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 affects 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 To do so, I leverage applications and admissions data to uncover unpublished GPA cutoffs used for transfer student admissions at each institution and then use these cutoffs as an instrument for transfer. In contrast to past work focused on freshmen 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. I investigate several mechanisms behind this result and find evidence for academic "mismatch" among two-year to four-year transfers, and substitution out of high-paying majors into lower-paying majors for four-year to flagship transfers.

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). The vast majority of research into the economic returns to higher education focuses only on a student's first or last institution, thereby failing to characterize a large population: students who transfer between institutions.[2]

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

This paper uses administrative data from Texas and a regression discontinuity (RD) design to study the causal effect 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 for academic "mismatch" among two-year to four-year transfers, and substitution out of high-paying majors into lower-paying majors for four-year to flagship transfers.

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

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[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. See also Rothstein and Schanzenbach (2022) for a discussion a researchers moving from using the term "quality" to "resources in the K-12 context.

[2]Notable exceptions include Andrews et al. (2014) and Monaghan and Attewell (2015). I review these and other papers in the transfer literature below.

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 four-year universities. I show that my cutoff estimation uncovers clear increases in the probability of transfer admission at certain GPA cutoffs and, intuitively, that these GPA cutoffs increase with university selectivity. I then use the detected cutoffs in an RD design to estimate the effect of a student's being narrowly granted transfer admission relative to being narrowly denied transfer admission across a variety of colleges.

My results show that among two-year college students who apply to transfer to 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 these narrowly accepted two-year to four-year transfer students. While the confidence intervals are wide, the point estimates for the average annual earnings impacts are around -7,000 dollars and persist over time since transfer: they are negative and statistically significant six to ten years after intended transfer, and the point estimates remain large and negative (although not statistically significant) 11-15 years later.

To be clear, I estimate a local average treatment effect (LATE) 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 illustrating several counterfactual pathways taken by narrowly denied students. Some students who are denied transfer admission never transfer, but many 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 transfer relative to never transferring and the effect of transferring earlier versus later) with only one-third of compliers below the cutoff falling into the "never transfer to a four-year college" category. While I am unable to separately estimate treatment effects by counterfactual pathway using the regression discontinuity, I use a complementary analysis using a "selection on observables" identification strategy to provide suggestive evidence that the effects of transferring are null to negative even when limiting the comparison group to those who never transfer to a four-year college.

It is useful for interpretation of the LATE to note that GPA cutoffs I estimate are relatively low: a GPA of 1.9 on average. Marginal transfer students are academically weaker

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[3]I implement several tests to check the validity of this assumption in subsection 3.3 and find that students above and below the cutoff appear similar.

than the average student of the college that they transfer to. Their statewide high school standardized test scores in math and English Language Arts (ELA) fall at the 28th and 24th percentile, respectively, in the distribution of scores of students who began as freshmen of the college to which they transfer. This raises the question of whether transfer students may suffer from being academically "mismatched." I create a binary proxy of "mismatch" based on whether marginal transfer students test scores are in the bottom quartile of freshmen students at their four-year college and use the RD to show that the negative impacts of transfer on earnings are concentrated among the more "mismatched" sample.

This result stands in contrast to most recent empirical work testing the "mismatch hypothesis" (Black et al., 2021; Bleemer, 2022, 2024; Barahona et al., 2023) but aligns with some of the results in Carlana et al. (2024) who study an affirmative action policy in Chile and find negative impacts for marginally eligible men.[4] [5] However, I note two differences in my study's setting from the past literature. First, prior studies focus on freshmen college admissions. We may expect the potential negative effects of academic under-preparation to be magnified when studying transfer students, since they may have been under prepared not only by their high school education but also their initial college. Second, while the students in my sample are academically marginal, they are no more disadvantaged in terms of their race-ethnicity or socioeconomic status than the average four-year college student, implying that they are not the same students who would be targeted by race or SES-based affirmative action policies.[6]

Moving to four-year to four-year transfers, I find that among students who apply to transfer to a flagship college, those who are marginally admitted do not have any change in their long-term bachelor's degree completion. Point estimates on their earnings are large and negative, though not statistically significant in most specifications. I explore mechanisms and find that compliers in this group substitute out of higher-paying and into lower-paying majors. In particular, they are less likely to major in business or health and are more likely to complete degrees in general liberal arts or social sciences. This is likely a result of admissions restrictions (e.g., the business school is more selective than the college of liberal arts) or issues in how courses are transferred (e.g., a course may transfer for general credit but not satisfy major requirements). 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.

This paper contributes to the transfer literature by providing a credible causal estimate of transfer on educational and labor market outcomes for marginally admitted students. Since

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[4]Carlana et al. (2024) find positive effects for marginally eligible women, as well as the average (i.e., not marginal) targeted student of both genders. My results (of marginal students) also suggest that negative effects are concentrated among men.

[5]My results also align with those of Arcidiacono et al. (2012); Arcidiacono and Lovenheim (2016) who find that less prepared minority students admitted through affirmative action are less likely to major in STEM, although they do not directly look at labor market outcomes. See also the topic of "mismatch" in law school (Sander and Stuart, 2012; Arcidiacono et al., 2016).

[6]Further, I do not find evidence of meaningful heterogeneous impacts by race-ethnicity or socioeconomic status.

it is difficult to find exogenous variation in transfer, most quantitative work on transfer is either descriptive, assumes selection on observables, or imposes structural assumptions on the selection process (Hilmer, 2000; Light and Strayer, 2004; Andrews et al., 2014; Monaghan and Attewell, 2015; Jenkins and Fink, 2016; Xu et al., 2018; Zhu, 2020; Bleemer, 2024; Velasco et al., 2024a,b). The most closely related to this paper are Andrews et al. (2014) who provide descriptive evidence using the same administrative data from Texas and Andrews and Thompson (2017) who also use a regression discontinuity design to estimate the effect of transferring to the University of Texas - Austin (UT–Austin) for a more narrow population than I study.[7] Some studies use quasi-experimental policy variation to estimate the effects of various policies on transfer and degree completion but do not estimate impacts on labor market outcomes (Baker, 2016; Boatman and Soliz, 2018; Shaat, 2020; Bloem, 2022; Baker et al., 2023; Shi, 2023). 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).[8]

My findings also complement the transfer literature that uses qualitative methods (e.g., interviews or focus groups) to 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; Ellis, 2013; Elliott and Lakin, 2021; Wang, 2021; Schudde and Jabbar, 2024). 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 (Bailey et al., 2017; Schudde et al., 2021b; Sutcliffe et al., 2025). Even within a university, each department sets how credits are transferred and whether they satisfy major requirements (Schudde et al., 2021a). 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 freshmen students may devote fewer resources to transfer students, because transfer students are not usually counted in graduation rates or other performance metrics that go into accountability measures and college rankings (Handel and Williams, 2012; Jenkins and Fink, 2016).[9]

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[7]Specifically, they study 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 and credit requirements. While they have some labor market data, they do not directly look at earnings as an outcome due to insufficient cohorts with enough years of longer-term earnings. Andrews (2016) is a closely related short paper considering the effects of CAP on major choice.

[8]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.

[9]My own conversations with administrators at 4-year universities in Texas revealed that attention and resources are much more focused on freshmen students than transfer students (e.g., the university has a goal of a 70 percent on-time graduation rate, but the measurement of on-time 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.

Finally, my work relates to the literature on the effect of access to colleges of varying resource levels (often referred to as "quality", see footnote 1), especially those that use regression discontinuity designs (Hoekstra, 2009; Cohodes and Goodman, 2014; Zimmerman, 2014; Goodman et al., 2017; Smith et al., 2020; Kozakowski, 2023; Mountjoy, 2025). I contribute to this literature by estimating the effect of *transferring* to a well-resourced college, since prior work has only considered the quality or 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 and Lovenheim, 2016; Aucejo et al., 2022; Bleemer, 2022), which has not previously considered transfer students.

The rest of this paper proceeds as follows: section 2 describes the Texas administrative data and section 3 details the estimation of admissions cutoffs, regression discontinuity design, and identification. In section 4, I present the main RD results for two-year to four-year transfers, elaborate on how to interpret the local average treatment effects (subsection 4.4), explore heterogeneity and mechanisms (subsection 4.5), and show that results are robust to a wide range of alternative specifications (subsection 4.6). In section 5, I repeat the main RD estimates, interpretation, mechanisms, and robustness for four-year to four-year transfer, with a focus on those applying to transfer to a flagship university. Finally, section 6 discusses policy implications and concludes.

2 Data

I use administrative data from the Education Research Center (ERC) at the University of Texas at 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.[10] In addition to including detailed student-level data on background characteristics (e.g., gender, race, economic disadvantage status[11], exact high school of attendance, 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, which I use to empirically detect GPA cutoffs used in transfer admissions (described in more detail in subsection 3.1). 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.[12]

Texas has two flagship institutions: the University of Texas at Austin and Texas A&M

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

[11]Economic disadvantage is primarily determined by eligibility for free or reduced-price lunch in high school

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

University. By almost any measure of college quality/resources used in the literature, these are the two top public universities in the state.[13] 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 non-flagship university.[14] [15]

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.[16]

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

Since the earnings data come from Texas administrative records, they do not capture earnings for individuals working in another state or self-employed individuals.[17] 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 measurement error since they are not all true zeros, so the second measure ("conditional" earnings) averages over only nonzero quarters. 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.[18] For all measures,

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[13]Using the college quality 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).

[14]For four-year to four-year transfer applicants, 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.

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

[16]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.

[17]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.

[18]Here, "positive" earnings are defined as earnings above an annual earnings floor of $3,250 in 2011 dollars.

I convert earnings to real 2012 dollars using the personal consumption expenditures price index and winsorize each quarter of earnings at the 99th percentile (among the full distribution of earnings of Texas workers). I also implement robustness checks where I proxy for out-migration following Grogger (2012) and find no evidence that my main effects are driven by selection bias due to differential migration between transfer and non-transfer students.

3 Empirical Strategy

In this section, I focus on the econometric strategy used to estimate causal effects. For a conceptual framework that describes the channels through which we may expect transfer to affect students' outcomes, see Appendix A.

3.1 Detection of Admissions Cutoffs

As noted in Altmejd et al. (2021), many colleges use minimum SAT cutoffs in admissions decisions without making these cutoffs publicly known. Similarly, institutions use college GPA cutoffs in their admissions decisions for transfer students. Some colleges in Texas publicly post GPA cutoffs on their transfer admission web pages, either as minimum GPA requirements to be admitted for transfer or as "guaranteed" admission standards above which students will be automatically admitted. If I were to use these posted cutoffs as an instrument for transfer, we might worry about selection into transfer application or GPA manipulation around the cutoffs. However, the application and admissions data reveal that large discontinuities in admission rarely exist at the colleges' publicly posted cutoffs, and often exist at much lower cutoffs.[19] Thus, rather than use publicly posted cutoffs, I estimate them from the admissions data. In addition to increasing statistical power by allowing me to identify cutoffs even for colleges that don't post them, this has the advantage of avoiding problems of manipulation since the cutoffs are unknown to students.[20]

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.[21] 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.

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).

[19]Sometimes the admissions data even contradict what the college has publicly posted. For example, a college may list a minimum required GPA of 2.5 but admit many transfer applicants with GPAs below 2.5.

[20]One might worry that the cutoffs could be informally known through guidance counselors, etc. If that were the case, we would expect to see an increased density of applicants right above the cutoffs and/or students with systematically different observable characteristics right above the cutoff. I do not find evidence of either in subsection 3.3.

[21]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. 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.

These cutoffs may vary across years within a given college, so I search for thresholds separately in each institution and year from 1999 to 2019. For a given institution and year, I also separately search by whether the student applies to transfer from a two-year or four-year institution (i.e., sector) since these transfer processes are different and admissions officers may treat GPAs from two-year college differently from those from four-year universities. Since I do not know which colleges use admissions thresholds and I want to limit false positives, I search for cutoffs in each college–year–sector combination only if it contains at least 500 transfer applications. Among this set, separately for every potential GPA threshold from 1.5 to 3.8, I estimate the following local linear regression with a bandwidth of 1.0 and a uniform kernel:

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

where $Accept_{icts}$ is an indicator for application i to college c from a student in sector s in year t being accepted and T_{cts} is a potential threshold used in admissions decisions. β_1 estimates the magnitude of any potential discontinuity in application acceptance at the given threshold T_{cts}. I want to use T_{cts} as a threshold only if there is strong evidence of a jump in admissions at that point, so I keep only thresholds for which the p-value of the test that β_1 is equal to zero is less than 0.01. If there is more than one threshold with a p-value less than 0.01, I take the one with the maximum t-statistic.[22]

I identify 23 colleges that use admissions cutoffs for two-year students and eight colleges that use admissions cutoffs for four-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 two-year and four-year colleges, respectively.[23] As expected, cutoffs are much higher for flagship universities than non-flagships. For some colleges, I do not identify a cutoff for every year, which we might observe if the cutoff was not binding in some years. It's also possible that there are some true cutoffs that I do not detect. This is not a problem for my identification strategy; excluding those cutoffs will weaken the first stage

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[22]This procedure is similar to the ones used to identify discontinuities in Hoekstra (2009), Andrews et al. (2017), Altmejd et al. (2021), Brunner et al. (2021), and Mountjoy (2025), among others. 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.

[23]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 papers 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 regression discontinuity treatment effects with a cutoff of 2.0 in two samples: 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.

but not bias effects. To further test my cutoff estimation strategy, in subsection 4.6, I run a permutation test comparing the reduced form from my estimated cutoffs to a range of "fake" cutoffs and find that the estimated effects from my cutoffs are larger than 95 percent of the estimated effects from the "fake" cutoffs, providing additional assurance that my strategy is working well and identifying real effects.

3.2 Regression Discontinuity

To formalize the use of these admissions cutoffs to estimate the effects of transfer on students' outcomes, consider a standard potential outcomes framework where some individuals from a population receive a binary treatment $D = TransferTarget_{ict}$, which is equal to one if student i transfers to 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. The potential treatments, $D_1(r)$ and $D_0(r)$, are a function of the value of the student's running variable (i.e., their GPA) $R = r$ relative to the target college's transfer admission cutoff T_{cts}. Their potential outcome is Y_0 if they do not transfer to target college c in year t, and Y_1 if they do. We observe $Y = Y_0 + D(Y_1 - Y_0)$, and the object of interest is the causal effect of treatment, $E[Y_1 - Y_0 | R = T_{cts}, D_1(T_{cts}) = 1, D_0(T_{cts}) = 0]$. This is the local average treatment effect of transferring to target college c in year t among compliers, i.e., marginal applicants who would transfer if their GPA was just above the transfer admission cutoff, but not if it was just below. This parameter is identified by the fuzzy RD estimand,

$$\frac{\lim_{r \downarrow T_{ct}} \mathbb{E}[Y \mid R = r] - \lim_{r \uparrow T_{ct}} \mathbb{E}[Y \mid R = r]}{\lim_{r \downarrow T_{ct}} \mathbb{E}[D \mid R = r] - \lim_{r \uparrow T_{ct}} \mathbb{E}[D \mid R = r]} \tag{2}$$

under standard assumptions (Hahn et al., 2001), discussed below.

Intuitively, the estimand is fuzzy 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 implies that crossing the threshold is a weaker instrument for transfer than if admission were determined fully by GPA and may change our interpretation of the estimated effects, 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.[24] Cattaneo et al. (2016) show that this pooled RD estimand identifies a well-identified average of college-year-specific LATEs, with higher weights on college-years with more applicants near the cutoff and with larger first-stage discontinuities. In all analyses, I keep applicants from two-year and four-year colleges separate. In some specifications, I also separately estimate effects between flagship

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[24]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 3% of my sample) and results are not sensitive to dropping them.

institutions (UT–Austin and Texas A&M) and the rest of the target colleges.[25] I also explore heterogeneity by student demographics and academic history in subsection 4.5.[26]

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 counterfactual pathway they would have followed otherwise. I elaborate on this and how it affects the interpretation of my results in subsection 4.4 and subsection 5.4. Another issue for interpretation is that cutoffs may also vary by major.[27] However, note that in the pooled two-year to four-year results, compliers are well-spread across a wide range of majors, including high-earning majors, so results are not specific to a small group of majors (see subsubsection 4.5.3). Conversely, for four-year to flagship transfers, compliers substitute out of higher-paying majors such as business immediately after transfer, suggesting that they did not gain admission to those majors (see subsection 5.5).

In the baseline specification, I estimate Equation 2 with a local linear regression that includes application college-by-year fixed effects to ensure that comparisons are made only between individuals who applied to the same college in the same year, a vector of student characteristics (gender, race, ethnicity, economic disadvantage status, high school standardized test scores in math and English Language Arts (ELA), year of high school graduation, and cumulative credits at the time of application), fixed effects for major at the time of application, and sending college fixed effects.[28] I use 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. I explore the sensitivity of results to a range of these choices for my main outcomes in subsection 4.6 and subsection 5.6.[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.

3.3 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

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[25]While I estimate a number of cutoffs for both flagship colleges for two-year applicants, I estimate only one cutoff at Texas A&M for four-year applicants, implying that results for four-year to flagship transfers are heavily weighted by UT-Austin. Results are not sensitive to dropping the one Texas A&M cutoff.

[26]In the context of freshmen admissions, Mountjoy (2025) finds heterogeneity by students' "fallback" options, i.e., whether they have gained admission to another four-year college. However, this is less relevant in the transfer context, where the vast majority of students who apply to transfer only apply to one college in a given year (in my sample, 83% of students only apply to transfer to one college).

[27]In principle, I could separately estimate cutoffs by major within a college-year, but there are practical issues that prevent me from doing so. First, I do not observe the major that students apply to and many students have undeclared majors before transfer or may end up applying to a different major than they currently have declared. I could attempt to estimate students' intended major based on their course choices (Zhu, 2025), but this data is not available until 2012 so is unavailable for the majority of my sample. Second, even if I was able to observe the major that students applied to, I likely would not have enough power to reliably estimate major-specific cutoffs.

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

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

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 two-year colleges and the bottom two subfigures are for applicants from four-year colleges. The outcome in the left subfigures is acceptance to a target institution. In the right subfigures, 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 in Table 1. The first column 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 outcome in second column is 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 second measure as the first-stage, so the results can be interpreted as the effect of transferring to a target college on various outcomes. This specification treats students who are accepted for admission but choose not to transfer as "never-takers." The results in the second column show that, while not all accepted students transfer, there is still a sizable jump in transfer rates at the discontinuity. Among students who applied to a target college, students with GPAs just above their colleges' cutoff are 12 percentage points more likely to transfer to that college than students just below the cutoff. The third and fourth columns show that applicants from four-year colleges who are just above their respective cutoffs are 21 percentage points more likely to be accepted and 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 manipulate their GPAs accordingly, then some more motivated students may increase their GPA to ensure that they are just above the cutoff. Or, again assuming students know the cutoffs, we might worry about selection into applying for transfer where students This would lead to biased results on the effect of transferring since the difference in outcomes between students just above and just below the cutoff may be more related to their difference in motivation or other unobservable characteristics than to the difference in transfer admission.[30] Given that

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[30]Another concern is that my bandwidth is large enough that there is bias. This is not an identification issue but an issue in estimation that is present to some degree in all empirical applications. I address this issue by using optimal bandwidth values as calculated by Calonico et al. (2020), using triangular weights so that observations

admissions thresholds are not publicly known, this scenario seems unlikely. Nevertheless, to investigate possible manipulation, I use perform tests that are standard in the RD literature.

The first test is to look at the density of the running variable around the cutoff to see whether there is bunching on one side (McCrary, 2008; Cattaneo et al., 2020). However, even absent manipulation, using GPA as the running variable is expected to produce some lumpiness in the distribution since grades are assigned in whole numbers (e.g., 3.0 corresponds to a "B" grade). Panels (a) and (c) of 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:

$$\frac{f(GPA|x)}{f(GPA)} \tag{3}$$

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 are economically disadvantaged in high school (which is primarily determined by whether they received free or reduced-price lunch). 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[31] and 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, economic disadvantage, standardized math and ELA 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 bachelor's 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.,

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closer to the cutoff are given more weight, and by examining the sensitivity of my results to changes in bandwidth in subsection 4.6 and subsection 5.6.

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

the year before they intend to transfer).[32]

In Figure 4, I show binned scatterplots analogous 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 A3 shows the corresponding table and verifies that there are no statistically significant discontinuities. Appendix Table A4 separates four-year applicants to those who apply to flagships versus nonflagships, and also shows no evidence of discontinuities in the covariate-predicted outcomes.

Since my earnings data come from administrative records from 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 higher 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.[33]

Appendix Table A5 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 (in subsection 4.6), align with my main estimates, which provides additional assurance that out-migration from Texas does not drive my main effects.

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

[33]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.

4 Two-Year to Four-Year Transfer Results

4.1 Summary Statistics of Sample and Compliers

In this section, I focus on results for two-year college students who apply to transfer to a four-year college. To help contextualize which types of students contribute the identifying variation for the main effects, Table 2 gives summary statistics on the background characteristics of my analysis sample as well as for several comparison groups. The first column includes all 9.6 million students who attended public high schools in Texas from 1993 to 2023. The second and third columns narrow this sample to include only students who enrolled in a two-year or four-year college in Texas for at least one semester, respectively. The final three columns correspond 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 column uses the baseline RD specification but replaces the outcome with pre-determined covariates to describe compliers following the method of Abadie (2002). "Math HS test score" and "ELA HS test score" refer to student test scores on 10th grade state standardized tests, which have been normalized within each statewide cohort to be mean zero with 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 which they were enrolled.

Focusing first on the students' standardized high school test scores and college GPAs, compliers are negatively selected among all transfer applicants and more representative of the average two-year college student then the average four-year college student, implying that they are academically weaker than the populations at the target colleges for which they apply. However, they are much more representative of the four-year college students in terms of their race-ethnicity and economic disadvantage, so they don't necessarily correspond to the same students that are often referenced in affirmative action "mismatch" debates. The average complier would enter the four-year college after about two years worth of credits attempted, but Figure A2 shows a good amount of variation in the number of semesters and credits students have attempted.[34]

4.2 Bachelor's Degree Completion

The reduced-form and instrumental variable (IV) results for the first main outcome of interest, bachelor's degree completion, are shown in Table 3. 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]

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[34]Note that Figure A2 plots the number of semesters enrolled and credits attempted at the end of the fall semester before the year of intended transfer, so most students would have one more semester of enrollment before transferring.

[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.

The first row gives the reduced-form effect of crossing the threshold on bachelor's completion. For example, the interpretation of the third column 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.

IV 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. This may seem counterintuitive since most two-year colleges do not award bachelor's degrees. However, these rates of bachelor's completion for untreated compliers are large because many students who are narrowly denied admission at a target college still end up transferring to a four-year college eventually. I return to this issue and talk about how it affects the interpretation of the estimates in subsection 4.4.

A subset of the reduced-form effects are also shown graphically in Figure A3 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 are much less pronounced over the longer time horizons.

4.3 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 time since transfer, and from specifications that allow for effect heterogeneity by 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 with a mean of 8.22 years since intended transfer. Table 4 shows the results, where I use 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 result on the effect of transfer

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[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 D for an illustration of this estimation.

[37]See section 2 for details on the earnings measures and the motivation for using each.

for compliers at the cutoff.

Table 4 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 5 shows these results graphically with binned scatterplots, where 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.

To offer a sense of how the effects change as individuals gain work experience and progress in their careers, Table 5 presents 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. If the negative effects of transferring are concentrated in early years after transfer (when transfer students are more likely to enrolled in college) but become positive over time, it may imply that the lifetime effect of transfer is positive. However, Table 5 shows that even in the longer term, the earnings effects are null to negative. The strongest negative effects are six to ten years after intended transfer, by which time virtually all students are done with their schooling. The point estimates remain large and negative 11-15 years out, and show some modest but inconsistent evidence of catch up in later years.[38]

4.4 Interpretation of Estimates

4.4.1 Decomposition of Local Average Treatment Effect

The main regression discontinuity IV estimates that I have presented identify a local average treatment effect (LATE). 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.

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[38]Appendix Table A23 shows that dynamics are similar when limiting the sample to individuals who are observed for at least 11 years after intended transfer.

While the treatment of transferring to target college c in year t is clearly defined, the counterfactual determining Y_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 non-target 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,

$$LATE_{RD}(Y) = Pr(Nev)\omega_{Nev} + Pr(O_t)\omega_{O_t} + Pr(T_{\tau>t})\omega_{T_{\tau>t}} \tag{4}$$

where $Pr(Nev)$ 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., non-target) 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.

4.4.2 Fraction of Compliers in Each Counterfactual Pathway

Although the separate treatment effects (ωs) are not identified, the proportion of compliers who would fall into each category, $Pr(Nev)$, $Pr(O_t)$, and $Pr(T_{\tau>t})$ are identified and can be estimated using the method of Abadie (2002) (see Appendix D 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 Table 6, labeled *% of Compliers*, 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, while around one-fifth transfer to a non-target college at time t and half transfer in a later year. Therefore, the IV results 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 non-target college, and the effect of transferring earlier relative to later.

4.4.3 Observational 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), controlling for a fourth-order polynomial of GPA, demographics, high school test scores, sending college fixed effects, and all the covariates included in the baseline specification.[39] I present OLS estimates among two samples: all college students in Texas who apply to transfer to a four-year college, and two-year applicants in my regression discontinuity sample within the 0.3 grade point bandwidth of the admissions cutoff. 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 likely 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 akin to upper bounds on the true causal impacts of each treatment effect.

Results are shown in Table 6, where the label at the top of each column gives the counterfactual pathway of untreated students. For example, the sample in 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 (i.e., students following a different counterfactual pathway are excluded from the sample in this column). For comparison, the final column gives the OLS estimate of transferring to a target college relative to all students do who do not transfer to a target college in year t (i.e., pooled across all counterfactuals). $E[Y_0]$ gives the average earnings for untreated students (e.g., in the first column, 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.

Focusing on the first 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 for my full set of covariates. The estimate in panel A of Table 6 indicates that when estimated on the all Texas two-year to four-year transfer applicants, two-year students who transfer to a target college earn approximately 1,600 dollars less per year than those who apply to transfer to a target college but never transfer. In the RD sample, the estimate is around -700 dollars, and is less precisely estimated. Since students who are accepted for transfer are likely positively selected yet the OLS estimates are null to negative, this lends additional evidence that the true causal effect of transferring to a target college relative to never transferring is negative, or at least, not positive.

The second and third columns show inconsistent evidence across the two samples of

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[39]The covariates from the baseline specification are gender, race-ethnicity, economic disadvantage status, high school standardized test scores in math and ELA, 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.

the effects of transferring to a target college relative to transferring to a non-target college in year t or to transferring in a year later than t, while the final column shows null to small negative effects relative to the pooled counterfactual. Appendix Table A6 separates these OLS estimates out by years since intended transfer. Both the full Texas sample and the RD sample show large, statistically significant negative estimates of transferring to a target college relative to never transferring to a four-year college in the short run. These estimates remain null to negative up to 10 years after intended transfer, but contrary to the regression discontinuity evidence, become statistically significant and positive in the longer run. The OLS estimates for the pooled counterfactual in the last column broadly follow the same pattern, although with smaller magnitudes. This discrepancy between the OLS and RD estimates 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 compliers.

4.5 Heterogeneity and Mechanisms

Next, I return to the regression discontinuity specification to further explore heterogeneity in the main earnings estimates (e.g., those in Table 4), and investigate some mechanisms that might be contributing to the lack of positive effects. For brevity, here I summarize the main takeaways from these mechanisms analyses, but further details are included in Appendix B.

4.5.1 Academic Preparation and "Mismatch"

First, I demonstrate that, as expected, marginal transfer students attend more selective and higher-resourced colleges than transfer applicants who were marginally rejected. Appendix Table A7 shows that both immediately after transfer and over the course of their college career, compliers attend colleges where their peers have higher standardized test scores, bachelor's degree completion rates, and earnings. While attending colleges with higher-achieving peers could have positive effects on transfer students' outcomes, it also raises the possibility "mismatch" where transfer students could be harmed if they are not academically prepared enough to keep up at their target institutions.

This "mismatch" hypothesis has been a long-running debate in higher education (Sowell, 1972), often in relation to race-based affirmative action policies (Arcidiacono and Lovenheim, 2016). I make two points in how the current setting is different from settings of past literature. First, note that the potential negative effects of academic mismatched may be magnified with transfer students as compared to first-time-in-college students. While both first-time and transfer students may have been under-prepared by their high school education, transfer students may have additionally been under-prepared by their initial college. Further, even if the classes at their initial college and the more well-resourced college were equally academically rigorous, there may be less continuity between the lower- and upper-division coursework for students who transfer between colleges (e.g., differences in topics covered). Second, recall from Table 2 that while compliers have lower GPAs and standardized test scores than the average four-year college student, they are no more disadvantaged racially or economically than the average four-year

college student. This implies that they are not necessarily the same students who would benefit from race- or SES-based affirmative action policies that prior literature has focused on.[40]

To shed light on whether transfer students in my sample may be suffering negative consequences of mismatch in the labor market, I create a measure of mismatch based on where the transfer students' statewide standardized test score falls in the distribution of scores of the freshmen students at their target college. I create a binary measure of mismatch that takes a value of one if both their math and ELA scores are in the bottom quartile of their target college's freshmen scores.[41] I then use the regression discontinuity to separately estimate the effects of transferring to a target college on earnings for these two groups.

Table 7 shows that the negative effects of transfer on earnings are concentrated among the more "mismatched" students (shown in Panel A). While point estimates for both groups are negative, for the mismatched students they are much larger and statistically significant. Appendix Figure A4 shows these results graphically, and the visual drop at the cutoff is even more striking than for the full sample. Appendix Table A8 lends additional evidence that academic under-preparation may be at play by showing that the negative earnings effects are also concentrated among students who had taken fewer credits at the time of transfer. This, combined with the fact that many untreated compliers eventually transfer, suggests that part of the negative effects on earnings may be driven by the timing of transfer (i.e., transferring too early) rather than transfer itself.[42] Appendix Table A9 also provides modest evidence that marginal transfer students' GPAs fall relative to their college peers in the semesters after they transfer.

More generally, we may expect to find heterogeneous effects by the selectivity or resources of target colleges. Unfortunately, I do not have enough power to separately estimate impacts by individual target colleges, but Appendix Table A10 gives suggestive evidence that the negative effects are concentrated among students who transfer to flagship colleges.

4.5.2 Heterogeneity by Demographics

We may also expect heterogeneity in effects 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). For two-year applicants, I find evidence of heterogeneity by gender. Appendix Table A11 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,

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[40]In fact, I do not find meaningful heterogeneity when splitting the sample by URM or economic disadvantage. See Appendix Table A13 and Table A14.

[41]On average, compliers' math and ELA scores are at the 28th and 24th percentile, respectively.

[42]This 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).

shown in Appendix Table A12, where increases in bachelor's degree completion are concentrated among women. Appendix Table A13 and Table A14 show that there not much heterogeneity in earnings effects when splitting by underrepresented minority or economic disadvantage status.

4.5.3   Field of Study and Industry

While transfer could potentially lower future earnings by pushing students into lower-paying majors, for two-year applicants that does not seem to be the case. In appendix Table A15, I group students' declared major in their first semester after intended transfer into 13 mutually exclusive categories (including "not enrolled"), then use those categories as an outcome in separate regressions so that the coefficients can be interpreted as the percentage-point change in the probability that a student will declare that major right after intended transfer. The primary substitution is out of "general/undeclared" and "not enrolled" into social sciences, humanities, and other majors. Note that two-year transfer students are *not* less likely to be in high-earnings majors like science, engineering, and business than their marginally denied peers. Note also that the $E[Y_0|C]$ and $E[Y_1|C]$ give the share of below- and above-threshold compliers, respectively, and reveal that marginal transfer students are well-spread across a wide range of majors, implying that results are not driven by a small set of majors. Appendix Table A16 and Table A17 focus on field of bachelor's degree (within six years of intended transfer) and show that transfer is not causing students to graduate with lower-earnings majors or work in lower-paying industries.[43]

4.5.4   Employment and Experience

Appendix Table A18 shows estimated effects of transfer on several proxies of employment, separately for men and women. Across all measures, men who are marginally admitted to a target college work less than men who are marginally denied. Combining these results with those on the indicator of dropping out of the earnings data in Table A5 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. These cumulative decreases in employment lead to decreases in years of experience by mid-career, shown for men in appendix Table A19.

4.5.5   Networks

When students transfer, especially if it involves moving further from home, 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). Appendix Table A20 shows that marginal transfer students attend colleges with many fewer of their peers from their high school's graduating cohort, suggesting that a loss of networks could contribute to the null to negative impacts of transfer on earnings.

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[43]See Appendix B for details on construction of these variables.

4.6 Robustness

Appendix Table A21 shows the sensitivity of results to various choices on bandwidth, polynomial of the running variables, standard error clustering, and kernel. Overall, estimates are stable across specifications, especially for earnings outcomes. The baseline bandwidth of 0.3 is around the optimal bandwidth values using a local linear regression as calculated by Calonico et al. (2020) for most outcomes, but the point estimates are stable at around -7,000 to -8,000 dollars for all bandwidths from 0.2 through 0.45.[44] For specifications that use a local quadratic function of the running variable, I present estimates from both the baseline bandwidth (0.3) and for the recalculated optimally-chosen bandwidths when using a local quadratic regression (0.81 for bachelor's completion and 0.72 for earnings). While the point estimate on earnings is somewhat smaller and statistically insignificant than the baseline at the smaller bandwidth, it is large and statistically significant for the optimally chosen bandwidth. Estimates are also similar when using alternative standard error clustering and kernel weighting.

Appendix Table A22 additionally presents earnings outcomes using various sets of controls. The "none" columns include no controls, while the "FEs" columns include only the application college-year fixed effects (i.e., fixed effects for each separate cutoff). Results are somewhat noisier, but point estimates are similar to the baseline specification. The last two columns show conditional and sandwich earnings measured in logs which generally align with the baseline estimates in levels. Appendix Table A23 additionally shows that the dynamics of earnings effects over time since intended transfer are similar to the baseline estimates when limiting the sample to individuals who are observed for at least 11 years after transfer.

Porter and Yu (2015) show that when estimating a regression discontinuity with an unknown discontinuity point, the convergence rates are such that the asymptotic distribution of the second-stage RD estimate is not affected by the fact that the cutoffs are estimated rather than known. A variety of empirical papers have used this result to estimate RDs without adjusting second-stage standard errors, including Mountjoy (2025) who uses a very similar procedure with the same Texas administrative data (on the effect of freshmen college admissions as opposed to transfers). Still, there may be some concern that the search error is not ignorable in my setting due to some differences between the setup from Porter and Yu (2015).[45] To address this, I run a permutation test where I estimate the reduced form using a wide range of "fake" discontinuity points on the running variable, and plot the resulting reduced form estimates in Figure A5. If my estimated effects are real, we would expect the absolute value of estimate from the "real" estimated cutoff to the be larger than most of the "fake" cutoffs, but less of an outlier if the standard errors were underestimated. For both bachelor's degree completion and earnings, my estimated cutoffs are around the 95th percentile of the distribution after removing cutoffs that are within 0.01 grade points of the estimated "real" cutoffs.[46] This lends additional

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[44]The point estimates for the smallest bandwidth is significantly smaller, although still negative, at -4,600 dollars.

[45]Specifically, Porter and Yu (2015) assumes that a discontinuity point exists, but its location is unknown. This is why I only include estimated cutoffs with a statistically significant first stage ($p < 0.01$). Additionally, my own conversations with Jack Porter indicate that the theoretical results would likely extend to my setup.

[46]Without dropping the "fake" cutoffs within 0.01 grade points of the "real" cutoff, they "real" cutoff is around the 96th percentile of the distribution.

comfort that my cutoff estimation is working well and not contributing meaningful noise to the second-stage RD estimates.

Finally, I showed in Table A5 that being marginally accepted for transfer does not impact a proxy variable for migration out of Texas. To further test whether results may be driven by differential migration out of Texas, 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 A24, align with my main estimates, which provides additional assurance that out-migration from Texas does not drive my main effects.

5 Four-Year to Flagship Transfer Results

In this section, I focus on results for four-year college students who apply to transfer to a flagship college, but results of the main outcomes (bachelor's degree completion and earnings) for four-year college students who apply to non-flagships and results pooled across the two groups can be found in the appendix.[47] Although Texas had two flagship colleges (UT-Austin and Texas A&M), for transfer students from four-year universities, I only identify one cutoff for Texas A&M, so results can be thought of as primarily the effect of transferring to UT-Austin.

5.1 Summary Statistics of Sample and Compliers

Appendix Table A25 gives summary statistics on the analysis sample and compliers and comparison groups.[48] It shows that compared to the average UT-Austin student, four-year to flagship transfer compliers are more likely to be male, less likely to be economically disadvantaged, less likely to be Asian, and more likely to be White. Their standardized test scores, while higher than the average of all four-year college students, are much lower than the average UT-Austin student. Compliers' test scores fall at the 21st and 25th percentile in the distribution of UT-Austin students' math and ELA test scores, respectively. Thus, as with the two-year to four-year compliers, four-year to flagship compliers are academically weaker but *not* more economically or racially disadvantaged than the average flagship student, implying that this is a different population than that of the affirmative action "mismatch" debates.

5.2 Bachelor's Degree Completion

Table 8 shows the estimated reduced form and instrumental variables effects of transfer on bachelor's degree completion within one to six years of intended transfer. First, note that the base completion rates are very high among this group: although only 26 percent of students

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[47]The reason for focusing on four-year to flagship transfers, as opposed to all four-year to four-year transfers, is that many untreated compliers in four-year to four-year nonflagship transfer to a two-year college instead of remaining at their current four-year college. Thus, the comparisons made in four-year to four-year nonflagship results are more akin to the effect of transferring to a four-year college relative to transferring to a two-year college. In contrast, most untreated compliers in the four-year to flagship sample remain at their current four-year institution. See Table A28.

[48]See subsection 4.1 for an explanation of the structure of the table.

have completed a bachelor's degree within one year, this figure climbs to 88 percent for completion within four years. While the point 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. The corresponding reduced form results are shown graphically in Figure A7.[49]

5.3 Earnings

Table 9 shows the effects of transferring from a four-year college to a flagship college, pooled across all person–year observations, where the mean of years since intended transfer is 8.75. 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 6 supports this conclusion.[50] Table 10 presents the earnings effects separated by time since intended transfer. The results show that the negative effects are not a short-term effect and grow over time, implying that marginal four-year to flagship transfer students are unlikely to "catch up" to marginally rejected students in later years.

5.4 Counterfactual Pathways and Observational Estimates

As with the two-year to four-year transfer students, to interpret the four-year to flagship regression discontinuity results, we need to understand more about what complier's counterfactuals would have been had they been marginally rejected for transfer. Table 11 shows the fraction of compliers who would follow each counterfactual pathway (estimated following Abadie (2002)), as well as the "selection on observables" estimates of transferring to UT-Austin relative to each pathway.[51] The counterfactual pathways for four-year to flagship applicants are similar to those for two-year to four-year applicants, but additionally include two categories for students who transfer from a four-year college to a two-year college either in year t or later. The estimates for "% of Compliers" in Table 11 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 different four-year college in some year later than t, or would have transferred to a two-year college in year t.[52]

The remaining rows of Table 11 show the results of estimating the effect of transferring to UT-Austin relative to each counterfactual pathway using ordinary least squares (OLS), controlling for a third-order polynomial of GPA, demographics, high school test scores, sending college fixed effects, and all the covariates included in the baseline specification. Panel A

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[49]Results for all four-year to four-year applicants, and for four-year to nonflagship applicants, can be found in appendix Table A26.

[50]Results for all four-year to four-year applicants, and for four-year to nonflagship applicants, can be found in appendix Table A27.

[51]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.

[52]Appendix Table A28 shows that for those who apply to transfer to non-flagship schools, many students below the cutoff instead transfer to a two-year college, and very few never transfer. This motivates focusing on four-year to flagship transfer applicants, since results for other four-year to four-year transfer applicants are mostly relative to transferring from a four-year to a two-year. It would be interesting to estimate the effect of transferring between non-flagship four-year colleges relative to never transferring, but the data do not contain the variation to reliably estimate that using the RD.

includes all Texas four-year college students who apply to transfer to UT-Austin, while Panel B limits the sample to those within the RD bandwidth (i.e., 0.4 grade points) of the cutoff. As in subsubsection 4.4.3, since these estimates do not account for unobservable factors that influence transfer admission and transfer, these are not causal effects and they are likely biased upwards. Focusing first on the effects relative to never transferring, both samples indicate that transferring to UT-Austin has a negative effect on future earnings. Appendix Table A29 presents estimates separately by years since intended transfer and shows that the negative point estimates persist for all year bins, although they are smaller and not statistically significant in the longer run.[53] Conversely, both Table 11 and appendix Table A29 show that relative to transferring to a four-year college in a year later than t, four-year college students who transfer to UT-Austin may see earnings increases.

5.5 Mechanism: Field of Study

As I did with the two-year to four-year transfer students in subsection 4.5, I use the regression discontinuity specification to further explore heterogeneity and mechanisms behind the main earnings estimates (e.g., those in Table 9). In this section, I focus on the mechanism for which I find the strongest evidence: differences in field of study. In contrast to the two-year to four-year transfer students, marginal four-year to flagship transfer students end up majoring in vastly different fields than their counterparts who were denied transfer admission. In particular, Table 12 shows that transfer causes large decreases in the probability that students have a declared major in business or health, two of the most lucrative majors. Thirty-one percent of untreated compliers majored in business or health in the semester of intended transfer, compared to just three percent of treated compliers. This is likely because they were not admitted to the business or health majors at UT-Austin, which are more selective than other majors at the university.[54] Most of the substitution is into the "general" category, which includes undeclared majors, implying that many students who would have majored in business or health at a less selective college instead switch into a more general liberal arts major or move to being undeclared majors at the time of transfer. Appendix Table A30 shows that these differences persist to graduation and flagship transfer students are much less likely to complete a bachelor's degree in business than their marginally rejected peers. Appendix Table A31 shows that these differences in field of study and industries worked can account for 20 to 40 percent of the negative earnings point estimates.[55]

5.6 Robustness

Appendix Table A32 shows the sensitivity of results to various choices on bandwidth, polynomial of the running variables, standard error clustering, and kernel. Estimates for bachelor's completion are somewhat inconsistent with some positive and some negative point estimates, none of which are statistically significant. Point estimates for earnings are negative in all specifications,

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[53]Counterfactual pathways "Transfer Other 4y Now" and "Transfer 2y Later" are omitted from Appendix Table A29 since they have such low complier shares.

[54]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).

[55]See Appendix B for details on the variable construction.

but smaller and noisier in specifications with a smaller bandwidth or quadratic running variable. Appendix Table A33 additionally presents earnings outcomes using various sets of controls and logs of earnings as opposed to levels. Again, all point estimates are negative but the magnitude and statistical significance varies, especially for the log specification of conditional earnings. Together, these results certainly do not give any evidence of positive effects on earnings and are suggestive (but not conclusive) of negative effects. Finally, both Appendix Table A5 and Appendix Table A24 show that results are unlikely to be driven by selective migratoin out of Texas.

6 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 institution–year-specific GPA thresholds used in transfer admissions. I then pool data across colleges and years with cutoffs and use a regression discontinuity 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 non-flagship 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 the lack of increases in earnings for four-year students who transfer to the flagship appears to be driven by substitution into lower-paying majors, 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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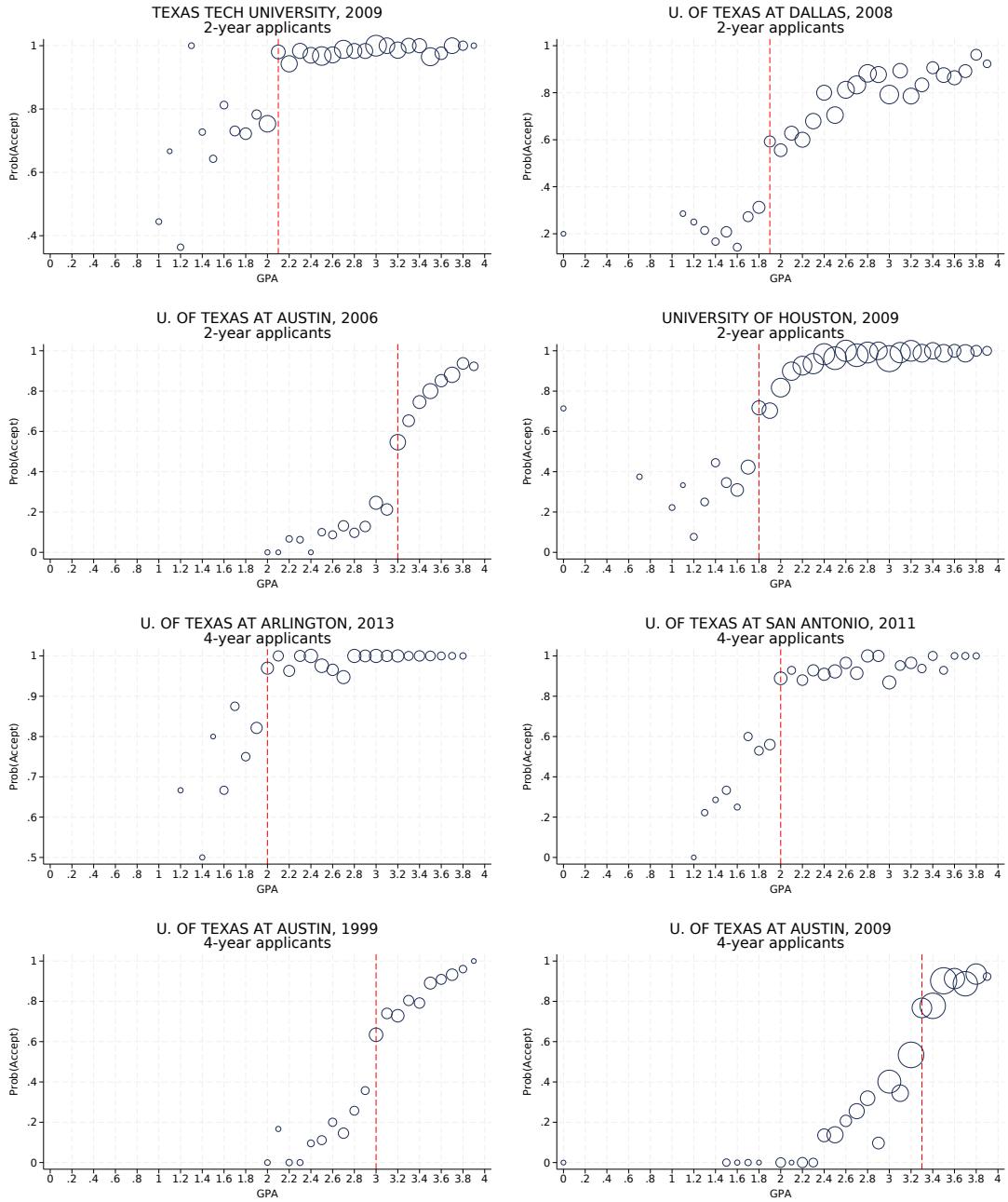
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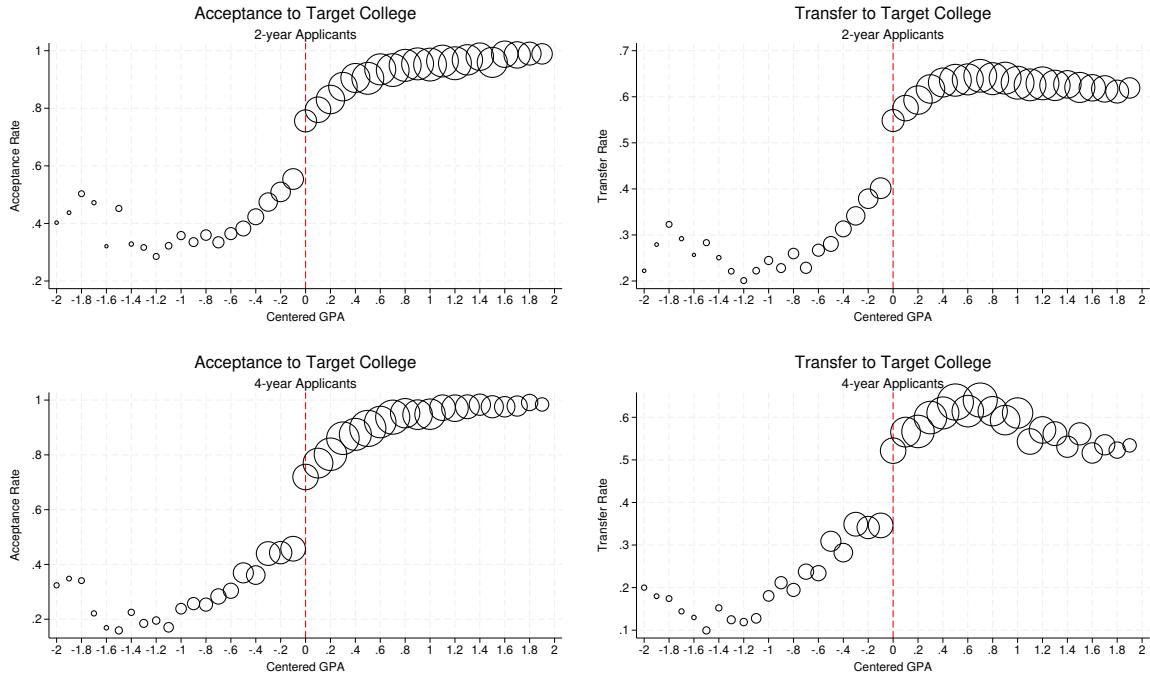
7 Tables and Figures

Figure 1: Examples of Identified GPA Cutoffs in Transfer Admissions
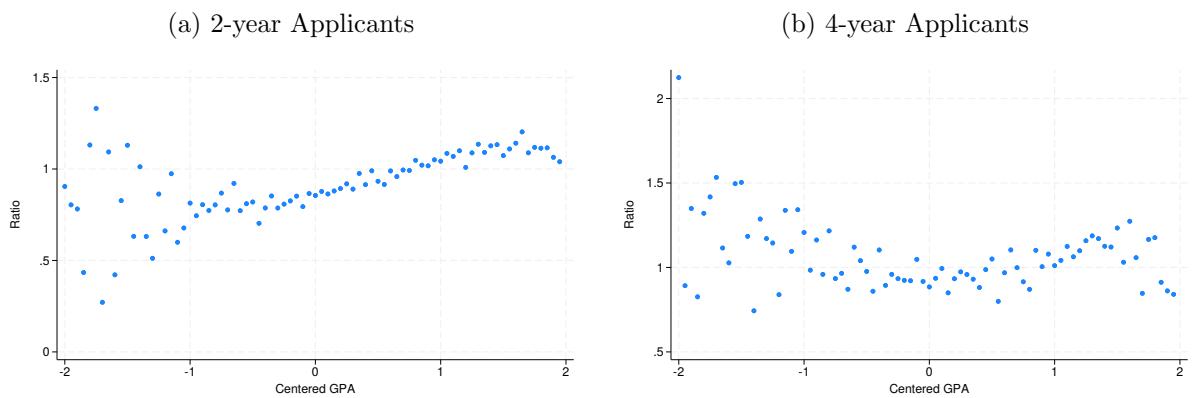
Notes: Each subfigure shows an example of an estimated discontinuity for a particular institution, year, and sector (2-year/4-year) of applicants. The subfigures are binned scatterplots of applicant acceptance rates, where each bin is 0.1 grade points. Circle sizes are proportional to the number of applications 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
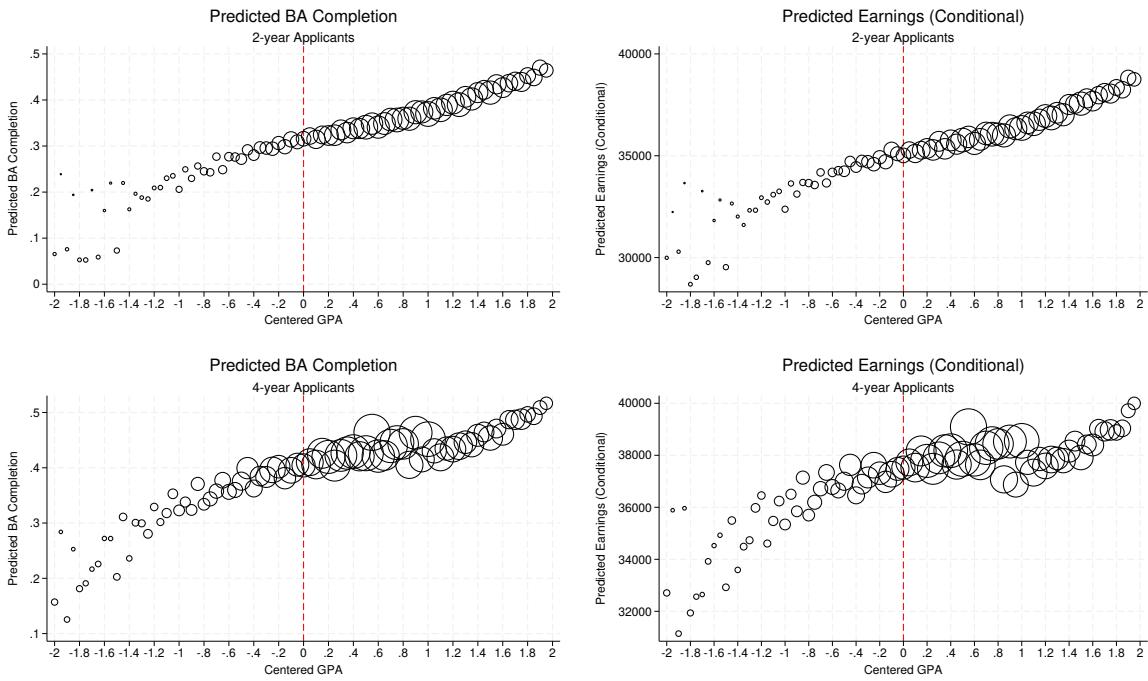
Notes: Binned Scatterplots of transfer application acceptance and enrollment on centered GPA. Centered GPA is created by subtracting the college–year-specific cutoff from each student's GPA for the application she submits. Circle sizes are proportional to the number of applications in each bin.

Figure 3: Density Smoothness Tests

(a) 2-year Applicants   (b) 4-year Applicants
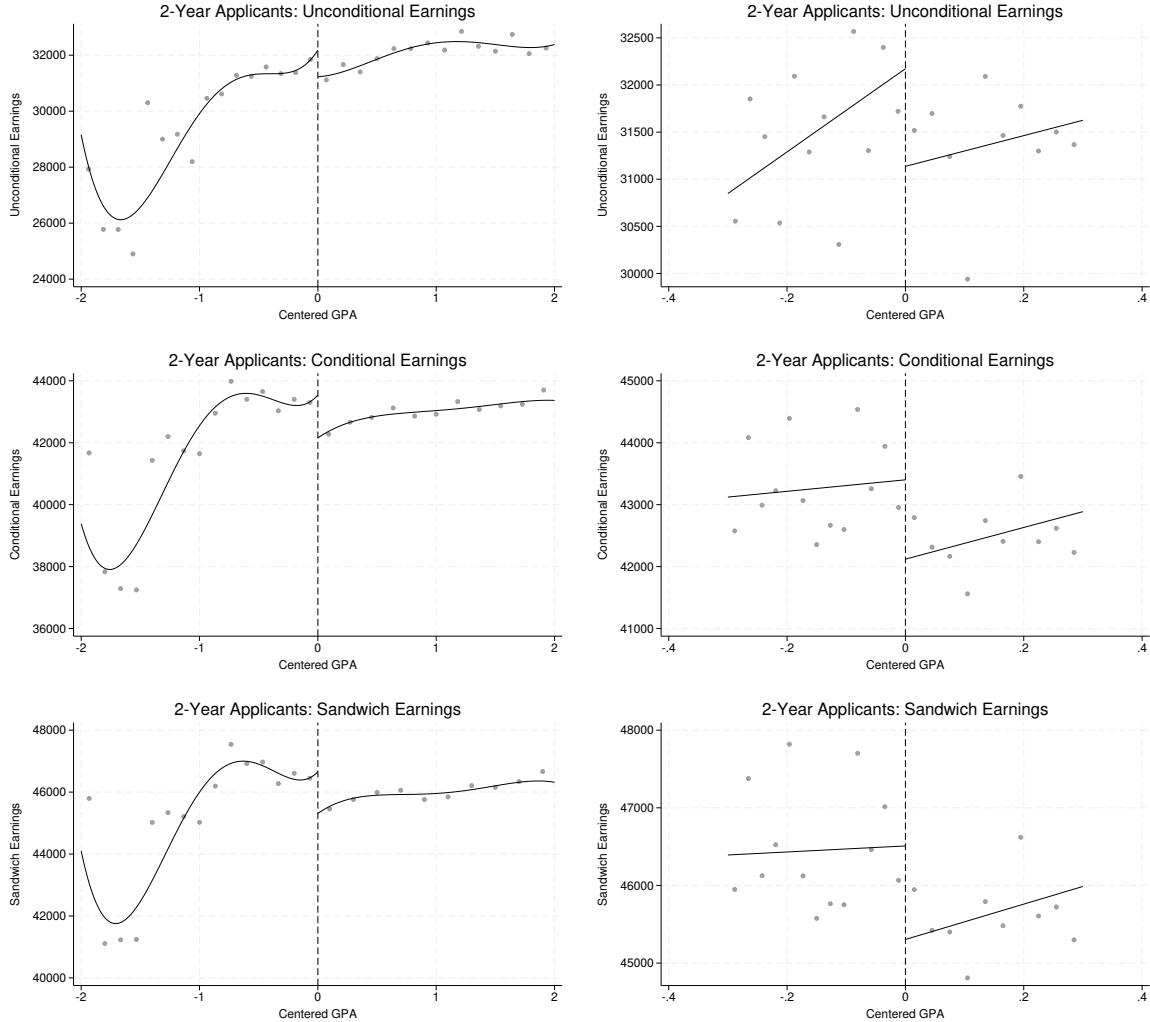
Notes: Each figure shows the ratios of conditional to unconditional densities in 0.05 grade point bins relative to the admissions cutoff. Conditional densities condition on whether students are economically disadvantaged, $Pr(GPA|EconDis)/Pr(GPA)$.

Figure 4: Balance Tests
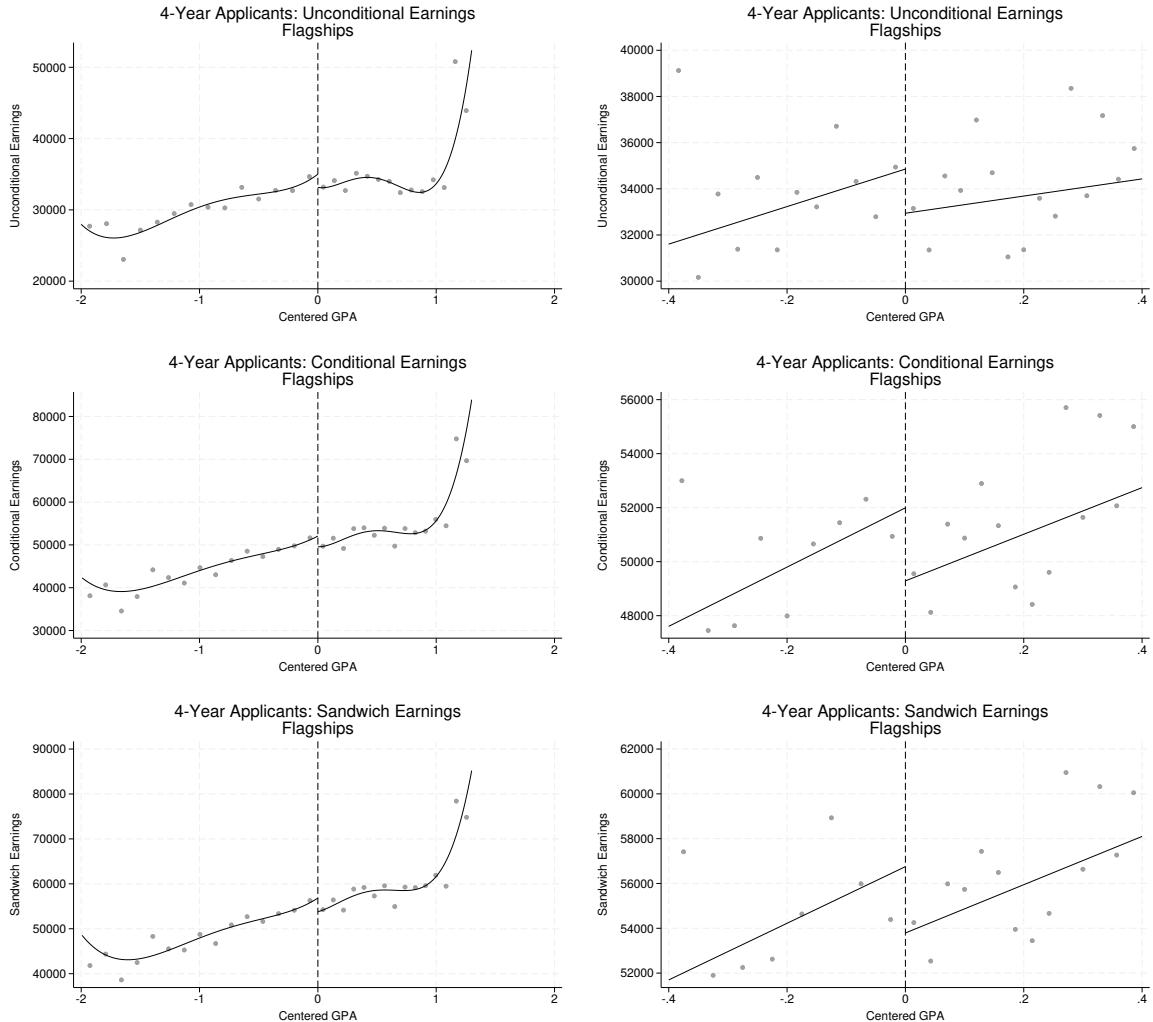
Notes: Binned scatterplots of covariate-predicted bachelor's degree completion and covariate-predicted earnings on centered GPA. Predicted bachelor's completion (within 6 years of high school graduation) and conditional earnings estimated on full sample of Texas high school graduates who enroll in a Texas postsecondary institution with the following covariates: gender, race/ethnicity, economic disadvantage status, standardized math and ELA 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 the 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: 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 6: 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 to flagship 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. 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: Summary Statistics of Analysis and Comparison Samples

	All HS students	2-year college students	4-year college students	2-year to 4-Year Transfer Applicants		
				Applicants to target college	Within the BW	Compliers
Male	0.51	0.47	0.45	0.46	0.52	0.53
Economic Disadvantage	0.40	0.36	0.24	0.23	0.19	0.26
Nat. American	0.00	0.00	0.00	0.00	0.00	0.00
Asian	0.04	0.03	0.07	0.06	0.05	0.05
Afr. American	0.13	0.13	0.12	0.09	0.11	0.11
Hispanic	0.42	0.41	0.29	0.31	0.27	0.24
White	0.39	0.42	0.51	0.54	0.55	0.59
Two or More Races	0.01	0.01	0.01	0.01	0.00	0.00
Math HS test score (std.)	0.00	-0.06	0.59	0.32	0.22	0.23
ELA HS test score (std.)	0.00	0.02	0.50	0.33	0.26	0.23
Cumulative GPA		2.02	2.64	2.80	2.18	2.00
Cumulative Credits		27.66	62.06	50.75	54.46	64.5
N	9,643,530	2,496,742	1,658,867	354,179	43,413	

Notes: All HS students includes all students who attended a public high school in Texas between 1993 and 2023; 2-year and 4-year college students enrolled in public colleges of that type for at least one semester. 2-year to 4-Year Transfer Applicants applied to transfer from a two-year college to a four-year target college. Within the BW limits the sample to those within 0.3 grade points of the target college's GPA cutoff. The means of compliers are estimated from the main IV specification using the method of Abadie (2002). Note that sample sizes are smaller than the analysis sample because students with missing test scores are excluded from the summary statistics.

Table 3: 2-Year Applicants: Bachelor's Completion, Years Since Intended Transfer

| | BA within X years since intended transfer | | | | | |
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs
$\mathbb{1}(GPA_i \geq T_{ct})$	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

Notes: *** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(GPA_i \geq T_{ct})$ gives reduced-form estimates; $TransferTarget$ gives instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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). $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table 4: 2-Year Applicants: Annual Earnings, Pooled across All Years

	Unconditional	Conditional	Sandwich
$\mathbb{1}(GPA_i \geq T_{ct})$	-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

Notes: *** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(GPA_i \geq T_{ct})$ gives reduced-form estimates; $TransferTarget$ gives instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at the 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. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated.$E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table 5: 2-year Applicants: Annual Earnings, by Number of Years Since Transfer

	Unconditional	Conditional	Sandwich
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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table 6: 2-Year Applicants: Observational Estimates of Transfer to Target College on Conditional Earnings, Relative to Counterfactuals

	Estimate of Transfer to Target College Relative to			
	Never Transfer 4y	Transfer Other 4y Now	Transfer 4y Later	All CFs
% of Compliers	32%	19%	50%	100%
Panel A: All Texas 2-4 Transfer Applicants (OLS Estimates)				
$TransferTarget$	-1,591***	462**	-171*	-549***
	(124)	(184)	(99)	(81)
$E[Y_0]$	40,977	38,872	42,524	42,489
Obs	3,140,840	2,898,397	3,374,089	4,445,953
Panel B: RD Sample 2-4 Transfer Applicants (OLS Estimates)				
$TransferTarget$	-712	-895**	1,221***	117
	(488)	(428)	(319)	(306)
$E[Y_0]$	41,233	44,746	42,132	42,757
Obs	338,738	354,192	395,483	535,539

Notes: *** p<0.01, ** p<0.05, * p<0.1. Outcome is average conditional earnings pooled across 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. Transfer 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. All CF = all counterfactual pathways. *% of Compliers* gives the fraction of compliers who follow each counterfactual pathway. Panel A is the sample of all 2-year college students in Texas who apply to transfer to a target college. Panel B limits the sample to students whose GPA is within 0.3 grade points of the admission cutoff of the target college to which they applied. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table 7: 2-year Applicants: Earnings, By HS Test Score Relative to Target College

	Unconditional	Conditional	Sandwich
Panel A: HS test score <25th percentile of target college			
$TransferTarget$	-14,068**	-15,006**	-13,832*
	(7,050)	(7,035)	(7,553)
$E[Y_0|C]$	38,136	50,296	57,961
Obs	344,072	266,960	257,117
Panel B: HS test score >25th percentile of target college			
$TransferTarget$	-3,472	-2,961	-2,473
	(4,161)	(4,187)	(4,594)
$E[Y_0|C]$	37,948	49,924	56,548
Obs	346,195	268,579	259,358

Notes:*** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at person–year level. Sample of transfer applicants from two-year colleges. Top (bottom) panel gives estimates for applicants whose high school standardized test scores are lower (higher) than the 25th percentile in the distribution of their target colleges' first-year students' test scores. 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, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table 8: 4-Year to Flagship Applicants: Bachelor's Completion in Years Since Intended Transfer

	BA within X years since intended transfer					
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs
$\mathbb{1}(GPA_i \geq T_{ct})$	-0.029*	0.019	-0.019	-0.013	-0.0059	-0.0017
	(0.015)	(0.022)	(0.018)	(0.017)	(0.017)	(0.019)
$TransferTarget$	-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

Notes: *** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(GPA_i \geq T_{ct})$ gives reduced-form estimates; $TransferTarget$ gives instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Outcome is bachelor's attainment measured in years since the intended transfer semester. Sample of transfer applicants to flagship colleges. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table 9: 4-Year to Flagship Applicants: Annual Earnings, Pooled across All Years

	Unconditional	Conditional	Sandwich
$\mathbb{1}(GPA_i \geq T_{ct})$	-987	-1,683	-2,096*
	(901)	(1,042)	(1,122)
$TransferTarget$	-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

Notes: *** p<0.01, ** p<0.05, * p<0.1. $\mathbb{1}(GPA_i \geq T_{ct})$ gives reduced-form estimates; $TransferTarget$ gives instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at the 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. Sample limited to transfer applicants from four-year colleges to flagship institutions. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table 10: 4-Year to Flagship Applicants: Annual Earnings, by Number of Years Since Transfer

	Unconditional	Conditional	Sandwich
$TransferTarget$			
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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table 11: 4-Year to Flagship Applicants: Observational Estimates of Transfer to UT-Austin on Conditional Earnings, Relative to Counterfactuals

	Estimate of Transfer to UT-Austin Relative to					
	Never Transfer	Transfer Other 4y Now	Transfer 4y Later	Transfer 2y Now	Transfer 2y Later	All CFs
% of Compliers	55%	<1%	21%	26%	4%	100%
Panel A: All Texas 4-Year to UT-Austin Transfer Applicants (OLS Estimates)						
$TransferTarget$	-2,967***	-263.90	4,925***	1,305	3,299*	-1,057*
	(645.7)	(1,056)	(853.6)	(1,205)	(1,865)	(538.8)
$E[Y_0]$	53,196	48,193	42,723	43,421	40,307	49,220
Obs	145,047	104,831	107,806	102,555	97,349	180,433
Panel B: RD Sample 4-Year to Flagship Transfer Applicants (OLS Estimates)						
$TransferTarget$	-2,669***	-1,532	4,251***	1,558	1,601	-306.90
	(719.0)	(1,285)	(1,092)	(1,225)	(1,519)	(526.6)
$E[Y_0]$	55,241	50,776	44,886	45,479	47,011	50,704
Obs	77,605	63,284	58,096	53,419	51,773	111,854

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Outcome is average conditional earnings pooled across 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 transfer 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. Transfer 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 2-year college in the year in which she applied to transfer to a target college. Transfer 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. All CF = all counterfactual pathways. *% of Compliers* gives the fraction of compliers who follow each counterfactual pathway, estimated following Abadie (2002). Panel A is the sample of all 4-year college students in Texas who apply to transfer to UT-Austin. Panel B limits the sample to students whose GPA is within 0.4 grade points of the admission cutoff of the target college to which they applied. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table 12: 4-year To Flagship Applicants: Major at Intended Transfer Semester

	General	Science	Engineer	Health	Business	Educ	SocSci
$TransferTarget$	0.32***	-0.10	-0.03	-0.13*	-0.15***	-0.00	0.12
	(0.11)	(0.11)	(0.05)	(0.07)	(0.04)	(0.01)	(0.08)
$E[Y_0|C]$	0.13	0.13	0.11	0.15	0.16	0.01	0.04
$E[Y_1|C]$	0.45	0.03	0.08	0.02	0.01	0.01	0.16
Obs	11,040	11,040	11,040	11,040	11,040	11,040	11,040

	CompSci	Vocational	Art	Human	Other	Not Enrolled	
$TransferTarget$	-0.02	0.01	0.04	0.05	-0.02	-0.08	
	(0.03)	(0.01)	(0.04)	(0.09)	(0.08)	(0.08)	
$E[Y_0|C]$	0.02	-0.01	-0.04	0.10	0.11	0.08	
$E[Y_1|C]$	0.00	-0.00	-0.00	0.15	0.09	0.00	
Obs	11,040	11,040	11,040	11,040	11,040	11,040	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample of 4-year to flagship transfer applicants. Regression discontinuity instrumental variables estimates 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. $E[Y_0|C]$ and $E[Y_1|C]$ give the mean value of the dependent variable for compliers in the untreated and treated groups, respectively, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

A 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.

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., students will learn more at a college with better instructors). 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. Conversely, students may suffer from transferring to a college where they are not as well-matched. Note that the potential negative effects of academic "overmatch" may be magnified with transfer students as compared to first-time-in-college students. While both first-time and transfer students may have been under-prepared by their high school education, transfer students may have additionally been under-prepared by their initial college. Further, even if the classes at their initial college and the more well-resourced college were equally academically rigorous, there may be less continuity between the lower- and upper-division coursework for students who transfer between colleges (e.g., differences in topics covered).

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 due to 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), credit loss (e.g., lack of time to complete all requirements for more intensive majors and still graduate on time), or lack of continuity in coursework for a given major across colleges. This change in major could affect students' human capital accumulation and earnings. Finally, transferring may have a negative impact on students earnings because of the disruption to both the student's academic environment and social networks.

Students will choose to transfer only if they expect that it will positively impact the sum of their expected earnings and non-pecuniary benefits. However, students do not have full information about their human capital and how well they are matched with each college. Thus, it is possible for students to make "mistakes" due to information frictions.[56] Students with worse information will be more likely to choose transfers which have worse payoffs.

B Additional Details on Measures used in Heterogeneity and Mechanisms

Appendix Table A7 focuses on the selectivity and resources of colleges that compliers attend. While there are many ways to measure college selectivity/resources, I use as summary measures each college's average bachelor's degree completion (within six year of high school graduation) and earnings, as well as average high school math standardized test score. I also create rough "college value-added" measure for each target college by estimating their students' bachelor's degree completion and earnings after controlling for a wide range of student observable characteristics. Appendix Table A8 splits the sample by if they have above or below the sample median number of credits at the time that they apply for transfer. Appendix Table A9 measures

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[56]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).

students GPA relative to their current college peers. To create this measure, I rank all students within a college by GPA in each semester using the GPA only of classes taken in the current semester, rather than cumulative GPA. I then use the student's rank as the outcome in the regression, where a higher fraction is better ranked, e.g., where 0.75 corresponds to having a GPA that is higher than 75 percent of the GPAs of one's peers in the current college.

In Appendix Table A16 and Table A30, each column is a separate regression where the outcome is an indicator variable for a student completing her degree in the given major. Students who do not complete a bachelor's degree within six years of transfer fall into the "no degree" category. In Appendix Table A17 and Table A31, for the first column, I use data on the earnings of all bachelor's degree holders in Texas to calculate average predicted earnings for each major category as measured 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.[57] 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. For the second column, I similarly create predicted earnings by 2-digit industry using the earnings records of all workers in Texas (not just the transfer sample). 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.[58] In the third column, I combine these analyses by predicting earnings of major-industry pairs.

Appendix Table A18 shows estimated effects of transfer on several proxies of employment (actual employment and hours worked are not observable in the administrative data). "Any Employment" is 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.

I measure experience in Table A19 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. I show experience accumulated by eight and 13 years after intended transfer, since these are the midpoints of the 6-10 and 11-15 year earnings bins, where we see the largest negative earnings effects for two-year applicants.
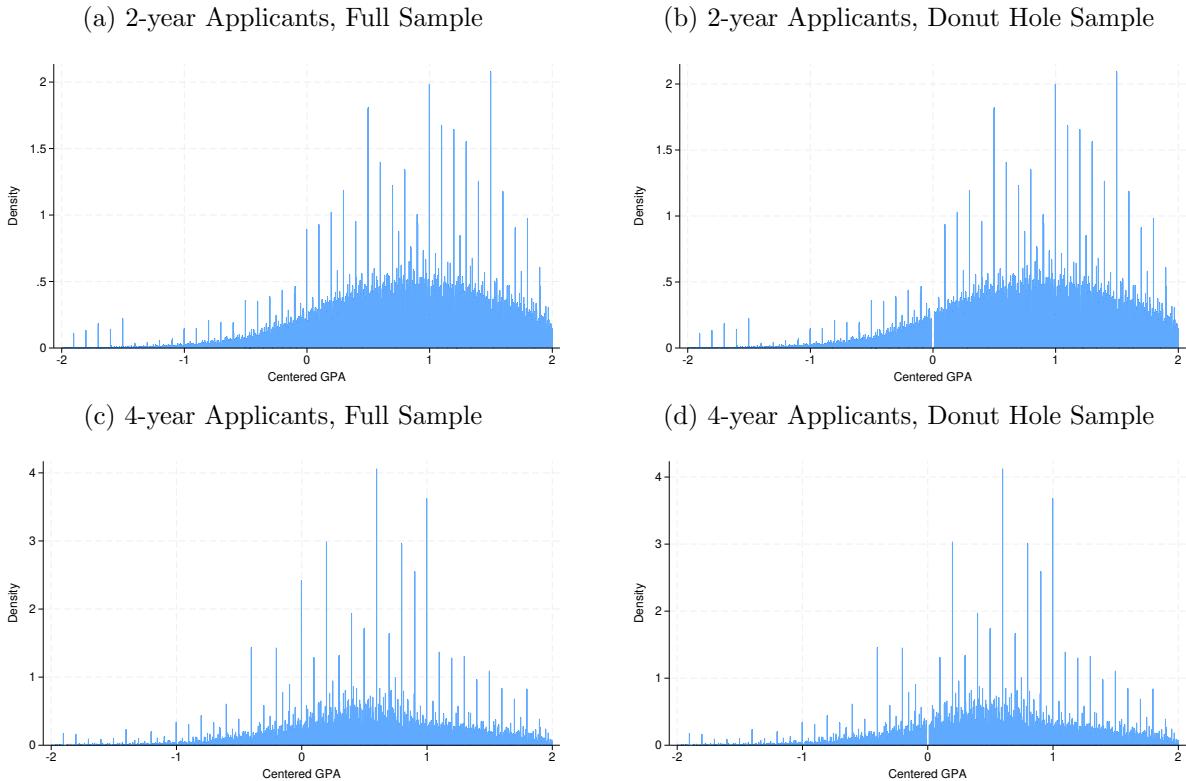
In Appendix Table A20, I construct measures of networks based on each students' high school graduating cohort. For each student, I measure how many other students who graduated in the same cohort from the same high school ("high school peers") are attending their college of enrollment. % HS Peers measures the fraction of students enrolled in their college who are high school peers, while # HS Peers measures the total number of high school peers who are enrolled in their college. The left panel focuses on students' college of enrollment at the time of intended transfer, and the right panel focuses on their last college of attendance.

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[57]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.

[58]If a worker has earnings in two different industries within one year, I use the one with higher earnings.
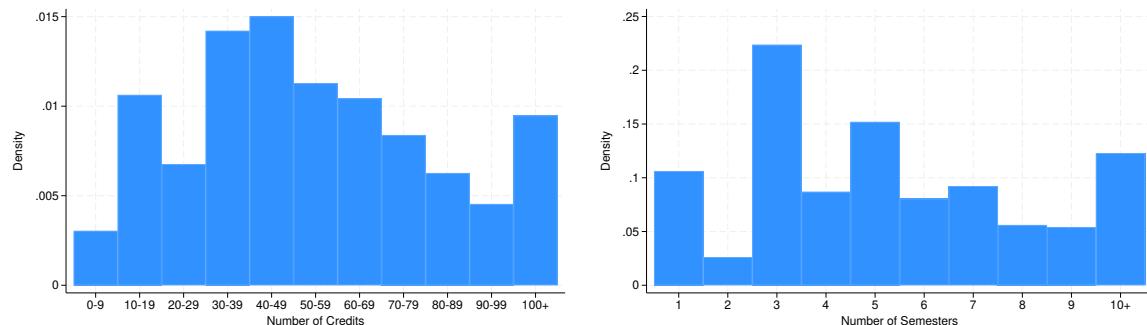
C Supplementary Tables and Figures

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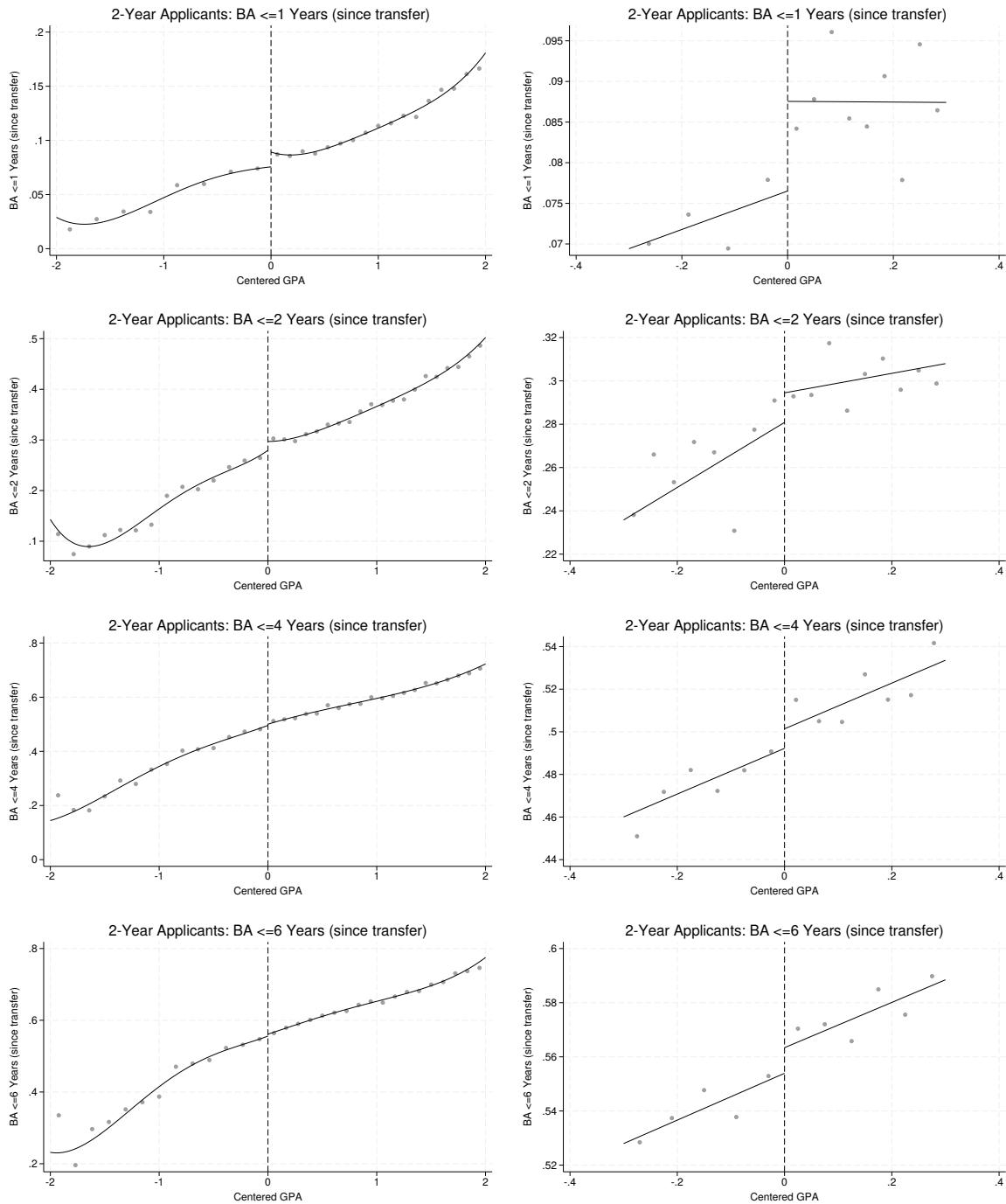
Notes: Histograms of applicants' GPAs after centering on the relevant college–year-specific admissions cutoff. Top row shows two-year applicants, and bottom row shows four-year applicants. Both figures on the right drop all students within 0.01 grade points of the cutoff.

Figure A2: 2-Year Applicants: Distribution of Credits Attempted and Semesters Enrolled at Time of Transfer Application
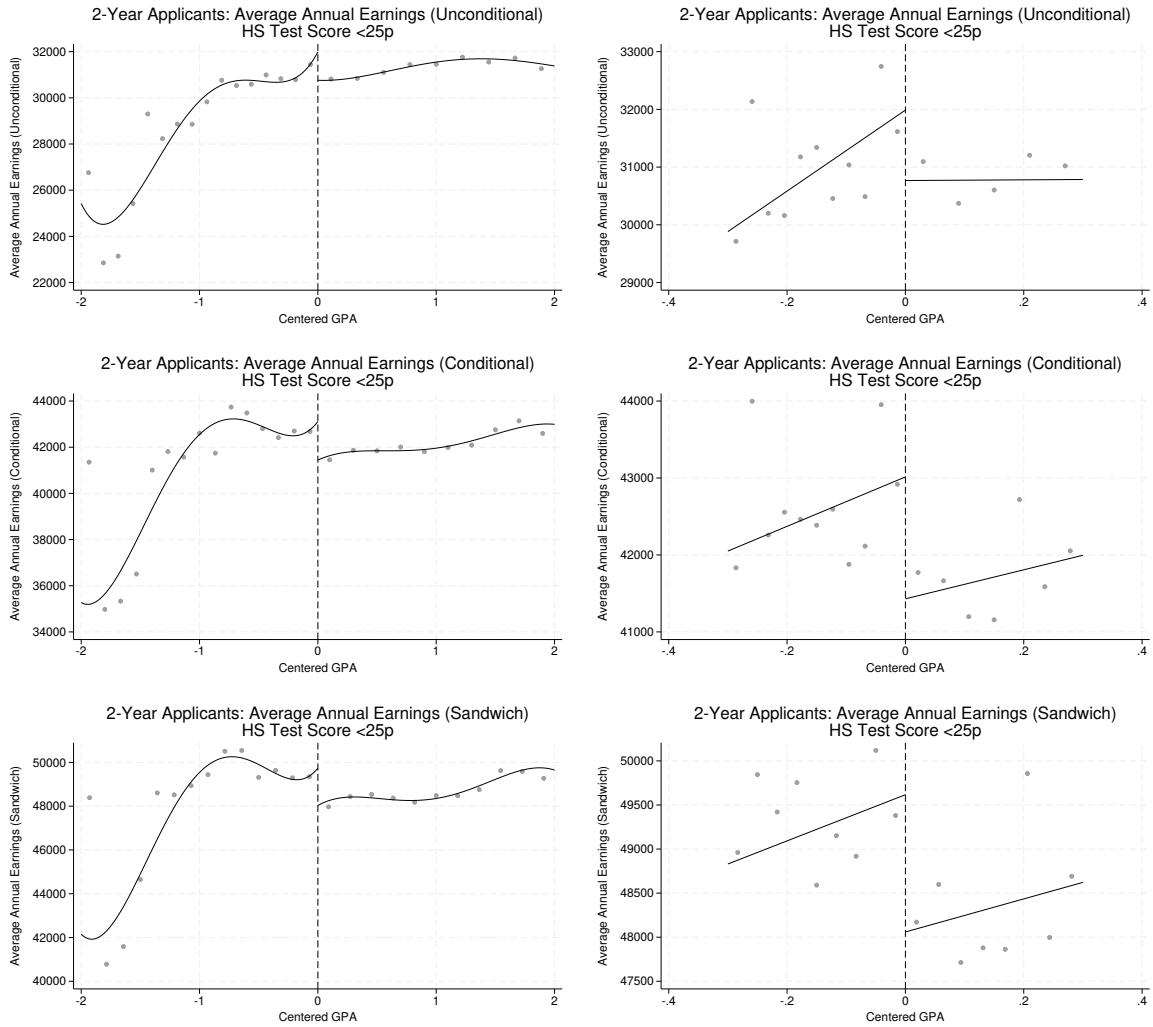
Notes: Distribution of the students' number of credits attempted and the number of semesters enrolled at the time of transfer application (i.e., the end of the fall semester before the fall in which they intend to transfer).

Figure A3: 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 A4: Annual Earnings, Pooled across All Years, 2-Year Applicants , "Mismatch" Sample

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 whose HS standadrized test score is below the 25th percentile of the target college to which they apply to transfer. 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 A5: 2-Year Applicants: Permutation Test

(a) Bachelor's Completion within 6 Years
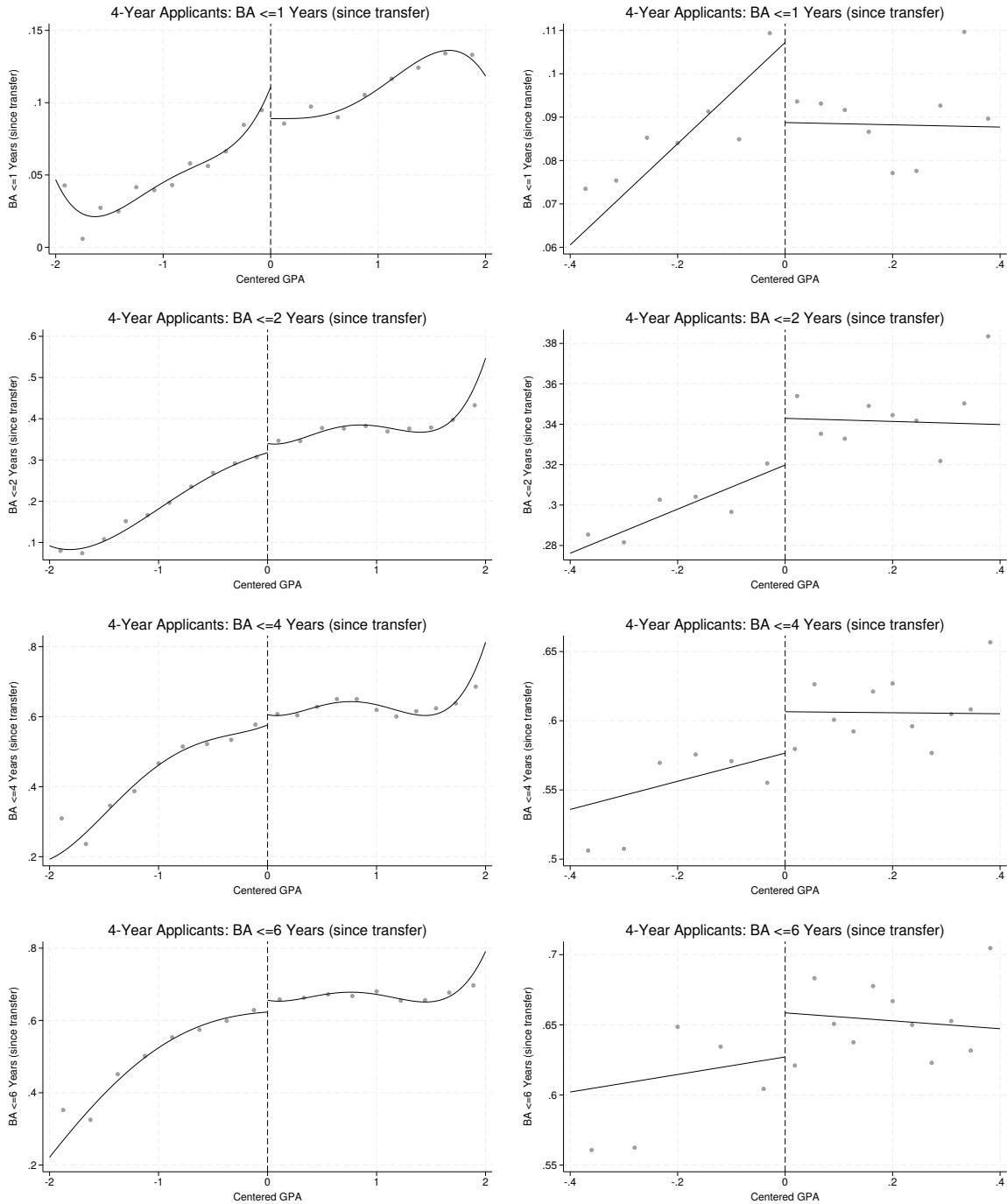
(b) Earnings
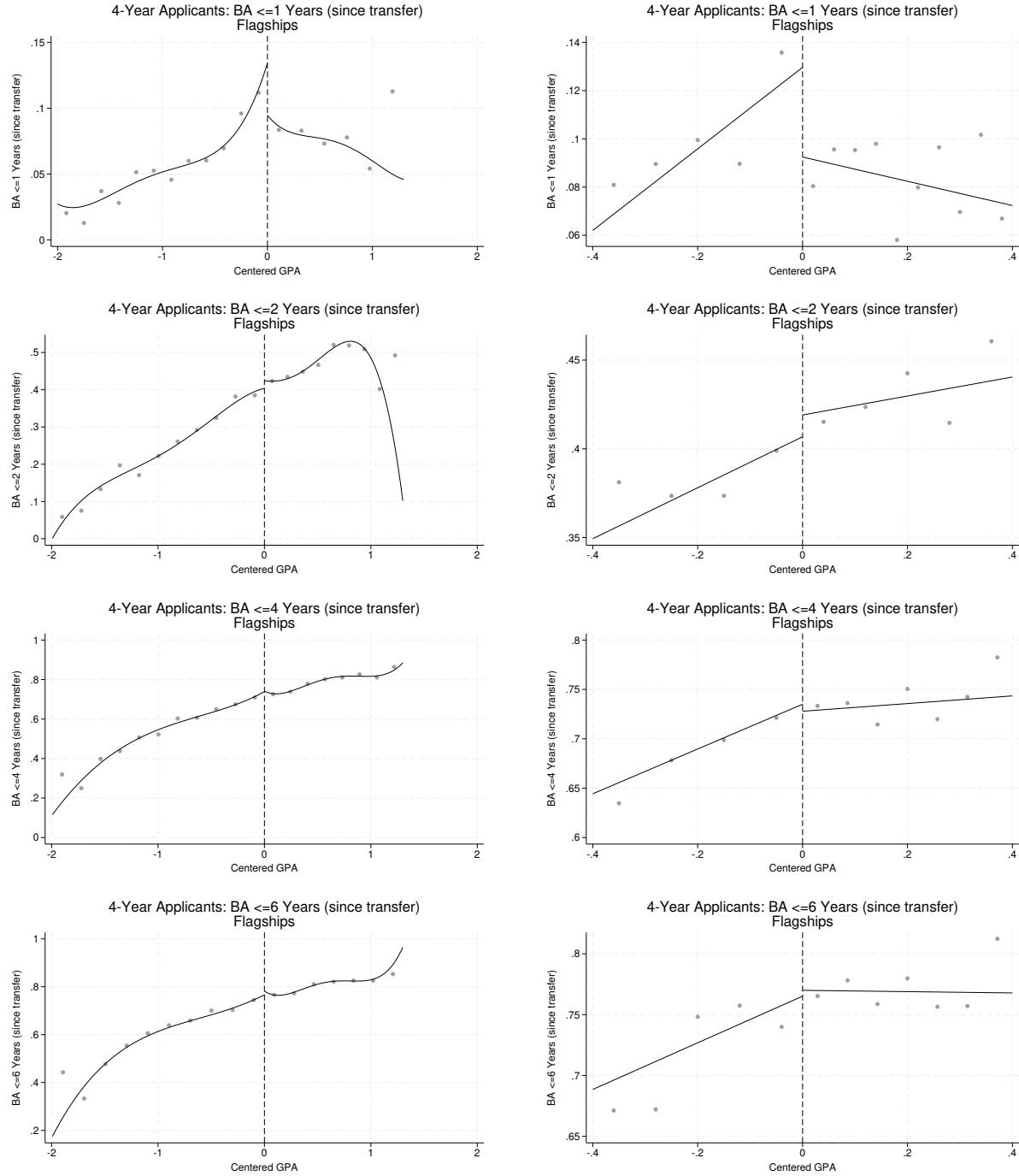
Notes: Plot shows reduced form coefficients from 1000 "fake" discontinuity points where estimation is the same as the main RD, except the cutoff is randomly chosen uniformly between normalized GPAs of -1 and 2.

Figure A6: 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 A7: 4-Year Applicants to Flagships: Bachelor's Completion in Years Since Intended Transfer
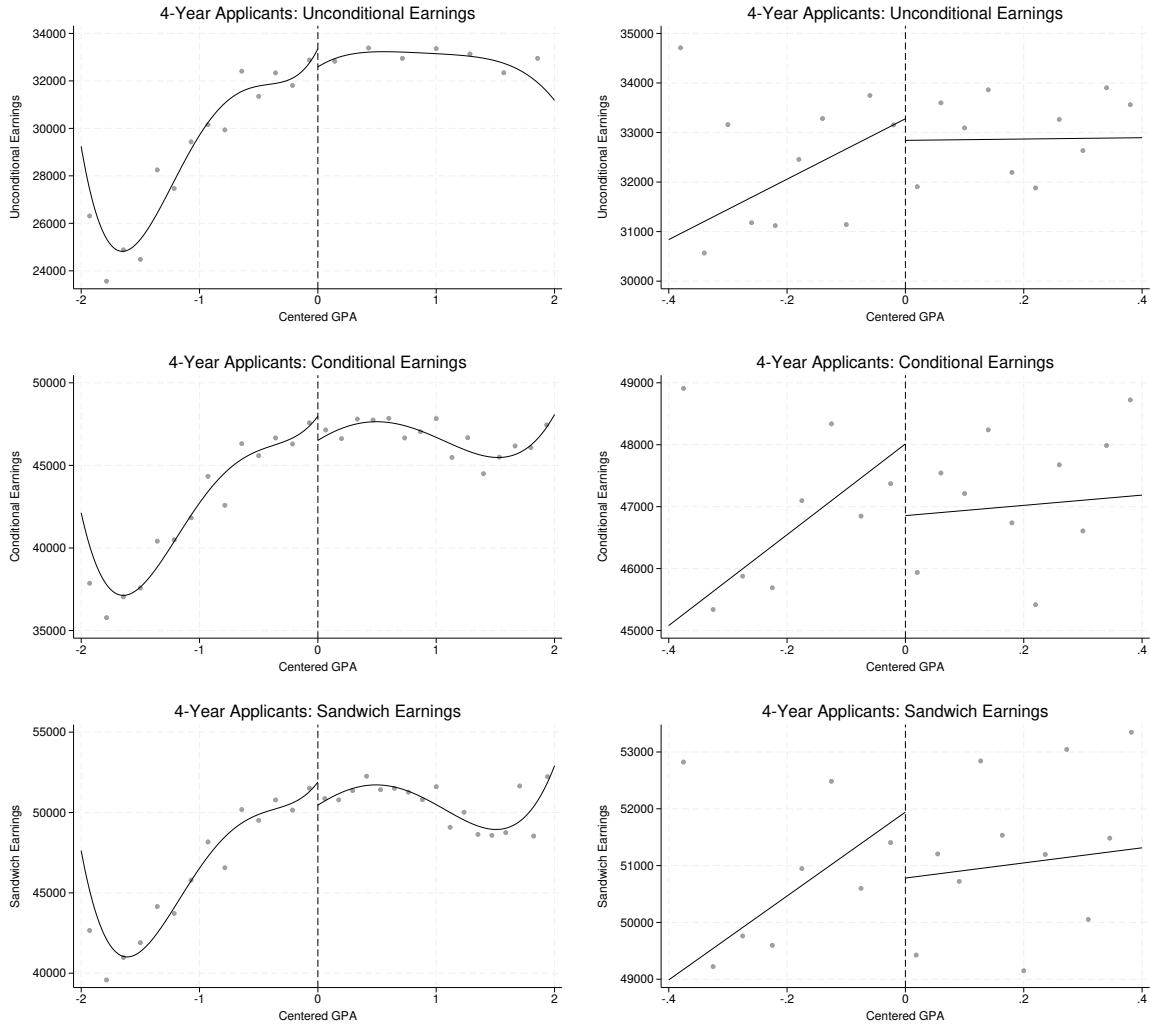
Notes: Binned scatterplots of bachelor's degree completion outcomes on centered GPA created with Stata package rdplot, with bins chosen using the integrated mean squared error-optimal evenly spaced method using polynomial estimators. Left panel includes all applicants within 2 grade points of the cutoff 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 A8: 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).
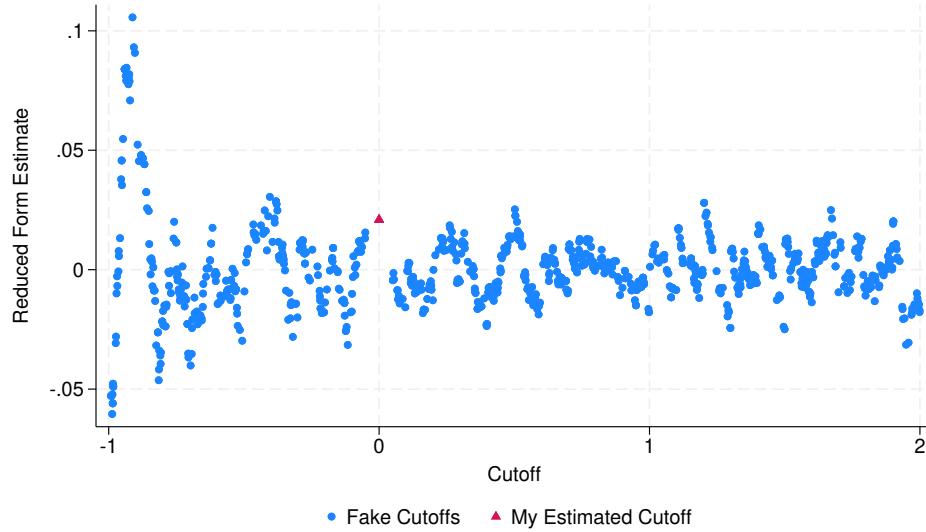
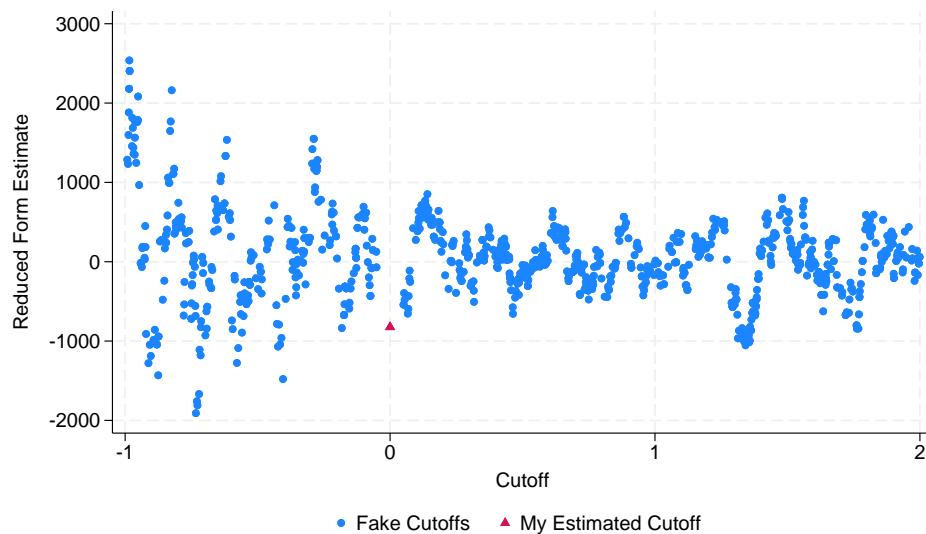Figure A9: Annual Earnings, Pooled across All Years, 4-Year Applicants

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 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
Non-flagship				
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 3.1. 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 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 3.1. 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: 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 (**??**); $TransferTarget$ gives instrumental variable estimates from equation (**??**). 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 ELA 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 A4: Balance Test, by Flagship Status

	4-Year to Flagship		**4-Year to Non-Flagship**	
	BA Completion	Conditional Earnings	BA Completion	Conditional Earnings
$TransferTarget$	0.057	1,766	-0.088	-825
	(0.045)	(2,169)	(0.064)	(2,197)
p-val	0.21	0.42	0.17	0.71
Obs	11,038	11,038	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 (**??**); $TransferTarget$ gives instrumental variable estimates from equation (**??**). 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 ELA 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. Left panel gives applicants to flagships; right panel applicants to non-flagships.

Table A5: 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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 A6: 2-Year Applicants: Observational Estimates of Transfer to Target College on Conditional Earnings, by Years Since Intended Transfer

	Estimate of Transfer to Target College Relative to							
	Never Transfer 4y		Transfer Other 4y Now		Transfer 4y Later		All CFs	
	All TX	RD	All TX	RD	All TX	RD	All TX	RD
TransferTarget								
1–5 years	-4,404***	-3,511***	-24	-427*	-344***	774***	-1,681***	-748***
	(93)	(265)	(113)	(253)	(69)	(215)	(57)	(180)
$E[Y_0\mid C]$	30,511	29,570	25,550	25,536	26,561	24,674	27,103	26,227
Obs	1,345,519	137,751	1,214,494	140,717	1,395,481	156,396	1,852,758	215,198
6–10 years	-471***	482	423*	-823	93	1,559***	8	599*
	(161)	(560)	(221)	(517)	(119)	(344)	(98)	(319)
$E[Y_0\mid C]$	46,585	44,699	42,993	48,231	45,484	44,973	46,163	45,826
Obs	923,011	102,829	860,417	107,627	1,006,864	121,356	1,317,803	163,975
11–15 years	1,688***	2,447**	1,029***	-1,072	-134	1,582***	442***	1,058*
	(250)	(1,013)	(364)	(818)	(186)	(602)	(147)	(594)
$E[Y_0\mid C]$	55,550	54,715	52,910	62,419	57,110	58,298	57,488	58,675
Obs	534,766	60,297	504,054	64,294	594,031	72,209	780,177	96,373
16+ years	2,951***	2,498	2,697***	-2,469*	-434	822	536**	160
	(421)	(1,802)	(579)	(1,285)	(309)	(1,093)	(249)	(1,090)
$E[Y_0\mid C]$	63,580	63,499	60,037	73,521	67,173	68,994	67,042	69,243
Obs	337,399	37,861	319,302	41,554	377,583	45,521	495,099	59,993

Notes: *** p<0.01, ** p<0.05, * p<0.1. Observations are at person–year level. Each row limits to observations within given range of years since 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. All CF = all counterfactual pathways. 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. Transfer 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. All TX columns are the sample of all 2-year college students in Texas who apply to transfer to a target college. RD columns limit the sample to students whose GPA is within 0.3 grade points of the admission cutoff of the target college to which they applied. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application–college–year level in parentheses.

Table A7: 2-Year Applicants: College Peers and Value-Added Measures

	Mean of College Peers'			College Value-Added	
	HS Math	BA Completion	Earnings	BA Completion	Earnings
Panel A: College at Time of Intended Transfer					
$TransferTarget$	0.46***	0.34***	9,404***	0.27***	5,875***
	(0.03)	(0.02)	(703)	(0.02)	(508)
$E[Y_0|C]$	0.18	0.24	34,086	0.27	34,539
Obs	46,648	46,648	46,648	53,721	53,721
Panel B: Last College Attended					
$TransferTarget$	0.19***	0.13***	4,252***	0.11***	2,850***
	(0.04)	(0.03)	(881)	(0.03)	(615)
$E[Y_0|C]$	0.35	0.38	37,394	0.37	36,607
Obs	53,634	53,634	53,634	53,634	53,634

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Sample of 2-year transfer applicants. "Mean of College Peers'" give college-level average high school math standardized test score, bachelor's degree completion (within six year of high school graduation) and conditional earnings. "College Value-Added" measure for each target college by estimating their students' bachelor's degree completion and earnings after controlling for a wide range of student observable characteristics. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002) Standard errors clustered at the application–college–year level in parentheses.

Table A8: 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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 expected value of the outcome for compliers when untreated for the estimate directly above it, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A9: 2-year Applicants: Relative Rank from 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)
$E[Y_0|C]$	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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A10: 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: Non-flagship			
$TransferTarget$	-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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 non-flagship colleges. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A11: 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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 expected value of the outcome for compliers when untreated for the estimate directly above it, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A12: 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
Panel A: Women						
$TransferTarget$	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
Panel B: Men						
$TransferTarget$	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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A13: 2-year Applicants: Annual Earnings, Pooled across All Years, by Underrepresented Minority Status

	Unconditional	Conditional	Sandwich
Panel A: Underrepresented Minority Students			
$TransferTarget$	-4,406	-5,895	-5,197
	(6,973)	(6,201)	(6,248)
$E[Y_0 \mid C]$	38,744	47,043	49,948
Obs	243,346	197,054	189,935
Panel B: Not Underrepresented Minority Students			
$TransferTarget$	-8,198*	-6,187	-4,529
	(4,762)	(5,052)	(5,066)
$E[Y_0 \mid C]$	38,763	51,576	54,291
Obs	447,426	338,823	326,866

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at person–year level. Sample of transfer applicants from two-year college. Panel A gives estimates for underrepresented minority applicants; Panel B for non-URM applicants. Earnings definitions follow the main text. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A14: 2-year Applicants: Annual Earnings, Pooled across All Years, by Economic Disadvantage

	Unconditional	Conditional	Sandwich
Panel A: Economically Disadvantaged Students			
$TransferTarget$	-4,243	-5,613	-5,680
	(5,945)	(5,417)	(5,383)
$E[Y_0 \mid C]$	33,793	42,874	46,556
Obs	111,165	89,728	86,443
Panel B: Not Economically Disadvantaged Students			
$TransferTarget$	-8,142*	-7,006	-5,406
	(4,796)	(4,808)	(4,837)
$E[Y_0 \mid C]$	39,415	51,824	54,493
Obs	579,607	446,149	430,358

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at person–year level. Panel A gives estimates for economically disadvantaged applicants; Panel B for non-disadvantaged applicants. Earnings definitions and estimation methodology follow the main text. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A15: 2-year Applicants: Major at Intended Transfer Semester

	General	Science	Engineer	Health	Business	Educ	SocSci
$TransferTarget$	-0.12*	0.014	-0.0075	0.0044	0.028	0.0078	0.078**
	(0.068)	(0.033)	(0.030)	(0.030)	(0.046)	(0.020)	(0.039)
$E[Y_0|C]$	0.35	0.06	0.02	0.06	0.11	0.01	<0.01
$E[Y_1|C]$	0.23	0.07	0.01	0.06	0.14	0.02	0.08
Obs	53,698	53,698	53,698	53,698	53,698	53,698	53,698

	CompSci	Vocational	Art	Human	Other	Not Enrolled
$TransferTarget$	-0.018	0.011	0.0013	0.12***	0.12**	-0.24***
	(0.021)	(0.019)	(0.021)	(0.038)	(0.054)	(0.048)
$E[Y_0|C]$	0.02	0.03	0.00	0.04	0.05	0.24
$E[Y_1|C]$	<0.01	0.04	<0.01	0.16	0.17	<0.01
Obs	53,698	53,698	53,698	53,698	53,698	53,698

Notes: *** p<0.01, ** p<0.05, * p<0.1. Sample of 2-year transfer applicants. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2 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. $E[Y_0|C]$ and $E[Y_1|C]$ give the mean value of the dependent variable for compliers in the untreated and treated groups, respectively, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A16: 2-year Applicants: Field of Degree at Graduation

	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)
$E[Y_0|C]$	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)
$E[Y_0|C]$	<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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2 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 expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A17: 2-Year Applicants: Predicted Earnings by Major and Industry

	Major	Industry	Major and Industry
$TransferTarget$	1,742	-765	-106
	(1,536)	(1,361)	(2,369)
Obs	38,663	415,162	408,382

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Sample of 2-year applicants. Major defined as field of bachelor's degree within 6 years of transfer, and industry is defined at the 2-digit NAICSs code in each year of earnings. Predicted conditional earnings are estimated using all Texas workers as described in the text. Standard errors clustered at the application–college–year level in parentheses.

Table A18: 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)
$E[Y_0|C]$	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)
$E[Y_0|C]$	0.91	0.68	3.26	2.98
Obs	362,132	362,132	362,132	362,132

Notes:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A19: 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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 measured 8 years after intended transfer; bottom panel 13 years after intended transfer. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A20: 2-Year Applicants: Share and Number of HS Peers at College

	Intended Transfer Semester		Last College	
	% HS Peers	# HS Peers	% HS Peers	# HS Peers
$TransferTarget$	-0.063***	-30.7***	-0.026	-11.6
	(0.017)	(10.6)	(0.018)	(11.5)
$E[Y_0|C]$	0.15	72.78	0.14	68.33
Obs	53,698	53,698	53,698	53,698

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Intended Transfer Semester gives outcomes at the college of enrollment in the semester for which the student applied to transfer. Last College gives outcomes in last college of enrollment. % HS Peers measures the fraction of students enrolled in their college who are high school peers; # HS Peers measures the total number of high school peers who are enrolled in their college. $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors in parentheses.

Table A21: 2-Year Applicants: Sensitivity to Alternative Specifications

	Baseline	Bandwidth						Polynomial		SE Clustering		Kernel	
Panel A: BA within 6 years													
$TransferTarget$	0.18**	0.13	0.17	0.19**	0.16*	0.14*	0.13*	0.15	0.12	0.18*	0.18*	0.18**	0.12
	(0.088)	(0.15)	(0.12)	(0.095)	(0.080)	(0.075)	(0.069)	(0.16)	(0.087)	(0.096)	(0.099)	(0.087)	(0.087)
Obs	39,141	18,462	24,685	31,767	45,691	52,623	59,210	39,115	110,005	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**	-5,816	-9,336**	-7,148*	-7,148*	-7,284*	-8,966**
	(3,878)	(6,177)	(4,763)	(4,086)	(3,571)	(3,442)	(3,213)	(6,475)	(4,550)	(3,841)	(4,260)	(3,882)	(3,805)
Obs	535,877	252,756	337,830	434,070	625,711	721,360	812,092	535,539	1,328,780	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	0.72; 0.81	0.30	0.30	0.30	0.30
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Quad	Quad	Linear	Linear	Linear	Linear
Kernel	Tri	Tri	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	Appl Coll	Appl Coll	GPA Bin	Send Coll	Appl Coll	Appl Coll

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Sample of two-year college applicants. Panel A outcome is bachelor's completion within 6 years; Panel B outcome is conditional earnings. Quad = quadratic polynomial. Tri = triangular kernel; Uni = uniform kernel; Epa = Epanechnikov kernel. Standard errors in parentheses. Clustering levels listed in the bottom row.

Table A22: 2-Year Applicants: Sensitivity to Alternative Specifications

	Unconditional	Conditional	Sandwich	Log(Cond)	Log(Sand)
$TransferTarget$	-8,864	-10,869	-10,244		
	(6,591)	(8,352)	(8,454)		
	-7,772*	-8,379**	-7,643*	-0.182*	-0.123
	(4,149)	(4,173)	(4,181)	(0.102)	(0.0799)
$E[Y_0]$	39,185	52,071	55,227		
	38,773	50,814	53,893		
Obs	690,772	535,877	516,801	535,877	516,801
	690,772	535,877	516,801		
Controls	None	None	None	All	All
	FEs	FEs	FEs		

Notes:*** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at person–year level. Sample of transfer applicants from two-year colleges. Columns with "none" have no controls included, those with "FEs" include only fixed effects for application–college–year, and "all" have the full set of baseline controls. 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 expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A23: 2-year Applicants: Annual Earnings, by Number of Years Since Transfer for Individuals Observed 11+ Years

	Unconditional	Conditional	Sandwich
TransferTarget			
1–5 years	-2,948	-3,242	-2,806
	(2,618)	(2,613)	(2,704)
$E[Y_0 \mid C]$	21,788	27,465	30,626
Obs	162,960	132,601	124,628
6–10 years	-8,575*	-12,440**	-10,406**
	(5,118)	(4,894)	(4,891)
$E[Y_0 \mid C]$	42,514	55,903	57,524
Obs	162,960	127,562	124,525
11–15 years	-9,165	-8,249	-5,824
	(6,868)	(7,042)	(6,928)
$E[Y_0 \mid C]$	50,548	69,255	70,253
Obs	131,261	96,455	94,533
16+ years	-21,481*	488.3	6,915
	(12,689)	(12,044)	(12,015)
$E[Y_0 \mid C]$	67,593	77,096	76,800
Obs	131,261	96,455	94,533

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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 expected value of the outcome for compliers when untreated for the estimate directly above it, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A24: 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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A25: Summary Statistics of Analysis and Comparison Samples

| | 4-year college students | UT-Austin college students | 4-year to Flagship Transfer Applicants | | |
			Applicants to target college	Within the BW	Compliers
Male	0.45	0.46	0.51	0.51	0.55
FRPL	0.24	0.13	0.12	0.11	0.09
Nat. American	0.00	0.00	0.00	0.00	0.00
Asian	0.07	0.19	0.18	0.16	0.05
Afr. American	0.12	0.05	0.04	0.04	0.05
Hispanic	0.29	0.19	0.23	0.22	0.20
White	0.51	0.56	0.54	0.56	0.68
Two or More Races	0.01	0.01	0.01	0.01	0.01
Math test score	0.59	1.08	0.87	0.84	0.77
ELA test score	0.50	0.80	0.66	0.64	0.62
Cumulative GPA	2.6	3.1	3.31	3.2	3.2
Cumulative Credits	62	61	26.79	29.20	25.10
N	1,658,867	158,345	24,421	8,763	

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 Flagship Transfer applicants limits to those who applied to a flagship. Within the BW limits the sample to those within 0.4 grade points of the target college's GPA cutoff. The means of compliers are estimated from the main IV specification using the method of Abadie (2002). Note that sample sizes are smaller than the analysis sample because students with missing test scores are excluded from the summary statistics.

Table A26: 4-Year Applicants: Bachelor's Completion in Years Since Intended Transfer

	BA within X years since intended transfer					
	1 yr	2 yrs	3 yrs	4 yrs	5 yrs	6 yrs
Panel A: All 4-Year Applicants						
$TransferTarget$	-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
Panel B: 4-Year Applicants to Non-flagships						
$TransferTarget$	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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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). $E[Y_0|C]$ gives the expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A27: All and Non-flagship 4-Year Applicants: Annual Earnings, Pooled across All Years

	Unconditional	Conditional	Sandwich
Panel A: All 4-Year Applicants			
$TransferTarget$	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
Panel B: 4-Year Applicants to Non-flagships			
$TransferTarget$	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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at the person–year level. Unconditional earnings give average annual earnings over all quarters after the intended transfer year, where missing values are coded as zero. 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 expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A28: 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 transfer 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 A29: 4-Year Applicants: Observational Estimates of Transfer to UT-Austin on Conditional Earnings, by Years Since Intended Transfer

| | Estimate of Transfer to UT-Austin Relative to | | | | | | | | | |
| | Never Transfer | | Transfer 4y Later | | Transfer 2y Now | | Transfer 2y Later | | All CFs | |
	All TX	RD	All TX	RD	All TX	RD	All TX	RD	All TX	RD
TransferTarget										
1–5 years	-2,326***	-2,195***	1,849***	2,084***	84.56	572.8	1,188	119.6	-1,347***	-988.6***
	(360.3)	(438.1)	(468.2)	(628.0)	(592.2)	(557.4)	(941.4)	(991.4)	(289.8)	(320.0)
$E[Y_0|C]$	25,987	27,026	18,759	19,056	19,661	20,470	18,803	21,895	22,890	24,008
Obs	58,312	28,805	42,838	21,417	41,012	19,841	39,124	19,193	71,630	40,679
6–10 years	-4,209***	-3,934***	5,662***	4,769***	3,146**	3,016*	7,398***	2,590	-1,383**	-582.9
	(793.3)	(730.6)	(1,095)	(1,579)	(1,424)	(1,681)	(2,408)	(2,515)	(648.6)	(840.5)
$E[Y_0|C]$	60,967	61,658	44,799	45,796	45,663	47,096	41,240	46,753	54,905	54,622
Obs	44,819	23,624	33,682	17,644	32,097	16,286	30,587	15,826	55,449	33,711
11–15 years	-3,464**	-2,092	8,872***	6,450**	2,198	3,153	5,225	2,724	-793.8	436.3
	(1,392)	(1,263)	(1,777)	(2,220)	(2,282)	(2,349)	(3,960)	(2,100)	(1,138)	(961.9)
$E[Y_0|C]$	79,039	79,309	60,352	62,395	64,042	64,955	62,298	65,582	74,106	72,037
Obs	27,355	15,650	20,578	11,836	19,589	10,920	18,515	10,620	34,121	22,607
16+ years	-906.5	-1,191	5,152	4,950	-939.9	-2,374	-5,507	3,865	74.80	1,078
	(2,118)	(3,495)	(3,265)	(3,066)	(4,070)	(3,369)	(6,512)	(4,988)	(1,789)	(2,214)
$E[Y_0|C]$	88,632	88,774	72,448	76,637	74,927	76,149	79,320	79,265	84,804	82,718
Obs	14,443	9,526	10,600	7,199	9,751	6,371	9,009	6,134	19,119	14,856

Notes: *** p<0.01, ** p<0.05, * p<0.1. Observations are at person–year level. Each row limits to observations within given range of years since 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 transfer to any college in years observed. Transfer 4y Later = transfer applicant transferred to a non-target college in year for which she applied to transfer to target college. Transfer Other 4y = 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 2-year college in the year in which she applied to transfer to a target college. Transfer 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. All CF = all counterfactual pathways. RD columns limit the sample to students whose GPA is within 0.3 grade points of the admission cutoff of the target college to which they applied. $E[Y_0]$ gives the average earnings for untreated students. Standard errors clustered at the application-college–year level in parentheses.

Table A30: 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. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2 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 expected value of the outcome for compliers when untreated, estimated following Abadie (2002). Standard errors clustered at the application–college–year level in parentheses.

Table A31: 4-Year to Flagship Applicants: Predicted Earnings by Major and Industry

	Major	Industry	Major and Industry
$TransferTarget$	-2,392	1,902	-4,721
	(3,222)	(2,284)	(5,614)
Obs	8,060	96,585	90,659

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Sample of 4-year to flagship transfer applicants. Major defined as field of bachelor's degree within 6 years of transfer, and industry is defined at the 2-digit NAICSs code in each year of earnings. Predicted conditional earnings are estimated using all Texas workers as described in the text. Standard errors clustered at the application–college–year level in parentheses.

Table A32: 4-Year to Flagship Applicants: Sensitivity to Alternative Specifications

	Baseline	Bandwidth						Polynomial		SE Clustering		Kernel	
Panel A: BA within 6 years													
$TransferTarget$	-0.013	0.10	0.052	0.016	-0.046	-0.058	-0.073	0.26	0.21	-0.013	-0.013	-0.033	-0.13
	(0.15)	(0.19)	(0.17)	(0.15)	(0.13)	(0.12)	(0.11)	(0.27)	(0.24)	(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	10,068	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**	-4,928	-6,565	-11,299	-11,299	-11,912	-17,966**
	(7,289)	(8,949)	(8,083)	(7,722)	(6,748)	(6,283)	(5,765)	(11,556)	(10,531)	(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	133,101	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	0.48	0.40	0.40	0.40	0.40
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Quad	Quad	Linear	Linear	Linear	Linear
Kernel	Tri	Tri	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	Appl Coll	Appl Coll	GPA Bin	Send Coll	Appl Coll	Appl Coll

Notes: *** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. 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. Quad = quadratic polynomial of running variable. 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 A33: 4-Year to Flagship Applicants: Sensitivity to Alternative Specifications

	Unconditional		Conditional		Sandwich		Log(Cond)	Log(Sand)
$TransferTarget$	-11,353	-12,902**	-15,255	-17,203**	-16,480	-18,717**	-0.0223	-0.183
	(13,632)	(6,178)	(18,644)	(8,484)	(18,276)	(8,760)	(0.143)	(0.141)
$E[Y_0]$	43,487	44,821	61,505	63,287	65,718	67,615		
Obs	156,524	156,524	111,855	111,855	106,460	106,460	111,855	106,460
Controls	None	FEs	None	FEs	None	FEs	All	All

Notes:*** p<0.01, ** p<0.05, * p<0.1. Instrumental variable estimates from the main fuzzy RD specification described in subsection 3.2. Observations are at person–year level. Sample of transfer applicants from two-year colleges. Columns with "None" have no controls included; columns labeled "FEs" include fixed effects for application–year (application–college–year in your original note); columns labeled "All" include the full set of baseline controls. 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.

D 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 my main estimating equation, collectively referred to as $\mathbb{X}$,

$$E(TransferTarget_{ict}|GPA_i, \mathbb{X}_i) = \sigma_0 + \sigma_1 \mathbb{1}(GPA_i \geq T_{ct}) + m(GPA_i) + u_{ict} \qquad (5)$$

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 $\mathbb{X}$,

$$E[(1 - D_i)NeverTransfer_{ict}|GPA, \mathbb{X}] = \psi_0 + \psi_1 \mathbb{1}(GPA_i \geq T_{ct}) + n(GPA_i) + \omega_{ict} \qquad (6)$$

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 (6) is given by,

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

$$(7)$$

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