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 To do so, I leverage applications and admissions data to uncover unpublished GPA cutoffs for transfer student admissions at each institution and then use these cutoffs as an instrument for transfer.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 do not find positive 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, and show suggestive evidence of *negative* returns. The mechanisms include transfer students' substitution out of high-paying majors into lower-paying majors, reduced employment and labor market experience, decreases in academic performance relative to college peers, 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 of transfer from either a two- or four-year college to a four-year college on students' degree completion and earnings. Surprisingly, I find no evidence of positive 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. In fact, for both of these groups, I find suggestive evidence of *negative* returns. I investigate several mechanisms behind this result and find evidence of transfer students' substitution out of high-paying majors into lower-paying majors, as well as reduced employment and labor market experience. I also find modest evidence for decreases in academic performance relative to college peers 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 a variant of methods from 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

[3]I implement several tests to check the validity of this assumption in subsection 5.3 and find that students above and below the cutoff appear similar.

being narrowly denied transfer admission across a variety of colleges. I explore effect heterogeneity along colleges' level of resources by separately estimating effects for flagship colleges and less-resourced institutions.

My results show that among both two-year college students who apply to transfer to four-year colleges and four-year college students who apply to transfer to nonflagship four-year colleges, those who are narrowly accepted for transfer admission are significantly more likely to earn a bachelor's degree than those narrowly denied admission. However, I surprisingly find zero to 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 -\$7,000 for two-year to four-year transfers and -\$11,000 for four-year nonflagship to four-year flagship transfers. Statistical significance varies across specifications, but the point estimates are consistently large and negative, and they persist over time since transfer. For two-year to four-year transfers, the negative estimates are large and statistically significant 6-10 years after transfer, and for four-year to flagship transfers, the negative impacts persist for 11 years or more.

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 counterfactual 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 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. Relatedly, I find suggestive evidence 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 find no evidence for my main effects being driven by selective out-migration from Texas or changes in industry of employment.

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

[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 result of restrictions on how major-specific courses are counted for transfer or on admission to the business school (transfer students may be broadly admitted to a university but not to a specific major). Past work has shown that major-specific barriers exist for non-transfer students as well: Bleemer and Mehta (2024) 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.

performance metrics that go into accountability measures and college rankings (Handel and Williams, 2012; Jenkins and Fink, 2016).[6]

The rest of this paper proceeds as follows: section 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 presents the main RD results, section 7 elaborates on how to interpret results, section 8 explores mechanisms behind the main earnings results, and section 9 discusses policy implications and 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 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,

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

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 on the effect of access to colleges of varying resource levels (often referred to as "quality", see footnote 1), espeically those that use regression discontinuity designs (Hoekstra, 2009; Cohodes and Goodman, 2014; Zimmerman, 2014; Goodman et al., 2017; Smith et al., 2020; Kozakowski, 2023; Bleemer, 2024; Mountjoy, 2024). A subset of papers in this literature concern academic "mismatch" or "overmatch," when students attend colleges that are more academically selective/demanding than their own academic qualifications. While most of these studies find positive effects of attending more selective universities even for "overmatched" students (Black et al., 2021; Bleemer, 2022, 2024), some papers find negative effects (Arcidiacono et al., 2012, 2016).[9] I contribute to these literatures by estimating the effect of transferring to a well-resourced college, since prior work has only considered the qual-

[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).

[9] See also the topic of "mismatch" in law school (Sander and Stuart Taylor, 2012; Arcidiacono and Lovenheim, 2016)

ity/resources of one's initial institution. 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 (2021) 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.[10] They estimate the effect of transferring to the University of Texas - Austin (UT–Austin) through the Coordinated Admissions Program (CAP), which allows students who were initially rejected from UT–Austin to transfer in after completing their first year at a UT branch campus with a specified minimum GPA. However, CAP serves a relatively narrow population of students who (1) initially apply to UT–Austin, (2) are offered CAP and decide take up the program by June 1 following their final year of high school, (3) begin the following fall at another UT branch with the intention of transferring to UT–Austin one year later, and (4) complete the other CAP course/credit requirements. My work adds to this literature by including a broader set of students who begin at any four-year college in Texas and may not make the decision to transfer until later in their college career. Additionally, I explore the effects of transferring to a broader set of universities, including those that are less resourced than UT–Austin.

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

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.[11]

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.

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

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

each college. Thus, it is possible for students to make "mistakes" due to information frictions.[12] 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 covering all Texas public high school students matched to data on all within-state postsecondary enrollment, degree completion, and earnings from 2000 to 2024.[13] 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.[14]

The ERC data allow me to identify four-year public colleges in Texas that use college GPA cutoffs in their transfer admissions decisions. As noted in Altmejd et al. (2021), many colleges use minimum SAT cutoffs in admissions decisions without making these cutoffs publicly known. Similarly, some institutions use college GPA cutoffs in their admissions decisions for transfer students. Although these cutoffs are sometimes made

[12]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).

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

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

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.[15]

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.[16] 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.[17] [18]

My primary outcomes of interest are bachelor's degree completion, observed through 2023, and earnings, observed through the first quarter of 2024. I define degree completion relative to the year in which the student intends to transfer. For example, in the 2010–2011 academic year, the student submits an application to transfer the following year; that is, she would like to enroll in fall of the 2011–2012 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.[19]

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

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

[16]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).

[17]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.

[18]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.

[19]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.

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.[20] Therefore, if a worker does not appear in the earnings data, she may really have zero earnings, or she may have earnings that are not observed. To account for this, I use three different measures of annual earnings. First, to fully capture any effects on the extensive margin of employment, I use an "unconditional" earnings measure, which codes earnings for quarters in which workers do not appear as zero. However, this might induce bias since they are not all true zeros, so the second measure ("conditional" earnings) averages over only nonzero quarters.[21] 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.[22] 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

[20] 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.

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

[22] 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).

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 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.[23] 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

[23] 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.

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.[24]

I identify eight colleges that use admissions cutoffs for four-year students and 23 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. Appendix Tables A1 and A2 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.[25] 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.

[24] This procedure is similar to the ones used to identify discontinuities in Altmejd et al. (2021), Brunner et al. (2021), Andrews et al. (2017), and Mountjoy (2024). I test the sensitivity of this procedure by considering analyses with stricter p-value thresholds (i.e., less than 0.001 and less than 0.0001) and obtain qualitatively similar results.

[25] 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.

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.[26]

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.[27] However, I keep applicants from two-year and four-year colleges separate in all specifications. I also estimate some specifications in which I separate out

[26]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.

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

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 target college.[28] 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 $\theta_{s(i,t)}$.[29] Because the admissions thresholds may be measured with noise, I use a donut-hole specification that drops observations within 0.01 grade points of the cutoff.

I then generate reduced-form estimates of the effect of crossing a target college's

[28] 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.

[29] 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.

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 on outcome Y_{ict} of a student being just above a target college's GPA cutoff relative to 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 7.

5.3 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 transferring to that institution.

Next, I more directly show evidence of relevance by presenting first-stage results from equation (2) in Table 1. 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 A3 and A4 show that the results are similar across a range of these choices for my main outcomes.[30][31] The first column of Table 1 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

[30]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.

[31]Results are also not sensitive to varying the set of control variables (i.e., including no controls or only college-year fixed effects).

be interpreted as the effect of transferring to a target college on various outcomes. This specification treats students who are accepted for admission but choose not to transfer as "never-takers." The results in the second column show that, while not all accepted students transfer, there is still a sizable jump in transfer rates at the discontinuity. Among students who applied to a target college, students with GPAs just above their colleges' cutoff are 12 percentage points more likely to transfer to that college than students just below the cutoff. The third and fourth columns show that applicants from four-year colleges who are just above their respective cutoffs are 21 percentage points more likely to be accepted and 15 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.[32] 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.

[32] 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 A3 and A4.

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 Appendix Figure A1 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 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 3, 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 a balance test using composite measures of students' predicted bachelor's degree completion and 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[33] and

[33]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.

regress bachelor's degree completion within six years of high school graduation, or average annual conditional (i.e. dropping quarters without any earnings) earnings 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 BA completion/earnings for my analysis sample. When matching these measures to my analysis sample, I use characteristics of the students' college experiences as measured in the semester when they submitted their transfer applications (i.e., the year before they intend to transfer).[34]

In Figure 4, I show binned scatterplots analagous to Figure 2 where the outcome is covariate-predicted bachelor's completion/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. For both two-year and four-year applicants, while these covariate-predicted measures increase as GPA increases, there is no discontinuity at the admission cutoff. Appendix Table A5 shows the corresponding table and verifies that there are no statistically significant discontinuities. Appendix Table A6 separates four-year applicants to those who apply to flagships versus nongflagships, and also shows no evidence of discontinuties in the covariate-predicted outcomes.

6 Main Regression Discontinuity Results

6.1 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

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

shown in Table 2, where the top panel shows results for applicants from two-year colleges and the bottom panel is for applicants from four-year colleges. Degree completion is measured 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.[35] 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 2.0 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. However, the reduced form estimate is difficult to interpret because it applies to a mix of "compliers," whose transfer behavior would be changed by crossing the cutoff; "always takers," who would transfer even if they were just below the cutoff; and "never takers," who would not transfer even if they were just above the cutoff (Angrist et al., 1996). The second row gives the IV estimates that isolate compliers by scaling up the reduced-form estimates by the first stage.

For two-year applicants, estimates are positive and statistically significant across the board, and the magnitude of the effect is stable at approximately 17 percentage points from two to six years after intended transfer. The $E[Y_0|C]$ row underneath gives the estimated base rate, i.e., the expected value of the outcome for compliers when untreated.[36] 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

[35]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.

[36]Note that, because this value is for untreated compliers, it is estimated following the method of Abadie (2002) rather than taken directly from the data. See Appendix B for an illustration of the estimation of $E[Y_0|C]$.

college eventually. I return to this issue and talk about how it affects the interpretation of the estimates in section 7.

A subset of the reduced-form effects are also shown graphically in Figure 5 with binned scatterplots. The left panels show the relationship between centered GPA and earnings for a wide range of GPAs with a fourth-order global polynomial regression fit, while the right panels zooms in on the analysis sample and fits local linear regression lines on each side of the discontinuity. While there are clear visual increases in bachelor's degree completion within one or two years, the effects is much less pronounced over the longer time horizons.

Figure 6 and Panel B of Table 2 show results for four-year to four-year transfer applicants. There is some evidence of positive impacts on bachelor's degree completion, although not statistically significant. Table 3 shows the same outcomes for four-year applicants, but it separates applicants to flagship colleges from nonflagship target colleges and reveals that the average effects mask heterogeneity between these two groups. While the point estimates are positive in every column for students who transfer to non-flagship 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 26 percent of students have completed a bachelor's degree within one year, this figure climbs to 88 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 18 and 36 percentage points more likely to complete bachelor's degrees within two to six years of intended transfer. Although the statistical significance of these estimates varies over the time frames, the magnitudes are large across the board, especially when we consider the base rates of bachelor's completion for this subgroup. Four years after intended transfer, bachelor's degree compeltion is only eight percent for compliers below the threshold, but over 40 percent ($0.08 + 0.34 = 0.42$) for students who transfer. The corresponding reduced form results are shown graphically in Appendix Figures A2 and A3. Appendix Table A7 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.

6.2 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 1–24 years. Table 4 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 do not earn more than two-year college students who were marginally denied transfer admission to target colleges. In fact, there is suggestive evidence that transferring causes these students to earn *less* than they would have had they not transferred. The point estimates are consistently negative across all three earnings measures, although the statistical significance varies. The magnitudes are substantial: the dollar amounts are around -$7,000 per year, and a comparison with the base rates shows that they correspond to reductions in annual earnings of 10 to 20 percent. Figure 7

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

shows these results graphically with binned scatterplots, where once again the left panels show the relationship between centered GPA and earnings for a wide range of GPAs with a fourth-order global polynomial regression fit, while the right panels zooms in on the analysis sample and fits local linear regression lines on each side of the discontinuity. In both sets of plots, there is a visual drop in earnings at the discontinuity.

Focusing on four-year to four-year transfers in the bottom panel of Table 4, once again the results show no evidence of positive effects and some evidence of negative effects, but estimated imprecisely. Table 5 shows these results broken down by flagship status and reveals that any negative effects appear to be driven by students who apply to transfer to flagship institutions. Although the estimates are imprecise, the point estimates suggest negative returns for students at four-year colleges who are marginally admitted to a flagship. The visual evidence in Figure 8 supports this conclusion. Meanwhile, the bottom panel of Table 5 shows inconsistent evidence for the effect of being admitted for transfer to a nonflagship target institution. Although the point estimate on unconditional earnings is large, it is negative for the other two earnings measures. Appendix Table A8 shows the effects for two-year applicants broken down by flagship status, showing larger decreases for students transferring to flagship universities. However, the point 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 any 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 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 on the results for two-year applicants broken down by gender since these are where I find the most evidence of heterogeneity.[38] Appendix Table A9 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, shown in Appendix Table A10 where 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 6 and Table 7 present the earnings effects separately by the time since intended transfer. To reduce variance, I estimate the effects in five-year earnings bins rather than individual years since transfer. The first bin corresponds to average annual earnings one to five years after transfer. For some individuals who complete their degree or drop out within one year of transferring, this will not include any years when they are still enrolled in college. For others, it may include some years of enrollment. I do not include the intended transfer year, as nearly all individuals are still enrolled at that time. The second bin averages earnings over six to ten years after transfer, after which virtually all students are done with their schooling and thus gives estimates of early-career earnings effects. Finally, the third and fourth bins show longer term results.

If the negative effects of transferring are concentrated in early years after transfer but become positive over time, it may imply that the lifetime effect of transfer is positive. However, Table 6 and Table 7 show that even in the longer term, the earnings effects are null to negative for both two-year students transferring to any four-year college and for four-year students transferring to flagship colleges. For two-year applicants, the negative effecst are strongest six to ten years after transfer. The point estimates remain large and negative 11-15 years out, but show some modest but inconsistent evience of catch up in later years. For four-year students transferring to flagships, the effects appear to become more negative over time.

[38] I explore heterogeneity by student race-ethnicity and free of reduced-price lunch status but do not find meaningful differences.

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, 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., since I observe data through 2023, I would mark a person as out-migrating if they have no earnings in Texas from 2019 to 2023). I repeat this exercise with a window of 10 years rather than five.[39]

Table 8 shows that for both two-year to four-year and four-year to flagship transfer applicants, there is no statistically significant effect of transferring to a target college on out-migration from the Texas workforce, and if anything, transfer makes individuals *less* likely to out-migrate. This suggests 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 A11, align with my main estimates, which provides additional assurance that out-migration from Texas does not drive my main effects.

[39] 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.

7 Interpretation of Estimates

7.1 Summary Statistics of Sample and Compliers

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 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.

To help contextualize which types of students contribute the identifying variation for the main effects, Appendix Tables A12 and A13 give summary statistics on the background characteristics of my analysis samples as well as for several comparison groups. The first column of Appendix Table A12 includes all 9.6 million students who attended public high schools in Texas from 1993 to 2023. The second column narrows this sample to include only students who enrolled in college in Texas for at least one semester, and the third column narrows further to includes only those who enrolled at a two-year public college in Texas for at least one semester. The final four columns narrow further to the first population of interest in this study: students at two-year colleges who have applied to transfer to a four-year target college between 1999 and 2019. While this full population includes about 350,000 students, only around 53,000 have a GPA close enough to the cutoff (i.e., within 0.3 grade points) to be used in my analysis. The final two columns use the baseline RD specification given in equation (4), but replace the outcome Y with pre-determined covariates X to describe the students on the margin of transfer admission in column 6 and the subset of these marginal applicants who are compliers (i.e., transfer to the target university if and only if they have a GPA just above the cutoff) in column 7. Appendix Table A13 repeats this exercise for students enrolled at four-year colleges who apply to transfer to any target college in columns 2-5 or to a flagship college in columns 7-10. Columns 1 and 6 give average characteristics of all students who ever attend a four-year college or UT-Austin.[40] "Math HS test score" and "Reading HS 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. For transfer applicants, cumulative GPA and credits give their cumulative college GPA and attempted credits at the time they they applied for transfer. For the broader college samples, cumulative GPA and credits are from one randomly drawn semester in

[40] I focus on UT-Austin and not both flagships since my flagship results are dominated by UT-Austin since I only detect one admissions cutoff for Texas A&M.

which they were enrolled.

The 2-year to 4-year transfer compliers shown in Appendix Table A12 are more representative of the average 4-year college student than the average 2-year college student in terms of their race-ethnicity and likelihood of receiving free or reduced-price lunch in high school. However, compliers' high school standardized test scores and college GPAs are more closely aligned with the average overall college student and the average 2-year college student. Relative to all 2-year applicants to target colleges, compliers have lower test scores and GPAs, and have attempted more credits.

Appendix Table A13 shows that compared to the average UT-Austin student, 4-year to flagship transfer compliers are more likely to be male, less likely to be Asian, more likely to be Hispanic, have lower test scores and have accumulated less credits than the average UT-Austin student. Compared to all marginal applicants (including always-takers, never-takers, and compliers), compliers are more likely to be White, less likely to be Asian or to receive free or reduced-price lunch, and have fewer credits.

Overall, these characteristics highlight that among applicants to better-resourced colleges (two-year to four-year and four-year to flagship transfer applicants), compliers are negatively selected academically compared to the populations at the target college for which they apply, indicating that they might be "over-matched." However, they are not necessarily more disadvantaged as measured by free or reduced-price lunch status or race-ethnicity.

7.2 Decomposition of Local Average Treatment Effect

Next, we need to understand the counterfactual for compliers. 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 are observable in the data for untreated 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,

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

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 two terms are defined analogously, where O_t defines transferring to some other (i.e., nontarget) four-year college in year t, and $T_{\tau>t}$ defines transferring to a four-year college (target or other) in some year τ later than t.

7.3 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, $Pr(Nev)$, $Pr(O_t)$, and $Pr(T_{\tau>t})$ are identified and can be estimated using the method of Abadie (2002) (see Appendix B for an illustration in this setting). This tells us how much weight is being put on each treatment effect in the combined IV estimate. If the vast majority of untreated compliers were to fall into one category, e.g., if almost all students who are rejected from a target college in year t never transfer to a four-year college, we could interpret the effects as being close to the effect of transferring to a target college relative to never transferring. However, the first row of Appendix Table A14 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 and there are nontrivial shares across all three categories. 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 complier shares 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.

Appendix Table A15 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 Appendix Table A15 shows that the most common counterfactual for students who apply to transfer to a flagship college is to never transfer, although there are nontrivial shares of compliers who would have transferred to a nontarget four-year college in some year later than t, or would have transferred to a two-year college in year 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 of 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.

7.4 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. Instead, to help interpret the RD results, I separately estimate ω_{Nev}, ω_{O_t}, and $\omega_{T_{\tau>t}}$ using ordinary least squares (OLS) with the sample of all college students in Texas who apply to transfer to a four-year college. In these specifications, I control for a fourth-order polynomial of GPA, demographics, high school test scores, sending college fixed effects, and all the other covariates included in equation (2).[41] 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 9 and Table 10 give the results for two-year applicants to any target college and four-year applicants to UT-Austin, respectively, 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 to a target college in year t or (2) never transfer to a four-year college. Students following a different counterfactual pathway are not included in this column. 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

[41] The full list of covariates is as follows: fourth-order polynomial of college GPA, 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.

for my full set of covariates. $E[Y_0]$ gives the average earnings for untreated students, i.e., those who never transfer to a four-year college. Results are pooled across 1–24 years after intended transfer, analogous to those in Table 4. The estimates in Table 9 indicate that, on average, two-year students who transfer to a target college earn approximately 1,700 dollars less per year than those who apply to transfer to a target college but never transfer. The corresponding estimates for four-year to flagship transfers in Table 10 is around -1,500 dollars. Since students who are accepted for transfer are likely positively selected yet the estimated effects are still negative, this lends additional evidence that for both of these groups, the true causal effect of transferring to a target college relative to never transferring is negative.

However, results for two-year applicants are more mixed when looking at the dynamics of earnings over time in Appendix Table A16. Unlike the regression discontinuity results, the selection on observables estimates of transferring relative to never transferring are positive and statistically significant in the longer run. This discrepancy may be because the selection on observables 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 A17 shows persistently negative returns to transferring from a four-year to UT-Austin relative to never transferring, although estimates are smaller and statistically insignificant in the long run.

The remaining columns of each table give the OLS estimates of the effect of transferring to a target college in year t relative to following the other possible counterfactual pathways. For two-year applicants, there appear to be small negative returns to transferring to a target college in year t relative to waiting until later to transfer. 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 A18. This finding also aligns with prior research on the relationship between community college transfer timing and earnings, which shows that community college students who transfer after obtaining an associate's degree earn more, on average, than those who transfer without any degree (Belfield, 2013; Kopko and Crosta, 2016). For four-year to flagship applicants, returns to transferring to the flagship in year t appear to be positive relative to all other counterfactual pathways aside from never transfer.

8 Mechanisms

Next, I turn to an exploration of *why* the regression discontinuity estimates of the returns to transferring to a target college show no positive effects. Although these analyses are more speculative than the main results presented in section 6, they help shed light on factors that may contribute to the lack of positive earnings effects for two-year students who transfer to four-year colleges and four-year students who transfer to flagship schools. I find strong evidence for the changes in field of study from high-earning to lower-earning majors and decreases in employment and experience. I find modest evidence for decreases in college performance relative to college peers and changes in proximity of support networks. I do not find evidence for changes in industry of work. For all of the following mechanisms analyses, I return to the IV specification as in equation (4) but use alternative outcomes.

8.1 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 11, where I group students into 13 mutually exclusive categories based on the field of their bachelor's degree: general (e.g., liberal arts), sciences, engineering, health, business, education, social sciences, computer science, vocational studies, art, humanities, and others. Students who do not complete a bachelor's degree within 6 years of transfer fall into the "no degree" category. Each column is a separate regression where the outcome is an indicator variable for a student completing 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.[42] They are also less likely to major in a vocational field. These students appear to substitute into general liberal arts or social science degrees.

To quantify how these changes in major might affect earnings, I use data on the earnings of all bachelor's degree holders in Texas to calculate average predicted earnings for each major category as measued by its 2-digit CIP code. Specifically, using years when individuals were the same age as those in my analysis sample, I regress earnings on fixed effects for each major category to create a measure of average predicted earnings given the degree field.[43] 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. Appendix Table A19 shows the results for four-year applicants to flagship colleges across predicted versions my three measures of earnings (unconditional, conditional, sandwich) and reveals that changes in major can account for around half of the total earnings effect. Thus, while changes in major are an important mechanism, they are not the whole story. Additionally,

[42]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 (2024) show that using GPA to restrict who can access business and other lucrative majors is common across many universities.

[43]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.

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 A20 shows that there is no clear pattern of two-year to four-year transfer students moving from high-earning to lower-earning majors.

8.2 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. 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 8).

I present results for two-year applicants, separately for men and women, in Table 12. I find that across all employment measures, men who are marginally admitted to a target college work less than men who are marginally denied. They are 14 percentage points less likely to be employed, 18 percentage points less likely to be continuously employed, and work over half a quarter less each year. Combining these results with those on the indicator of dropping out of the earnings data in Table 8 implies that men who transfer are not more likely to exit the labor force completely (or move out of Texas), but they have more spells of unemployment and might switch between jobs more frequently. Meanwhile, for women, transfer does not have statistically significant impacts on employment, and if anything, points estimates imply positive impacts. Appendix Table A21 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.

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. Since negative employment effects are concentrated among two-year male applicants, I focus on this group in Appendix Table A22 and show experience accumulated by 8 and 13 years after intended transfer.[44] The results show that 8 years after intended transfer, men who were marginally accepted for transfer have accumulated one less year with any positive earnings, almost five fewer quarters with any earnings, and over five fewer quarters as part of continuous employment spells. By 11 years after intended transfer, these decreases in accumulated experience have nearly doubled.[45]

8.3 Enrollment, College Resources, and Match

I focus on students applying to transfer to higher-resourced colleges, which we may expect to have positive effects based on prior research focused on first-time-in-college students who attend better-resourced colleges. In this section, I explore why those positive effects may not carry over to transfer students. First, I show that compared to margnially denied transfer applicants, marginally accepted transfer students attend much better-resourced

[44]I choose 8 and 13 years as the midpoints of the 6-10 and 11-15 year earnings bins, where we see the largest negative earnings effects for two-year applicants.

[45]Estimates for women, not shown, are positive but statistically insignificant.

colleges in the short term, but this difference decreases over time.

Appendix Table A23 shows the treatment effects on two-year college students' sectors of enrollment for the four semesters following the intended transfer semester. In the first semester, as expected, marginally accepted transfer students are much (81 pp) more likely to be enrolled at a four-year college. However, they were also much (58 pp) less likely to be enrolled at a two-year college, illustrating that most students who were marginally denied admission stayed enrolled at a two-year college rather than dropping out entirely. The final row shows combines these two sectors to show "any college" enrollment and reveals that 77 percent of untreated compliers were enrolled somewhere, compared to 100 percent of treated compliers (by construction). Over time, the differences between the treated and untreated group lessen, such that four semesters later, marginal transfer students were only 21 percentage points more likley to be enrolled at a four-year college. This reflects a combination of treated compliers dropping out and untreated compliers transferring to a four-year college in later years. Appendix Table A24 adds up students' total number of enrolled semesters and credit attempted, and shows that by six years after intended transfer, treated compliers have only completed 0.39 more semesters (statistically insignificant) and attempted 10.2 more credits (marginally statistically significant) than untreated compliers.

Appendix Table A25 further investigates the differences between the types of colleges and college peers that treated and untreated compliers are exposed to over time. I use my measures of students' covariate-predicted bachelor's degree completion and earnings to compute each *college's* average students' predicted outcomes to get a sense of how treated and untreated compliers compare to their college peers. I also include each college's average students' high school math standardized test score. The left panel of Table A25 shows that at the time of intended transfer, there are large increases in the predicted outcomes of college peers for both two-year transfer applicants and four-year transfer applicants to the flagship colleges. For example, for two-year applicants, the average predicted earnings of the college attended by a treated transfer applicant is over $10,000 more than the average predicted earnings of the college attended by an untreated transfer applicant. In the right panel, I repeat this exercise each transfer applicant's *last* college attended. Although treated compliers are still at colleges where their peers' average outcomes are higher, the magnitude is around half for each measure. This illustrates how transfer applicants who are marginally rejected may be able to "catch up" by transferring to a better-resourced college later.

So, marginal transfer students are attending colleges that have higher levels of resources and higher-achieving peers, which past work has generally found to be beneficial for first-time-in-college students (Bleemer, 2024). However, we may not see positive earnings effects of transfer if students are academically under-prepared for the higher-resourced college (e.g, "over-matched"). While this is difficult to test directly, I investigate this by examining their GPAs in the subsequent semesters after transferring. In order to compare them to their peers at their current college, 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. The results are shown for the first four semesters after the intended transfer in Appendix Table A26. The results show transfer students' relative GPAs are lower in the first and second semesters after transfer, implying that they are performing worse than their peers. Looking at semesters three and four, there do not appear to be any persistent effects. However, the interpretation of these results, especially in the later semesters, is not straighforward becuase of selective attrition from the sample of students with GPAs (due to differences in the drop-out and graduation rates between the treatment and control groups).

8.4 Potential Loss of Networks

When students transfer, they may lose access to their existing support networks. Qualitative literature has shown that transfer students have difficulties adjusting to their new environment and integrating socially into their new college (Flaga, 2006). While I cannot directly measure loss of networks, I shed some light on this mechanism by investigating how transfer affects students' likelihoods of attending college near their hometowns. I use students' high school location as a proxy for their hometown. I calculate travel time (driving) from each student's high school to the last college that she attends.[46] Table A27 shows the results for two-year applicants, which lend some modest evidence that students are attending college further from home. Point estimates imply that marginal transfer students are around 13, and 5, percentage points less likely to attend a college within 30, and 60, minutes of their high school, but the effects are estimated imprecisely. To the extent that being geographically near support networks is beneficial for students, this may contribute to the lack of positive earnings effects. 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 as well.

8.5 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 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 major as described in subsection 8.1. 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 A28 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.

[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). Travel time is computed as the driving time in minutes with OpenRouteService.

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

9 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 transfer on educational and labor market outcomes. First, I use detailed application and admissions data from all public four-year universities in Texas to uncover the institution–year-specific GPA thresholds used in transfer admissions. I then pool data across colleges and years with cutoffs and use an RD design to estimate the effects of a student's being marginally admitted for transfer, net of the difference in student characteristics between those who do and do not transfer. My results show that, for my sample, transferring does not lead to earnings increases. If anything, I find that 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 earnings *decreases* compared to students who were marginally denied transfer admission.

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 could have zero, or even negative, impacts on marginal students' earnings. This suggests that care must be taken in the structuring of transfer systems and the design of transfer policies.

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

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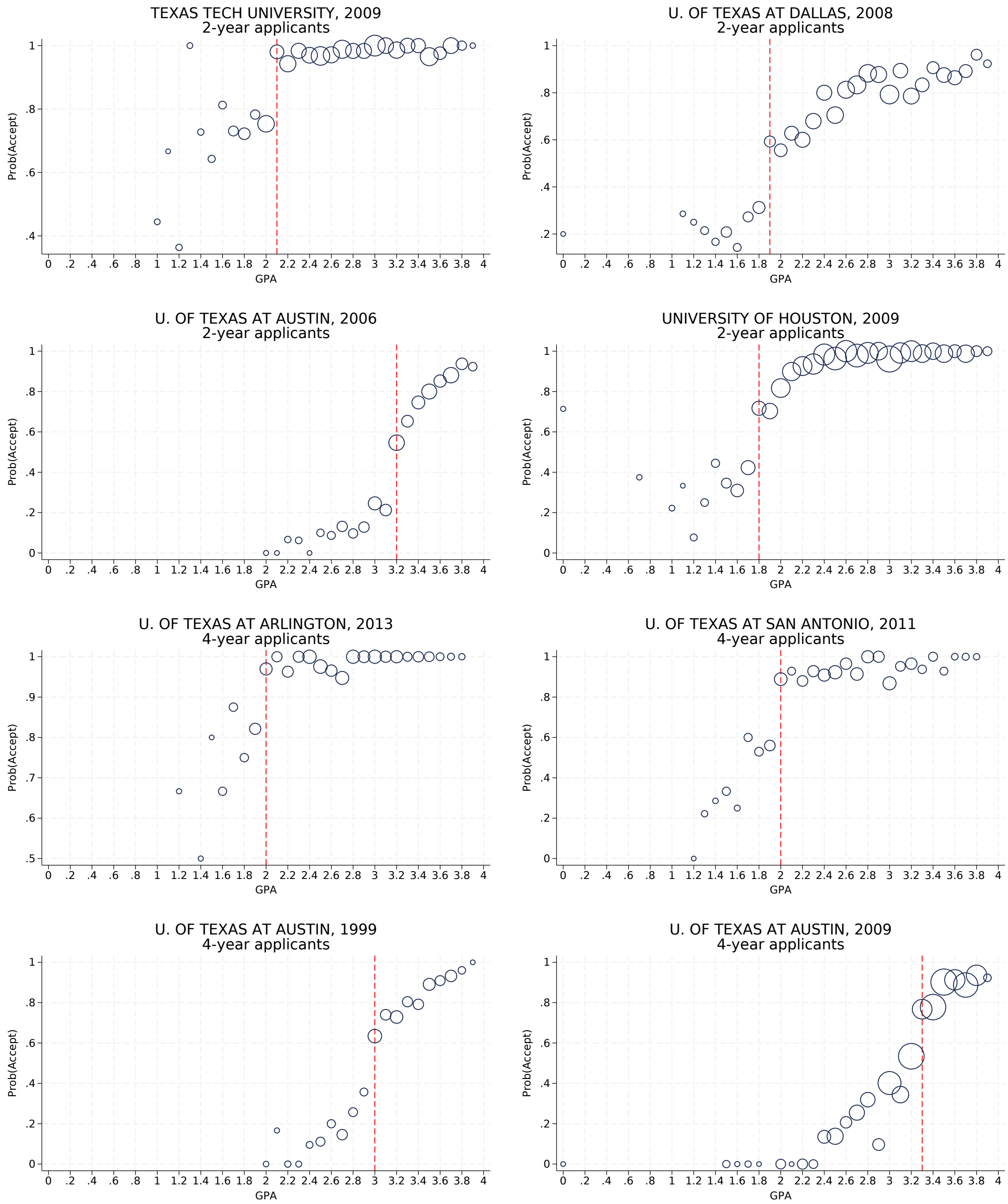
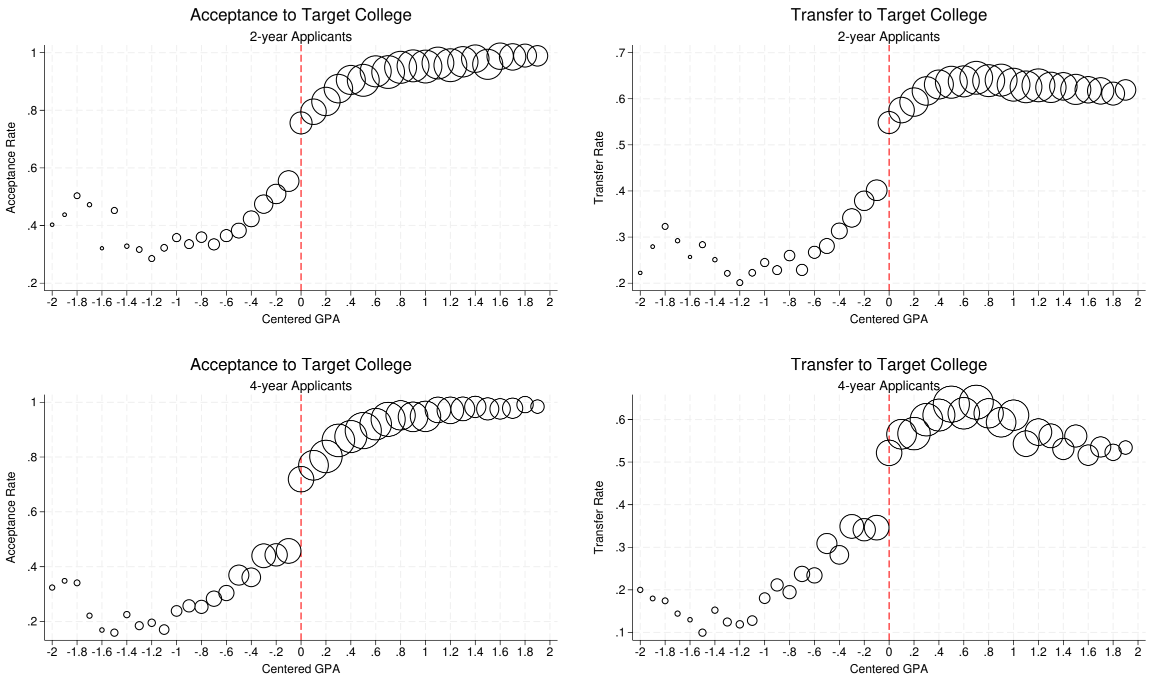
10 Tables and Figures

Figure 1: Examples of Identified GPA Cutoffs in Transfer Admissions



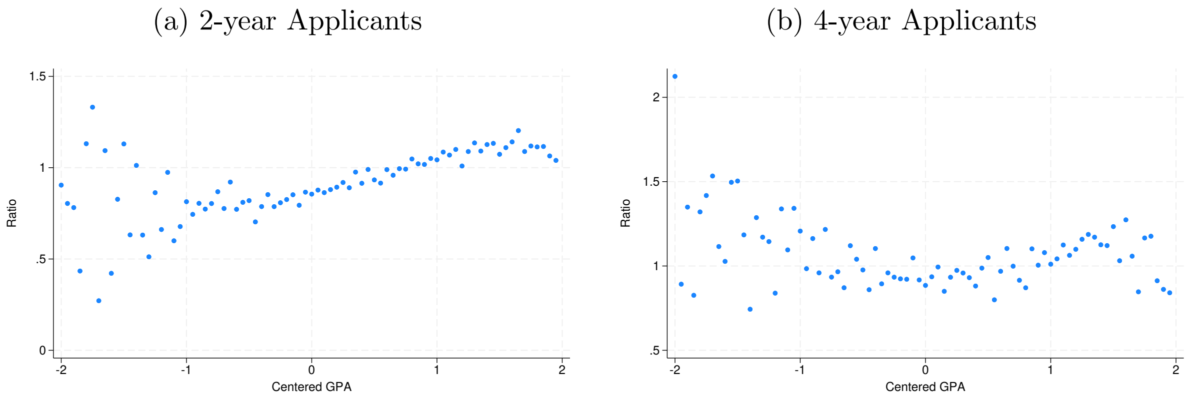
Notes: Each subfigure shows an example of an estimated discontinuity for a particular institution, year, and sector (2-year/4-year) of applicants. The subfigures are binned scatterplots of applicant acceptance rates, where each bin is 0.1 grade points. Circle sizes are proportional to the number of applications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

Figure 2: Identified Cutoffs in Transfer Admission, Pooled across Colleges and Years



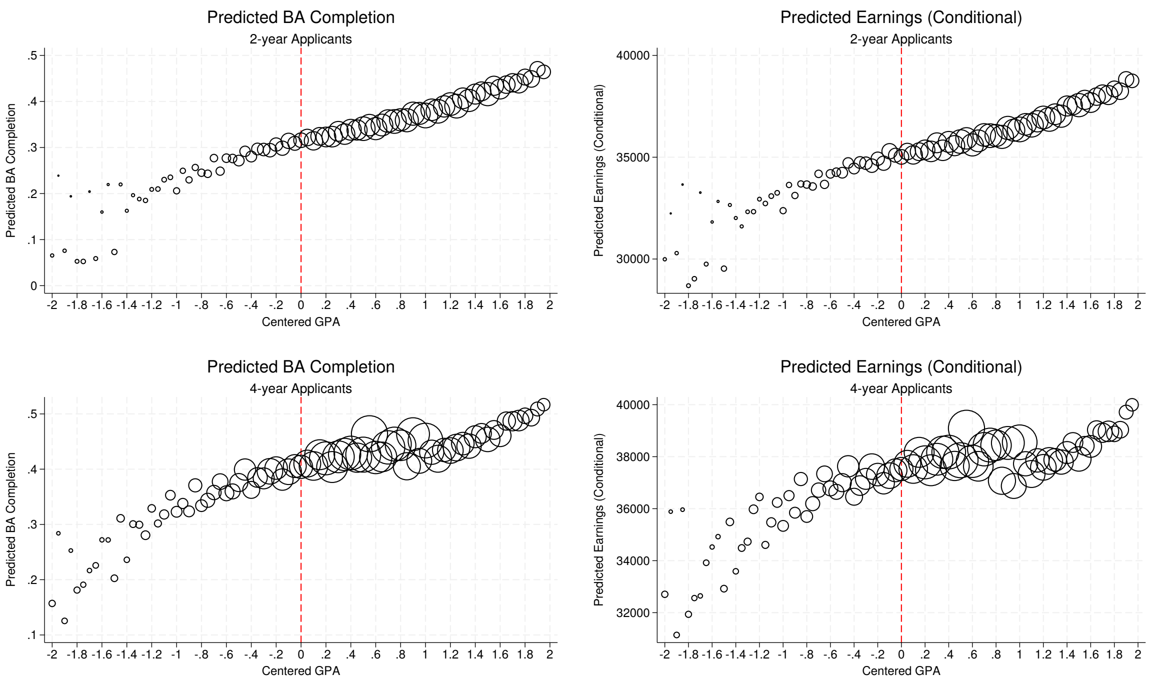
Notes: Binned Scatterplots of transfer application acceptance and enrollment on centered GPA. Centered GPA is created by subtracting the college–year-specific cutoff from each student's GPA for each application she submits. Circle sizes are proportional to the number of applications in each bin.

Figure 3: Density Smoothness Tests



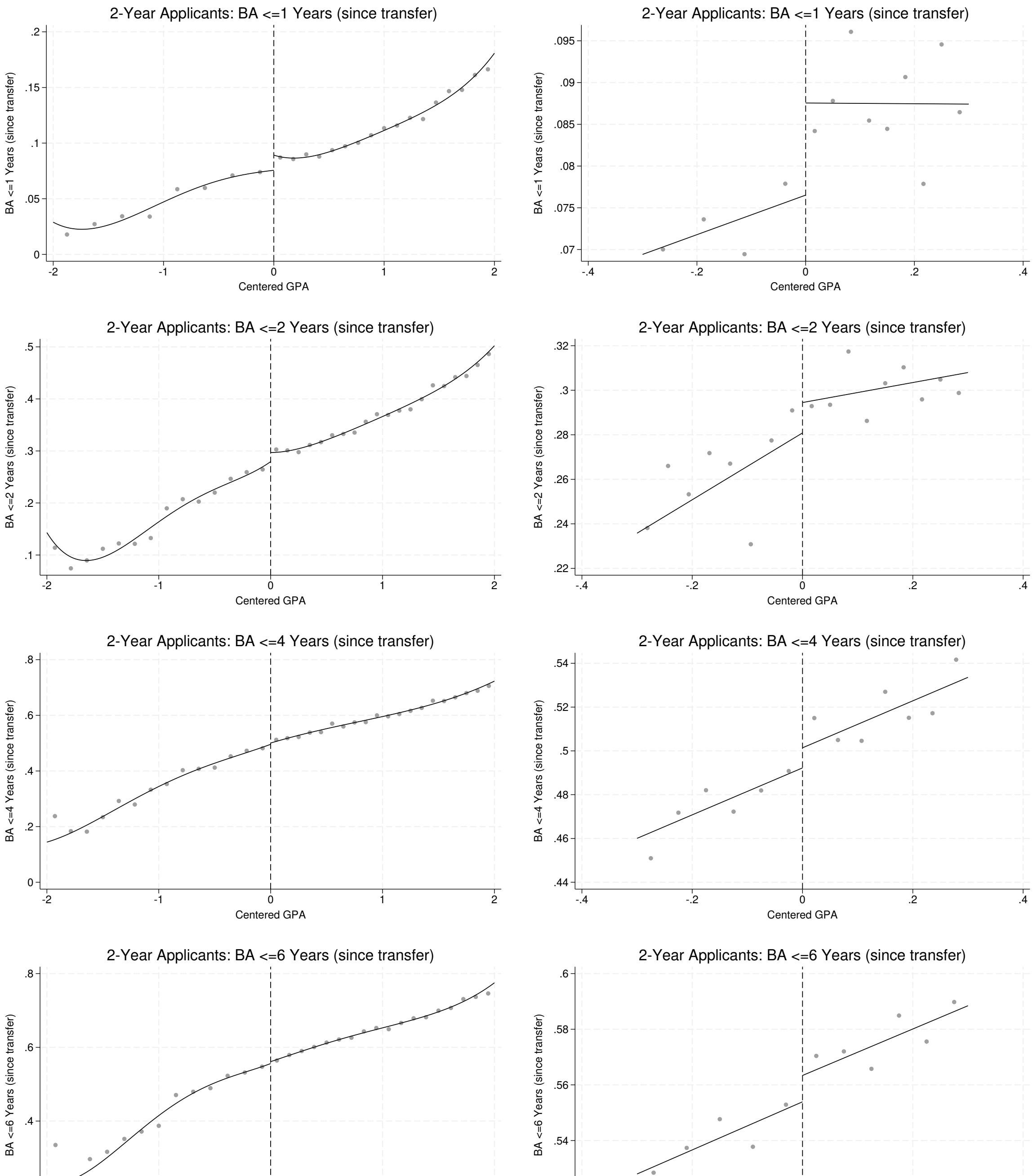
Notes: Each figure shows the ratios of conditional to unconditional densities in 0.05 grade point bins relative to the admissions cutoff. Conditional densities condition on whether students receive free or reduced-price lunch, $Pr(GPA|FRPL)/Pr(GPA)$.

Figure 4: Balance Tests



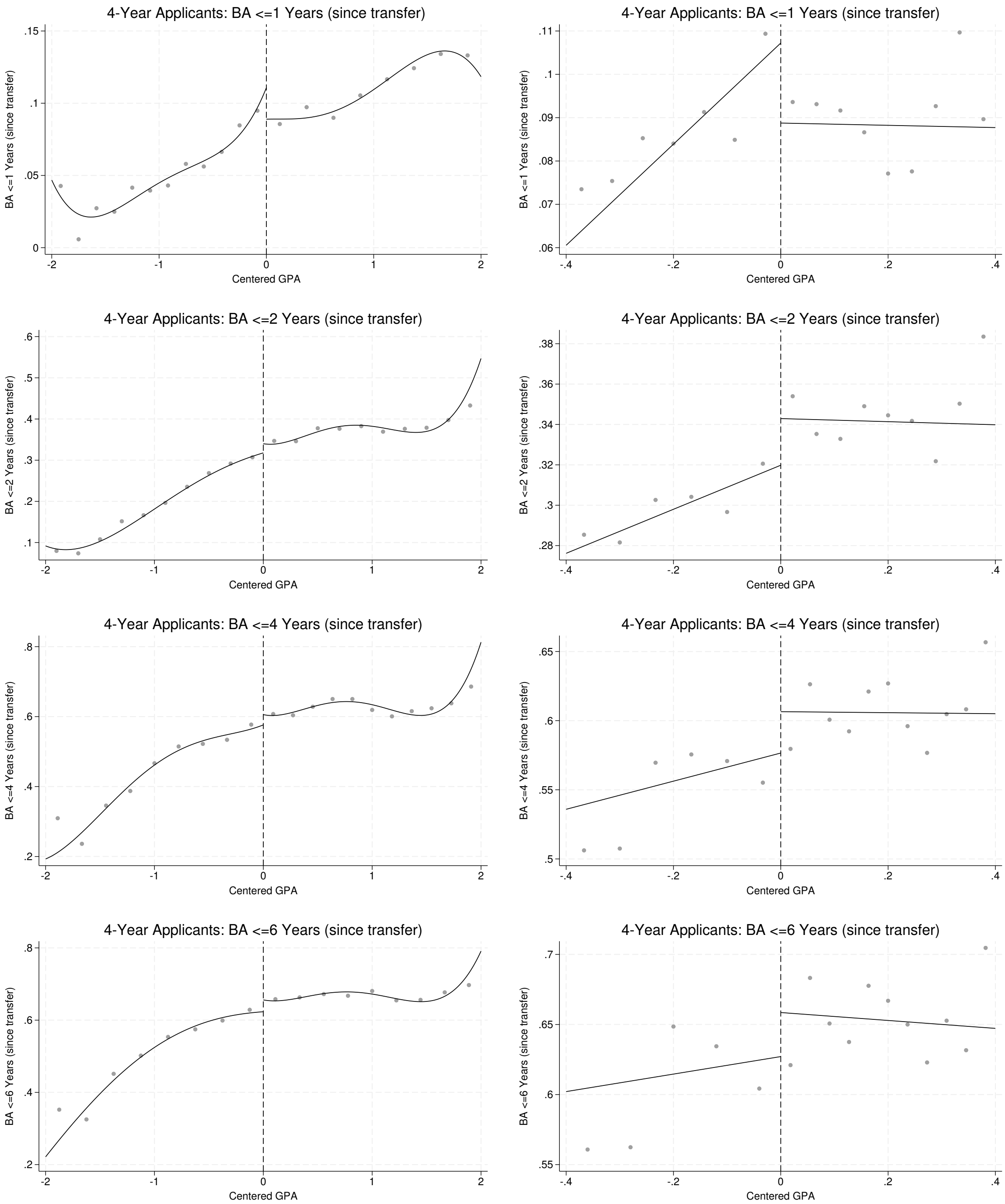
Notes: Binned scatterplots of covariate-predicted bachelor's degree completion and covariate-predicted earnings on centered GPA. Predicted bachelor's completion (within 6 years of high school graduation) and conditional earnings estimated on full sample of Texas high school graduates who enroll in a 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. 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. Top panel gives two-year transfer applicants; bottom gives four-year transfer applicants.

Figure 5: 2-Year Applicants: Bachelor's Completion in Years Since Intended Transfer



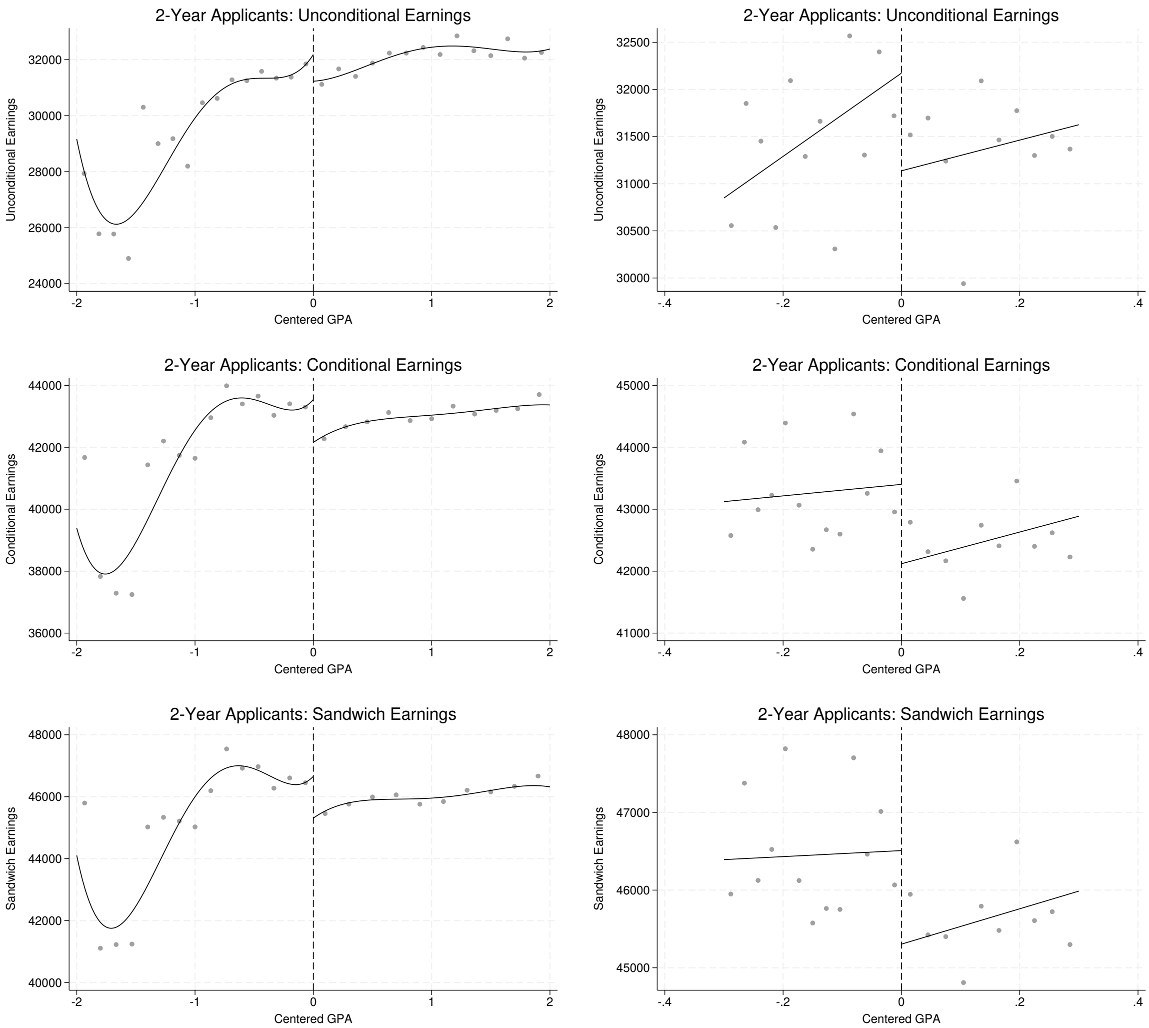
Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff and fits a global fourth-order polynomial on each side. Right panel includes only analysis sample and fits a local linear regression on each side. Sample of two-year transfer applicants. 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).

Figure 6: 4-Year Applicants: Bachelor's Completion in Years Since Intended Transfer



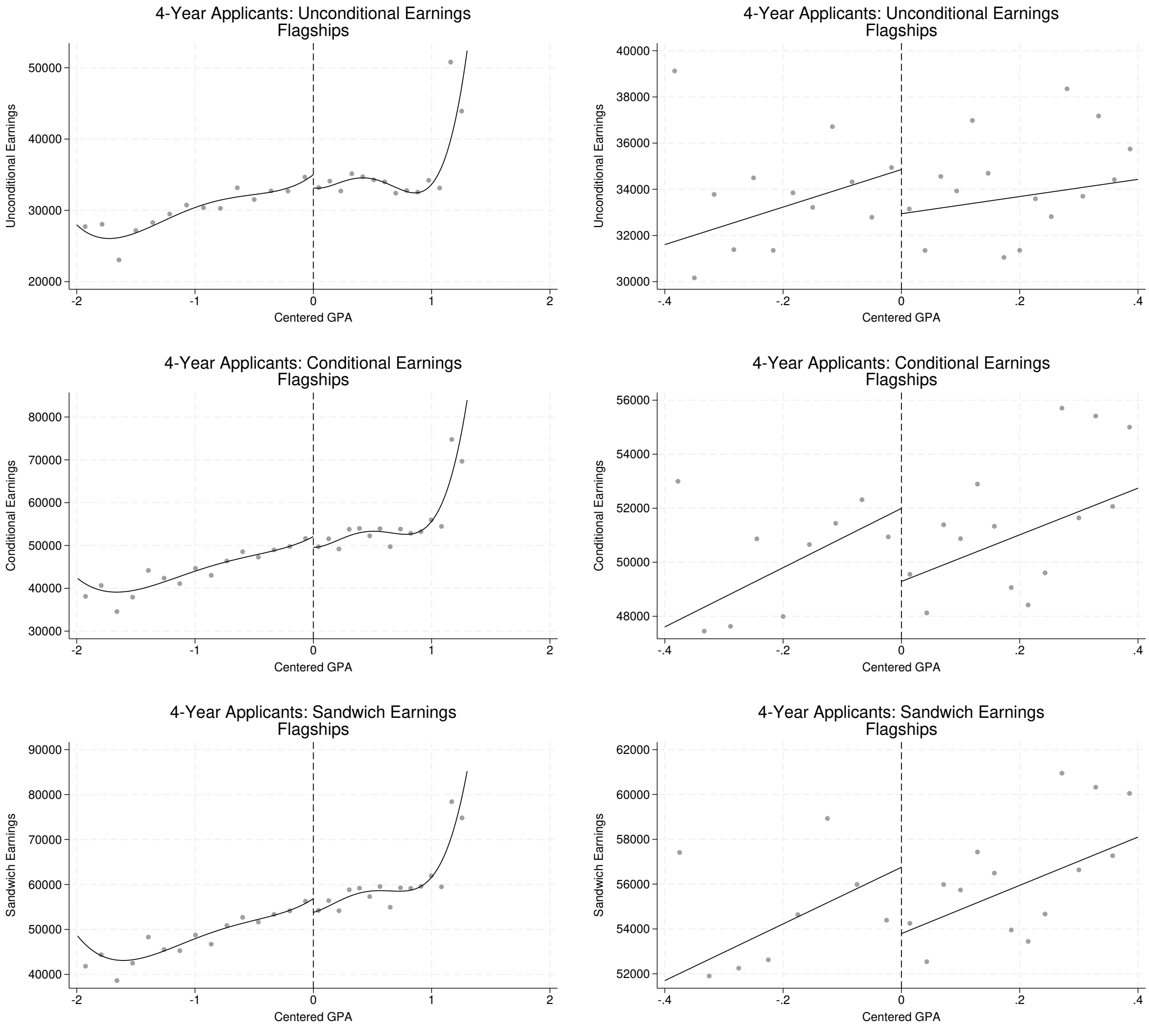
Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff and fits a global fourth-order polynomial on each side. Right panel includes only analysis sample and fits a local linear regression on each side. Sample of four-year transfer applicants. 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).

Figure 7: Annual Earnings, Pooled across All Years, 2-Year Applicants



Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff and fits a global fourth-order polynomial on each side. Right panel includes only analysis sample and fits a local linear regression on each side. Sample of two-year transfer applicants. 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.

Figure 8: Annual Earnings, Pooled across All Years, 4-Year Applicants to Flagships



Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff and fits a global fourth-order polynomial on each side. Right panel includes only analysis sample and fits a local linear regression on each side. Sample of four-year transfer applicants. 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.

Table 1: 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***	0.15***
	(0.006)	(0.008)	(0.012)	(0.014)
F Statistic	562.45	229.49	308.24	110.71
Observations	53,726	53,726	22,003	22,003

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.

Table 2: 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
Panel A: 2-year Applicants						
$\mathbb{1}(GPA_i \geq T_{cy})$	0.0099**	0.019**	0.020**	0.020**	0.019**	0.021**
	(0.0051)	(0.0090)	(0.0096)	(0.0092)	(0.0097)	(0.0100)
$TransferTarget$	0.086**	0.17**	0.17**	0.17**	0.17**	0.18**
	(0.044)	(0.077)	(0.081)	(0.078)	(0.085)	(0.088)
$E[Y_0\|C]$	0.02	0.22	0.35	0.43	0.48	0.47
Obs	53,726	50,545	48,027	44,652	41,979	39,141
Panel B: 4-year Applicants						
$\mathbb{1}(GPA_i \geq T_{cy})$	-0.015	0.025*	0.020	0.022	0.017	0.020
	(0.0096)	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)
$TransferTarget$	-0.10	0.17*	0.14	0.15	0.12	0.14
	(0.066)	(0.099)	(0.091)	(0.090)	(0.090)	(0.092)
$E[Y_0\|C]$	0.15	0.16	0.36	0.45	0.47	0.51
Obs	22,003	20,669	20,230	18,942	17,944	16,996

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 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 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.

Table 3: 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
Panel A: Flagship						
$TransferTarget$	-0.21*	0.15	-0.15	-0.10	-0.048	-0.013
	(0.12)	(0.18)	(0.15)	(0.14)	(0.14)	(0.15)
$E[Y_0\|C]$	0.26	0.30	0.79	0.88	0.86	0.85
Obs	11,040	10,304	10,304	9,752	9,362	8,879
Panel B: Nonflagship						
$TransferTarget$	0.014	0.18	0.36***	0.34**	0.25*	0.26*
	(0.071)	(0.12)	(0.14)	(0.13)	(0.13)	(0.14)
$E[Y_0\|C]$	0.03	0.02	<0.01	0.08	0.12	0.19
Obs	10,963	10,365	9,926	9,190	8,582	8,117

Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). 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). Sample of transfer applicants from four-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 4: Annual Earnings, Pooled across All Years

	Unconditional	Conditional	Sandwich
Panel A: 2-year Applicants			
$\mathbb{1}(GPA_i \geq T_{cy})$	-900**	-825*	-674
	(455)	(444)	(447)
$TransferTarget$	-7,821**	-7,148*	-5,835
	(3,986)	(3,878)	(3,888)
$E[Y_0\|C]$	38,882	50,436	53,228
Obs	690,772	535,877	516,801
Panel B: 4-year Applicants			
$\mathbb{1}(GPA_i \geq T_{cy})$	49	-1,099	-1,231
	(708)	(757)	(790)
$TransferTarget$	324	-6,698	-7,371
	(4,623)	(4,751)	(4,875)
$E[Y_0\|C]$	36,369	51,527	54,657
Obs	299,396	222,492	213,063

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. 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.

Table 5: 4-year Applicants: Annual Earnings, Pooled across All Years, by Flagship Status

	Unconditional	Conditional	Sandwich
Panel A: Flagships			
$TransferTarget$	-6,961	-11,299	-13,885*
	(6,118)	(7,289)	(7,821)
$E[Y_0\|C]$	31,315	44,086	46,041
Obs	156,524	111,855	106,460
Panel B: Nonflagship			
$TransferTarget$	7,853	-2,279	-1,388
	(6,689)	(6,067)	(5,975)
$E[Y_0\|C]$	40,031	58,067	62,754
Obs	142,872	110,637	106,603

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. Both panels are limited to applicants from four-year colleges; 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 6: 2-year Applicants: Annual Earnings, by Number of Years Since Transfer

	Unconditional	Conditional	Sandwich
$\underline{TransferTarget}$			
1-5 years	-2,379	-2,581	-2,794
	(2,502)	(2,473)	(2,550)
$E[Y_0\|C]$	21,587	28,147	31,931
Obs	265,439	215,282	203,343
6-10 years	-8,874*	-13,420***	-12,223**
	(4,725)	(4,536)	(4,541)
$E[Y_0\|C]$	42,846	56,653	59,012
Obs	209,544	164,062	160,101
11-15 years	-9,165	-8,249	-5,824
	(6,868)	(7,042)	(6,928)
$E[Y_0\|C]$	50,548	69,255	70,253
Obs	131,261	96,455	94,533
16+ years	-21,481*	488	6,915
	(12,689)	(12,044)	(12,015)
$E[Y_0\|C]$	67,593	77,096	76,800
Obs	84,528	60,078	58,824

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Observations are at person–year level. Each row limits to observations within given range of years since intended transfer. 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 7: 4-year Applicants to Flagship Colleges: Annual Earnings, by Number of Years Since Transfer

	Unconditional	Conditional	Sandwich
$\underline{TransferTarget}$			
1-5 years	-2,197	-3,392	-4,160
	(3,509)	(3,964)	(4,626)
$E[Y_0\|C]$	15,263	20,877	24,578
Obs	54,464	40,679	36,736
6-10 years	9,373	4,890	-3,103
	(10,023)	(11,058)	(10,918)
$E[Y_0\|C]$	27,342	47,227	55,973
Obs	46,570	33,712	32,938
11-15 years	-13,404	-28,830*	-28,020*
	(12,160)	(14,960)	(16,216)
$E[Y_0\|C]$	65,388	95,467	95,570
Obs	33,049	22,607	22,218
16+ years	-26,676***	-29,243**	-29,203**
	(9,928)	(14,070)	(14,137)
$E[Y_0\|C]$	74,134	108,956	112,856
Obs	22,441	14,857	14,568

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Observations are at person–year level. Each row limits to observations within given range of years since intended transfer. 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 8: Out-Migration

	2-year Applicants No Earnings in Last		4-year Applicants To Flagships No Earnings in Last	
	5 yrs	10 yrs	5 yrs	10 yrs
$TransferTarget$	-0.001	-0.03	-0.05	-0.06
	(0.051)	(0.051)	(0.11)	(0.091)
$E[Y_0\|C]$	0.12	0.10	0.20	0.15
Obs	48,025	35,749	10,304	8,273

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Left panel is 2-year transfer applicants, right panel is 4-year transfer applicants to flagships. Out-migration proxy constructed to be equal to one if the individual has no earnings in the last 5/10 years for which they could be observed in the data, and zero otherwise. Standard errors clustered at the application–college–year level in parentheses.

Table 9: All TX 2-year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals

	Effect of Transfer to Target College Relative to		
	Never Transfer 4y	Transfer Other 4y Now	Transfer 4y Later
$TransferTarget$	-1,732***	396**	-243**
	(126)	(192)	(100)
$E[Y_0]$	44,150	41,749	45,537
Obs	3,034,017	2,799,125	3,259,015

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 1–24 years after intended transfer. Effects of transferring to target college relative to each counterfactual pathway listed at the top of the column, estimated by ordinary least squares with controls for all covariates. Never Transfer 4y = transfer applicant did not enroll in any four-year college in years observed. Transfer Other 4y = transfer applicant transferred to a non-target college in year for which she applied to transfer to target college. Tranfsfer 4y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a four-year college in a later year. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table 10: All UT-Austin 4-year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals

	Effect of Transfer to Target College Relative to				
	Never Transfer	Transfer Other 4y Now	Transfer 4y Later	Transfer 2y Now	Transfer 2y Later
$TransferTarget$	-1,542***	3,318***	4,145***	2,083***	1,117
	(486)	(522)	(627)	(729)	(1,025)
$E[Y_0\|C]$	54,299	44,916	45,890	44,560	42,715
Obs	373,565	331,852	192,183	184,775	133,901

Notes:*** p<0.01, ** p<0.05, * p<0.1. Sample of all 4-year college students in Texas who apply to transfer to the University of Texas at Austin. Outcome is average sandwich earnings pooled across the 1–24 years after intended transfer. Effects of transferring to target college relative to each counterfactual pathway listed at the top of the column, estimated by ordinary least squares with controls for all covariates. Never Transfer = transfer applicant did not tranfser to any college in years observed. Transfer Other 4y = transfer applicant transferred to a non-target college in year for which she applied to transfer to target college. Tranfsfer 4y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a four-year college in a later year. Transfer 2y Now = transfer applicant transferred to a two-year college in year for which she applied to transfer to target college. Tranfsfer 2y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a two-year college in a later year. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table 11: 4-year Applicants to Flagship Colleges: Field of Degree

	General	Science	Engineer	Health	Business	Educ	SocSci
$TransferTarget$	0.13**	0.098	-0.01	-0.12	-0.18**	0.013**	0.20
	(0.061)	(0.13)	(0.069)	(0.076)	(0.079)	(0.0065)	(0.15)
$E[Y_0\|C]$	0.01	0.03	0.10	0.10	0.19	<0.01	0.04
Obs	8,812	8,812	8,812	8,812	8,812	8,812	8,812

	CompSci	Vocational	Art	Human	Other	No Grad
$TransferTarget$	-0.051	-0.036**	-0.032	0.0017	-0.029	0.017
	(0.038)	(0.016)	(0.048)	(0.12)	(0.082)	(0.14)
$E[Y_0\|C]$	0.02	0.03	0.05	0.15	0.15	0.14
Obs	8,812	8,812	8,812	8,812	8,812	8,812

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. 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.

Table 12: 2-year Applicants: Employment, Pooled across All Years, By Gender

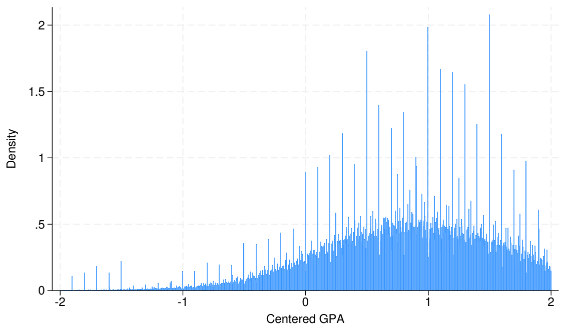
	Any Employment	Continuous Employment	Quarters Worked	Sandwich Quarters Worked
Panel A: Women				
TransferTarget	0.08	0.04	0.32	0.29
	(0.08)	(0.08)	(0.31)	(0.31)
$E[Y_0 \vert C]$	0.70	0.48	2.45	2.19
Obs	328,640	328,640	328,640	328,640
Panel B: Men				
TransferTarget	-0.14**	-0.18**	-0.58**	-0.62**
	(0.07)	(0.07)	(0.27)	(0.28)
$E[Y_0 \vert C]$	0.91	0.68	3.26	2.98
Obs	362,132	362,132	362,132	362,132

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Observations are at person–year level. Sample of 2-year applicants; top panel includes only women and bottom panel only men. 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.

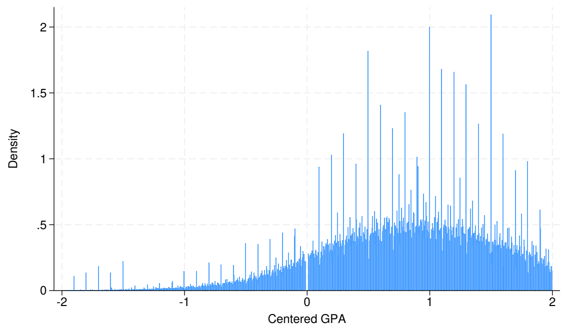
A Supplementary Tables and Figures

Figure A1: Density of Applicant GPAs

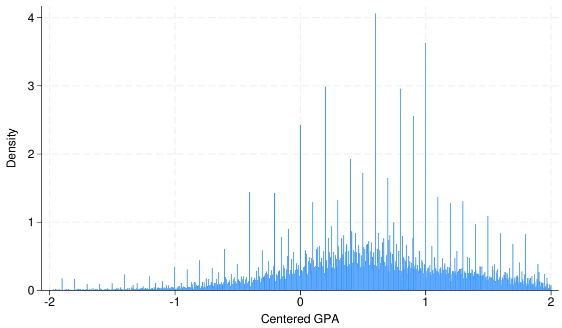
(a) 2-year Applicants, Full Sample



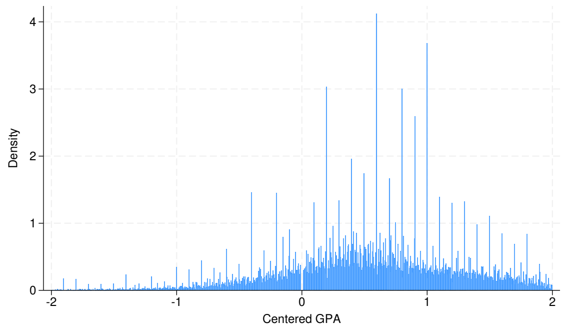
(b) 2-year Applicants, Donut Hole Sample



(c) 4-year Applicants, Full Sample

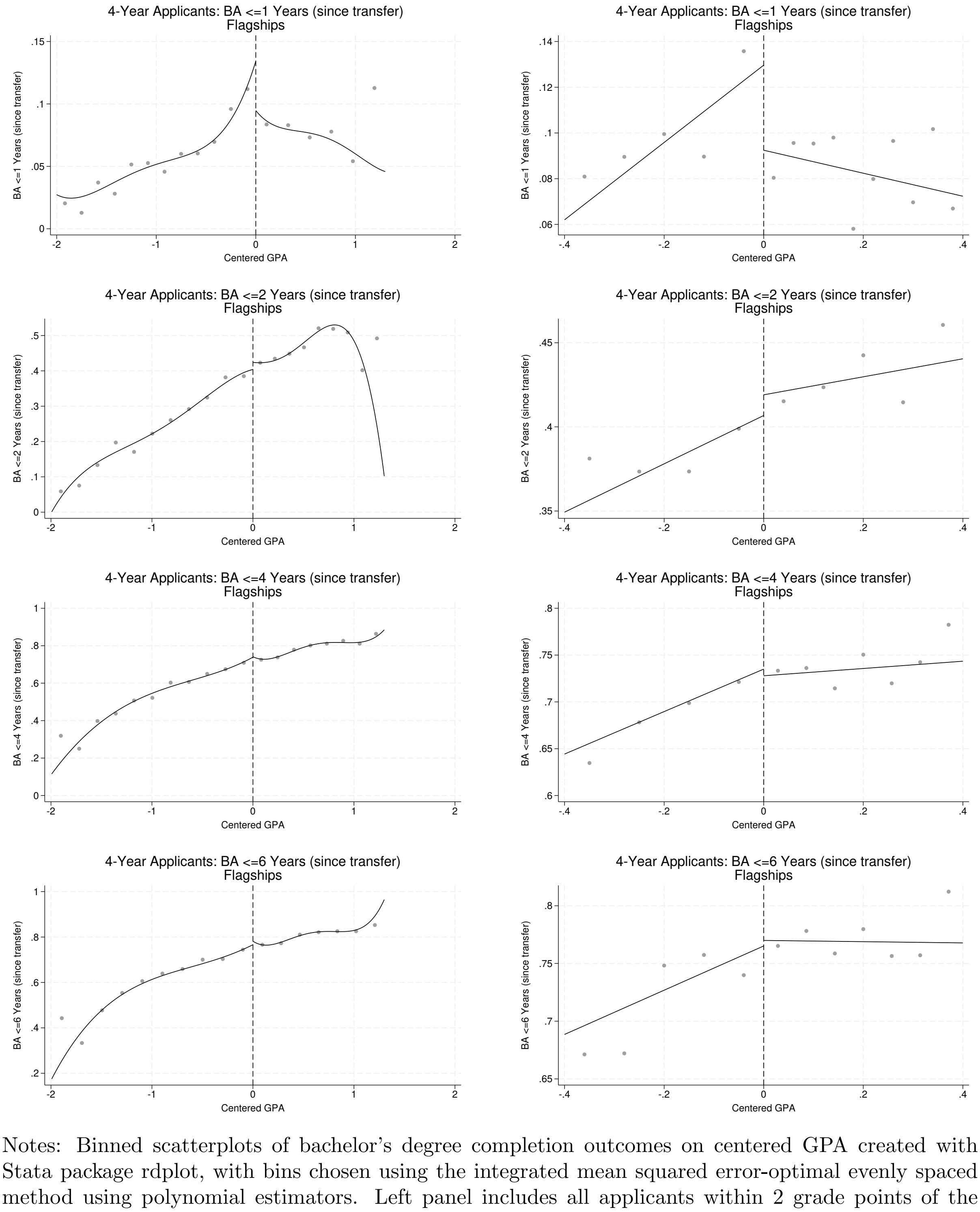


(d) 4-year Applicants, Donut Hole Sample



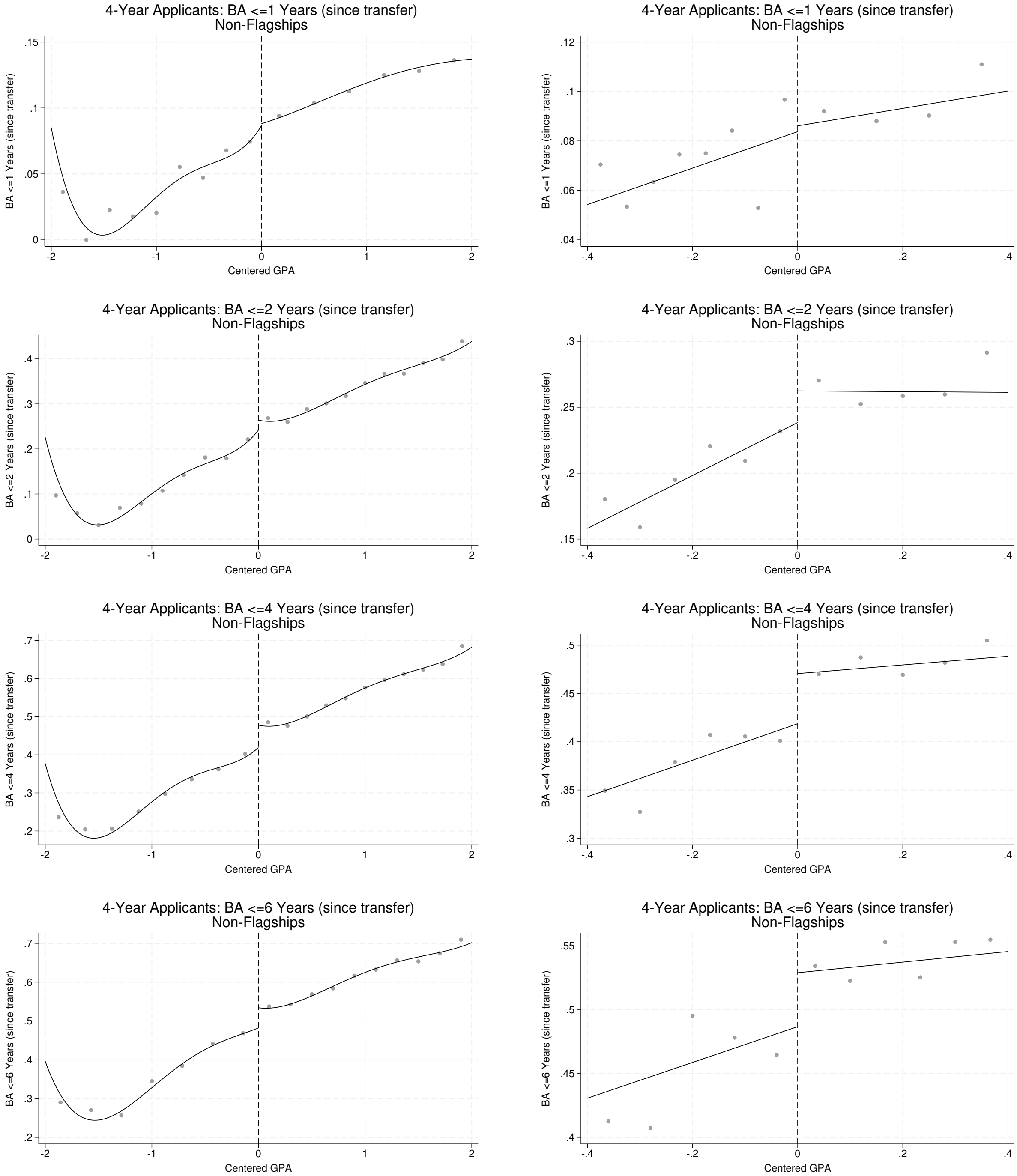
Notes: Histograms of applicants' GPAs after centering on the relevant college–year-specific admissions cutoff. Top row shows two-year applicants, and bottom row shows four-year applicants. Both figures on the right drop all students within 0.01 grade points of the cutoff.

Figure A2: 4-Year Applicants to Flagships: Bachelor's Completion in Years Since Intended Transfer



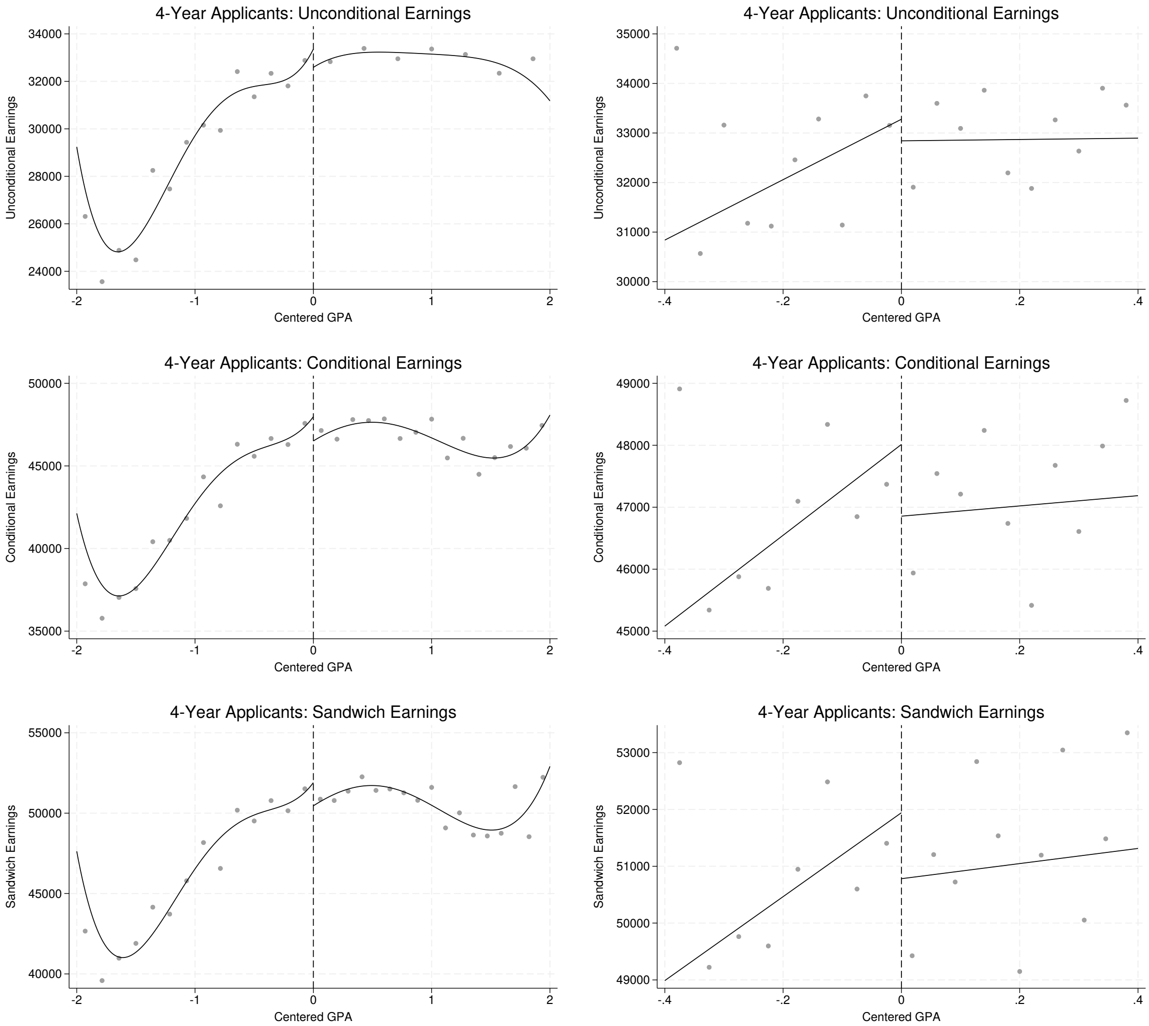
Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff and fits a global fourth-order polynomial on each side. Right panel includes only analysis sample and fits a local linear regression on each side. Sample of four-year transfer applicants to flagships. 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).

Figure A3: 4-Year Applicants to Non-Flagships: Bachelor's Completion in Years Since Intended Transfer



Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff and fits a global fourth-order polynomial on each side. Right panel includes only analysis sample and fits a local linear regression on each side. Sample of four-year transfer applicants to non-flagships. 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).

Figure A4: Annual Earnings, Pooled across All Years, 4-Year Applicants



Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff and fits a global fourth-order polynomial on each side. Right panel includes only analysis sample and fits a local linear regression on each side. Sample of four-year transfer applicants. 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.

Table A1: 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
Non-flagship				
Texas State University	15	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	**94**	**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.

Table A2: 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	15	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	10	1.7	1.5	1.8
Texas A&M Univ-Corpus Christi	6	1.7	1.5	2.0
Texas A&M University-Commerce	6	1.7	1.6	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	11	1.7	1.5	2.0
U. of Texas-Permian Basin	1	1.5	1.5	1.5
U. of Texas-Rio Grande Valley	6	1.6	1.5	1.8
University of Houston	21	1.9	1.8	2.2
University of North Texas	10	1.7	1.5	3.1
West Texas A&M University	2	1.6	1.6	1.6
Total	**238**	**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.

Table A3: 2-Year Applicants: Sensitivity to Alternative Specifications

	Baseline	Bandwidth						SE Clustering		Kernel	
Panel A: BA within 6 years											
$TransferTarget$	0.18**	0.13	0.17	0.19**	0.16*	0.14*	0.13*	0.18*	0.18*	0.18**	0.12
	(0.088)	(0.15)	(0.12)	(0.095)	(0.080)	(0.075)	(0.069)	(0.096)	(0.099)	(0.087)	(0.087)
Obs		18,462	24,685	31,767	45,691	52,623	59,210	39,141	39,141	39,141	39,141
Panel B: Earnings (Conditional)											
$TransferTarget$	-7,148*	-4,600	-7,133	-7,185*	-7,813**	-8,018**	-8,009**	-7,148*	-7,148*	-7,284*	-8,966**
	(3,878)	(6,177)	(4,763)	(4,086)	(3,571)	(3,442)	(3,213)	(3,841)	(4,260)	(3,882)	(3,805)
Obs	535,877	252,756	337,830	434,070	625,711	721,360	812,092	535,877	535,877	535,877	535,877
BW	0.30	0.15	0.20	0.25	0.35	0.40	0.45	0.30	0.30	0.30	0.30
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Epa	Uni
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). Sample of two-year college applicants. Outcome is bachelor's degree completion within 6 year of transfer in the top panel; conditional earnings in the bottom panel. Tri = Triangular kernel. Uni = Uniform kernel. Epan = Epanechnikov kernel. Appl coll = standard errors clustered at application–college–year level, GPA bin = standard errors clustered at GPA distance to the cutoff in 0.01 bin, Send coll = standard errors clustered at sending college-year level.

Table A4: 4-Year Applicants To Flagships: Sensitivity to Alternative Specifications

	Baseline	Bandwidth						SE Clustering		Kernel	
Panel A: BA within 6 years											
$TransferTarget$	-0.013	0.10	0.052	0.016	-0.046	-0.058	-0.073	-0.013	-0.013	-0.033	-0.13
	(0.15)	(0.19)	(0.17)	(0.15)	(0.13)	(0.12)	(0.11)	(0.16)	(0.16)	(0.14)	(0.15)
Obs	8,879	5,515	6,663	7,761	10,115	10,851	11,870	8,879	8,879	8,879	8,879
Panel B: Earnings (Conditional)											
$TransferTarget$	-11,299	-8,394	-8,951	-10,112	-13,044*	-13,155**	-12,940**	-11,299	-11,299	-11,912	-17,966**
	(7,289)	(8,949)	(8,083)	(7,722)	(6,748)	(6,283)	(5,765)	(7,832)	(7,666)	(7,261)	(8,857)
Obs	111,855	5,515	6,663	7,761	10,115	10,851	11,870	111,855	111,855	111,855	111,855
BW	0.40	0.25	0.30	0.35	0.45	0.50	0.55	0.40	0.40	0.40	0.40
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Epa	Uni
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). Sample of four-year college applicants to flagships. Outcome is bachelor's degree completion within 6 year of transfer in the top panel; conditional earnings in the bottom panel. Tri = Triangular 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 A5: Balance Test

	2-year Applicants		4-year Applicants	
	BA Completion	Conditional Earnings	BA Completion	Conditional Earnings
$\mathbb{1}(GPA_i \geq T_{cy})$	0.00026	-203	0.00068	170
	(0.0075)	(269)	(0.015)	(484)
$TransferTarget$	0.0022	-1,756	0.0043	1,064
	(0.065)	(2,283)	(0.093)	(3,025)
p-val	0.97	0.44	0.96	0.73
Obs	53,653	53,653	22,000	22,000

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). Predicted bachelor's completion (within 6 years of high school graduation) and conditional 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. Left panel gives sample of two-year applicants; right panel sample of four-year applicants.

Table A6: Balance Test, by Flagship Status

	4-year Applicants	
	BA Completion	Conditional Earnings
Panel A: Flagship		
$TransferTarget$	0.057	1,766
	(0.045)	(2,169)
p-val	0.21	0.42
Obs	11,038	11,038
Panel B: Non-Flagship		
$TransferTarget$	-0.088	-825
	(0.064)	(2,197)
p-val	0.17	0.71
Obs	10,962	10,962

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). Predicted bachelor's completion (within 6 years of high school graduation) and conditional 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. Sample limited to four-year applicants. Top panel gives applicants to flagships; bottom panel applicants to non-flaghips.

Table A7: 2-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
Panel A: Flagship						
$TransferTarget$	0.0585	0.298*	0.177	0.206	0.239	0.278*
	(0.0921)	(0.165)	(0.155)	(0.130)	(0.159)	(0.163)
$E[Y_0\|C]$	0.03	0.35	0.63	0.72	0.75	0.72
Obs	13,336	12,372	12,061	11,222	10,746	10,039
Panel B: Nonflagship						
$TransferTarget$	0.0883*	0.110	0.168*	0.173*	0.161	0.157
	(0.0493)	(0.0867)	(0.0963)	(0.0951)	(0.102)	(0.105)
$E[Y_0\|C]$	0.02	0.18	0.24	0.30	0.35	0.36
Obs	40,390	38,173	35,966	33,430	31,233	29,102

Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). 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). Sample of transfer applicants from two-year college. Top panel gives estimates for transfer applicants to flagship colleges and bottom panel for applicants to non-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.

Table A8: 2-year Applicants: Annual Earnings, Pooled across All Years, by Flagship Status

	Unconditional	Conditional	Sandwich
Panel A: Flagships			
$TransferTarget$	-18,664**	-14,330*	-13,350
	(8,746)	(8,593)	(8,489)
$E[Y_0\|C]$	49,519	57,666	60,368
Obs	184,341	138,694	133,263
Panel B: Nonflagship			
$TransferTarget$	-2,937	-4,111	-2,632
	(4,252)	(4,092)	(4,146)
$E[Y_0\|C]$	34,791	47,349	50,048
Obs	506,431	397,183	383,538

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. Both panels are limited to applicants from two-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.

Table A9: 2-year Applicants: Annual Earnings, Pooled across All Years, by Gender

	Unconditional	Conditional	Sandwich
Panel A: Women			
$TransferTarget$	1,552	-3,268	-2,990
	(4,917)	(4,890)	(5,008)
$E[Y_0\|C]$	26,068	39,448	42,319
Obs	328,640	255,216	245,538
Panel B: Men			
$TransferTarget$	-14,935**	-8,592	-6,564
	(6,079)	(5,566)	(5,655)
$E[Y_0\|C]$	49,469	57,604	60,323
Obs	362,132	280,661	271,263

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.

Table A10: 2-Year Applicants: Bachelor's Completion in Years since Intended Transfer, by Gender

	BA within X years since intended transfer					
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs
<u>Panel A: Women</u>						
$TransferTarget$	0.12	0.23**	0.21*	0.28**	0.26**	0.26**
	(0.071)	(0.11)	(0.12)	(0.12)	(0.12)	(0.13)
$E[Y_0\|C]$	0.01	0.21	0.38	0.42	0.47	0.50
Obs	25,799	24,197	22,961	21,267	19,929	18,556
<u>Panel B: Men</u>						
$TransferTarget$	0.073	0.12	0.14	0.068	0.082	0.11
	(-0.052)	(-0.092)	(-0.099)	(-0.095)	(-0.11)	(-0.11)
$E[Y_0\|C]$	0.04	0.22	0.32	0.42	0.48	0.43
Obs	27,927	26,348	25,066	23,385	22,050	20,585

Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). 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). Sample of transfer applicants from two-year college. Top panel gives estimates for women and bottom panel 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 A11: Annual Earnings, Pooled Across All Years, Individuals Unlikely To Migrate

	Unconditional	Conditional	Sandwich
Panel A: 2-Year Applicants			
$TransferTarget$	-8,414**	-7,642*	-6,116
	(4,097)	(4,022)	(3,987)
$E[Y_0 \| C]$	39,798	50,968	53,428
Obs	652,670	505,824	488,297
Panel B: 4-Year Applicants to Flagships			
$TransferTarget$	-7,317	-11,173	-13,427
	(6,579)	(8,206)	(8,678)
$E[Y_0 \| C]$	43,721	62,852	67,746
Obs	144,028	102,901	98,213

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. 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 A12: Summary Statistics

				2-year to 4-Year Transfer Applicants			
	All HS students	All college students	2-year college students	Applicants to target college	Within the BW	Marginal at cutoff	Compliers
Male	0.51	0.46	0.47	0.46	0.52	0.58	0.50
FRPL	0.40	0.31	0.36	0.23	0.20	0.18	0.24
Nat. American	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Asian	0.04	0.04	0.03	0.06	0.06	0.06	0.05
Afr. American	0.13	0.13	0.13	0.09	0.11	0.11	0.14
Hispanic	0.42	0.36	0.41	0.31	0.28	0.22	0.29
White	0.39	0.46	0.42	0.54	0.54	0.62	0.52
Two or More Races	0.01	0.01	0.01	0.01	0.01	0.00	0.00
Math HS test score (std.)	0.00	0.21	-0.06	0.27	0.19	0.31	0.18
Reading HS test score (std.)	0.00	0.21	0.02	0.27	0.22	0.31	0.19
Cumulative GPA		2.3	2.0	2.8	2.2	2.2	2.0
Cumulative Credits		41	28	51	54	49	64
N	9,643,530	4,155,734	2,496,742	354,464	53,725		

Notes: All HS students includes all students who attended a public high school in Texas between 1993 and 2023, all college students includes those who enrolled in a college in Texas for at least one-semester, and 2-year college students enrolled in a public two-year college for at least one semester. 2-year to 4-Year Transfer Applicants applied to transfer to a four-year target college while they were enrolled at a two-year college. Within the BW limits the sample to those that were within 0.3 grade points of the target college's GPA cutoff. The means of marginal applicants are estimated as the intercept in the reduced form RD specification, equation (3). The means of compliers are estimated from the main IV specification, equation (4), using the method of Abadie (2002).

Table A13: Summary Statistics, Continued

		4-year to 4-Year Transfer Applicants					4-year to Flagship Transfer Applicants			
	4-year college students	Applicants to target college	Within the BW	Marginal at cutoff	Compliers	UT-Austin college students	Applicants to target college	Within the BW	Marginal at cutoff	Compliers
Male	0.45	0.46	0.51	0.52	0.57	0.46	0.51	0.51	0.50	0.51
FRPL	0.24	0.18	0.17	0.19	0.13	0.13	0.12	0.11	0.15	0.07
Nat. American	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
Asian	0.07	0.11	0.12	0.15	0.06	0.19	0.18	0.17	0.22	0.08
Afr. American	0.12	0.11	0.12	0.18	0.05	0.05	0.04	0.04	0.04	0.05
Hispanic	0.29	0.25	0.24	0.23	0.32	0.19	0.23	0.24	0.25	0.27
White	0.51	0.52	0.51	0.43	0.57	0.56	0.54	0.54	0.48	0.58
Two or More Races	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.01	0.02
Math test score	0.59	0.55	0.54	0.43	0.66	1.1	0.74	0.72	0.65	0.76
Reading test score	0.51	0.45	0.43	0.36	0.37	0.81	0.54	0.53	0.38	0.47
Cumulative GPA	2.6	2.9	2.7	2.6	2.7	3.1	3.3	3.3	3.2	3.3
Cumulative Credits	62	43	44	50	39	60	27	29	35	25
N	1,658,992	73,825	22,003			158,374	24,424	11,038		

Notes: All 4-year college students includes all Texas public high school students who enroll in a public four-year college for at least one semester; All UT-Austin college students includes those who enroll at University of Texas at Austin. 4-Year to 4-Year Transfer Applicants applied to transfer to a four-year target college while they were enrolled at a four-year college; 4-Year to Flagship Transfer applicants limits to those who applied to a flagship. Within the BW limits the sample to those that were within 0.4 grade points of the target college's GPA cutoff. The means of marginal applicants are estimated as the intercept in the reduced form RD specification, equation (3). The means of compliers are estimated from the main IV specification, equation (4), using the method of Abadie (2002).

Table A14: 2-year Applicants: Fraction of Compliers in Each Counterfactual Category

	Never Transfer 4y	Transfer Other 4y Now	Transfer 4y Later
All 2-year	0.32	0.19	0.50
Male	0.43	0.25	0.34
Female	0.19	0.12	0.68

Notes: Estimated fraction of compliers who fall into each mutually exclusive counterfactual pathway following the method of Abadie (2002). Sample of all two-year applicants. Second row limits to men; third to women. Never Transfer 4y = transfer applicant did not enroll in any four-year college in years observed. Transfer Other 4y = transfer applicant transferred to a non-target college in year for which she applied to transfer to target college. Tranfsfer 4y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a four-year college in a later year.

Table A15: 4-year Applicants: Fraction of Compliers in Each Counterfactual Category

	Never Transfer	Transfer Other 4y Now	Transfer 4y Later	Transfer 2y Now	Transfer 2y Later
All 4-year	0.34	0.075	0.17	0.34	0.076
Flagships	0.55	<0.01	0.21	0.26	0.038
Non-flagships	0.09	0.18	0.16	0.45	0.12

Notes: Estimated fraction of compliers who fall into each mutually exclusive counterfactual outcome following the method of Abadie (2002). Sample of all four-year applicants. Second row limits to applicants to flagships; third row to non-flagships. Never Transfer = transfer applicant did not tranfser to any college in years observed. Transfer Other 4y = transfer applicant transferred to a non-target college in year for which she applied to transfer to target college. Tranfsfer 4y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a four-year college in a later year. Transfer 2y Now = transfer applicant transferred to a two-year college in year for which she applied to transfer to target college. Tranfsfer 2y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a two-year college in a later year.

Table A16: All TX 2-year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals

	Effect of Transfer to Target College Relative to		
	Never Transfer 4y	Transfer Other 4y Now	Transfer 4y Later
$TransferTarget$			
1-5 years	-4,397***	-156	-328***
	(94)	(121)	(70)
$E[Y_0]$	33,685	28,652	29,621
Obs	1,275,652	1,148,387	1,320,188
6-10 years	-780***	382*	-62
	(160)	(225)	(116)
$E[Y_0]$	49,435	45,311	48,060
Obs	902,940	842,418	985,228
11-15 years	1,348***	1,006***	-237
	(252)	(372)	(184)
$E[Y_0]$	58,483	55,164	59,697
Obs	524,513	494,914	583,101
16+ years	2,718***	2,651***	-507
	(434)	(576)	(310)
$E[Y_0]$	66,558	62,436	69,915
Obs	330,764	313,263	370,352

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 annual sandwich earnings estimated in 5-year bins since 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. Never Transfer 4y = transfer applicant did not enroll in any four-year college in years observed. Transfer Other 4y = transfer applicant transferred to a non-target college in year for which she applied to transfer to target college. Tranfsfer 4y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a four-year college in a later year. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table A17: All UT-Austin 4-year Applicants: OLS Estimates of Transfer to Target College on Sandwich Earnings, Relative to Counterfactuals

	Effect of Transfer to Target College Relative to				
	Never Transfer	Transfer Other 4y Now	Transfer 4y Later	Transfer 2y Now	Transfer 2y Later
$TransferTarget$					
1-5 years	-2,205***	-738	2,715***	1,377**	2,682***
	(403)	(964)	(522)	(626)	(961)
$E[Y_0\|C]$	31,356	28,784	22,741	23,105	23,058
Obs	50,879	33,743	36,945	35,507	33,760
6-10 years	-3,669***	2,038	4,618***	3,936***	6,644***
	(803)	(1,991)	(1,214)	(1,402)	(2,504)
$E[Y_0\|C]$	64,139	55,170	48,571	47,987	45,440
Obs	42,212	28,840	31,818	30,377	28,925
11-15 years	-2,463*	1,794	8,676***	3,204	7,114*
	(1,349)	(3,851)	(2,104)	(2,437)	(3,996)
$E[Y_0\|C]$	82,621	72,885	63,771	66,056	64,934
Obs	25,590	17,392	19,266	18,469	17,386
16+ years	-1,149	404	4,224	-2,242	-378
	(2,274)	(6,480)	(3,683)	(4,171)	(5,875)
$E[Y_0\|C]$	92,416	83,211	76,990	78,070	85,228
Obs	13,229	8,166	9,549	8,938	8,177

Notes:*** p<0.01, ** p<0.05, * p<0.1. Sample of all 4-year college students in Texas who apply to transfer to UT-Austin. Outcome is average annual sandwich earnings estimated in 5-year bins since 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. Never Transfer = transfer applicant did not tranfser to any college in years observed. Transfer Other 4y = transfer applicant transferred to a non-target college in year for which she applied to transfer to target college. Tranfsfer 4y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a four-year college in a later year. Transfer 2y Now = transfer applicant transferred to a two-year college in year for which she applied to transfer to target college. Tranfsfer 2y Later = transfer applicant does not transfer in the year for which she applied to transfer to target college, but transfers to a two-year college in a later year.

Table A18: 2-year Applicants: Annual Earnings, Pooled across All Years, by Amount of Credits

	Unconditional	Conditional	Sandwich
Panel A: Less Credits			
$TransferTarget$	-15,380***	-14,169**	-13,474**
	(5,811)	(5,938)	(6,080)
$E[Y_0\|C]$	41,016	54,289	57,354
Obs	349,194	267,147	256,680
Panel B: More Credits			
$TransferTarget$	-2,211	-2,812	-1,101
	(5,256)	(4,726)	(4,752)
$E[Y_0\|C]$	38,691	49,395	52,078
Obs	341,578	268,730	260,121

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 A19: 4-year Applicants to Flagship Colleges: Predicted Annual Earnings Based on Field of Degree

	Predicted Unconditional	Predicted Conditional	Predicted Sandwich
$TransferTarget$	-3,832**	-3,550*	-3,415
	(1,521)	(2,017)	(2,170)
$E[Y_0 \mid C]$	13,340	10,837	10,964
Obs	8,203	8,203	8,203

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Sample includes all four-year applicants to flagship colleges who are 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.

Table A20: 2-year Applicants: Field of Degree

	General	Science	Engineer	Health	Business	Educ	SocSci
$TransferTarget$	0.052	0.012	-0.029	0.0010	0.078	0.0033	0.0012
	(0.05)	(0.03)	(0.03)	(0.03)	(0.06)	(0.01)	(0.05)
$E[Y_0 \mid C]$	0.04	<0.01	0.06	0.03	0.08	<0.01	0.09
Obs	38,701	38,701	38,701	38,701	38,701	38,701	38,701

	CompSci	Vocational	Art	Human	Other	No Grad
$TransferTarget$	0.028*	-0.023	0.0093	0.069	0.0091	-0.21**
	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.09)
$E[Y_0 \mid C]$	<0.01	0.02	0.01	0.03	0.07	0.60
Obs	38,701	38,701	38,701	38,701	38,701	38,701

Notes:*** p<0.01, ** p<0.05, * p<0.1. Sample of 2-year transfer applicants. 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.

Table A21: 4-year Applicants to Flagships: Employment, Pooled across All Years

	Any Employment	Continuous Employment	Quarters Worked	Sandwich Quarters Worked
Flagship				
$TransferTarget$	-0.049	0.0089	-0.12	-0.066
	(0.069)	(0.062)	(0.26)	(0.25)
$E[Y_0\|C]$	0.79	0.52	2.70	2.39
Obs	156,524	156,524	156,524	156,524

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Sample of 4-year applicants to flagships. 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 A22: 2-year Male Applicants: Experience

	Years Worked	Quarters Worked	Sandwich Quarters Worked
Panel A: 8 Years after Intended Transfer			
$TransferTarget$	-1.035*	-4.81**	-5.32**
	(0.57)	(2.44)	(2.64)
$E[Y_0\|C]$	7.17	26.19	23.77
Obs	22,050	22,050	22,050
Panel B: 13 Years after Intended Transfer			
$TransferTarget$	-1.912*	-8.171*	-9.404*
	(1.056)	(4.542)	(4.857)
$E[Y_0\|C]$	11.88	43.74	40.67
Obs	13,592	13,592	13,592

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Sample of male applicants from 2-year colleges. 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. Top panel gives experience variables measued 8 years after intended transfer; bottom panel 13 years 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.

Table A23: 2-year Applicants: College Enrollment, by Semesters Since Intended Transfer

	Number of Semesters			
	1	2	3	4
Enrollment				
4-year college	0.81***	0.53***	0.24***	0.21***
	(0.050)	(0.063)	(0.069)	(0.073)
$E[Y_0\|C]$	0.19	0.35	0.44	0.48
2-year college	-0.58***	-0.35***	-0.11**	-0.076
	(0.057)	(0.057)	(0.051)	(0.047)
$E[Y_0\|C]$	0.58	0.38	0.20	0.18
Any college	0.23***	0.17***	0.11*	0.14**
	(0.047)	(0.057)	(0.065)	(0.067)
$E[Y_0\|C]$	0.77	0.74	0.65	0.67
Obs	53,726	53,726	53,726	53,726

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Sample of 2-year applicants. The outcomes are enrollment in a 4-year college, in a two-year college or in any college, 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.

Table A24: 2-year Applicants: Semesters Enrolled and Credits Attempted

	Total Semesters Enrolled	Total Credits Attempted
$TransferTarget$	0.39	10.20*
	(0.61)	(5.90)
$E[Y_0\|C]$	11.50	123.00
Obs	39,103	39,103

Notes: *** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Sample of 2-year applicants. Outcomes are total number of semesters enrolled and total number of credits attempted 6 years 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.

Table A25: Predicted Outcomes of Average College Peers

	Average College Peer at Time of Intended Transfer			**Average College Peer of Last College Attended**		
	Average HS Math Test Score	Average Predicted BA Completion	Average Predicted Earnings	Average HS Math Test Score	Average Predicted BA Completion	Average Predicted Earnings
Panel A: 2-Year Applicants						
$TransferTarget$	0.48***	0.37***	10,186***	0.21***	0.14***	5,023***
	(0.032)	(0.026)	(783)	(0.044)	(0.036)	(990)
$E[Y_0\|C]$	0.23	0.24	33,795	0.35	0.33	35,666
Obs	46,648	46,648	46,648	53,721	53,721	53,721
Panel B: 4-Year Applicants to Flagship						
$TransferTarget$	0.60***	0.37***	14,617***	0.29***	0.14**	6,260***
	(0.052)	(0.033)	(1,389)	(0.090)	(0.071)	(2,303)
$E[Y_0\|C]$	0.44	0.41	37,017	0.53	0.44	38,464
Obs	10,165	10,165	10,165	11,040	11,040	11,040

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Top panel is 2-year transfer applicants; bottom is 4-year transfer applicants to flagships. Left panel gives average college peer characteristics of enrolled college in intended transfer semester; right panel gives average college peer characteristics of last enrolled college. Standard errors clustered at the application–college–year level in parentheses.

Table A26: 2-year Applicants: Relative Rank from Final GPA, by Number of Semesters Since Intended Transfer

	Number of Semesters			
	1	2	3	4
$TransferTarget$	-0.24***	-0.10*	-0.025	0.0027
	(0.092)	(0.062)	(0.082)	(0.068)
$E[Y_0 \mid C]$	0.61	0.52	0.38	0.44
Obs	11,580	11,258	9,968	9,617

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Sample of 2-year applicants. The outcome 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.

Table A27: 2-year Applicants: Distance and Travel Time from High School to College

	Within 30 min	Within 60 min
$TransferTarget$	-0.13*	-0.05
	(0.068)	(0.069)
$E[Y_0 \mid C]$	0.42	0.62
Obs	53,726	53,726

Notes:*** p<0.01, ** p<0.05, * p<0.1. IV estimates from equation (4). Sample of 2-year applicants. Outcome is an indicator variable for the last college of enrollment being within 30/60 minutes driving time of students' high school. $E[Y_0|C]$ gives the untreated mean value of the dependent variable for compliers. Standard errors clustered at the application–college–year level in parentheses.

Table A28: 2-Year Applicants: Predicted Earnings by Industry, Pooled Across Years

	Predicted Unconditional	Predicted Conditional	Predicted Sandwich
$TransferTarget$	-636	-765	-706
	(1,262)	(1,361)	(1,387)
Obs	415,162	415,162	415,162

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 illustrates how to estimate the fraction of untreated compliers who will follow each counterfactual pathway using the method of Abadie (2002). 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 $C = \mathbb{1}(\text{Complier})$, $AT = \mathbb{1}(\text{Always-taker})$, and $NT = \mathbb{1}(\text{Never-taker})$. 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 the size of the discontinuity in equation (8) is given by,

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

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