Switching Schools: Effects of College Transfers*

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Abstract

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 To do so, I leverage applications and admissions data to uncover the unpublished GPA cutoffs for transfer student admissions at each institution and then use these cutoffs as an instrument for transfer.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' substitution out of high-paying majors into lower-paying majors, reduced employment and labor market experience, and potential loss of support networks.

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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.

[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 2.

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 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.

[3] I implement several tests to check the validity of this assumption in section 6 and find that students above and below the cutoff appear similar.

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.

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

[4]See Altonji et al. (2016) and Martellini et al. (2023) for estimates of pay differentials by major in the US and global contexts, respectively.

[5]This is likely a results of restrictions on how major-specific courses are counted for transfer or

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]

on admission to the business school (transfer students may be broadly admitted to a university but not to a specific major). 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

These findings have important implications for policies regarding college transfer and transfer student support. One response would be to raise the GPA cutoffs for transfer admission at these colleges so that marginally admitted students have a better chance of success. Another response is to increase support for transfer students. Some randomized controlled trials of comprehensive support programs in higher education have shown encouraging results. For example, the City University of New York's (CUNY's) Accelerated Study in Associate Programs (ASAP) had large positive impacts on graduation rates for low-income community college students (Weiss et al., 2019). This suggests that similar efforts for transfer students at four-year colleges may be effective. Additionally, results from prior research show that even marginal students who attend better-resourced colleges from the beginning of their college career see earnings benefits (Hoekstra, 2009; Zimmerman, 2014). This implies that an avenue for improvement may be to extend the support and programming that first-time-in-college students are offered to transfer students. My research also suggests that limiting barriers to lucrative majors may also help improve transfer students' earnings outcomes.

The rest of this paper proceeds as follows: section 2 reviews related literature, section 3 lays out a conceptual framework to offer context to the empirical results, section 4 describes the data, section 5 details the empirical framework, section 6 discusses identification, section 7 presents the main RD results, section 8 elaborates on how to interpret results, section 9 explores mechanisms behind the main earnings results, and section 10 concludes.

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

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.

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

[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).

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; Smith et al., 2020; Kozakowski, 2023). I also add to the literature that considers the interaction between field of study and college 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,

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

including those that are less resourced than UT–Austin.

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 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.

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

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.

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.

[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.

(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 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 subsection 5.1, 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 gives summary statistics on the background characteristics for my analysis sample (described in section 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–

[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.

Table 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.

2011 academic year, the student submits 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

[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.

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.

5 Empirical Strategy

5.1 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

[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).

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:

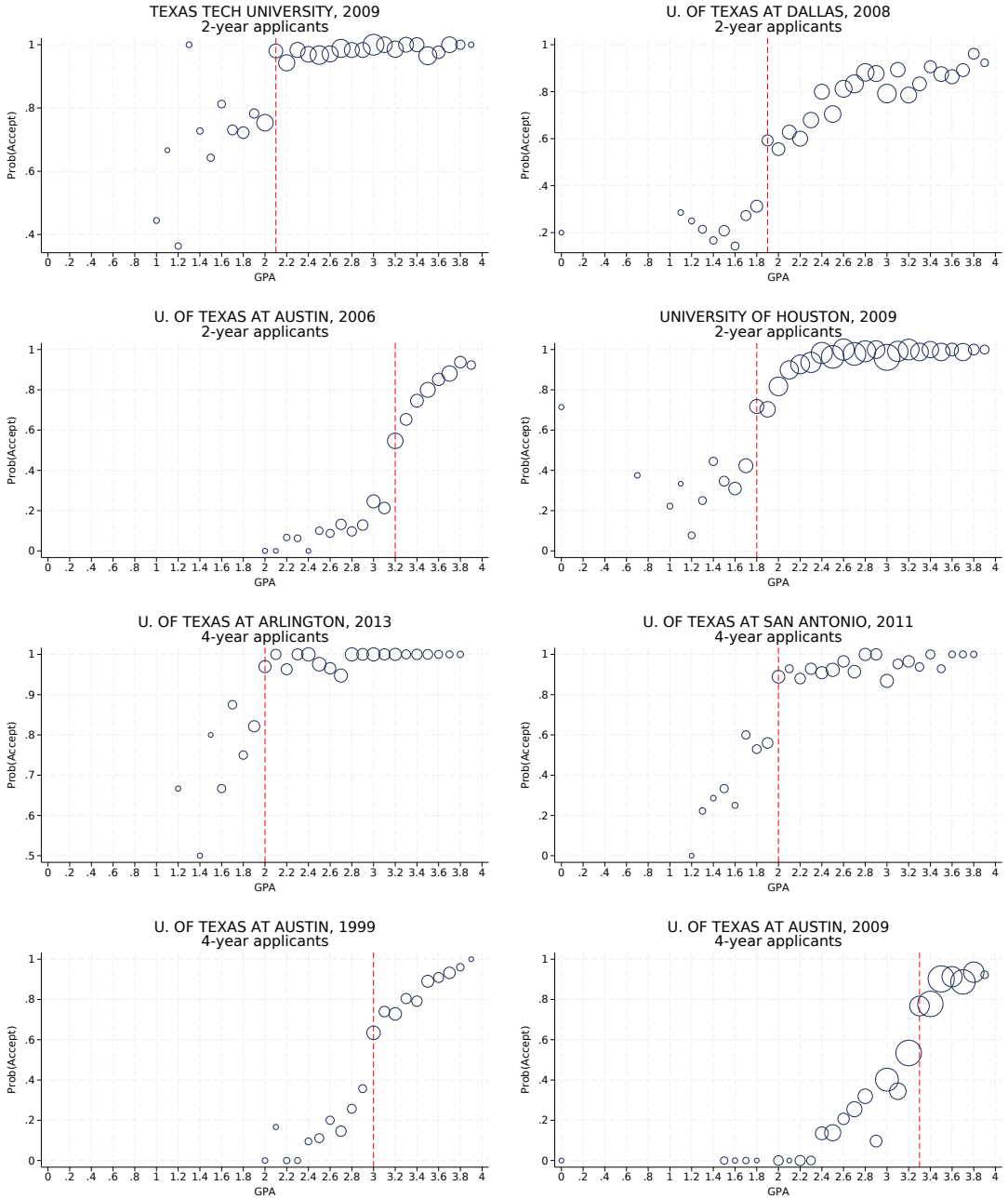
$$Accept_{icts} = \beta_0 + \beta_1 \mathbb{1}(GPA_i \geq T_{cts}) + f(GPA_i) + \varepsilon_{icts} \tag{1}$$

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]

[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

Figure 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.

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 Figure 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 2 and Table 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]

results.

[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.

Table 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 subsection 5.1. 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.

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 applicants from two-year and four-year colleges

[26]Since some students may apply for transfer to multiple colleges, some individuals are included in

Table 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 subsection 5.1. 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.

separate in all specifications. I also estimate some specifications in which I separate out applications to flagship universities to explore heterogeneity by college resources.

5.2 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:

$$\begin{aligned} TransferTarget_{ict} = \alpha_0 + \alpha_1 \mathbb{1}(GPA_i \geq T_{ct}) + f(GPA_i) \\ + \Omega X_i + \gamma_{ct} + \kappa_{m(i,t)} + \theta_{s(i,t)} + \epsilon_{ict} \end{aligned} \tag{2}$$

where $TransferTarget_{ict}$ 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 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.

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.

[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.

[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.

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

$$\begin{aligned} Y_{ict} = \delta_0 + \delta_1 \mathbb{1}(GPA_i \geq T_{ct}) + g(GPA_i) \\ + \Lambda X_i + \pi_{ct} + \nu_{m(i,t)} + \phi_{s(i,t)} + \upsilon_{ict} \end{aligned} \tag{3}$$

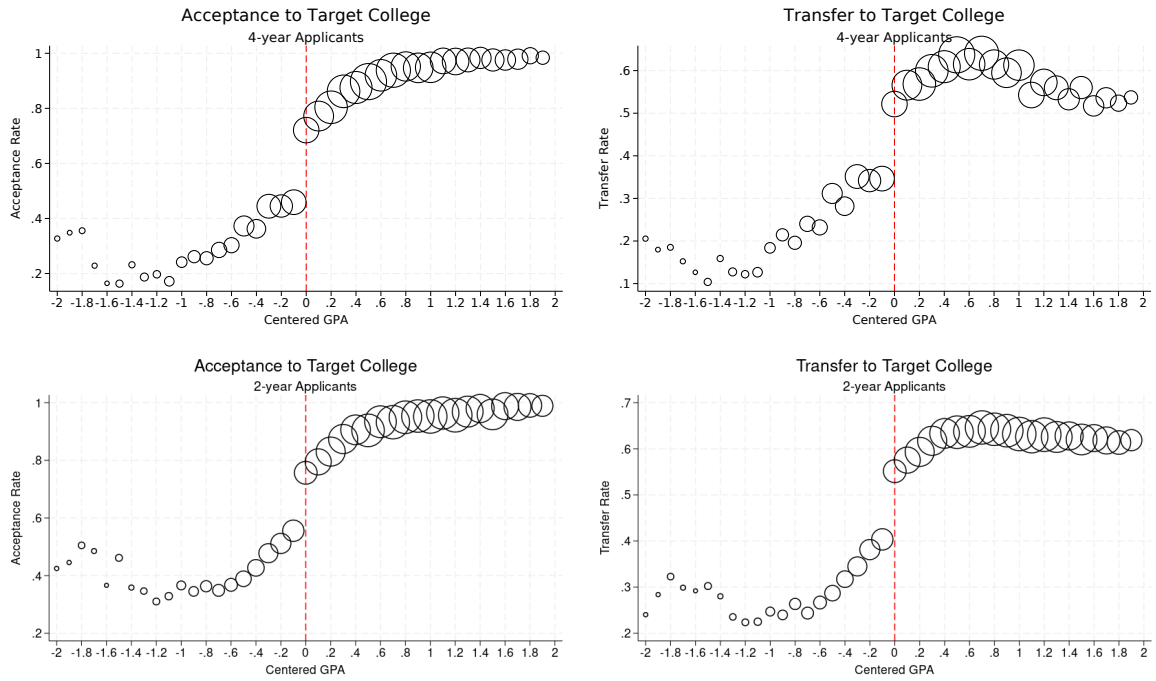
The coefficient of interest δ_1 measures the effect of a student being just above a target college's GPA cutoff on outcome Y_{ict} relative to the outcomes when she falls just below the target college's GPA cutoff. The main outcomes of interest are degree completion and earnings. Analogous to the first 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:

$$\begin{aligned} Y_{ict} = \eta_0 + \eta_1 \widehat{TransferTarget}_{ict} + h(GPA_i) \\ + \Gamma X_i + \zeta_{ct} + \mu_{m(i,t)} + \lambda_{s(i,t)} + \xi_{ict} \end{aligned} \tag{4}$$

where $\widehat{TransferTarget}_{ict}$ is the predicted value from Equation 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 8.

Figure 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.

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 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

Table 4: First-Stage Results

	2-year Applicants		4-year Applicants	
	Accept	Transfer	Accept	Transfer
$\mathbb{1}(GPA_i \geq T_{cy})$	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 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.

transferring to that institution.

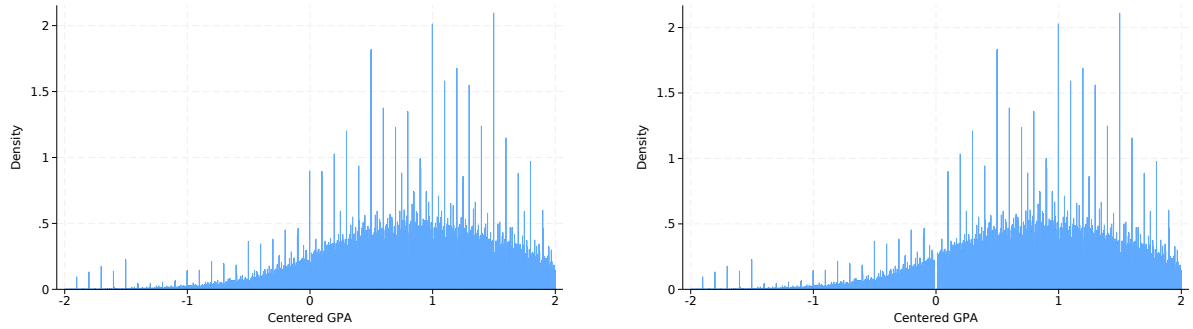
Next, I more directly show evidence of relevance by presenting first-stage results from Equation 2 in Table 4. Through all analyses presented in the main body, I use a local linear specification with a triangular kernel, a bandwidth of 0.3 for two-year applicants and 0.4 for four-year applicants, and 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 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

[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 3: Density of Applicant GPAs

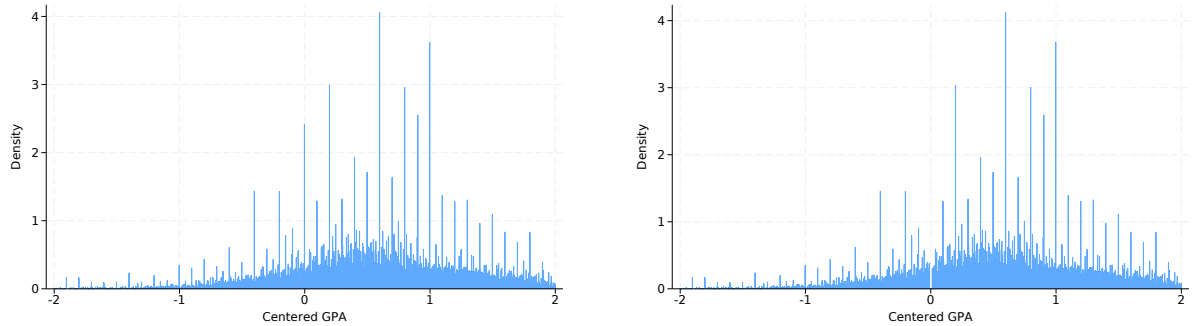
(a) 2-year Applicants, Full Sample

(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.

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, 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 grades are assigned in whole numbers (e.g., 3.0 corresponds to a "B" grade). Panels (a) and (c) of Figure 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:

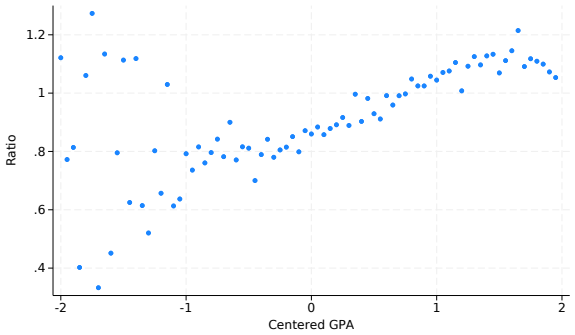
$$\frac{f(GPA|x)}{f(GPA)} \tag{5}$$

where $f(GPA|x)$ and $f(GPA)$ are the conditional and unconditional densities of the centered GPAs, respectively. The idea is that, if the spikes in the GPA distribution

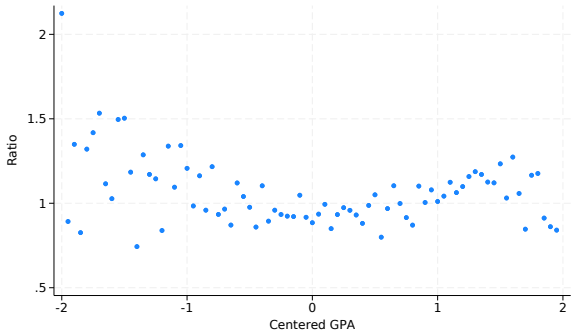
[30] Another concern is that my bandwidth is large enough that there is bias. This is not an identification issue but an issue in estimation that is present to some degree in all empirical applications. I address this issue by using optimal bandwidth values as calculated by Calonico et al. (2020), using 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 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, $Pr(GPA|FRPL)/Pr(GPA)$. Ratios computed within 0.05 grade point bins.

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 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 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

[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.

[32]I use each of the three annual earnings measured described in section 4.

Table 5: Balance Test

	2-year Applicant Predicted Earnings			4-year Applicant Predicted Earnings		
	Unconditional	Conditional	Sandwich	Unconditional	Conditional	Sandwich
$\mathbb{1}(GPA_i \geq T_{cy})$	-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
$TransferTarget$	-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 3 and instrumental variable (IV) estimates of Equation 4, where the outcome is predicted average annual earnings across unconditional, conditional, and sandwich earnings measures (see section 4 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.

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 5, I estimate Equation 3 and Equation 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.

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

I also 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.

7 Main Regression Discontinuity Results

7.1 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 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 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.

[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.

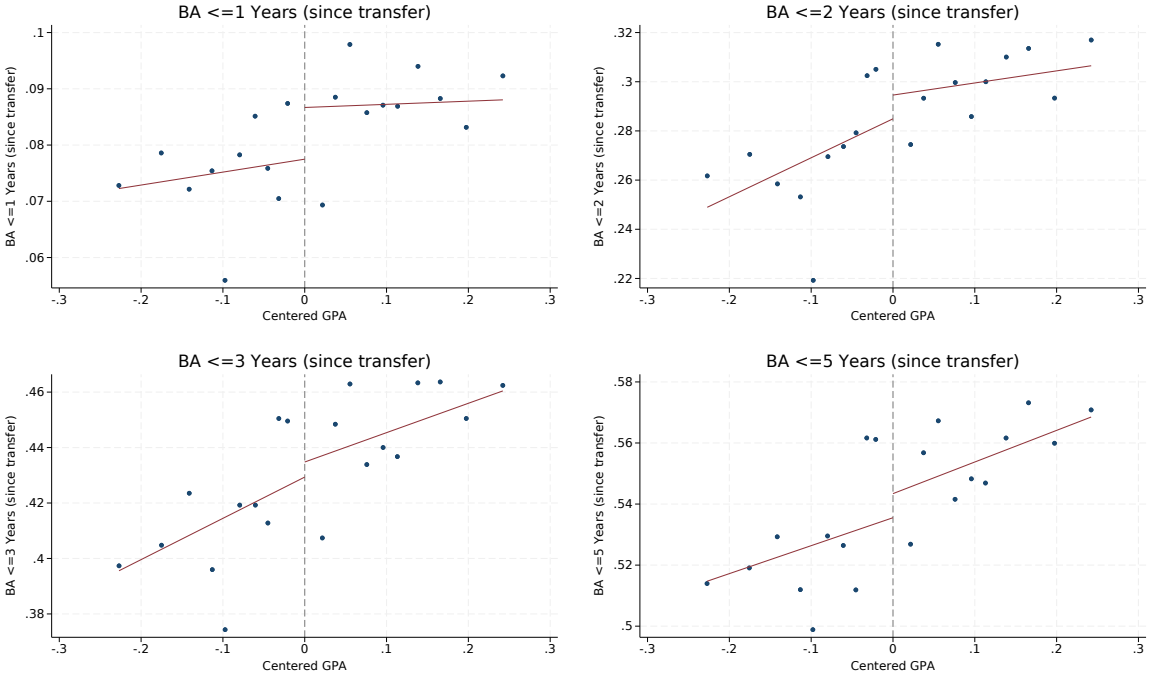
Table 6: Bachelor's Completion in Years Since Intended Transfer, Reduced-Form and Instrumental Variable Results

	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							
$\mathbb{1}(GPA_i \geq T_{cy})$	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)
$TransferTarget$	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)
$E[Y_0\|C]$	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							
$\mathbb{1}(GPA_i \geq T_{cy})$	-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)
$TransferTarget$	-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)
$E[Y_0\|C]$	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

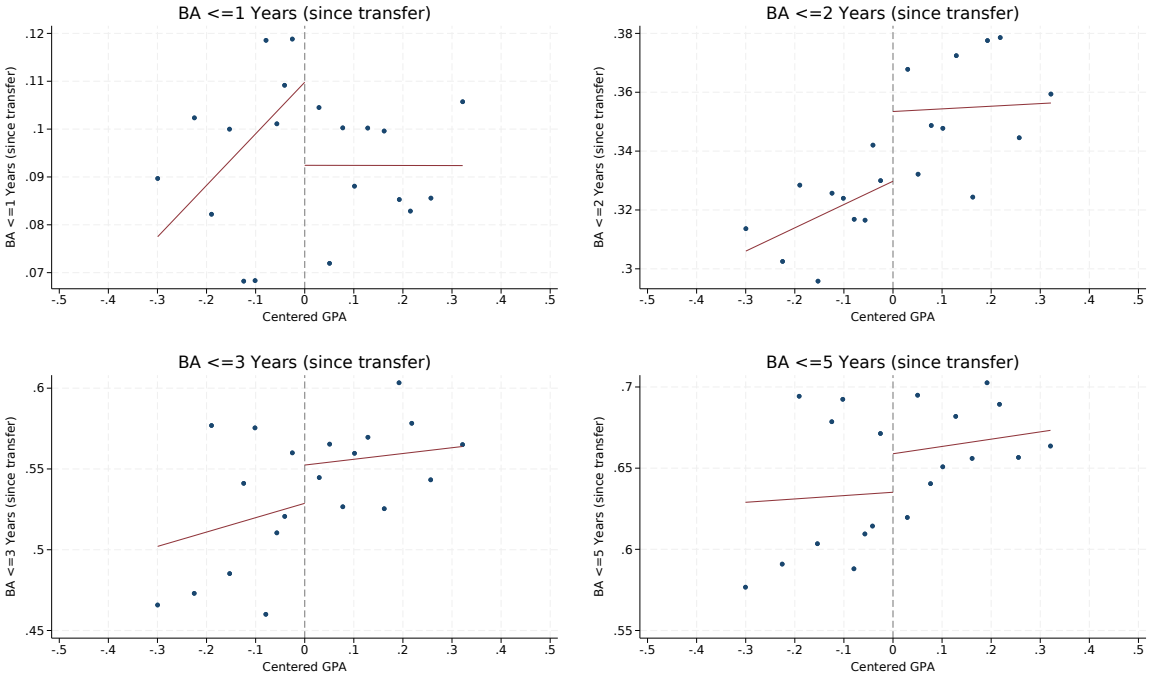
Notes: *** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(GPA_i \geq T_{cy})$ gives reduced-form estimates from equation (3); $TransferTarget$ gives instrumental variable estimates from equation (4). Outcome in rows 1–6 is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 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. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated. Standard errors clustered at the application–college–year level in parentheses.

Figure 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.

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 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 8.

Table 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 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

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

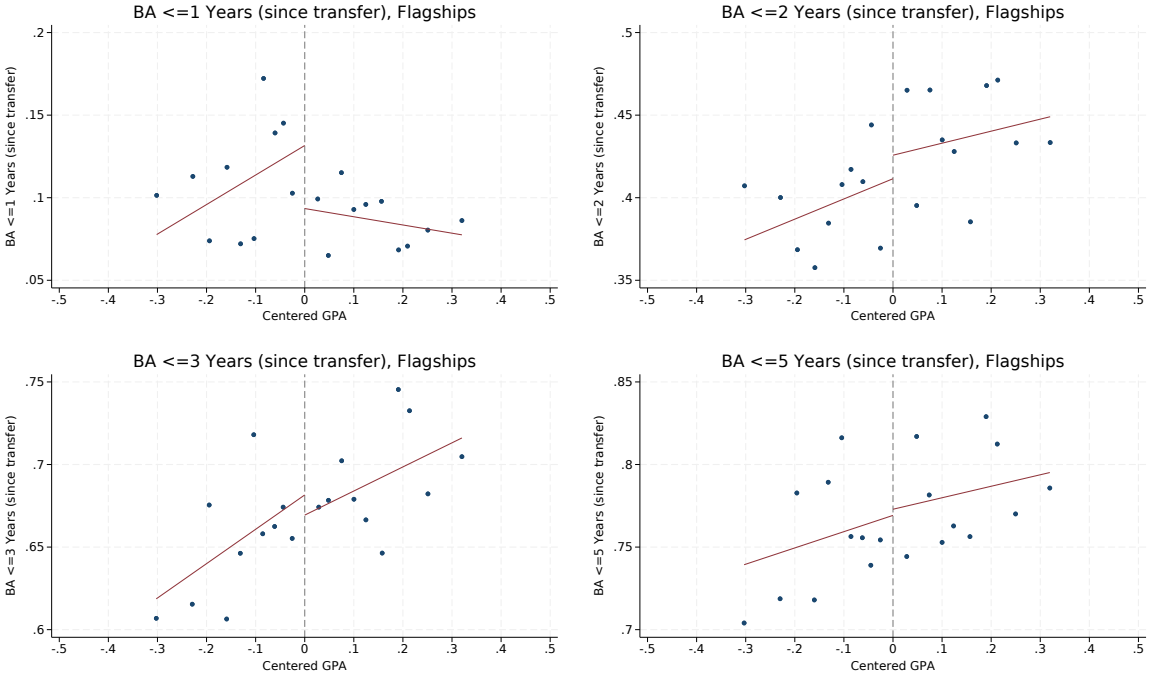
Table 7: 4-Year Applicants: IV bachelor's Completion in Years Since Intended Transfer, by Flagship Status

	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

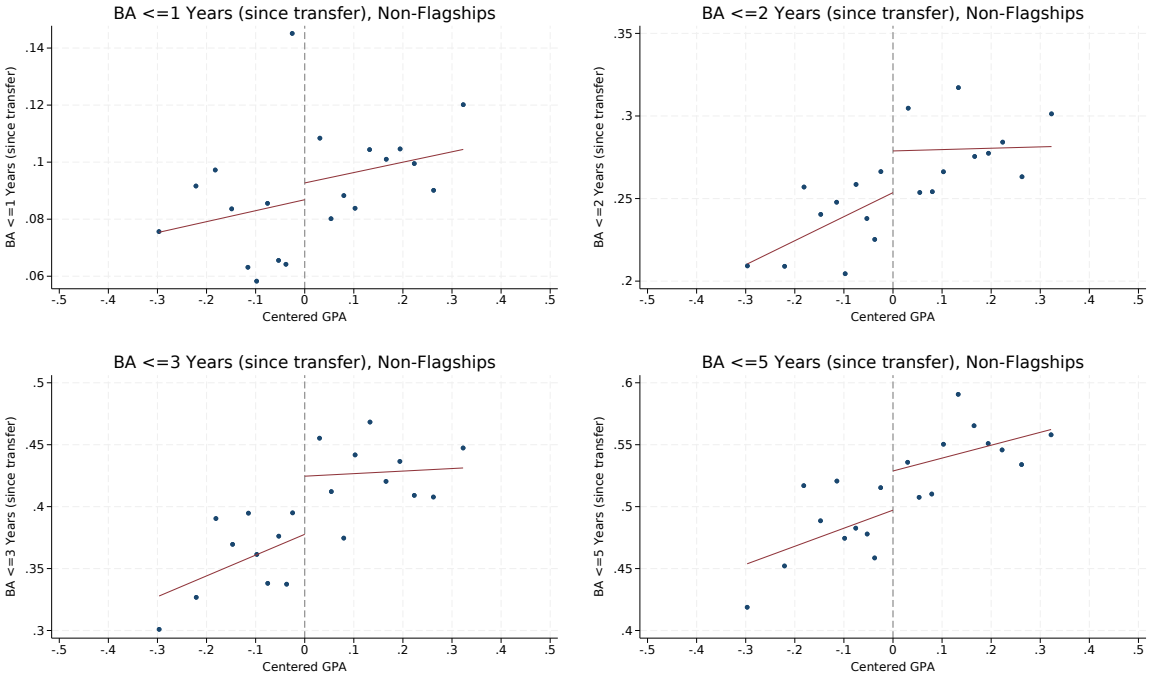
Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Outcome in rows 1–6 is bachelor's attainment measured in years since the intended transfer semester (e.g., 2 yrs indicates earning a bachelor's within 2 years of the semester for which the student applied for transfer). Yrs to BA 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.

Figure 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.

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 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 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]

7.2 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 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 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

[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.

[37] See section 4 for details on the earnings measures and the motivation for using each.

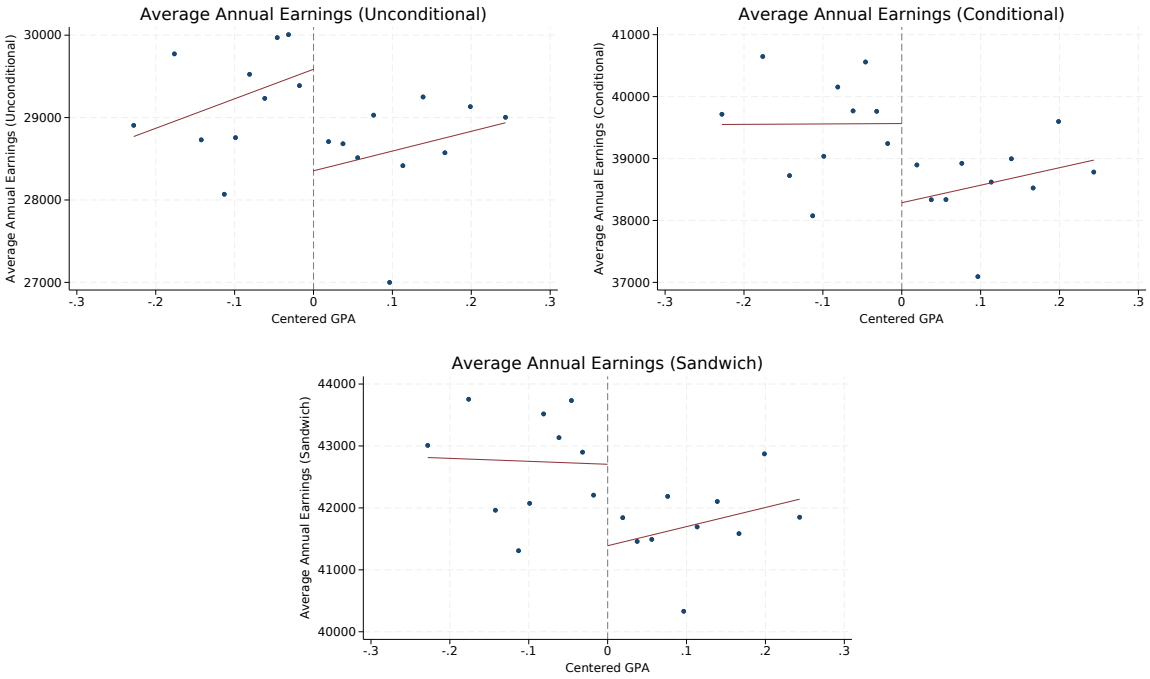
Table 8: Annual Earnings, Pooled across All Years

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

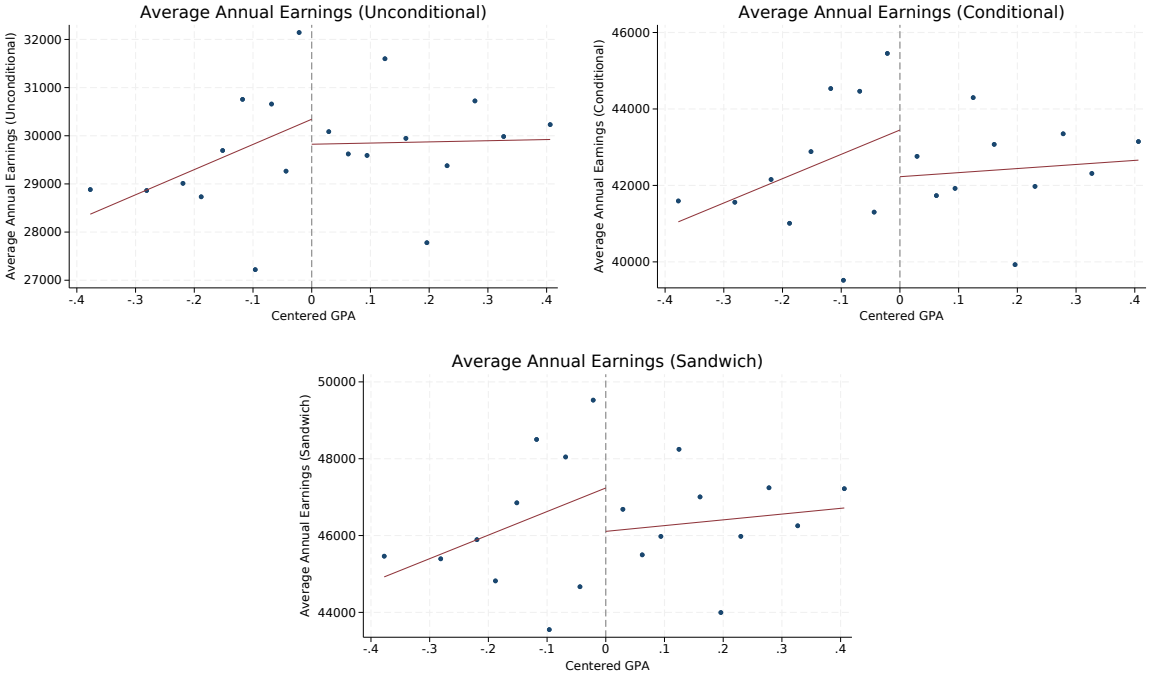
Notes:*** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(GPA_i \geq T_{cy})$ gives reduced-form estimates from equation (3); $TransferTarget$ gives instrumental variable estimates from equation (4). Observations are at person–year level. Unconditional earnings give average annual earnings over all quarters 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.

Figure 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.

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 8 shows suggestive evidence of decreases in earnings for transfer from four-year colleges as well, but these estimates are not statistically significant. Figure 7 shows these results graphically with binned scatterplots and local linear regression lines fit on each side of the discontinuity.

Table 9 and Figure 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 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

Table 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

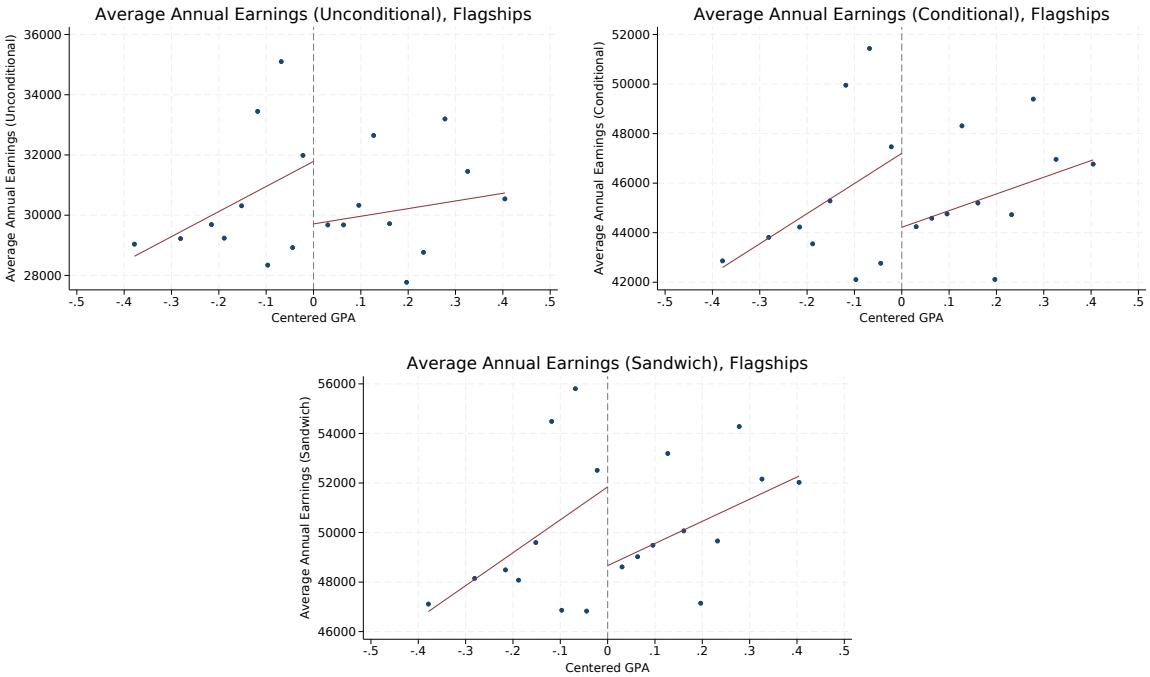
Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Observations are at person–year level. Unconditional earnings give average annual earnings over all quarters after the intended transfer year, where an observation with a missing value in the earnings records for a quarter is coded 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.

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 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.

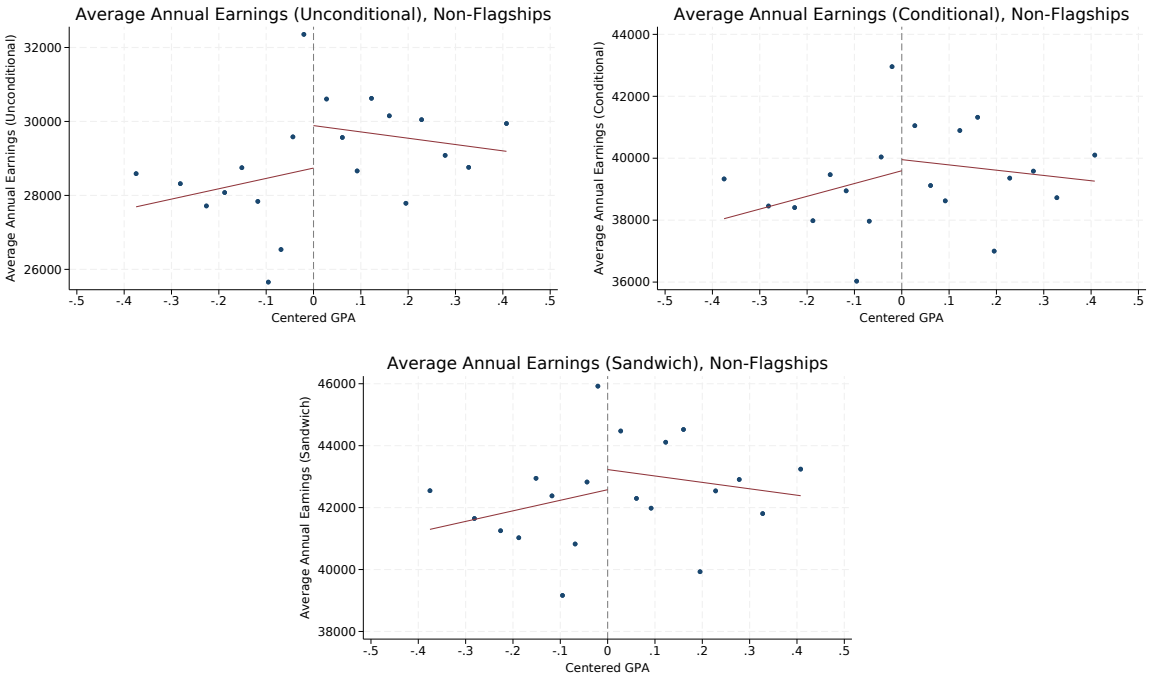
To offer a sense of how the effects change as individuals gain work experience and progress in their careers, Table 11 and Table 12 present the earnings effects separately by the time since intended transfer. To reduce variance, I estimate the effects in five-year earnings bins rather than individual years since transfer. The first bin corresponds

Figure 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 10: 2-year Applicants: Annual Earnings, Pooled across All Years, by Gender

	Unconditional	Conditional	Sandwich
Panel A: Women			
$TransferTarget$	-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			
$TransferTarget$	-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 (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.

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 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 11 and Table 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 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).

Table 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 (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.

Table 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 (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.

Table 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 (4). Standard errors clustered at the application–college–year level in parentheses.

I repeat this exercise with a window of 10 years rather than five.[38]

Table 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 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.

8 Interpretation of Estimates

8.1 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 cutoff. More concretely, consider a standard potential

[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.

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 $Y_i = Y_i(D_i) = D_iY_i(1) + (1 - D_i)Y_i(1)$, 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,

$$\hat{\eta}_1 = Pr(Nev)\omega_{Nev} + Pr(O_t)\omega_{O_t} + Pr(TT_{\tau>t})\omega_{TT_{\tau>t}} + Pr(O_{\tau>t})\omega_{O_{\tau>t}} \tag{6}$$

where $\hat{\eta}_1$ is the estimate of η_1 from equation (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.

8.2 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 Appendix 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

Table 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.

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

Table 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.

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.

8.3 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

[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.

Table 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.

for demographics, high school test scores, sending college, and all the other covariates included in Equation 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 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 16 and Table 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 $TransferTarget$ 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,

[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 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.

i.e., those who never transfer to a four-year college. Table 16 shows estimates that are pooled across all 1–21 years after intended transfer (analogous to those in Table 8), while Table 17 separately estimates effects by time since transfer (analogous to those in Table 11). These two tables give mixed evidence on the effect of transferring to a target college relative to never transferring. The estimates in Table 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 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 16 and Table 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 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).

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 7, 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 4 but use alternative outcomes that may shed light on explanations for the negative earnings effects.

9.1 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 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

Table 18: 4-year Applicants to Flagship Colleges: Field of Degree

	General	Science	Engineer	Health	Business	Educ	SocSci
$TransferTarget$	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
$TransferTarget$	-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 (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.

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 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

[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.

Table 19: 4-year Applicants to Flagship Colleges: Predicted Annual Earnings Based on Field of Degree

	Predicted Unconditional	Predicted Conditional	Predicted Sandwich
$TransferTarget$	-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 (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.

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 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 19 shows the results for four-year applicants to flagship colleges across predicted versions of the same three measures of earnings presented in Table 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.

[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.

9.2 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 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 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 13).

Table 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 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

Table 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)
$E[Y_0\|C]$	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 (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.

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 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 22, I show the years of experience accumulated by 11 years after transfer.[45]

[43] I explore this mechanism in subsection 9.3.

[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.

[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 21: 2-year Applicants: Employment, by Number of Years Since Intended Transfer

	Any Employment	Continuous Employment	Quarters Worked	Sandwich Quarters Worked
$\underline{TransferTarget}$				
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 (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 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 (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.

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.

9.3 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

Table 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 (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.

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 high school to the last college that she attends.[46] Table 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., 2022), 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.

[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.

9.4 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 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 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 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.

9.5 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

Table 24: 2-year Applicants: Final Cumulative GPA and Relative Semester GPA

	Final GPA		Relative Rank from GPA			
	All	Graduates	1	2	3	4
$TransferTarget$	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 (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.

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 subsection 9.1. 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 are statistically insignificant and economically small compared to the magnitudes of the earnings decreases.

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

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

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. 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.

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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 (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.

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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)
$E[Y_0\|C]$	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)
$E[Y_0\|C]$	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 (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.

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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 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 (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 (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 (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)
$E[Y_0\|C]$	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)
$E[Y_0\|C]$	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)
$E[Y_0\|C]$	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)
$E[Y_0\|C]$	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 (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. $E[Y_0|C]$ 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 (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
$TransferTarget$	49,278	55,348	30,907
	(92,063)	(91,436)	(46,342)
$TransferOther4y$	40,535	55,840	34,123
	(89,600)	(126,454)	(70,494)
$TransferTargetLater$	65,586	69,025	47,561
	(56,972)	(69,717)	(49,600)
$TransferOther4yLater$	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 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 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 (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 (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 (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 (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 2, collectively referred to as $\mathbb{X}$,

$$E(TransferTarget_{ict}|GPA_i, \mathbb{X}_i) = \sigma_0 + \sigma_1 \mathbb{1}(GPA_i \geq T_{ct}) + m(GPA_i) + u_{ict} \quad (7)$$

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 $\mathbb{X}$,

$$E[(1 - D_i)NeverTransfer_{ict}|GPA, \mathbb{X}] = \psi_0 + \psi_1 \mathbb{1}(GPA_i \geq T_{ct}) + n(GPA_i) + \omega_{ict} \quad (8)$$

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 $GPA_i \geq T_{ct}$ 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 8, is given by,

$$\begin{aligned}\psi_1 =& Pr(NT)E(NeverTransfer_{ict}|Z=1,NT) - Pr(NT)E(NeverTransfer_{ict}|Z=0,NT) \\ & - Pr(C)E(NeverTransfer_{ict}|Z=0,C)\end{aligned} \tag{9}$$

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 $Pr(C) = \sigma_1$, $E(NeverTransfer_{ict}|Z=0,C) = -\psi_1/\sigma_1$.