	0.065	0.020	0.015	0.081
Two-way FEs	yes	yes	yes	yes
Sector specific trend	yes	yes	yes	yes
State level control	yes	yes	yes	yes

Notes: 1. Sample of 4+ year degree granting institutions. 2. The outcome variables are 150% time graduation rate (=6 years) of cohort started with tuition regulation, 75 percentile of admitted students' SAT score, 25 percentile of admitted students' SAT score, and the percent of applicants submitted SAT score. 3. A private/public specific time trend is included. 4. Two-way fixed effects include institution fixed effects and year fixed effects. 5. State level controls include lag, lead and the current year of state-level unemployment rate. Two dummy variables - one if the majority of both Upper and Lower house are taken by Republicans and the other if by Democrats - are also included. 6. Standard errors clustered at the state level are in parentheses.

B Tuition Freezes/Caps 1990-2019

Tuition Caps and Freezes, 1990-2007

State	Type	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007
Alabama	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0	0
Alaska	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Arizona	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Arkansas	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
California	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
California	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Colorado	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Connecticut	1	–	–	–	–	–	–	–	–	–	–	0	–	–	–	–	–	–	–
Delaware	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Florida	0	–	–	–	–	–	0	–	–	–	–	–	–	–	–	–	–	–	–
Georgia	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Hawaii	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Idaho	0	–	–	–	–	–	–	–	–	–	–	–	–	–	0.1	0.1	0.1	0.1	0.1
Illinois	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Indiana	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Iowa	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Kansas	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Kentucky	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Louisiana	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Maine	2	–	–	–	–	–	–	–	–	–	–	0	0	0	0	0	0	–	–
Maine	3	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Maryland	1	–	–	–	–	–	–	–	–	0.04	0.04	0.04	0.04	0.04	–	–	–	–	0
Maryland	4	–	–	–	–	–	–	–	–	0.04	0.04	0.04	0.04	0.04	–	–	–	–	0
Massachusetts	5	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Michigan	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Minnesota	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Minnesota	7	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Minnesota	6	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Mississippi	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Missouri	8	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Montana	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Nebraska	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Nevada	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
New Hampshire	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0

New Jersey	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0.09	0.08	0.08	0.08
New Mexico	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
New York	9	–	–	–	–	0	0	–	0	0	0	0	0	0	0	–	0	0	0
New York	10	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
North Carolina	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	0	0.065
North Dakota	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
North Dakota	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Ohio	2	–	–	–	–	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	–	–	–	0.06	0.06
Ohio	11	–	–	–	–	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	–	–	–	0.06	0.06
Ohio	12	–	–	–	–	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	–	–	–	0.06	0.06
Oklahoma	2	–	–	–	–	–	–	–	–	–	–	–	–	0.07	0.07	–	–	–	–
Oklahoma	13	–	–	–	–	–	–	–	–	–	–	–	–	0.07	0.07	–	–	–	–
Oklahoma	14	–	–	–	–	–	–	–	–	–	–	–	–	0.07	0.07	–	–	–	–
Oregon	1	–	–	–	–	–	–	–	–	0	0	0	0	–	–	–	–	0.03	0.03
Pennsylvania	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Rhode Island	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Rhode Island	15	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
South Carolina	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
South Dakota	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
South Dakota	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Tennessee	1	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Texas	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Utah	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Vermont	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Virginia	0	–	–	–	–	–	0.03	0.03	0	0	0	-0.2	0	0	–	–	–	–	–
Washington	2	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Washington	11	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Washington	12	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
West Virginia	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–
Wisconsin	1	–	–	–	–	–	–	–	–	–	–	–	0	–	0.08	–	–	–	–
Wyoming	0	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–

Notes: This table lists states and years where state legislatures impose in-state tuition caps and freezes at public institutions. 1990-2013 data from Deming and Walters (2018). We collected 2014-2019 data through a combination of Lexis-Nexis searches of legislation and news articles, communication with state boards of education and legislatures, and verification using legislative records from state websites. Codes for Type: 99 means that the tuition is set by legislature. We do not include this case in the analysis. 1 - Applies only to four-year institutions in the state. 2 - Applies only to two-year institutions in the state. 3- Applies only to University of Maine System. 4- Applies only to St. Mary's college of Maryland. 5- Applies only to University of Massachusetts, Amherst. 6- Applies only to University of Minnesota System. 7- Applies to four-year Minnesota State System. 8- Applies only to four-year institutions whose tuition is above the average. 9- Applies only to CUNY (except 2003) and Cornell (all years). 10- Applies only to SUNY. 11- Applies only to State University. 12- Applies to regional campuses. 13- Applies only to Oklahoma research universities. 14- Applies only to Oklahoma regional institutions. 15 - Applies only to Rhode Island College and The Community College of Rhode Island.

Tuition Caps and Freezes, 2008-2019

State	Type	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Alabama	2	0	0	0	–	–	–	–	–	–	–	–	–
Alaska	0	–	–	–	–	–	–	–	–	–	–	–	–
Arizona	0	–	–	–	–	–	–	–	–	–	–	–	–
Arkansas	0	–	–	–	–	–	–	–	–	–	–	–	–
California	1	–	–	–	–	–	–				–		
California	2	–	–	–	–	–	–	99	99	99	99	99	99
Colorado	0	–	–	–	–	–	–	0.09	0.06	0.06	0.07	0.065	0
Connecticut	1	–	–	–	–	0	–	–	–	–	–	–	–
Delaware	0	–	–	–	–	–	–	–	–	–	–	–	–
Florida	0	0	–	–	–	–	–	–	–	–	–	–	–
Georgia	0	–	–	–	–	–	–	–	–	–	–	–	–
Hawaii	0	–	–	–	–	–	–	–	–	–	–	–	–
Idaho	0	0.1	0.1	0.1	–	0.1	0.1	–	–	–	–	–	–
Illinois	0	–	–	–	–	–	–	–	–	–	–	–	–
Indiana	0	–	–	–	–	–	–	–	–	–	–	–	–
Iowa	0	–	–	–	–	–	–	–	–	–	–	–	–
Kansas	0	–	–	–	–	–	–	–	–	0.036	–	–	–
Kentucky	0	–	–	–	–	–	–	–	–	–	–	–	–
Louisiana	0	–	–	–	–	–	–	–	–	–	–	–	–
Maine	2	–	–	–	0	–	0	–	–	–	–	–	–
Maine	3	–	–	–	–	–	–	–	–	–	–	–	–
Maryland	1	0	0	0	0.03	0.03	0.03	–	–	–	0.03	0.03	0.03
Maryland	4	0	0	0	0.03	0.03	0.03	0	–	–	0.03	0.03	0.03
Massachusetts	5	–	–	–	–	–	–	–	–	–	–	–	–
Michigan	1	–	–	–	–	0.071	0.04	0.0375	0.032	0.032	0.042	max(0.038, $475)	max(0.038, $490)
Minnesota	2	–	–	–	–	–	–	0	0	0	-0.01	0.01	0
Minnesota	7	–	–	–	–	–	–	0	0	–	0	–	0
Minnesota	6	–	–	–	–	–	–	0	0	–	–	–	0
Mississippi	0	–	–	–	–	–	–	–	–	–	–	–	–
Missouri	8	–	–	0	0	–	–	0.017	0.015	0.008	0.007	0.021	0.021
Montana	0	0	0	–	–	–	–	0	0	0	0	0	0
Nebraska	0	–	–	–	–	–	–		–	–	–	–	–
Nevada	0	–	–	–	–	–	–	–	–	–	–	–	–
New Hampshire	2	–	0	–	–	–	0	-0.05	0	0	0	0.025	–
New Jersey	1	–	–	0.03	0.04	–	–	–	–	–	–	–	–
New Mexico	0	–	–	–	–	–	–	–	–	–	–	–	–

New York	9	0	0	–	–	–	–	\$300	\$300	\$300	0	\$200	\$200
New York	10	–	–	–	–	–	–	\$300	\$300	\$300	0	\$200	\$200
North Carolina	1	0.065	0.065	0.065	0.065	0.065	0.065				–	–	–
North Dakota	1	–	–	–	–	–	–	–	–	0.025	0.025	0.04	0.04
North Dakota	2	–	–	–	–	–	–	–	–	0.025	0.025	0.04	0.04
Ohio	2	0.035	0.035	0.035	0.035	0.035	0.035	\$100	\$100	0	0	\$10/credit hour	\$10/credit hour
Ohio	11	0.035	0.035	0.035	0.035	0.035	0.035	max(0.02, \$188)	max(0.02, \$188)	0	0	0	0
Ohio	12	0.035	0.035	0.035	0.035	0.035	0.035	max(0.02, \$114)	max(0.02, \$114)	0	0	0	0
Oklahoma	2	–	–	0	–	–	–	0.05	0.06	0.047	0.086	0.071	0.038
Oklahoma	13	–	–	0	–	–	–	0	0.024	0.047	0.07	0.05	0.016
Oklahoma	14	–	–	0	–	–	–	0.057	0.056	0.049	0.086	0.041	0.046
Oregon	1	–	–	–	–	–	–	0.05	0.05	0.05	0.03	–	0.05
Pennsylvania	0	–	–	–	–	–	–	–	–	–	–	–	–
Rhode Island	1	–	–	–	–	–	–	0	0	–	–	–	–
Rhode Island	15	–	–	–	–	–	–	0	0	0	–	–	–
South Carolina	0	–	–	–	–	–	–	–	–	–	–	–	–
South Dakota	1	–	–	–	–	–	–	–	–	–	–	–	–
South Dakota	2	–	–	–	–	–	–	–	–	–	–	–	–
Tennessee	1	–	–	–	–	–	–	–	–	–	–	–	–
Texas	0	–	–	–	–	–	–	–	–	–	–	–	–
Utah	0	–	–	–	–	–	–	–	–	–	–	–	–
Vermont	0	–	–	–	–	–	–	–	–	–	–	–	–
Virginia	0	0.06	0.04	–	–	–	–	–	–	–	–	–	–
Washington	2	–	–	–	–	–	–	0	–	-0.05	-0.05	0.022	–
Washington	11	–	–	–	–	–	–	0	–	-0.05	-0.15	0.022	–
Washington	12	–	–	–	–	–	–	0	–	-0.05	-0.2	0.022	–
West Virginia	0	–	–	–	–	–	–	–	–	–	–	–	–
Wisconsin	1	–	–	–	–	0.055	0.055	0	0	0	0	0	0
Wyoming	0	–	–	–	–	–	–	–	–	–	–	–	–

Notes: This table lists states and years where state legislatures impose in-state tuition caps and freezes at public institutions. 1990-2013 data from Deming and Walters (2018). We collected 2014-2019 data through a combination of Lexis-Nexis searches of legislation and news articles, communication with state boards of education and legislatures, and verification using legislative records from state websites. Codes for Type: 99 means that the tuition is set by legislature. We do not include this case in the analysis. 1 - Applies only to four-year institutions in the state. 2 - Applies only to two-year institutions in the state. 3- Applies only to University of Maine System. 4- Applies only to St. Mary's college of Maryland. 5- Applies only to University of Massachusetts, Amherst. 6- Applies only to University of Minnesota System. 7- Applies to four-year Minnesota State System. 8- Applies only to four-year institutions whose tuition is above the average. 9- Applies only to CUNY (except 2003) and Cornell (all years). 10- Applies only to SUNY. 11- Applies only to State University. 12- Applies to regional campuses. 13- Applies only to Oklahoma research universities. 14- Applies only to Oklahoma regional institutions. 15 - Applies only to Rhode Island College and The Community College of Rhode Island.

3 WHO SCARS THE EASIEST? COLLEGE QUALITY AND THE EFFECTS OF GRADUATING INTO A RECESSION

3.1 Introduction

To what extent do the adverse effects of graduating into a recession vary with college quality? Economists have long been interested in the short and long-run consequences of exposure to negative economic shocks. Several papers have documented large and persistent negative labor market effects associated with graduating into a recession (e.g., Kahn (2010); Oreopoulos et al. (2012)), and other work has demonstrated that individuals graduating into a worse economy find it more difficult to match to a job that is compatible with their undergraduate field of study (Liu et al., 2016). However, comparatively less is known about how scarring effects differ between students who graduate from higher- versus lower-quality colleges. If these scarring effects are disproportionately concentrated among individuals graduating from better or worse schools, then this heterogeneity may have important implications for income inequality and inter-generational mobility. Additionally, while the existence of a college quality premium has been extensively documented in the economics literature, there has been less attention paid to whether and to what extent this premium varies over the business cycle.

We study this question in the context of the 2008 financial crash and the subsequent Great Recession using restricted-use versions of the National Surveys of College Graduates (NSCG). Using a fixed effects design and leveraging variation in unemployment rates at graduation across states and over time, we find that the earnings losses of entering the labor market during a recession are larger for graduates from high-quality institutions relative to their peers who graduate from lower quality colleges. We find that on average, a student who attends a one standard deviation higher quality college earns around \$3,700 more per year. However, this difference is \$384 (i.e., around 10%) smaller for students graduating in a state with a one percentage point higher unemployment rate.

We identify several mechanisms behind these results: substitution out of the labor force and into graduate school, decreased graduate degree completion, differences in the economic stability of fields of study between graduates of high- and low-quality colleges, and decreased labor mobility. First, graduates from higher quality colleges who graduate into a recession are more likely to enroll in graduate school than those from lower quality colleges. This lowers their labor force participation and earnings while they are enrolled in graduate school but may not necessarily mean long-term earnings decreases if they see large increases in earnings upon completion of their higher degree. However, we also provide evidence that although students from higher quality college are more likely to *enroll* in graduate school in the following years, they are *less* likely to *complete* a graduate degree, suggesting that the negative earnings effects are unlikely to reverse. In fact, we find qualitatively similar results when limiting the sample to individuals who are in the labor force and not currently enrolled in graduate school, although the magnitude of these results is attenuated from our main estimates. A similar pattern emerges when limiting the sample to individuals who are neither enrolled in graduate school nor have any graduate degrees.

Our next main mechanism is undergraduate field of study: students from high-quality colleges are more likely to major in fields that are more adversely affected by the recession (e.g. STEM, social science) while students from lower quality colleges are more likely to major in fields that are resilient over the business cycle (e.g., education). As a final mechanism behind our earnings results, we find suggestive evidence that graduating into a recession reduces labor mobility relatively more for higher-quality college graduates.

The remainder of the paper proceeds as follows. Section 3.2 reviews the relevant literature and details our contribution to it. Section 3.3 describes our data and empirical strategy for studying our research question. Section 3.4 presents our results and gives a more detailed comparison of our results to related recent work, and Section 3.5 discusses some broader implications of our findings before concluding.

3.2 Literature

This paper contributes to several strands of the economics literature, most directly to the literature that studies the persistent "scarring" effects of recession exposure upon labor market entry on individual outcomes (see von Wachter (2020) for a recent review). Graduating into recessions is associated with substantially depressed earnings for at least 10 years. While some work has suggested that these scarring effects fade after approximately a decade, other work has found that the effects can reemerge later in life and be near-permanent (Schwandt and von Wachter, 2019; Stuart, 2022). As data availability increases, a growing literature has studied the effects of the Great Recession by leveraging spatial variation in the shocks it induced, generally finding that the scarring effects associated with these shocks are severe (Rinz, 2019; Yagan, 2019; Rothstein, 2021).

A smaller subset of this literature considers heterogeneity of scarring effects between more and less advantaged groups of individuals. Schwandt and von Wachter (2019) find that scarring effects are larger for high school dropouts relative to individuals with more education, and larger for white individuals than non-white individuals. Arellano-Bover (2022) finds that the negative effects of bad labor market conditions on skill development are larger for individuals with lower parental education. Our findings that graduates of higher-quality colleges suffer greater losses than graduates of lower-quality colleges demonstrate that these patterns are not uniform, especially when focusing on four-year college graduates.

Two existing papers have also considered how the effects of graduating into a recession vary by college type. Both Oreopoulos et al. (2012), who study Canadian college students who graduated into recessions in the early 1980s and 90s, and Weinstein (2023), who studies graduates of selective colleges in the United States during the Great Recession, find that losses from graduating into a recession are larger among graduates of more selective colleges. However, compared to these studies, we use a broader sample that more comprehensively captures heterogeneity in scarring effects among college graduates. We include individuals with zero earnings, capturing the effect of graduating into a recession on individuals' labor force participation decision. This is particularly important in the context of the Great Recession, during which real wages stayed fairly stable while employment collapsed. Additionally, we use variation across the entire college quality distribution of the United States while Weinstein (2023) focuses on differences between graduates of elite (i.e., very high-quality) universities and the rest of the distribution. A deeper comparison between our work and these papers may be found in Section 3.4.

Our paper also fits into the broader literature on the relationship between college quality and earnings. Papers in this literature generally find that attending a higher-quality college increases earnings (Black et al., 2005; Hoekstra, 2009; Zimmerman, 2014; Smith et al., 2020; Kozakowski, 2023), with effects persisting up to 30 years after college attendance (Dillon and Smith, 2020).[1] Our primary contribution on top of these papers is to explore how differences in earnings between graduates of higher- and lower-quality colleges vary over the business cycle.

Our work explores many mechanisms behind the main earnings effects, many of which have been studied in previous literature. First, our work contributes to the literature on how recessions affect higher education enrollment. Barr and Turner (2013) and Long (2014) have both shown that the Great Recession led to increases in college enrollment for undergraduates. We find that these countercyclical enrollment patterns hold for graduate enrollment as well, particularly graduates from high-quality institutions. Bedard and Herman (2008) and Kahn (2010) also study the effects of economic downturns on graduate school enrollment and graduate degree attainment over earlier periods. Findings are mixed across recession measures and demographic groups. We add to these findings by investigating how graduate school enrollment during recessions varies by undergraduate college quality. Our findings of strong graduate enrollment effects among graduate of high-quality undergraduate institutions suggests that noisiness of previous estimates may be due to heterogeneity in effects by college quality.

Finally, our work relates to the literature on the relationship between labor markets conditions and major/occupation. Altonji et al. (2016) find that the negative effects of graduating into a recession are concentrated among lower-paying majors, but the effects of the Great Recession are more evenly distributed across majors than earlier recessions. We focus on broad major categories and find that STEM and social science graduates experience relatively worse effects of graduating in a worse economy, while education majors perform better. Prior work has also found that the teaching profession is more stable through recessions than other occupations (Kopelman and Rosen, 2016; Nagler et al., 2020; Deneault, 2023). Another potential mechanism is cyclical skill mismatch (i.e., when an individual is working in a field requiring different skills than the field in which they were trained). Previous literature has explored this, such as Liu et al. (2016) who find that the likelihood of skill mismatch at a worker's initial job is higher in worse economic conditions. We explore this effect and how it varies over college quality, but do not find any evidence for meaningful

[1] In contrast, a few other papers have found limited scope for college quality to increase earnings - see Dale and Krueger (2002, 2014); Mountjoy and Hickman (2021).

heterogeneity. We also investigate changes in geographical mobility as a mechanism for earning losses. Yagan (2014) found that migratory insurance, where individuals in heavily shocked areas move to more prosperous areas for economic opportunity, played a relatively small role in the Great Recession compared to earlier recessions. We find evidence that graduates of higher quality colleges are more prone to decreases in inter-state mobility, which could contribute to their larger earnings losses relative to lower quality college graduates.

3.3 Data and Empirical Strategy

We use restricted-access versions of the National Survey of College Graduates (NSCG) 2010, 2013, 2015, 2017, and 2019 accessed via the Census RDC (U.S. Census Bureau, 2023). The NSCG sample is drawn from the American Community Survey and includes individuals who have earned a bachelor's degree, reside in the United States or Puerto Rico, and are younger than 76 years old. We restrict our sample to include individuals who earned their BA between 2000 and 2012 to focus on the Great Recession while maintaining a reasonably narrow range of cohorts and ages among individuals in the sample.[2] These sample restrictions imply that respondent's earnings are measured between one and 19 years after they graduate from college. While the dynamics of how earnings impacts change over time is certainly of interest, unfortunately we do not have enough power to separately explore effects by time since graduation.

The restricted-use version includes information on the exact college from which respondents obtained their degrees which we link to the Integrated Postsecondary Education Data System (IPEDS) to construct the quality of the college attended (U.S. Department of Education, 2021). Following Dillon and Smith (2020), our college quality measure is an index combining the pseudo-median SAT score of entering students (midpoint of 25th and 75th percentiles), the applicant rejection rate, the student-faculty ratio, and the average salary of faculty engaged in instruction. We take the first principal component of this index and use it to calculate percentiles of our index across the enrollment-weighted distribution of four-year non-specialty colleges in the United States.[3] In our specifications, we use both a standardized version of this continuous measure and an alternative measure which includes indicators for each quality quartile.

Table A1 presents summary statistics for our sample. Linking individuals to their institutions of graduation requires that they graduate from a U.S. college, which forces us to drop any individuals in the sample that obtained their degree from an international school before moving to the U.S. We present summary statistics for our sample both with and without these dropped individuals — the restriction reduces the proportion of Asian individuals in the sample, but other variables such as income, unemployment, and rates of graduate degree attainment do not change meaningfully,

[2]We note that we only observe college graduates, so our analysis may be understating the degree to which labor market outcomes vary by college quality since higher-quality colleges boost graduation rates (see Dillon and Smith (2020), among others).

[3]Also following Dillon and Smith (2020), we use 2008 as our base year and calculate our college quality index for any college that has at least two of the four proxies.

suggesting that linking individuals to their exact institution of study does not inject meaningful selection into the sample. As the sample includes only those who obtained a college degree, it is not representative demographically or economically of the U.S. as a whole.

Table A2 presents summary statistics for the analysis sample, broken down by college quality quartile.[4] A few differences emerge. Demographically, the proportion of Asians increases with college quality, especially for top-quartile graduates, while earnings, graduate degree attainment, maternal education, and increase monotonically as we move up the quality distribution. Graduates of higher quality colleges are also generally more likely to move away from their state of graduation and/or state of birth.

To first understand the main effects of graduating into a worse labor market and graduating from a high-quality college, we estimate:

$$Y_{istr} = \phi_r + \gamma_s + \theta_t + \alpha_0 X_i + \alpha_1 U_{st} + \alpha_2 Q_i + \epsilon_{istr}, \tag{3.1}$$

where Y_{istr} is an outcome variable of interest for individual i who graduated in year t from a college in state s and was surveyed in year r. The specification includes survey year fixed effects ϕ_r to strip out macroeconomic trends, state of graduation fixed effects γ_s to control for differences in state means in the outcome variable, and cohort fixed effects θ_t to account for changes in outcomes common across all graduates of a particular year. Note that the combination of the survey year and cohort fixed effects controls for years of (potential) experience since graduation. We also include a vector of individual characteristics X_i, which includes indicators for race, sex, ethnicity, and mother's and father's education level. The main variables of interest are U_{st}, the unemployment rate of the state s from which individual i graduated in year of their graduation t, and Q_i, the quality of i's college of graduation. Thus, α_1 represents the effect of graduating into a labor market with a one percentage point higher unemployment rate. Individuals who graduate from a one standard deviation higher quality college earn α_2 dollars more, on average.[5] We include an error term ϵ_{istr} and cluster our standard errors at the state of graduation by cohort level.

To assess heterogeneity in the effects of the recession over college quality, we next estimate the specification:

$$Y_{istr} = \eta_r + \delta_s + \xi_t + \beta_0 X_i + \beta_1 U_{st} + \beta_2 Q_i + \beta_3 U_{st} Q_i + \beta_4 U_{st} X_i + \varepsilon_{istr}. \tag{3.2}$$

The parameter of interest β_3 quantifies the extent to which the impacts of graduating into a worse labor market differed for individuals based on the quality of the college from which they graduated. A positive sign would imply that graduates from higher-quality colleges were harmed relatively less by graduating into a bad labor market than their peers from lower-quality colleges. On the other hand, a negative sign would imply the college quality earnings premium is smaller during

[4]Note that since quality quartiles are created before individuals in our sample are merged to them, the number of individuals in each quartile need not be the same and indeed are not due to higher-quality colleges exhibiting substantially higher graduation rates.

[5]One standard deviation is equivalent to about a 30 percentile increase in the college quality distribution.

recessions than it is during good times. We also include the unemployment rate interacted with cohort fixed effects and individual controls, $U_{st}X_i$, to account for differing effects over the business cycle.

Because we do not have exogenous variation in college quality, we interpret our results as differences in the scarring effects of recessions between graduates of higher- and lower-quality colleges, but not necessarily the causal effect of attending a higher-quality college on scarring effects. We control for some of the selection into college quality by including the following individual characteristics in X_i: sex, race, ethnicity, mother's education level and father's education level. Even so, students likely sort into colleges based on unobservable factors, such as student ability. The direction of this selection is likely positive: if higher-ability students are more likely to attend higher-quality colleges and are also more likely to earn more, this would mean that our coefficient is upwardly biased. In our baseline results, correcting for this would have the effect of making the negative effect we uncover stronger. We also investigate whether selection into college varied considerably over the time range we study and find little evidence to this effect; see section A.

A natural question is whether the state-level unemployment rate is the most relevant measure of college graduates' labor market. In Section 3.4, we explore sensitivity of our results by using alternative unemployment rate measures. First, we employ data on where colleges' graduates locate from LinkedIn, collected by Conzelmann et al. (2022b), and find little difference in results from our baseline analysis using state-level unemployment rates (Conzelmann et al., 2022a). We also present results that use the national unemployment rate as the source of variation. An advantage of this analysis is that focuses directly on the effects of graduating into the Great Recession as opposed to being located in local labor markets that were more or less affected by it; its limitation is that it prevents us from being able to include cohort fixed effects due to immediate collinearity issues. Still, the general conclusions from this analysis confirm our main results, shown in section B.

Our primary outcomes of interest are earnings and labor force participation. In our main specification, we measure earnings in levels and winsorize at the 95th percentile to prevent the large right tail in earnings among college graduates from dominating our results. To measure labor force participation, we use estimate linear probability[6] models with the following indicator variables as outcomes: employed, unemployed, out of the labor force, current enrollment in graduate school, and "discouraged" (i.e., out of the labor force and not enrolled in graduate school).

3.4 Results

Baseline

Table A3 shows our baseline results from estimating equations (3.1) and (3.2) where the outcome is annual earnings. We do not condition on labor force participation, so individuals with zero earnings are included. The first column focuses on the separate effects of the unemployment rate and college

[6]We explore the sensitivity of our results under alternative models and alternate earnings outcomes in Section 3.4.

quality on annual earnings. The estimate of U_{st} implies that a one percentage point increase in the state unemployment rate upon graduation decreases annual earnings by around 759 dollars. On average, we find that students who graduate from a one-standard deviation higher-quality college earn 3,700 dollars annually.

The second column of Table A3 includes the interaction term of the unemployment rate and college quality and thus gives insight into how scarring effects vary with college quality. We find that a one percentage point increase in the unemployment rate shrinks the earnings advantage of a one standard deviation increase in quality by around 384 dollars, roughly 10 percent of the baseline college quality differences we find in the specification without the interaction. This result may be viewed graphically in Figure A1, where we estimate earnings returns to an increase in college quality separately for each graduation cohort from 2000 to 2012: the returns hold roughly steady from the cohorts of 2000 to 2007 before dropping sharply in 2008 when the recession began.

The final two rows of table Table A3 use our second measure of college quality, where we include indicators for each quartile of the enrollment-weighted college quality distribution.[7] As expected, we see in column 3 that earnings are strongly increasing in college quality. This specification suggests that the college quality earnings differences are nonlinear - while moving from the first quartile to the second quartile increases annual earnings by about 800 dollars (not statistically significant), moving from the second to third and from third to fourth quartile each increase earnings by around 4,000 dollars. The interaction terms align with our results from the continuous measure, showing that individuals who attended a higher-quality college experienced a larger earnings penalty from graduating into a recession than those who attended lower quality colleges.

Table A4 shows our main results for labor force participation. Binary variables are scaled by 100 so that effects can be interpreted as percentage point changes. We see in column 1 that employment sharply decreases during a downturn for individuals who attended higher quality colleges. The effect of a one standard deviation increase in college quality when the state unemployment rate is one percentage point higher is a 0.32 percentage point decrease in the probability of being employed. Column 2 shows that this is driven by individuals dropping out of the labor force rather than shifting into unemployment. The last two columns show that graduating from college during an economic downturn increases the probability of being enrolled in graduate school, and this effect is amplified for students who have graduated from high-quality colleges, especially those in the top quartile. In fact, the increased probability of being currently enrolled is roughly equal to the decrease in labor force participation. Moving back to column 3, we show the effects of being "discouraged," which we define as being out of the labor force and not enrolled. The interaction term between college quality and the unemployment rate at graduation is positive but statistically insignificant. For employed individuals, we additionally explore how the effect of graduating in a recession varies by college quality for their number of hours worked. Column 5 shows that, unlike the large extensive margin effects on labor force participation, the change in hours conditional on

[7] We prefer this to a subgroup analysis because the latter involves the generation of multiple smaller samples and implicit samples, which can make passing disclosure review from U.S. Census highly cumbersome.

being employed does not vary much over the college quality distribution. Among the employed, we also investigate the probability of working in one's field of study, but do not find any evidence that the probability of working outside one's field during a recession varies by college quality.[8]

Mechanisms and Heterogeneity

Given the results from Oreopoulos et al. (2012) and Weinstein (2023) that found that individuals from more selective colleges fared better when graduating into adverse labor market conditions, the nature of the heterogeneity we find may be surprising. We next aim to unpack the mechanisms driving our results so as to justify them and better situate them in the previous literature.

The results displayed in Table A4 suggest that substitution from labor force participation to graduate school enrollment may be an important driver of our earnings results. However, if graduates from high quality colleges who enroll in graduate school earn higher returns from their graduate degrees upon completion, they may eventually end up outearning their peers from lower quality colleges who did not enroll in graduate school. Next, we further delve into the graduate school enrollment results and investigate whether the higher enrollment is leading to higher graduate degree attainment.

Table A4 shows results for whether respondents are “currently enrolled" in graduate school at the time that they are surveyed. For some graduates, we observe this measure multiple times, since the NSCG is a panel for some respondents. In results presented in Table A5, we include only one observation per person and define the outcome as “ever observed enrolled", which takes a value of one if we observe respondents as currently enrolled at least once when responding to the survey. Note that this measure takes a value of zero for respondents who hold a graduate degree but completed it before they are surveyed. We prefer this measure since we are also interested in drop out from graduate school. We have no way of knowing if an individual enrolled in graduate school before dropping out if we do not observe them enrolling in the first place. Thus, we would be introducing bias by counting an individual who we observe with a graduate degree (but not enrolled) as “ever enrolled" but not counting an individual who enrolled and dropped out before we observe them.

Table A5 shows the results for “ever observed enrolled" in any graduate school, as well as broken down results by degree type (Master's, PhD, or professional). We concentrate on the quartiles college quality measure, since the result from Table A4 showed that the interaction effect of the unemployment rate at graduation and college quality on being “currently enrolled" was driven by graduates from the top quality quartile. The first column shows that relative to bottom quality quartile college graduates, graduates from the top quality quartile who graduated into a labor market with a one percentage point higher unemployment rate were 0.88 percentage points more

[8]The NSCG includes a question asking respondents, “To what extent was your work on your principle job related to your highest degree?" Option responses are closely related, somewhat related, and not related. We count responses of “not related" as working outside one's field.

likely to have ever enrolled in any graduate program. This effect is driven by enrollment in PhD programs and professional programs (e.g., law school, medical school).

Next, we examine the effect of graduating in a recession on the probability of completing a graduate degree. In Table A6, we include one observation for each individual and estimate whether they hold any graduate degree by the last time we observe them (unconditional on us ever observing them as being enrolled). We find that relative to bottom quality quartile college graduates, although graduates from high quality colleges are more likely to enroll in graduate school if they graduated into a worse labor market, they are *less* likely to hold a completed degree. This suggests that individuals from high quality colleges who graduate into a recession are likely to drop out of their graduate program before completing it and therefore that our main finding of earnings losses from graduating into a recession being concentrated among graduates from high quality colleges is unlikely to be reversed over time. Additionally, since these results are not conditional on being observed as enrolled, this is likely a combination of negative selection of students into graduate school during a recession, as well as an increased probability of dropout among students who would have pursued graduate school absent the recession.

The second mechanism that we uncover for our negative earnings effects is field of study. First, we show that there are differences in major choice across colleges that vary by the college's quality. Table A7 shows the percentage of each college quality quartiles' graduates who graduate with degrees in five broad major categories: STEM (i.e., science, technology, engineering, and math), social sciences, health, education, and business. Graduates from high quality colleges are much more likely to major in STEM: 28 percent of graduates from the top quartile choose a STEM major, compared to just 13 percent of graduates from the bottom quartile. High quality college graduates are also more likely to complete majors in the social sciences. On the other hand, graduates from lower quality colleges are more likely to major in health and business. They are also much more likely to major in education: bottom quartile graduates are over three times as likely to major in education as top quartile graduates.

These differences in majors have implications for how graduates from different colleges will fare when graduating into a worse labor market, since some majors are much more stable over the business cycle than others. In Table A8, we show how the returns to these majors vary with the unemployment rate that students face at graduation. In this specification, we do not include the interaction term of college quality with the unemployment rate, but rather include an interaction term of the unemployment rate with each major group.[9] Table A8 shows that while the earnings of individuals who major in STEM and social sciences tend to decline when the unemployment rate is higher, individuals who major in education actually earn more if they graduated into a labor market with a higher unemployment rate. Thus, part of the reason that we find stronger earnings losses from graduating into a recession among individuals who attended high quality colleges is because graduates from high quality college tend to major in subjects that are more sensitive to fluctuations in the business cycle.

[9]We sill include the main effect of college quality to capture averages differences in earnings across colleges.

Next, we explore heterogeneity in our results by sex.[10] Table A9 show results for separately estimating equation (3.2) for men and women. We find that the negative earnings effects of graduating into a recession for graduates from high quality colleges are stronger for women. We find that a one percentage point increase in the state unemployment rate decreases the returns to a one standard deviation increase in college quality by over 550 dollars for women, but only around eighty dollars for men.[11] We also examine differences between men and women in graduate degree enrollment and attainment. It appears that the positive effects we find for graduate enrollment are driven by women, while the negative effects for degree attainment are driven by men.

Finally, we investigate the interaction between college quality and economic conditions upon graduation on labor mobility. College graduates (particularly those from high-quality colleges) are highly geographically mobile, and this propensity to move for higher-paying jobs is an important recent driver of the college earnings premium (Diamond, 2016). However, research has indicated that the Great Recession depressed labor mobility, which offers another potential mechanism behind our main results. Table A11 probes this issue, and we find suggestive evidence that higher quality college graduates are less likely to move out of the state of their college by the time they are observed when they graduate into a worse labor market. The effects appear to be stronger for men: men from third and fourth quartile schools who experience a one percentage point higher unemployment rate upon graduation are each about two percentage points less likely to migrate, relative to men from bottom quality quartile colleges.

Taken together, our investigation points to several mechanisms behind our main result that earnings losses from recessions are relatively higher for graduates from higher quality colleges. First, graduates from high quality colleges substitute out of the labor force and into graduate school when they experience a worse labor market upon graduation. However, they are unlikely to complete these graduate degrees and are ultimately less likely to hold an advanced degree if they graduated into a recession. Second, graduates from lower quality colleges tend to major in fields that are more resilient to recessions. Finally, graduating into a recession may decrease labor mobility for high-quality college graduates, especially for men.

Robustness

We also estimate specifications where we restrict the sample to individuals with only a BA (i.e., those who have not obtained and are not currently enrolled in any graduate school) or to those who are both in the labor force and not currently enrolled in any graduate school. We do not prefer these specifications since they condition on endogenous variables, but still find them valuable in understanding how much of our main result is coming through labor force participation/graduate school enrollment. Results are presented in the first four columns of Table A12. We find qualitatively

[10]In additional analyses for which we have not disclosed the precise point estimates, we studied whether our results varied meaningfully by Census region (Northeast, Midwest, South, East) or race and ethnicity and found little evidence for either.

[11]However, we cannot reject the null that the estimates for men and women are equal to each other.

similar results to our main findings, although the estimates are smaller and often not statistically significant.

The final two columns of Table A12 address the question of what the most relevant labor market is for college graduates. We use "Grads on the Go" data, provided by Conzelmann et al. (2022b). For each college, they collect data from LinkedIn on where its graduates locate and provide the fraction of each college's graduates that live in each state. We use this data to construct college-specific unemployment rates for each year by multiplying each state's unemployment rate (in the relevant year of graduation) by the college's share of graduates residing in that state.[12] Results are very similar to our baseline specification using state-level unemployment rates.

In Table A13, we also experiment with measuring earnings in logs as well as log-plus-1 to avoid dropping zeros. We also include results where we use hourly wages, measured as total earnings divided by hours worked in the previous year, as the dependent variables instead of annual earnings. For our main binary outcomes, we additionally present average marginal effects from probit models (rather than the baseline linear probabilty models) in Table A14. In all cases our results hold qualitatively.

We also assess whether our results are sensitive to our measure of college quality by using each individual sub-index of college quality (faculty-student ratios, rejection rates, faculty salaries and test scores) as our measure of college quality instead. While we were not able to disclose the point estimates of this exercise, the sign and statistical significance of our estimates do not change relative to the baseline results when using any individual component. We also run specifications with the Barron's selectivity categories, although we note that the Barron's categories capture a different sort of heterogeneity than our main quality measure - they provide several small categories at the top of the quality distribution but group together around three quarters of the sample into one category for the lower/middle parts of the distribution. Still, the qualitative results from this exercise match our baseline results, although the estimates are not statistically different from each other.

As a final test, we also use coarser measures of time and recession severity to construct a 2X2 difference-in-difference setup to address potential lingering concerns about our baseline identification strategy. Our first difference is before/after the recession in 2008, and our second difference is based on the change in the state unemployment rate between 2007 and 2009, as in Yagan (2019). We characterize states as receiving a "bad shock" if the unemployment rate change is above the median. Table A15 show our results. The first column shows that relative to bottom quality quartile graduates, individuals who graduated after the recession from a top quartile college in a state with a bad recession shock earn around 6,000 dollars less than those in a state with a less severe recession shock, after accounting for the earnings differences between these states before the recession.

[12]Note that the timing of the college's shares in each location is slightly misaligned with the timing of our sample: the LinkedIn data uses graduates from 2010 to 2015.

Comparison to Other Studies

In this section, we take a more detailed look at explanations for differences in our results from two other studies that have considered how the effects of graduating into a recession vary across types of colleges. Oreopoulos et al. (2012) find that Canadian college students who graduated into a recession suffered smaller and less persistent earnings losses if they graduated from generally higher-earning majors and colleges. Several important differences between the setting and methods of Oreopoulos et al. (2012) and our work are worth highlighting: in addition to focusing on an earlier time period (graduates from 1976 to 1995 as opposed to 2000-2012) in a different country, the authors restrict their sample to only men with strictly positive earnings and no graduate degrees, thus missing any effects on women as well as considerably reducing the role that substitution from the labor force toward further education can play in their analysis.[13]

The setting in Weinstein (2023) is closer to ours, as he also uses variation from the Great Recession in the United States. However, there are several methodological differences that lead to our seemingly opposing results. The first is a difference in college quality/selectivity measures. Weinstein (2023) uses Barron's categories, which provide a high degree of detail at the top of the distribution but little variation in the middle and bottom of the distribution. The entirety of the top two categories that Weinstein uses (Ivy Plus and Barron's Tier 1 (Elite)), along with 95 percent of students in his third category (Barron's Tier 2 (Highly Selective)) fall within our top quality quartile. Meanwhile, his fourth category (Barron's Tiers 3-5), which is used as the base category in his analysis, spans all four of our quality quartiles.[14] Thus, we make broader comparisons across the college quality distribution while Weinstein's comparison is more akin to elite universities versus the rest of the distribution. In our view, this distinction allows our papers to be quite complementary to one another.

Second, we use different earnings measures. Our primary earnings measure is mean earnings in levels, which we choose to capture endogenous differences in labor force participation, while Weinstein's main measure is the log of each college's median income after restricting to positive earners, which may understate the role of substitution out of the labor force into graduate education in a similar manner to Oreopoulos et al. (2012). Third, the (implicit) weighting differs between our sample and Weinstein's. After applying the NSCG's sampling weights, our student-level data is nationally representative of bachelor's degree holders, so our results represent the mean impact across all college graduates. Weinstein's data is institution-level, so smaller universities carry more weight per student.

Since Weinstein uses public-use mobility report card data, we are able to directly show how these three differences affect results. When we use Weinstein's specification and data but change

[13]It is also worth acknowledging that the authors assess the robustness of their results to including workers with graduate degrees and find little change, and the authors do not find significant impacts on labor force participation. However, the importance and prevalence of graduate degrees increased considerably between the 1980s and the 2000s, which provides another potential explanation for why substitution out of the labor force into graduate education plays a larger role in our analysis.

[14]Specifically, 29 percent of students in Barron's Tiers 3-5 fall in our bottom quality quartile, while 32, 30, and 8 percent fall in our second, third, and top quartiles, respectively.

the college quality measure from Barron's categories to our quartiles measure, use mean earnings as the outcome variable, and weight by institution size, we broadly replicate our results. Details can be found in section C.

3.5 Conclusion

Graduating into a recession is associated with losses in earnings, but less is known about how these effects vary based on where an individual graduated from. We study how the effects of graduating into an economic downturn vary with college quality in the context of the Great Recession. Using restricted-use data from the National Survey of College Graduates, we find that graduation into worse economic conditions is associated with earnings losses that are concentrated among graduates from relatively high-quality colleges. We identify several mechanisms behind these results: first, graduates from high-quality colleges who graduate during a worse labor market are more likely to exit the labor force and enroll in graduate school. However, they are *less* likely to earn graduate degrees, implying increased levels of dropout both for marginal enrollees as well as those who would have enrolled absent the recession. Second, relative to lower-quality college graduates, graduates from high-quality colleges tend to major in fields that are more sensitive to business cycle fluctuations, so a recession affects the earnings of graduates from high-quality college more. Third, labor mobility appears to decrease for students from high-quality college when they graduate in a downturn. These findings suggest that who stands to lose the most from graduating into a recession may be more subject to context than previously thought.

These findings also may have considerable implications for how the Great Recession impacted the economic mobility for those who graduated into it. The backgrounds of students varies considerably over the college quality distribution: more than 10% of students in bottom-quartile colleges had parents in the bottom quintile in the national income distribution, while the corresponding statistic for students in top-quartile colleges was less than 5% — further, the proportion of students in these colleges with parents in the top income percentile was 0.8% and 7.7%, respectively.[15] Thus, the heterogeneity we find suggests a potential leveling of the playing field for individuals who graduated into the recession, at least among college graduates. Further investigations into how our results evolve as time passes will likely be worthwhile.

[15]These statistics obtained from using our measures of college quality in conjunction with college mobility report cards from Chetty et al. (2020).

3.6 Tables and Figures

Table A1: Summary Statistics

Variable	Mean/SD	Mean/SD
Age	34.65	34.36
	(8.14)	(8.01)
Asian	0.11	0.08
	(0.32)	(0.27)
Black	0.11	0.11
	(0.31)	(0.31)
White	0.79	0.83
	(0.41)	(0.37)
Hispanic	0.11	0.09
	(0.31)	(0.29)
Married	0.64	0.64
	(0.48)	(0.48)
Has MA	0.25	0.24
	(0.43)	(0.43)
Has Professional Degree	0.04	0.05
	(0.20)	(0.21)
Has PHD	0.02	0.02
	(0.14)	(0.13)
Mother college dummy	0.40	0.41
	(0.49)	(0.49)
STEM BA	0.21	0.19
	(0.41)	(0.39)
Undergraduate Loans ($1,000s)	10.34	10.38
	(19.82)	(19.43)
Total Income ($1,000s)	54.97	50.69
	(45.36)	(35.16)
Unemployed	0.03	0.03
	(0.18)	(0.17)
Not in Labor Force	0.08	0.08
	(0.28)	(0.27)
Currently Enrolled in Graduate Program	0.11	0.11
	(0.31)	(0.31)
Currently Enrolled in MA	0.06	0.06
	(0.23)	(0.23)
Currently Enrolled in Prof. Degree	0.01	0.01
	(0.10)	(0.10)
Currently Enrolled in PHD	0.02	0.01
	(0.13)	(0.12)
Moved from State of Graduation	0.41	0.36
	(0.49)	(0.48)
Moved from State of Birth	0.53	0.50
	(0.50)	(0.50)
Has Children	0.45	0.44
	(0.50)	(0.50)
Sample	Whole	Analysis
Observations	173,000	144,000

Notes: Data from 2010, 2013, 2015, 2017, and 2019 Waves of the National Survey of College Graduates; see text for details. Analysis sample contains individuals with undergraduate institutions that can be linked to IPEDS data. Cell counts rounded following disclosure avoidance protocols.

Table A2: Summary Statistics by College Quality

Variable	Mean/SD	Mean/SD	Mean/SD	Mean/SD
Quality Quartile	1	2	3	4
Age	36.60	35.34	33.73	32.61
	(9.25)	(8.88)	(7.25)	(6.37)
Asian	0.04	0.05	0.06	0.15
	(0.19)	(0.21)	(0.23)	(0.35)
Black	0.14	0.13	0.10	0.07
	(0.34)	(0.34)	(0.30)	(0.25)
White	0.83	0.83	0.86	0.81
	(0.37)	(0.37)	(0.35)	(0.39)
Hispanic	0.09	0.09	0.10	0.09
	(0.29)	(0.28)	(0.30)	(0.29)
Married	0.67	0.65	0.64	0.60
	(0.47)	(0.48)	(0.48)	(0.49)
Has MA	0.22	0.24	0.23	0.26
	(0.41)	(0.43)	(0.42)	(0.44)
Has Professional Degree	0.02	0.02	0.04	0.09
	(0.13)	(0.14)	(0.19)	(0.29)
Has PHD	0.01	0.01	0.01	0.03
	(0.08)	(0.11)	(0.11)	(0.17)
Mother college dummy	0.28	0.32	0.43	0.57
	(0.45)	(0.47)	(0.49)	(0.50)
STEM BA	0.13	0.15	0.18	0.28
	(0.34)	(0.35)	(0.39)	(0.45)
Undergraduate Loans (1000s)	12.98	11.48	9.55	8.48
	(21.29)	(19.71)	(18.68)	(18.28)
Total Income (1000s)	44.64	46.63	51.32	57.51
	(30.92)	(32.23)	(34.46)	(39.26)
Hourly Wage	22.85	23.95	25.88	28.57
	(16.61)	(17.41)	(18.08)	(20.06)
Unemployed	0.03	0.03	0.03	0.03
	(0.16)	(0.17)	(0.18)	(0.17)
Not in Labor Force	0.08	0.08	0.07	0.08
	(0.27)	(0.27)	(0.25)	(0.27)
Currently Enrolled in Graduate Program	0.09	0.09	0.10	0.13
	(0.29)	(0.29)	(0.30)	(0.33)
Currently Enrolled in MA	0.06	0.05	0.06	0.06
	(0.24)	(0.22)	(0.23)	(0.23)
Currently Enrolled in Prof. Degree	0.00	0.00	0.01	0.02
	(0.07)	(0.07)	(0.09)	(0.15)
Currently Enrolled in PHD	0.01	0.01	0.01	0.02
	(0.10)	(0.10)	(0.11)	(0.15)
Moved from State of Graduation	0.33	0.29	0.33	0.46
	(0.47)	(0.45)	(0.47)	(0.50)
Moved from State of Birth	0.44	0.45	0.50	0.57
	(0.50)	(0.50)	(0.50)	(0.49)
Has Children	0.53	0.46	0.43	0.37
	(0.50)	(0.50)	(0.50)	(0.48)
Observations	20000	28000	35000	60000

Notes: Data from 2010, 2013, 2015, 2017, and 2019 Waves of the National Survey of College Graduates; see text for details. Analysis sample contains individuals with undergraduate institutions that can be linked to IPEDS data. College quality defined following Dillon and Smith (2020). Cell counts rounded following disclosure avoidance protocols.

Table A3: Results for Earnings

VARIABLES	(1) Earnings	(2) Earnings	(3) Earnings	(4) Earnings
UR at Graduation	-759.1*	353.8	-759.5*	961.5
	(417.3)	(1,979)	(417.0)	(2,031)
College Quality Q2			801.7	2,093
			(935.7)	(2,911)
College Quality Q3			4,763***	9,410***
			(986.6)	(3,166)
College Quality Q4			8,797***	15,750***
			(1,041)	(3,067)
CQ Q2 X UR				-229.6
				(427.6)
CQ Q3 X UR				-760.3
				(472.1)
CQ Q4 X UR				-1,138**
				(463.7)
CQ (SD)	3,684***	6,037***		
	(382.7)	(1,150)		
CQ (SD) X UR		-383.9**		
		(177.8)		
Observations	144000	144000	144000	144000
R-squared	0.164	0.168	0.163	0.167

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equations (1) and (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate.

Table A4: Results for Employment Variables

VARIABLES	Employed	NILF	Discouraged	Hours	Work Outside Field	Enrolled	Enrolled
UR at Graduation	-1.364	-0.827	-0.787	56.37**	-0.244	0.424	0.0944
	(1.538)	(1.461)	(1.352)	(26.45)	(1.800)	(1.035)	(1.038)
CQ (SD)	1.747*	-2.124***	-1.354*	37.39*	-0.857	-0.547	
	(0.946)	(0.801)	(0.756)	(19.89)	(1.296)	(0.677)	
CQ (SD) X UR at Graduation	-0.321**	0.352***	0.113	-0.534	-0.134	0.357***	
	(0.151)	(0.122)	(0.114)	(3.143)	(0.202)	(0.112)	
College Quality Q2							-0.908
							(2.119)
College Quality Q3							-0.000939
							(2.339)
College Quality Q4							-0.872
							(2.015)
CQ Q2 X UR							0.166
							(0.334)
CQ Q3 X UR							0.213
							(0.411)
CQ Q4 X UR							0.808**
							(0.329)
Observations	144000	144000	144000	131000	131000	144000	144000
R-squared	0.026	0.032	0.045	0.054	0.017	0.044	0.044

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate. NILF: not in labor force. Enrolled: currently enrolled in any graduate program. Discouraged: not in the labor force and not enrolled in any graduate program. Work Outside Field: indicator variable for respondent working outside their field of undergraduate study.

Table A5: Ever Observed Enrolled Results

VARIABLES	(1) Any	(2) MA	(3) PHD	(4) Professional
UR at Graduation	-0.158	0.180	-0.239	-0.0625
	(0.684)	(0.592)	(0.191)	(0.194)
CQ Q2	-0.259	-0.146	0.713	-0.182
	(3.472)	(2.994)	(0.916)	(0.607)
CQ Q3	2.259	3.516	-0.220	-0.905
	(3.829)	(2.943)	(0.959)	(1.072)
CQ Q4	-0.876	0.994	-0.348	-1.976*
	(3.336)	(2.720)	(0.981)	(1.060)
CQ Q2 X UR at Graduation	-0.0450	-0.293	-0.0643	0.0149
	(0.542)	(0.494)	(0.122)	(0.0981)
CQ Q3 X UR at Graduation	-0.165	-0.753	0.110	0.239
	(0.641)	(0.499)	(0.138)	(0.176)
CQ Q4 X UR at Graduation	0.881*	-0.260	0.348**	0.689***
	(0.523)	(0.433)	(0.150)	(0.180)
Observations	75000	75000	75000	75000
R-squared	0.034	0.025	0.009	0.028

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate. Column headers indicate which type of postgraduate enrollment variable is considered in the given regression.

Table A6: Degree Attainment Results

VARIABLES	(1) Any	(2) MA	(3) PHD	(4) Professional
UR at Graduation	2.503	2.636	-0.0152	-0.118
	(1.778)	(1.816)	(0.396)	(0.713)
CQ Q2	4.382	2.366	0.970*	1.046
	(4.029)	(3.873)	(0.573)	(1.025)
CQ Q3	2.933	1.853	1.212**	-0.132
	(3.901)	(3.670)	(0.599)	(1.617)
CQ Q4	20.26***	5.842*	3.349***	11.07***
	(3.701)	(3.516)	(0.622)	(1.705)
CQ Q2 X UR at Graduation	-0.431	-0.244	-0.101	-0.0860
	(0.630)	(0.610)	(0.0932)	(0.132)
CQ Q3 X UR at Graduation	0.0677	-0.145	-0.108	0.321
	(0.600)	(0.569)	(0.0951)	(0.268)
CQ Q4 X UR at Graduation	-1.347**	-0.383	-0.243***	-0.721***
	(0.530)	(0.518)	(0.0903)	(0.231)
Observations	75000	75000	75000	75000
R-squared	0.068	0.039	0.022	0.045

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate. Column headers indicate which type of postgraduate enrollment variable is considered in the given regression.

Table A7: Major Choice by College Quality

Variable	Mean/SD	Mean/SD	Mean/SD	Mean/SD
Quality Quartile	1	2	3	4
STEM BA	0.13	0.15	0.18	0.28
	(0.34)	(0.35)	(0.39)	(0.45)
Soc Sci BA	0.12	0.14	0.18	0.25
	(0.33)	(0.35)	(0.38)	(0.43)
Health BA	0.08	0.08	0.06	0.05
	(0.27)	(0.27)	(0.24)	(0.21)
Education BA	0.14	0.12	0.09	0.04
	(0.35)	(0.33)	(0.28)	(0.18)
Business BA	0.26	0.23	0.21	0.13
	(0.44)	(0.42)	(0.41)	(0.34)
Observations	20000	28000	35000	60000

Notes: Data from 2010, 2013, 2015, 2017, and 2019 Waves of the National Survey of College Graduates; see text for details. Analysis sample contains individuals with undergraduate institutions that can be linked to IPEDS data. College quality defined following Dillon and Smith (2020). Cell counts rounded following disclosure avoidance protocols.

Table A8: Earnings Results: Heterogeneity by Field

VARIABLES	(1) Earnings	(2) Earnings	(3) Earnings	(4) Earnings	(5) Earnings
UR at Graduation	264.5	450.2	386.4	17.01	342.6
	(1,964)	(1,957)	(1,951)	(1,938)	(1,986)
CQ (SD)	3,277***	3,962***	3,781***	3,442***	4,046***
	(386.0)	(384.9)	(382.3)	(382.4)	(384.8)
STEM BA	11,150***				
	(1,928)				
STEM BA X UR	-491.0*				
	(285.7)				
Soc Sci BA		-931.3			
		(1,814)			
Soc Sci BA X UR		-712.4***			
		(267.7)			
Health BA			9,603***		
			(3,395)		
Health BA X UR			53.42		
			(493.1)		
Education BA				-15,820***	
				(3,031)	
Education BA X UR				1,280***	
				(450.0)	
Business BA					9,416***
					(2,924)
Business BA X UR					-276.5
					(444.9)
Observations	144000	144000	144000	144000	144000
R-squared	0.175	0.171	0.172	0.172	0.175

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate.

Table A9: Earnings Results: Heterogeneity by Sex

VARIABLES	(1) Earnings	(2) Earnings
UR at Graduation	2,397	-2,430
	(2,971)	(2,394)
CQ (SD)	4,844***	6,705***
	(1,713)	(1,380)
CQ (SD) X UR at Graduation	-80.20	-557.7***
	(258.9)	(205.7)
Observations	69000	75000
R-squared	0.157	0.107
Sample	Male	Female

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate.

Table A10: Enrollment and Degree Attainment Results by Sex

VARIABLES	(1) Ever Enrolled	(2) Ever Enrolled	(3) Grad Degree	(4) Grad Degree
UR at Graduation	0.644	-0.819	1.767*	0.440
	(0.938)	(0.938)	(1.022)	(1.016)
CQ Q2	2.533	-3.239	9.124*	0.899
	(4.647)	(4.891)	(5.343)	(5.859)
CQ Q3	8.657*	-3.567	7.425	-1.479
	(4.806)	(5.846)	(5.391)	(5.499)
CQ Q4	4.145	-4.955	27.59***	16.31***
	(4.191)	(4.997)	(5.343)	(4.846)
CQ Q2 X UR at Graduation	-0.835	0.572	-1.770**	0.479
	(0.744)	(0.769)	(0.795)	(0.935)
CQ Q3 X UR at Graduation	-1.409*	0.849	-1.077	1.106
	(0.750)	(0.973)	(0.833)	(0.845)
CQ Q4 X UR at Graduation	-0.161	1.624**	-2.575***	-0.475
	(0.677)	(0.796)	(0.798)	(0.696)
Sample	Male	Female	Male	Female
Observations	36000	39000	36000	39000
R-squared	0.039	0.039	0.061	0.070

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate. Column headers indicate which type of postgraduate enrollment variable is considered in the given regression.

Table A11: Migration Results

VARIABLES	(1) Migration	(2) Migration	(3) Migration
UR at Graduation	5.941***	5.427*	6.103**
	(1.992)	(3.023)	(2.468)
CQ Q2	0.281	-0.973	1.474
	(5.270)	(7.608)	(6.393)
CQ Q3	5.554	8.044	4.011
	(5.131)	(7.465)	(6.291)
CQ Q4	18.57***	22.23***	15.62**
	(4.768)	(6.934)	(6.543)
CQ Q2 X UR at Graduation	-0.733	-0.935	-0.649
	(0.801)	(1.122)	(1.014)
CQ Q3 X UR at Graduation	-1.155	-2.119*	-0.533
	(0.774)	(1.125)	(0.976)
CQ Q4 X UR at Graduation	-1.111	-1.956*	-0.455
	(0.716)	(1.044)	(1.006)
Sample	All	Male	Female
Observations	144000	69000	75000
R-squared	0.113	0.114	0.125

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate. Migration indicates respondent living in a state other than the state in which they obtained their first BA.

Table A12: Robustness of Results

VARIABLES	(1) Earnings	(2) Enrolled	(3) Earnings	(4) Earnings	(5) Earnings	(6) Earnings
UR at Graduation	-584.9	94.57	1,281	1,735	405.7	1,145
	(2,697)	(2,714)	(1,869)	(1,958)	(1,314)	(1,371)
CQ Q2		1,727		1,742		2,732
		(3,650)		(2,921)		(3,101)
CQ Q3		11,900***		10,230***		9,014***
		(3,668)		(3,170)		(3,371)
CQ Q4		12,300***		16,140***		15,760***
		(3,918)		(2,970)		(3,300)
CQ Q2 x UR at Graduation		-250.5		-122.8		-286.0
		(535.3)		(415.7)		(447.0)
CQ Q3 x UR at Graduation		-1,152**		-744.5		-635.5
		(535.6)		(469.6)		(494.9)
CQ Q4 x UR at Graduation		-775.5		-777.1*		-1,069**
		(578.0)		(442.1)		(490.2)
CQ (SD)	5,012***		6,440***		5,861***	
	(1,430)		(1,094)		(1,246)	
CQ (SD) X UR at Graduation	-278.4		-277.0*		-317.2*	
	(214.5)		(163.5)		(188.9)	
Observations	81000	81000	116000	116000	142000	142000
Sample	BA only	BA only	LF, not enr	LF, not enr	Baseline	Baseline
UR Measure	Baseline	Baseline	Baseline	Baseline	GOTG	GOTG
R-squared	0.163	0.163	0.202	0.202	0.169	0.169

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate. Enrolled: currently enrolled in any graduate program. BA only: excludes individuals who hold a graduate degree or are currently enrolled in a graduate program. LF, not enr: includes only individuals who are in the labor force and not enrolled in graduate school. GOTG: unemployment rate measured from Graduates On The Go data.

Table A13: Alternate Earnings Results

VARIABLES	(1) Log(Earnings)	(2) Log(Earnings+1)	(3) Wage	(4) Log(Wage)	(5) Log(Wage+1)
UR at Graduation	0.0250	-0.128	-0.778	-0.0141	-0.0564
	(0.0402)	(0.181)	(1.060)	(0.0396)	(0.0636)
CQ (SD)	0.109***	0.232**	3.159***	0.0992***	0.125***
	(0.0240)	(0.107)	(0.558)	(0.0205)	(0.0362)
UR at Graduation X CQ	-0.00686*	-0.0328*	-0.258***	-0.00684**	-0.0137**
	(0.00366)	(0.0171)	(0.0824)	(0.00301)	(0.00564)
Observations	131000	144000	144000	131000	144000
R-squared	0.138	0.041	0.121	0.132	0.065

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate.

Table A14: Probit Results

VARIABLES	(1) Employed	(2) NILF	(3) Discouraged	(4) Enrolled
UR at Graduation	-0.00564	0.00243	0.000677	-0.00150
	(0.00492)	(0.00406)	(0.00386)	(0.00366)
CQ (SD)	0.0158*	-0.0191**	-0.0104	0.00167
	(0.00921)	(0.00750)	(0.00694)	(0.00661)
UR at Graduation X CQ	-0.00296**	0.00320***	0.000551	0.00217**
	(0.00147)	(0.00116)	(0.00111)	(0.000980)
Observations	144000	144000	144000	144000

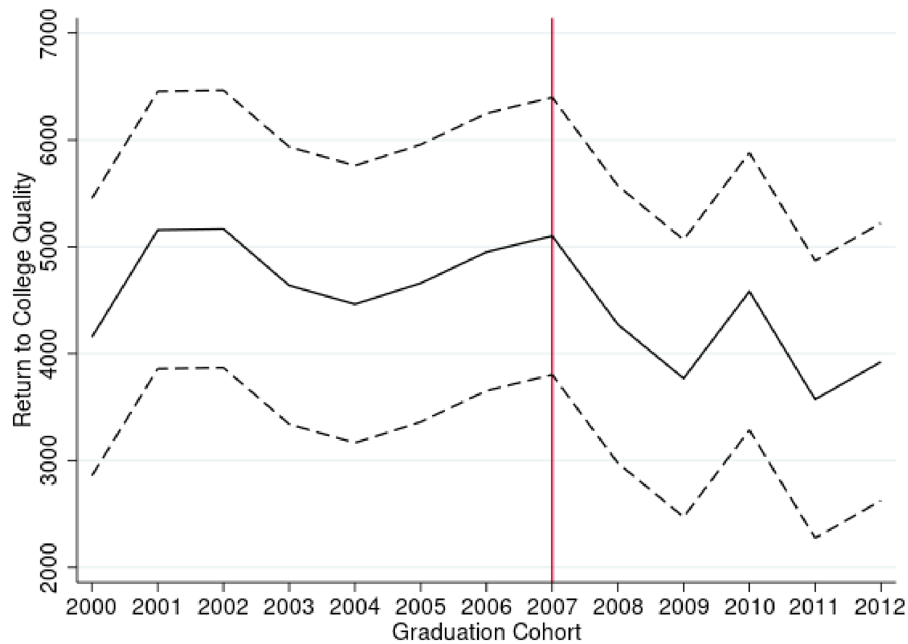
Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort; indicators for race, sex, and parental education; and cohort FEs, race, sex, and parental education indicators interacted with the unemployment rate. NILF: not in labor force. Enrolled: currently enrolled in any graduate program. Discouraged: not in the labor force and not enrolled in any graduate program.

Table A15: Difference-in-Differences Specification Results

VARIABLES	(1) Earnings	(2) Log(Earnings)	(3) Log(Earnings + 1)	(4) NILF	(5) Enrolled
Bad Shock X Post	1,882	0.0101	0.252	-0.0112	-0.0106
	(2,132)	(0.0498)	(0.215)	(0.0161)	(0.0167)
CQ Q2	982.1	0.0306	0.0298	-0.000145	-0.00221
	(1,026)	(0.0242)	(0.110)	(0.00913)	(0.00783)
CQ Q3	5,160***	0.101***	0.157	-0.0114	0.0121
	(1,068)	(0.0231)	(0.111)	(0.00856)	(0.00764)
CQ Q4	9,695***	0.168***	0.125	-0.00456	0.0344***
	(1,114)	(0.0245)	(0.115)	(0.00889)	(0.00796)
Bad Shock X Post X CQ Q2	-1,677	-0.0597	-0.162	-0.00257	0.0231
	(2,369)	(0.0584)	(0.242)	(0.0181)	(0.0211)
Bad Shock X Post X CQ Q3	-2,630	-0.0437	-0.150	0.0122	0.00855
	(2,493)	(0.0555)	(0.248)	(0.0197)	(0.0236)
Bad Shock X Post X CQ Q4	-5,915**	-0.0819	-0.437*	0.0410**	0.0428**
	(2,334)	(0.0552)	(0.241)	(0.0172)	(0.0188)
Observations	144000	131000	144000	144000	144000
R-squared	0.164	0.134	0.038	0.029	0.041

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Bad Shock is an indicator for graduating from a college in a state with an above median decline in the unemployment rate from 2007 and 2009. Post is an indicator for graduating in 2008 or later. Controls include state, cohort, and survey year fixed effects, race and sex indicators, and indicators for parental education. NILF: not in labor force. Enrolled: currently enrolled in any graduate program. Discouraged: not in the labor force and not enrolled in any graduate program.

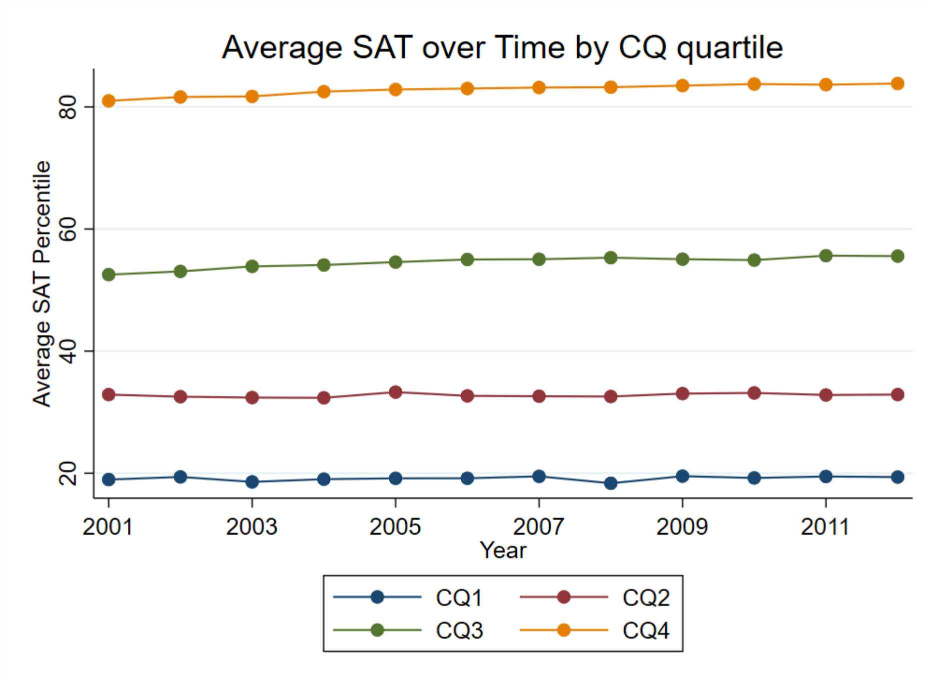
Figure A1: College Quality Returns by Graduation Cohort

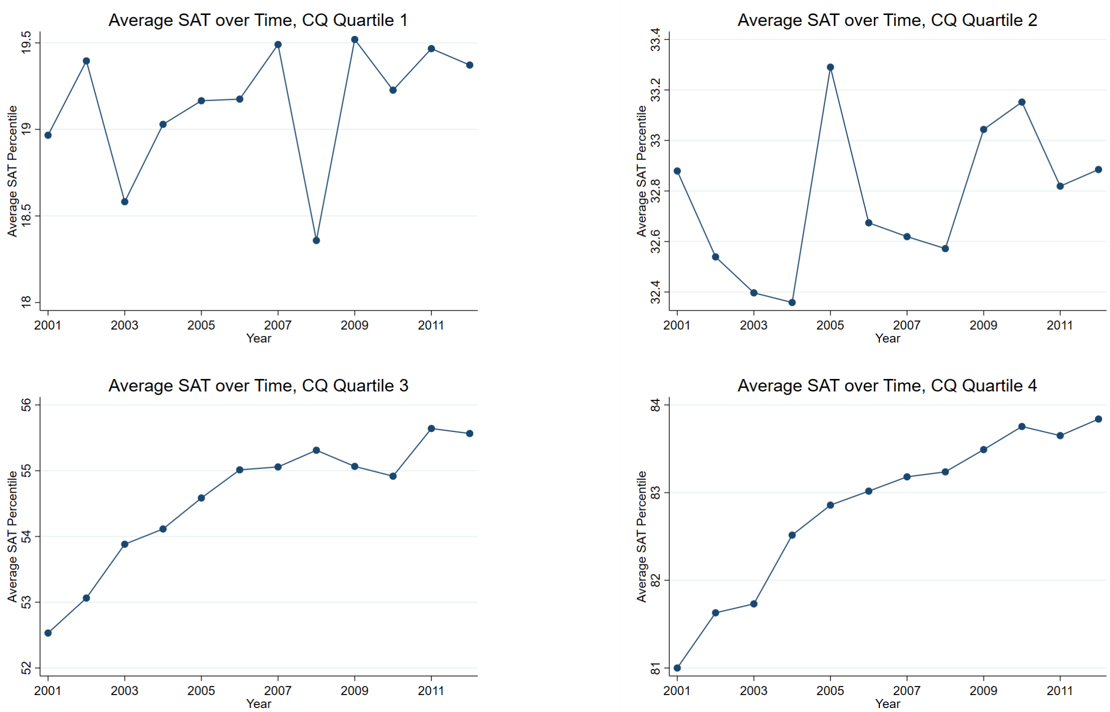


Notes: Standard errors clustered at the cohort-by-state-of-graduation level; dotted lines represent 95% confidence intervals. Table reports estimate of Equation (2). Data from 2010, 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include fixed effects for survey year, state, and cohort, as well as race and sex indicators, and indicators for parental education.

A Selection into College, 2000-2012

The following graphs show the mean of the 25th and 75th percentile of SAT scores of entering students over time, by college quality quartile as defined in section 3.3. Although there is some variation over our time period, it is relatively small and does not appear to be systematically related to the business cycle.




Average SAT over Time, CQ Quartile 1
Average SAT Percentile
18
18.5
19
19.5
2001
2003
2005
2007
2009
2011
Year
Average SAT over Time, CQ Quartile 2
Average SAT Percentile
32.4
32.6
32.8
33
33.2
33.4
2001
2003
2005
2007
2009
2011
Year
Average SAT over Time, CQ Quartile 3
Average SAT Percentile
52
53
54
55
56
2001
2003
2005
2007
2009
2011
Year
Average SAT over Time, CQ Quartile 4
Average SAT Percentile
81
82
83
84
2001
2003
2005
2007
2009
2011
Year

B Results for National-Level Unemployment Rate Specification

Table A16: Results for Earnings

VARIABLES	(1) Earnings	(2) Earnings	(3) Earnings	(4) Earnings
UR at Graduation	-213.6	-230.8	-220.5	987.4**
	(280.5)	(278.9)	(281.5)	(415.2)
College Quality Q2			1,175	5,009
			(1,024)	(3,479)
College Quality Q3			5,297***	15,070***
			(1,085)	(3,694)
College Quality Q4			9,823***	24,280***
			(1,159)	(3,616)
CQ Q2 X UR				-595.0
				(484.4)
CQ Q3 X UR				-1,521***
				(519.3)
CQ Q4 X UR				-2,253***
				(514.5)
CQ (SD)	4,109***	9,471***		
	(431.3)	(1,327)		
CQ (SD) X UR		-836.4***		
		(194.6)		
Observations	126000	126000	126000	126000
R-squared	0.172	0.174	0.171	0.173

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equations (1) and (2). Data from 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include race and sex indicators, indicators for parental education, indicators for a STEM undergraduate degree, state fixed effects, and a quartic polynomial in experience.

Table A17: Results for Employment Variables

VARIABLES	Employed	NILF	Unemployed	Enrolled	Discouraged	Hours	Work Outside Field
UR at Graduation	-0.200	0.028	0.172*	0.629***	0.093	-6.828	0.160
	(0.216)	(0.189)	(0.102)	(0.192)	(0.177)	(5.234)	(0.350)
CQ (SD)	2.734***	-3.120***	0.386	-1.380*	-1.767**	37.58*	-0.407
	(1.023)	(0.903)	(0.544)	(0.742)	(0.872)	(21.16)	(1.462)
UR at Graduation X CQ (SD)	-0.427***	0.484***	-0.057	0.431***	0.180	-0.618	-0.095
	(0.156)	(0.130)	(0.089)	(0.123)	(0.123)	(3.205)	(0.225)
Observations	126000	126000	126000	126000	126000	116000	116000
R-squared	0.025	0.031	0.011	0.040	0.042	0.058	0.019

Notes: Standard errors clustered at the cohort-by-state-of-graduation level are in parentheses. Table reports estimate of Equation (1) and (2). Data from 2013, 2015, 2017 and 2019 Waves of the National Survey of College Graduates; see text for details. College quality defined following Dillon and Smith (2020). Controls include race and sex indicators, indicators for parental education, indicators for a STEM undergraduate degree, state fixed effects, and a quartic polynomial in experience. NILF: not in labor force. Enrolled: currently enrolled in any graduate program. Discouraged: not in the labor force and not enrolled in any graduate program. Work Outside Field: indicator variable for respondent working outside their field of undergraduate study.

C Weinstein (2022) Replication and Comparison

To illuminate why our results differ from those of Weinstein (2023), we conduct an exercise where we use the same data (mobility report card) and specification as Weinstein but change the college quality measure from Barron's categories to our quartiles measure, use mean earnings as the outcome variable, and weight by institution size. Specifically, we replicate the following triple-difference event specification,

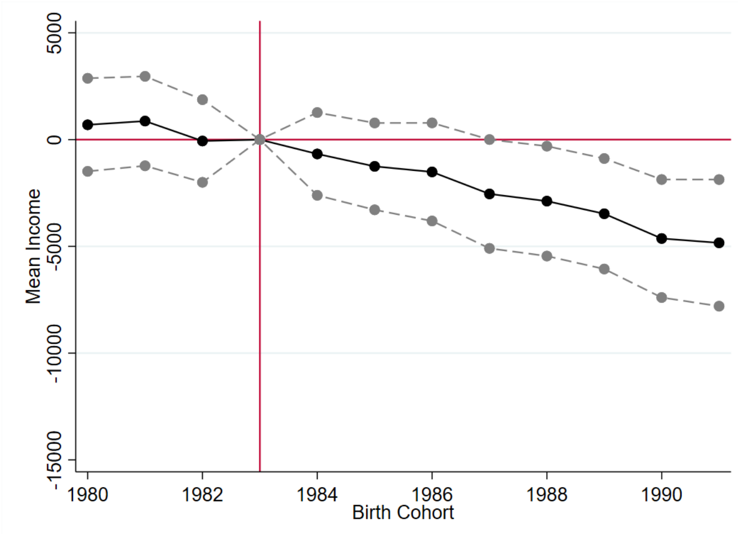
$$
\begin{aligned}
Y_{j_{ks}t} = \kappa_j + \beta_{st} + \gamma_{kt} + \lambda_{kt}\mathrm{Cohort}_t * \quad & \mathit{CollegeQuality}_j * \mathit{SevereRecession}_{j_{ks}} \\
& + \rho_{kt}\mathrm{Cohort}_t * Z_{jt} * \mathit{SevereRecession}_{j_{ks}} + X_{jt}\delta + u_{jt}
\end{aligned}
$$

where $Y_{j_{ks}t}$ is income measured in 2014 for graduates of university j, in birth cohort t, where university j is in college quality group k and commuting zone s. κ_j are university fixed effects, β_{st} are birth cohort-commuting zone fixed effects, and γ_{kt} are birth cohort-college quality group fixed effects. $\mathit{SevereRecession}_{j_{ks}}$ is an indicator for college j being located in a commuting zone with an above-median change in the unemployment rate between 2007 and 2009. Z_{jt} and X_{jt} are university-level controls for fraction of female students, log of students in the cohort, and several parental income variables. This specification is exactly the same as Weinstein's, except we have changed the outcome to mean earnings, changed the college quality measure to our quartiles measure, and weighted by institution size.

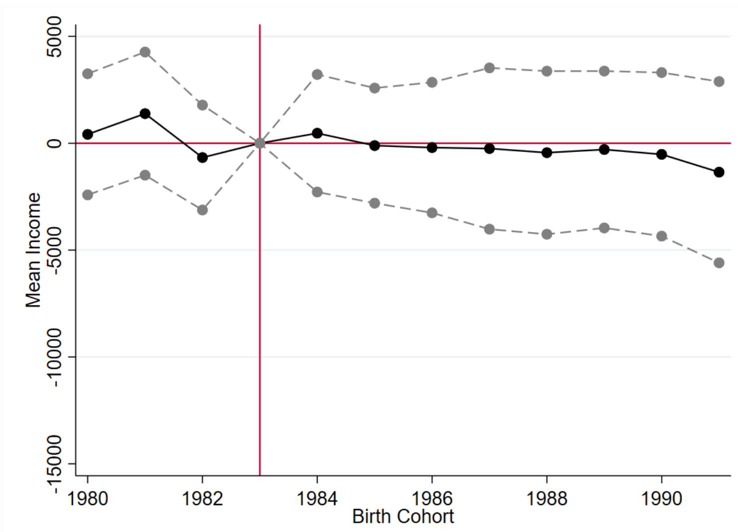
Figure A3 shows the results, where the bottom quality quartile is the omitted category. The interpretation for subfigure (c) is the following: for birth cohorts who would have graduated after the Great Recession, the difference in mean incomes between graduates from the top quality quartile and same-CZ bottom quality quartile is an additional 5 to 8 thousand dollars less in high-recession shock versus low-recession shock CZs relative to the 1983 (base, following Weinstein) cohort.

Figure A3: Recession Effects by College Quality, Relative to College Quality Quartile 1: Triple Differences Model

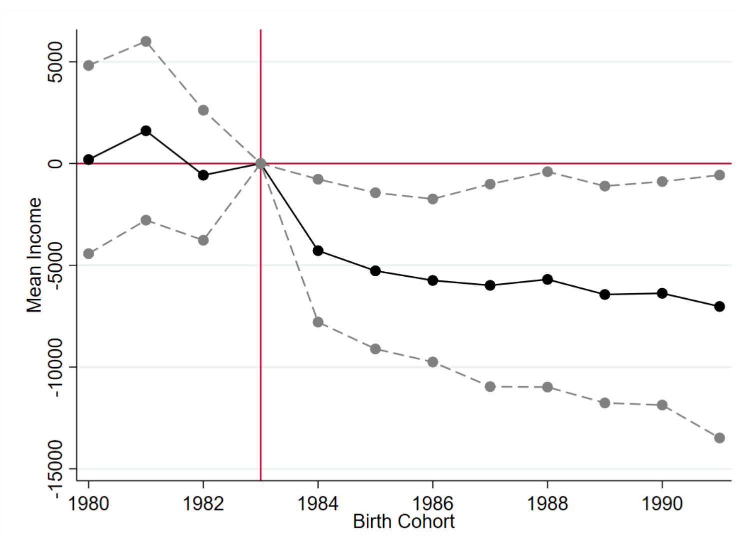
(a) College Quality Quartile 2



(b) College Quality Quartile 3



(c) College Quality Quartile 4



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