The Returns to College Admission for Academically Marginal Students

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Abstract

I combine a regression discontinuity design with rich data on academic and labor market outcomes for a large sample of Florida students to estimate the returns to college admission for academically marginal students. Students with grades just above a threshold for admissions eligibility at a large public university in Florida are much more likely to attend any university than below-threshold students. The marginal admission yields earnings gains of 22 percent between eight and fourteen years after high school completion. These gains outstrip the costs of college attendance, and are largest for male students and free lunch recipients.

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1 Motivation

The college wage premium has risen dramatically over the past 30 years. In 1980, college graduates earned roughly 50 percent more than high school graduates; by 2008, they earned 97 percent more.[1] A series of influential papers (e.g., Katz and Murphy (1992), Goldin and Katz (2008), Acemoglu and Autor (2011)) show that this change is at least in part the product of rapidly rising demand for skilled labor coupled with slower increases in supply. For instance, Goldin and Katz (2008, p. 297) estimate that between 1980 and 2005, the demand for college graduates increased by about 3.5 percent per year, while the relative supply of college graduates increased by only 2 percent per year. The net result was growth in the college wage premium at the rate of 0.9 percent per year.

Why has supply not kept pace with demand? One possible explanation is that the returns for students on the margin of college attendance are much lower than the average returns to college. This is consistent with the large body of evidence suggesting that many US primary and secondary schools do a poor job of preparing their students for college, and with evidence from structural models of schooling choice suggesting that relaxing financial constraints on postsecondary attendance would have little effect on educational attainment.[2] Alternatively, it may be the case that the returns to college for students on the margin of attendance are high, but that these students are constrained in some way. Possible constraints include short term credit constraints,[3] constraints based on limited access to or costly acquisition of information on the costs and benefits of college and the admissions process,[4] and constraints on the supply of places in appropriate postsecondary institutions (Bound and Turner 2007).

Distinguishing between these lines of reasoning is of critical importance for higher education policy. If many students are capable of making high-return human capital investments but cannot because they are constrained in some way, then policies aimed at relaxing these constraints will be enough to increase the supply of college graduates. If low marginal returns are the dominant story, then policies aimed at improving primary

[1]Source: Acemoglu and Autor (2011). Estimates adjust for changes in demographic composition.

[2]For evidence on college preparation, see Roderick, Nagaoka and Coca (2009). Structural models of schooling choice under credit constraints include Keane and Wolpin (2001) and Johnson (Forthcoming).

[3]See Belley and Lochner (2007), Stinebrickner and Stinebrickner (2008), Cameron and Taber (2004), or Lochner and Monge-Naranjo (2011). Long-term credit constraints, described in Carneiro and Heckman (2002) as children's inability to purchase better early-life inputs, likely also play a role in determining postsecondary educational attainment. These types of constraints are closely related to the low returns explanation, since they impede cognitive and non-cognitive development.

[4]See Avery and Kane (2004), Dynarski and Scott-Clayton (2008), and Jensen (2010).

and secondary education so that students emerge better-prepared for college are more appropriate. The key question is whether students who are only marginally prepared for college are able to realize economic returns large enough to justify the investment of time and money, and, if so, which constraints need to be relaxed so that more such students actually do make these investments.

This paper asks whether relaxing supply constraints through reductions in admissions standards at four year colleges would allow students to make investments with high private and social returns. I combine a rich dataset on high school, college, and labor market outcomes for a large sample of Florida high school students with a regression discontinuity design around a state-level GPA cutoff for admission to the Florida State University System (SUS) to estimate the returns to four-year college admission for students at the margin of admission to any SUS campus. I focus my analysis on Florida International University (FIU), an SUS campus that was especially generous in the way it computed the GPAs used for admissions during the period in question, and thus functioned as the SUS campus of last resort for many students.

I find that students just above the admissions threshold at FIU are 23.4 percentage points more likely to be admitted to FIU and 11.9 percentage points more likely to attend *any* SUS campus than students just below the admissions threshold. On average, students induced to attend college by threshold-crossing attend an SUS campus for an additional 3.8 years, and graduate at rates similar to those in the broader student population. Threshold-crossing produces a $372 gain in quarterly earnings between eight and fourteen years after high school completion, corresponding to a $1,593 increase in quarterly earnings per marginal admission. This is equal to 22 percent of expected earnings just below the threshold. Driving earnings gains are large effects for male students ($4,191 per marginal admission) and free lunch recipients ($2,695 per marginal admission). Gains for female students and students who do not receive free lunch are close to zero. Combining estimates of earnings effects with institution-level IPEDS data on the private and social direct costs of postsecondary attendance suggests that the private and social internal rates of return associated with the marginal college admission are substantially higher than market interest rates. I interpret my results as evidence that supply constraints on spots in state universities bind in the sense that they prevent students from making investments that would have high private and social returns.

This paper builds on existing work in a number of ways. Its main contribution is to present the first plausibly causal estimates of the earnings gains associated with access to four-year college for the policy-critical group of moderate- to low-achieving students at the margin of college attendance. The closest precedent in the literature on the earnings effects of education is Hoekstra (2009).[5] Hoekstra uses a test score admissions cutoff to estimate the returns to attending a flagship state university. His analysis differs from what is presented here in that a) students who are not admitted to the flagship university most likely attend other colleges, although Hoekstra cannot verify such attendance directly with the available data, and b) students near the admissions cutoff in his analysis have stronger academic backgrounds than students near the admissions cutoff in the present paper. The average combined SAT score for students near the cutoff in the Hoekstra study was roughly 1000 on the pre-1995 SAT,[6] which corresponds to a score of 1100 on the current test (College Board 2013). The average score for students near the cutoff in the present analysis is 839, a score that would place a student in the 21st percentile of college-bound seniors in 2011 (College Board 2011).

Other authors use regression discontinuity designs to estimate the labor market effects of schooling in other contexts. Öckert (2010) uses admissions cutoffs to estimate the effect of a year of college attendance on earnings for Swedish students applying to college in 1982. Ozier (2011) uses a test score cutoff to estimate labor market returns for students admitted to secondary school in Kenya. Although the designs in these papers are similar to the one employed here, the educational systems and labor markets they explore differ substantially from current conditions in the US. Such distinctions are important because, as discussed in Card (1999), Meghir and Rivkin (2011), and Carneiro, Heckman and Vytlacil (2011), credible use of instrumental variables estimates for policy evaluation depends on finding an instrument that shifts students across the same margin as the proposed policy. The instrument here is grade threshold-crossing for students with grades close to the cutoff value. This instrument focuses tightly on academically marginal students and offers the answer to a concrete policy question: how does college admission affect earnings for students who attend if we relax public university supply constraints through a marginal reduction in admissions standards?

An additional contribution this paper makes is to compare earnings gains to the private and social costs associated with the marginal admission. My calculations suggest that both the private and social internal rates of return to the marginal admission are large. This is because the early-career earnings losses associated with admission are relatively small compared to later gains, and because the increased costs of attending a four-year college are partially offset by decreases in expenditures on community college. This analysis draws on a match between college attendance microdata and

[5] Kane (2003) and Van der Klaauw (2002) also use regression discontinuity strategies in the context of college attendance, but focus on academic outcomes such as attendance and graduation rather than labor market outcomes.

[6] Personal communication with author, February 10th 2012.

panel data on institution-specific per-student tuition receipts (net of financial aid) and total educational expenditures. With the exception of Ockert (2010), who considers the effects of admissions on forgone earnings and the private receipt of educational subsidies, prior work in this literature does not address this question.

The paper proceeds as follows. Section two describes the policy environment that gives rise to the admissions cutoff, section three describes my econometric strategy, and section four describes the academic and labor market data I use in my analysis. In section five I present my core regression discontinuity results and estimates of internal rates of return. Section six concludes.

2 Policy environment

There are 11 campuses in the Florida State University System (SUS). In the late 1990s and early 2000s, when students in this analysis were applying to college, the SUS enrolled approximately twenty to twenty-five thousand first-time-in-college freshmen each year. The middle 50 percent of these enrollees had SAT scores ranging from roughly 1000 to 1250. These scores exceed scores for college-bound high school seniors nationwide, for whom the interquartile range in 2011 was 860 to 1170. This paper focuses on Florida International University, a large SUS campus located in Miami. Students at FIU had test scores similar to those of other SUS students and entering students across the country: during the period in question, FIU enrolled about 1,500 first-time-in-college students per year, with an interquartile SAT range of about 950 to 1200, depending on the year.[7] Outcomes for FIU students during this period were also similar to outcomes for college students nationally: the six-year graduation rate for FIU students in the 2001-2002 entering class was 49 percent, close to the 55 percent national graduation rate for students entering four-year public colleges in that year.[8] Table A1 presents descriptive statistics for enrolled and admitted students at FIU in the 2000-2001 school year.

Though SUS campuses are allowed substantial discretion in admissions policies, lower bounds on student qualifications are governed by statewide rules. To qualify for standard admission students must have grades above a sliding-scale cutoff value that decreases in standardized test scores. In practice, nearly all students with grades close

[7]For freshmen enrollment in 2000-2001, see State University System of Florida Board of Governors (2003). Henceforth I will refer to documents from this source using the acronym SUSBOG. For interquartile SAT ranges for enrolling students, see SUSBOG (2001). Equivalent statistics for all relevant years are available in SUSBOG (2012). For national SAT interquartile ranges, see College Board (2011).

[8]National graduation rates from NCES 2010, Table 341.

to the admissions cutoff had combined SAT scores of less than 970 and so faced a GPA cutoff of 3.0. See appendix table A2 for a mapping of SAT scores to GPA requirements.[9] Students with grades above the cutoff are not guaranteed admission. Similarly, students with grades below the cutoff value may still be admitted, but only through a 'student profile assessment' that considers factors like family background, high school quality, and special talents. The number of students admitted through profile assessment is limited to 10 percent of total system wide admissions.[10]

Though the same admissions statute applies to all SUS campuses, the rules used for GPA determination are not standardized across campuses. In the late 1990s and early 2000s, FIU was substantially more generous in its GPA calculations than other SUS schools. As a result, students just below the FIU cutoff were typically not eligible for standard admission at any SUS campus, and this asymmetry spilled over into admissions outcomes. FIU thus functioned as the SUS campus of last resort for students bound by the threshold-crossing admissions constraint: if they were not admitted to FIU, they were not admitted to any SUS campus.

[9] As noted in table A2, 19 percent of applicants with grades close to the admissions cutoff did not take the SAT. It is likely that many of these students took the ACT instead. There is a similar sliding scale of GPA cutoffs based on ACT scores. Because I do not have access to data on ACT scores, I assign non-SAT takers a grade cutoff of 3.0.

[10] Source: Florida Administrative Rule 6C-6.002. Notably, race, gender, and country of origin are excluded from profile assessments.

Table 1: FIU and FSU admissions GPAs for joint applicants

A. GPAs for joint applicants		
	Mean	SD
HS GPA	2.98	0.39
FIU GPA	3.40	0.50
FSU GPA	3.19	0.62
B. Status relative to grade cutoffs		
	FSU=1	FSU=0
FIU=1	0.231	0.462
FIU=0	0.004	0.303
C. Admissions outcomes		
	FSU=1	FSU=0
FIU=1	0.079	0.619
FIU=0	0.012	0.290

Panel A: Sample consists of all students who applied to both FIU and FSU for the year following their senior year in high school. HS GPAs are unweighted cumulative GPAs provided by high schools. FIU and FSU GPAs are university-computed and taken from applications data. N=5,618. Panels B and C: Sample consists of students who applied to both FIU and FSU for the year following their senior year and had FIU GPAs within 0.3 grade points of their individual-specific admissions cutoff. Cell values in panels B and C sum to one within each panel. N=1,614.

Table 1 illustrates this process using the sample of students who applied to both FIU and Florida State University (FSU), the SUS campus with which FIU had the largest number of same-year cross-applicants in the analysis dataset.[11] Panel A reports mean unweighted high school GPAs, FIU application GPAs, and FSU application GPAs for the set of 5,618 cross-applicants. The mean high school GPA for this group is 2.98, compared to a mean FIU GPA of 3.40 and a mean FSU GPA of 3.19. Clearly neither weighting procedure maps directly to unweighted grades computed by high schools, and the formula FIU uses to compute admissions GPAs from high school transcripts is more generous than the formula used by FSU.

The relative generosity of FIU GPAs has direct consequences for the status of applicants relative to their required grade cutoffs. Panel B of Table 1 displays the distribution of position relative to the cutoff for marginal FIU applicants–defined here as students with GPAs within 0.3 grade points on either side of the cutoff– who also applied to

[11]As reported in Table A3, similar grading asymmetries are present at all other SUS campuses with which FIU had a substantial number of cross-applicants.

FSU. Of the 69.3 percent of marginal FIU students whose grades surpassed the FIU cutoff, one third (23.1 percent) also surpassed the FSU cutoff. But of the 30.7 percent of marginal FIU students whose grades fell below the cutoff, only one in seventy-seven (0.4 percent) surpassed the FSU cutoff. Panel C presents parallel results for admissions. Of the 69.8 percent of marginal students who were admitted to FIU, one ninth (7.9 percent) were also admitted to FSU. But of the 30.2 percent of marginal students who were rejected from FIU, less than one in 27 was admitted to FSU. The net result of grading generosity at FIU is that students just above the grading threshold at FIU are much more likely to be admitted to any state university campus than students just below.

3 Econometric strategy

I recover estimates of the earnings effects of the marginal college admission using a fuzzy regression discontinuity (FRD) design that compares outcomes for students with grades just below the grade cutoff for FIU admission to outcomes for students with grades just above the cutoff. The intuition is that students with grades very close to the cutoff on either side are comparable in terms of the observable and unobservable (to the econometrician, in this dataset) determinants of wages, but that those just above the cutoff are more likely to be admitted to college.

In FRD designs, threshold crossing causes a discontinuous jump in the probability of treatment, but this jump is not from zero to one. The idea here is that some students with grades below the cutoff are admitted to college, and some students with grades above the cutoff are not. Because students whose admission status responds to threshold crossing may differ from other students with similar grades, the estimates I obtain should be interpreted as a local average treatment effect for students at the academic margin of admission. One way to think of this group is as the group of 'compliers' with the admissions cutoff policy (Angrist, Imbens and Rubin 1996).

I estimate specifications of the following form. Let y_i be post-college earnings for individual i, g_i be the distance between the grades for individual i and the cutoff he faces, $f()$ be some smooth function, and S_i be a dummy variable for college admission. I estimate the equation

$$y_i = \alpha + f(g_i) + \beta S_i + u_i, \tag{1}$$

instrumenting for S_i with $Z_i = 1[g_i \geq 0]$. As discussed in the next section, I use average quarterly dollar earnings between eight and fourteen years after high school completion (roughly ages 26 to 32) as the earnings outcome of interest in most cases. I also present results from modified versions of (1), in which I a) replace S_i with measures of educational attainment such as years of SUS attendance or the receipt of a BA degree, b) estimate the reduced-form effect of threshold-crossing by substituting Z_i for S_i, or c) add a vector of individual-specific controls X_i. The X_i may increase precision by decreasing the variance of residuals but are not required for identification.

When estimating this equation, I restrict my sample to students with grades within a relatively narrow window around the cutoff value. The goal of this restriction is to avoid identifying local effects using variation far from the cutoff value (Imbens and Lemieux 2008). I approximate the slope of earnings in grades $f(g_i)$ using polynomial functions. In general, I restrict coefficients on polynomial terms to be the same above and below the cutoff, although I also present some specifications in which coefficients are allowed to vary above and below. This restriction is motivated by the observations that a) there is little evidence that polynomial terms change above and below the cutoff in core specifications, and b) allowing coefficients to vary entails losses in the precision of discontinuity estimates in some cases. As is standard in the regression discontinuity literature (Lee and Lemieux 2010), I present results for a variety of window widths and polynomial degrees. My estimates are robust to the specifications I present here, as well as to other similar specifications.

Because the FIU admissions office rounds grades to the nearest hundredth of grade point, the distribution of the running variable g_i is discrete rather than continuous. Following Lee and Card (2008), I compute standard errors that allow for clustering within each value of g_i due to random misspecification error. Further, as I show in section 5.1, the grade distribution contains heaps at each tenth of a grade point (i.e., 2.9, 3.0, 3.1, etc.). In specifications using narrower bandwidths, a relatively small number of these heaps can account for a large fraction of the data. As discussed in Cameron, Gelbach and Miller (2008), inference using analytic cluster-robust variance estimators can lead to over-rejection when the number of clusters is small. To account for this I present the usual cluster-robust estimates of standard errors, but conduct inference using the clustered wild bootstrap-t procedure that Cameron et al. recommend. Inferences drawn using the wild bootstrap tend to be more conservative than those implied by the analytic cluster-robust variance estimator. Appendix B provides the details of the bootstrap procedure.

For this analysis to produce consistent and interpretable results, several conditions must hold. First, the interpretation of β as a mean effect for compliers requires the monotonicity condition that there are no individuals who are admitted if and only if they have grades below the cutoff (Angrist et al. 1996). This condition seems plausible. Second, threshold-crossing variable Z_i must be conditionally uncorrelated with unobservable earnings determinants u_i when g_i is within some narrow window around zero. As discussed in Lee and Lemieux (2010), this restriction will typically hold if a) applicants do not attempt to manipulate grades so as to just surpass the cutoff score, or b) applicants do attempt grade manipulation, but manipulation is imprecise. In either case, earnings determinants other than college attendance will change smoothly near the cutoff value, and the discontinuity will reflect only the desired treatment effect.

4 Data

I use data on six cohorts of public high school 12th graders from 15 Florida counties. The 15 counties include Miami-Dade and Broward counties, the two largest school districts in the state and among the largest in the country. Students in my sample graduated from high school between 1996 and 2002, with the 1997 cohort omitted. I obtained this data through an agreement with the Florida Department of Education.[12] The data include basic demographic information, high school, community college, and state university transcript and degree information, administrative application data for the state university system, and data from surveys administered to high school seniors on their post-high school plans.[13] The data also include earnings information from Florida Unemployment Insurance records through the first quarter of 2010. Appendix C describes the data sources and procedures used to construct key variables.

Strengths of this data include the detail of the academic records for public institutions and the relatively long panel component of the earnings data, which tracks students for up to 14 years after their 12th grade year, or approximately age 32. There are two main weaknesses. First, educational outcomes are censored for students who do not attend Florida public institutions. Second, earnings outcomes are censored for students who leave the state and for students who do not work. So long as censoring is uncorrelated with threshold-crossing, this will not compromise an analysis of earn-

[12] I did not have access to data from other counties at the time of this analysis. I did have access to data on the 2004 12th-grade cohort, but I exclude them from this analysis because I observe their earnings at most five years out of high school. This is too early to effectively evaluate the labor market effects of postsecondary education, particularly given that many students in this sample take more than four years to complete college.

[13] It is important to note that I do not have data on the timing of surveys within the senior year. Surveys were administered on different dates in different high schools, and data administrators did not maintain a record of the survey date. It is possible some surveys were administered before students were aware of admissions decisions.

ings effects for in-state labor market participants. However, censoring of educational and earnings outcomes could bias my analysis if the likelihood of censoring changes discontinuously around the grade cutoff. I address questions of earnings censoring in section 5.1 and find no evidence that the probability of censoring is related to threshold-crossing. Surveys on post-high school plans indicate that few students near the cutoff attend in-state private or out-of-state colleges. In section 5.2, I show that survey responses do not change discontinuously near the cutoff. The absence of differential earnings censoring also suggests a limited role for differential censoring of out-of-state educational outcomes. If students below the threshold were more likely to attend college out-of-state, they might also be more likely to stay out-of-state to work, which I do not observe.

Several data construction choices are important to highlight. First, I take mean quarterly dollar earnings for labor force participants between eight and fourteen years after high school (generally between the ages of 26 and 32) as the outcome variable of interest. Focusing on outcomes eight or more years after graduation gives students time to complete formal schooling and enter the labor market prior to earnings measurement. As I show in section 5.3, the gap in earnings between above- and below-cutoff students is relatively stable over this period, so averaging earnings seems reasonable. However, I also present robustness checks that estimate separate effects using earnings observations from between eight and ten and between eleven and fourteen years years following high school completion. I use dollar earnings (deflating to 2005 dollars using the quarterly PCE) rather than log earnings to facilitate comparisons with costs. To reduce the impact of very high earnings outliers on my results, I topcode mean earnings at the 99th percentile within each cohort. I present robustness checks that show that my findings are robust to raising or lowering the topcoding percentile.

Second, when counting years and terms of SUS and CC attendance for a particular student, I use attendance records from the first through sixth years after high school for that student. I choose this cutoff value so that I can construct measures of educational attainment that are consistent across cohorts and institution types. Figure A1 shows that, although some students continue to attend school more than six years after high school completion, differences in enrollment patterns between above- and below-cutoff students are fairly small beyond that point.[14] I classify students as having attended SUS or CC in a given year if they are ever enrolled in an institution of the relevant type

[14]Estimates of the effects of threshold crossing on SUS outcomes through seven years following high graduation are available upon request, and show that extending the analysis timeframe does not meaningfully affect estimated discontinuities.

during the year in question. I count terms of SUS and CC enrollment by summing full time terms (given a weight of one) and part time terms (given a weight of one half).

Table 2 presents sample means for key variables in the full sample of 12th graders, the sample of FIU applicants, the sample of marginal FIU applicants, and the subsample of marginal FIU applicants for whom outcome period earnings data are available. I label this last group the 'labor force sample.' FIU applicants are heavily Hispanic and similar to other high school graduates in terms of rates of free lunch receipt. In terms of academic performance as measured by high school grades, marginal FIU applicants resemble the broader population more than they do other FIU applicants. The mean SAT score for marginal applicants is 841, more than 100 points below the mean score for all applicants. 51 percent of marginal applicants attend an SUS institution and 50 percent attend a community college in the year following the 12th grade year, compared to 9 percent who express the intent to attend a private college in Florida or any college outside of Florida.[15] Finally, 80 percent of marginal applicants show up later in the labor force sample. These students tend to be similar in terms of observable characteristics to the full sample of marginal applicants. For consistency, I focus on these observations in the bulk of my analyses. I present evidence that threshold-crossing is uncorrelated with both selection into the labor force sample and the fraction of censored earnings observations in section 5.1.

[15]Students may attend both an SUS institution and a CC institution in the same year. Students are asked about their postsecondary plans during their senior year of high school. I do not know precisely when during this year they respond to the question.

Table 2: Sample description

	All	FIU	Marg.	LF sample
White	0.40	0.18	0.15	0.15
Black	0.27	0.26	0.33	0.32
Hispanic	0.28	0.5	0.47	0.48
Male	0.48	0.37	0.36	0.35
F/R Lunch	0.4	0.43	0.46	0.46
HS GPA	2.63	2.92	2.72	2.72
SAT	N/A	943	841	839
Attend SUS next year	0.16	0.59	0.51	0.51
Attend CC next year	0.31	0.37	0.5	0.51
Survey: attend non-FL college	0.06	0.04	0.03	0.03
Survey: attend FL priv. college	0.04	0.08	0.06	0.06
In LF sample	0.68	0.78	0.80	1.00
Frac. quarters with earnings obs.	0.53	0.64	0.67	0.83
N	351198	24690	8147	6542

Sample means for selected student populations. 'FIU' refers to all FIU applicants. 'Marg.' refers to marginal FIU applicants. 'LF sample' refers to marginal FIU applicants for whom outcome-period earnings data are available. 'Frac quarters with earnings obs.' is the fraction of quarters during the outcome period with uncensored (positive) earnings observations.

5 Results

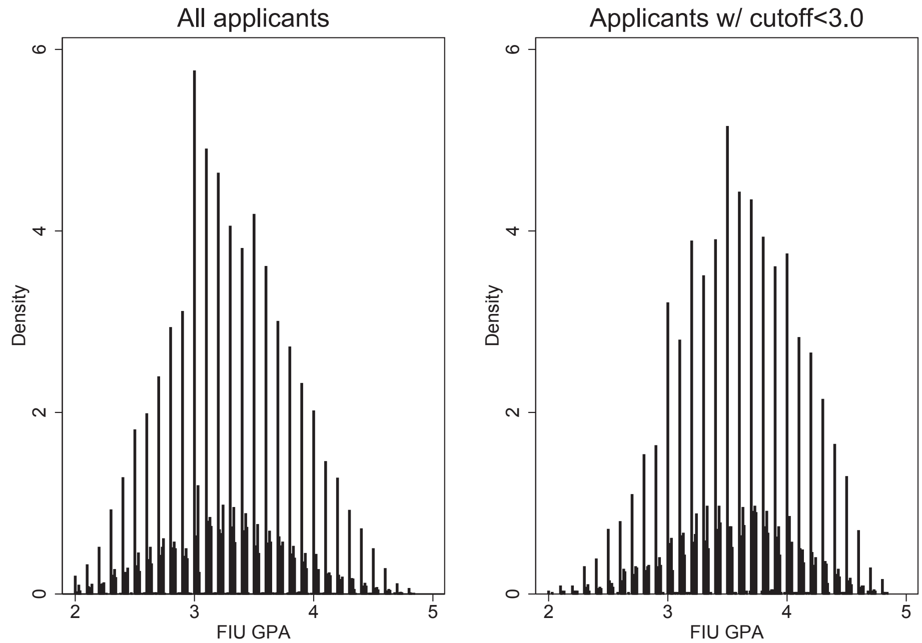
5.1 Robustness of the RD design

There are two major concerns about this research design. The first, standard in the regression discontinuity literature, is that students, teachers, or administrators may manipulate grades so that the distribution of unobservable earnings determinants is discontinuous at the grade cutoff. Because GPAs are computed within admissions offices and computation procedures vary across SUS campuses, it would likely be fairly difficult for students to calibrate their grades so that they end up above the cutoff for admission to a specific institution. But it is possible, and in principle it might also be possible for admissions officers to manipulate grade calculations in favor of particular students. If students with better earnings prospects clump above the cutoff, my estimates of earnings effects will be biased upward. Second, it is possible that there is differential selection into the labor force sample above and below the cutoff (i.e., differential censoring) due either to labor supply choices for Florida residents or to differential outmigration. There are number of possible stories about how this could bias estimation of earnings effects. If high earning below-threshold students are more likely to leave Florida for school, this would bias my estimates upwards. Alternatively, if high-earning above-threshold students are more likely to take out-of-state jobs, this would bias my estimates downwards.

To address these concerns, I consider two tests that are standard in the regression discontinuity literature. The first test is to look for discontinuities in the density of grades at the cutoff point (McCrary 2008). The argument is that if some students manipulate their grades to surpass the threshold, the density of the grade distribution will be higher just above the cutoff than just below. Unfortunately, this exercise is unhelpful if distributional discontinuities at the cutoff point can be traced to other factors. That is the case here. For most individuals, the relevant cutoff GPA is 3.0. This corresponds to an unweighted 'B' average– a benchmark grade level that teachers and FIU evaluators may be more likely to assign or students more likely to work to obtain for reasons exogenous to the admissions process than other nearby GPAs.

The empirical distribution of grades is consistent with this idea. The left panel of Figure 1 shows a histogram of FIU GPAs for all applicants with SAT scores. One thing that jumps out is the heaping of observations at each tenth of a grade point. I return to this below. Apropos of the McCrary test, the other notable feature of the distribution is a sharp discontinuity in the grade distribution at the 3.0 grade level. Formally, the null hypothesis of no discontinuity in the probability density function at that point is easily rejected at the one percent level. The discontinuity could be the result of strategic cutoff-crossing, or of an alternative process related to the 'B' grade. The jumps and drops in the density at non-cutoff points (e.g, at GPA of 3.5), suggest the latter story may be important.

Figure 1: GPA histograms



Histograms of admissions GPAs all sample students and sample students with cutoffs of less than 3.0. Students with grades below 2.0 are dropped. Separate columns are shown for each GPA bin; bin width is 0.01 grade points.

Looking only at students for whom the 3.0 cutoff is not in effect provides further evidence of this. The right panel of Figure 1 shows a histogram of FIU GPAs for students with cutoff GPAs of less than 3.0. Because these students by definition have higher SAT scores than students facing the 3.0 cutoff, the entire grade distribution is shifted to the right. However, there remains a sharp discontinuity at the 3.0 grade level, which cannot be the result of grade manipulation with respect to the admissions cutoff. The null hypothesis of continuity in the probability density function at 3.0 is rejected at the one percent level here as well.

A more informative visual test for grade manipulation in the context of a running variable that may be discontinuously distributed for exogenous reasons is to look for continuity in the ratios of the conditional densities to the unconditional density,

$$\frac{f(g|x)}{f(g)}. \tag{2}$$

$f(g)$ and $f(g|x)$ are the unconditional and conditional densities of g_i, respectively.

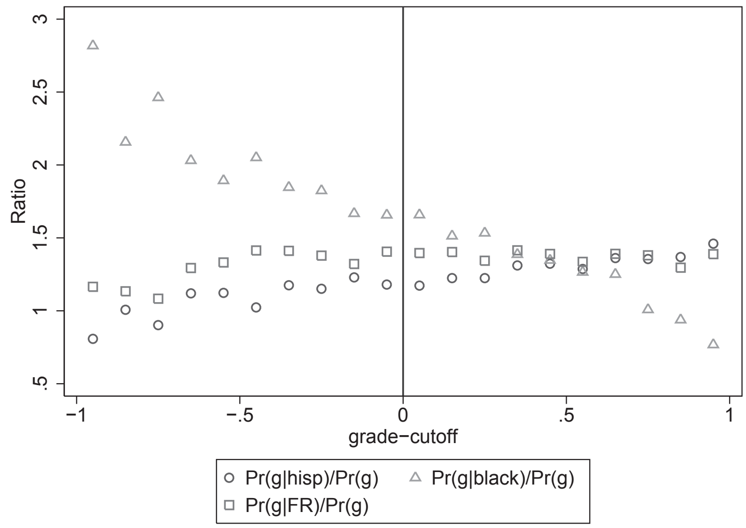
To understand this test, assume that observable and unobservable wage determinants (x, u) have some continuous unconditional joint distribution $h(x, u)$. A sufficient condition for unbiased RD estimation is that the conditional joint distribution $h(x, u|g)$ be continuous in g (Lee and Lemieux, 2010). Via Bayes' rule,

$$h(x, u|g) = h(x, u)\frac{f(g|x, u)}{f(g)} \tag{3}$$

Thus $h(x, u|g)$ is continuous if the ratio of the conditional to unconditional densities is continuous. Equation 2 tests this requirement using the observable wage determinants only. This test is in a sense more direct than looking only at the continuity of $f(g)$, since it focuses specifically on the object that determines the continuity of wage determinants in grades. The intuition is also clear. If discontinuities in the grade distribution are due to a process that is exogenous to the determination of the treatment, discontinuous jumps in the conditional distributions should be matched by discontinuous jumps in the unconditional distribution. The ratio of the two densities should be continuous even if each individual density is not.

Figure 2 presents the density ratios described in equation 2 for three different conditioning groups: black students, Hispanic students, and students who receive free or reduced price lunch. Each point represents the ratio of the proportion of observations in the sample of students with the stated characteristic to the proportion of all observations within a 0.1 grade-point bin. Consistent with a valid RD design, each density ratio is continuous around the cutoff value.

Figure 2: Ratios of conditional to unconditional grade densities



Ratios of conditional to unconditional grade densities by distance relative to the admissions cutoff. Densities are computed with bins with a width of 0.1 grade points.

The continuity of the density ratios is closely related to the second standard test of RD validity, which is to test for the balance of observable covariates across the threshold.[16] Figure 3 and Table 3 present estimates of the effects of threshold crossing on covariate means and selection into the analysis sample. Notably, these covariates include the number of other SUS campuses to which students applied in the year they applied to FIU, and the number campuses where they were eventually accepted.[17] If students are aware of their status relative to the grading threshold and the increased probability of FIU acceptance that threshold-crossing entails, threshold-crossing will at least in some cases be associated with a change in the expected value of sending out applications to other campuses, and therefore with application behavior. As part of this exercise, I also test whether threshold-crossing is associated with any change in the probability of presence in the labor force sample.

Here and in what follows, I present results obtained using five different regression discontinuity specifications. The 'Main' specification uses observations within 0.3 grade

[16]To see this, consider some binary variable $X \in \{0,1\}$. Then substituting for $f(g|X=1)$ using Bayes' rule yields $\frac{f(g|X=1)}{f(g)} = \frac{Pr(X=1|g)}{Pr(X=1)} = \frac{E[X|g]}{E[X]}$. Thus the density ratio for a given g is equal to the conditional mean of X at that point multiplied by a scalar that is the same for all g.

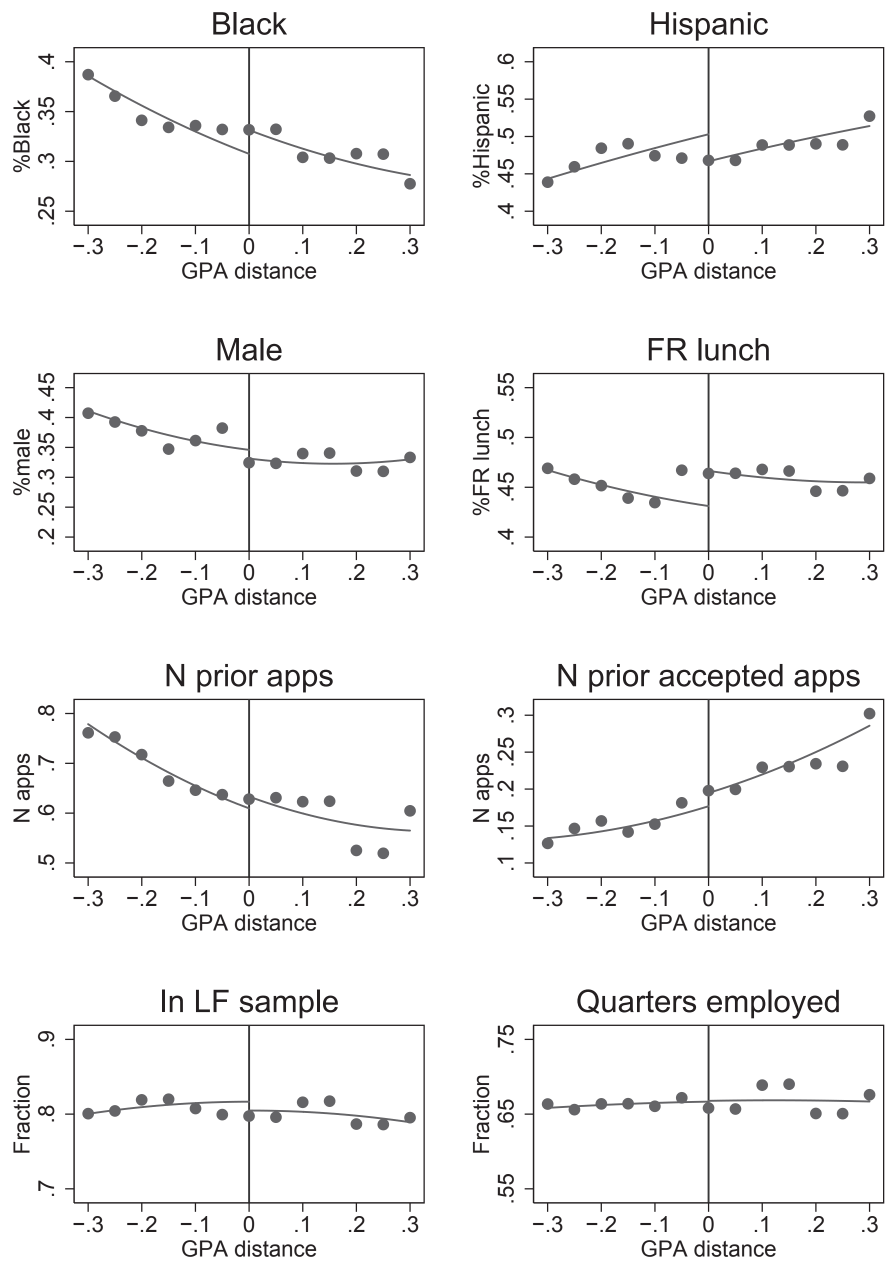
[17]I consider only applications prior to or contemporaneous with the FIU application. Clearly the results of FIU applications will affect students' application decisions in subsequent terms.

points on either side of the threshold and controls for a second degree polynomial in distance from the cutoff. The 'Controls' specification is identical to the main specification, but adds controls for gender, race, free lunch status, and 12th-grade cohort. The 'BW=0.15' specification uses observations within 0.15 grade points above and below the cutoff and allows for a linear trend in distance from the cutoff. The 'BW=0.5' specification uses observations within 0.5 grade points on either side of the cutoff and allows for a quartic polynomial in distance from the cutoff. Finally, the 'Local Linear' specification is identical to the main specification but allows for linear slope terms in distance from the cutoff that differ above and below the threshold. Results are generally consistent across specifications, so I focus on the main specification in the text and when constructing fitted values in figures. Recall from section 3 that regression tables report analytic cluster-robust standard errors, but that p-values come from a clustered wild bootstrap-t procedure. For this reason, standard errors and p-values may move in opposite directions in some cases.

I find no evidence of discontinuities in covariates or a linear index of covariates at the threshold: out of the thirty hypothesis tests in panels A and B of Table 3, three reject the null at the ten percent level. Nor do I find evidence of differential selection into post-college employment, whether measured as the presence of at least one valid earnings observation or as the fraction of valid earnings observations. Threshold-crossing does not appear to affect whether students participate in the in-state labor market. These findings are consistent with a valid RD design that is also unbiased by censoring on the outcome variable.

The absence of differential selection into the earnings sample also provides insight into problems with interpretation of first-stage results that might arise due to the censoring of out-of-state educational outcomes. If below-threshold students were more likely to leave Florida to attend college, one might expect many them to remain out of state after college, leading to an increase in labor force participation at the cutoff value. That this is not evident here suggests that this kind of educational outcome censoring is not affected by threshold-crossing. This is consistent with the analysis of survey results presented in section 5.2 below.

Figure 3: Covariate balance and employment effects



Means of demographic variables and labor force participation by distance relative to the cutoff. Lines are fitted values based on Main specification. Dots, shown every 0.05 grade points, are rolling averages of values within 0.05 grade points on either side that have the same value of the threshold-crossing dummy.

Table 3: Validity of RD design

Dependent Var.	Main	Controls	BW=0.5	BW=0.15	Loc. Lin.
A. Student characteristics					
Black	0.024		0.017	0.027	0.027
	(0.018)		(0.020)	(0.019)	(0.022)
Hisp.	-0.036*		-0.018	-0.022	-0.038*
	(0.021)		(0.022)	(0.024)	(0.022)
F/R lunch	0.035		0.036	0.018	0.037
	(0.024)		(0.025)	(0.026)	(0.028)
Male	-0.015		-0.020	-0.054**	-0.007
	(0.017)		(0.019)	(0.020)	(0.018)
Index	6.2		19.8	20.2	1.9
	(31.3)		(31.4)	(40.6)	(35.6)
N	6542		9659	3294	6542
B. Other SUS applications					
Acceptances	0.018	0.016	0.013	0.007	0.022
	(0.023)	(0.022)	(0.025)	(0.025)	(0.026)
Total apps	0.024	0.015	-0.002	-0.013	0.034
	(0.042)	(0.037)	(0.045)	(0.050)	(0.044)
N	6542	6542	9659	3294	6542
C. Labor force participation					
In LF sample	-0.012	-0.017	-0.021*	-0.018	-0.013
	(0.012)	(0.013)	(0.014)	(0.017)	(0.013)
Fraction of quarters in LF	0.001	0.000	-0.010	-0.029	0.002
	(0.015)	(0.016)	(0.015)	(0.011)	(0.015)
N	8147	8147	12085	4083	8147

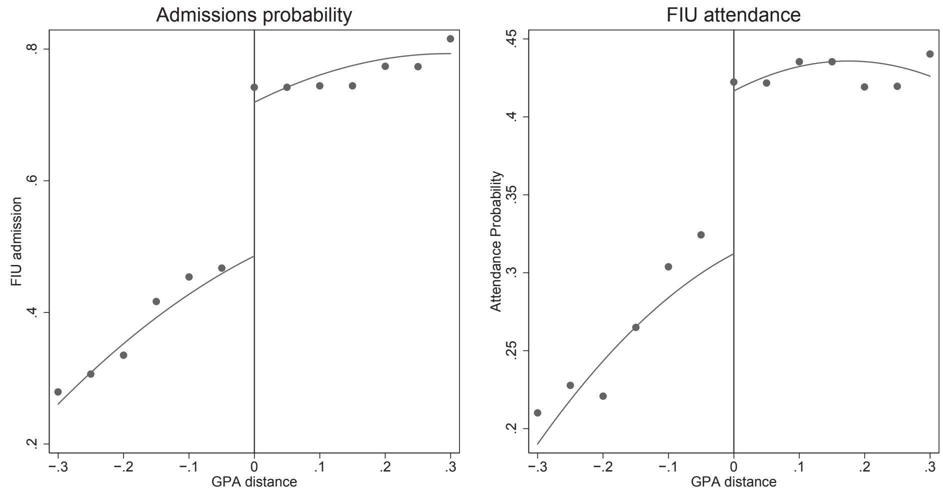
Significance: ***: 1% **: 5% *10%. Standard errors are clustered within grade bins. p-values are calculated using a clustered wild bootstrap-t procedure described in section 3 and Appendix B. 'Controls' specification is omitted from panel A because dependent variables are part of the control set. 'Index' is a linear index of race dummies, free lunch status and gender dummies, and cohort effects, with weights given by coefficients from a regression of earnings on these variables plus a quadratic in distance from the cutoff. Panel C looks at labor force participation 8 to 14 years after HS. 'In LF sample' is a dummy equal to one if a marginal applicant shows up later in the earnings sample. 'Fraction of quarters in LF' is equal to the proportion of non-censored quarterly observations for each student.

Before moving on, I briefly turn to the implications that heaping in the grade distribution has for the analysis. Heaping will only bias regression discontinuity estimates to the extent that it creates imbalances in earnings determinants across the threshold. Standard tests show little evidence of this. However, Barreca, Guldi, Lindo and Waddell (2011a) argue that if heaping is associated with determinants of the outcome variable it can create biases even when the regression discontinuity passes standard balance tests. Barreca et al. (2011a) and Barreca, Lindo and Waddell (2011b) consider several ways to correct for possible biases, including 'donut' RDs that omit heaped points and separate intercepts and trends for heaped and unheaped data. I implement these tests in section 5.5.

5.2 Academic outcomes

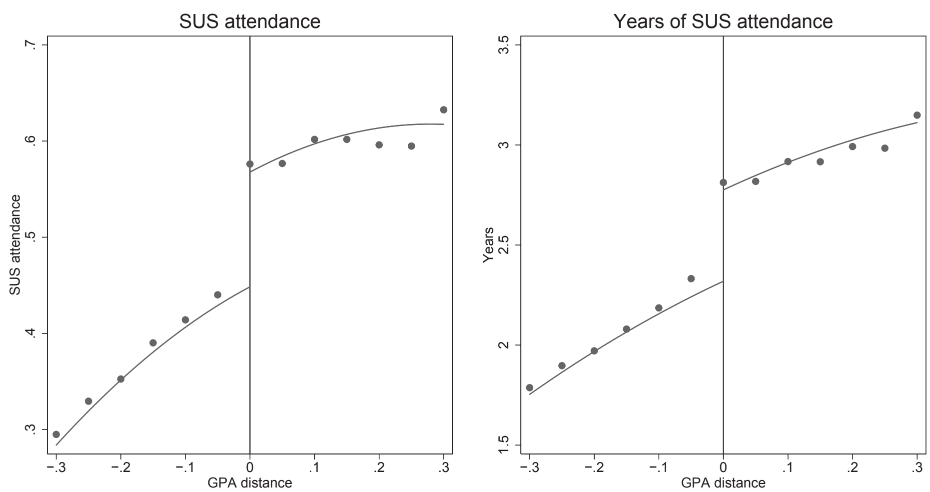
Table 4 presents regression discontinuity estimates of the effects of threshold-crossing on academic outcomes including SUS admissions, attendance, and graduation, as well as community college attendance and survey responses about post-college plans. Figure 4 shows the effect of threshold crossing on admission to FIU and FIU attendance. Students above the threshold are 23.4 percentage points more likely to be admitted to FIU and 10.4 percentage points more likely to attend than students just below the cutoff. As shown in Figure 5, students just above the cutoff are 11.9 percentage points more likely to attend *any* SUS campus, and attend for an average of 0.457 more years than students just below. This indicates a high degree of SUS persistence amongst policy compliers: admitted students attend an SUS campus for an average of 1.95 (i.e., 0.457/0.234) years more than students who were not admitted, or 3.8 years for each additional first-year enrollee. That the jump in SUS attendance at the cutoff is of similar size to (and statistically indistinguishable from) the jump in FIU attendance suggests that students at this margin are not substituting FIU attendance for attendance at another SUS campus when granted FIU admission; if this were the case the effect on overall SUS attendance (i.e., at attendance any campus) would be less than the effect on FIU attendance.

Figure 4: Admissions and FIU attendance



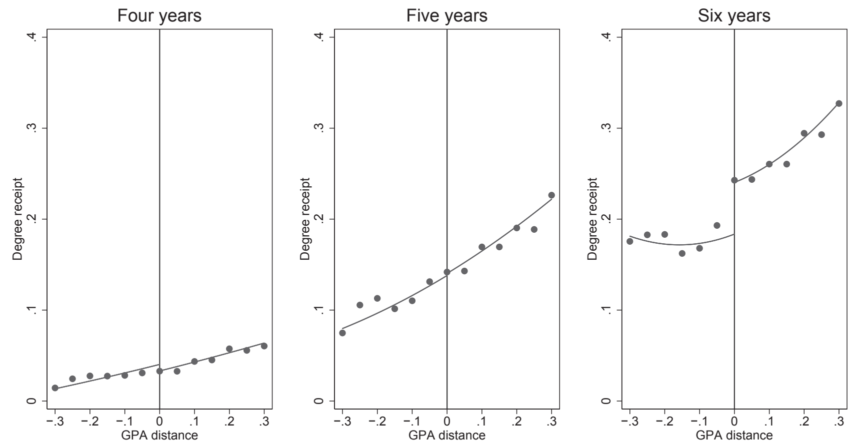
Lines are fitted values based on Main specification. Dots, shown every 0.05 grade points, are rolling averages of values within 0.05 grade points on either side that have the same value of the threshold-crossing dummy.

Figure 5: SUS attendance and persistence



Lines are fitted values based on Main specification. Dots, shown every 0.05 grade points, are rolling averages of values within 0.05 grade points on either side that have the same value of the threshold-crossing dummy.

Figure 6: SUS BA receipt by years elapsed since high school



Lines are fitted values based on Main specification. Dots, shown every 0.05 grade points, are rolling averages of values within 0.05 grade points on either side that have the same value of the threshold-crossing dummy.

Students affected by threshold crossing attend state universities with relatively low intensity. Threshold-crossing is associated with an additional 0.644 full-time-equivalent SUS terms, or 1.41 terms per year of SUS attendance. This translates to delayed SUS graduation. As shown in Figure 6 and Panel B of Table 4, threshold crossing has no effect on the probability students will have graduated from college by four or five years after high school. However, by six years after high, school, a 5.7 percentage point gap in SUS graduation has opened up. Note that the p-value associated with this gap is 0.13. This corresponds to a six year graduation rate of 48 percent, statistically indistinguishable from the 49 percent six year rate for all FIU students reported in Table A1.

Panel C of Table 4 presents the effects of threshold crossing on other academic outcomes. Threshold-crossing substantially reduces community college attendance. Threshold-crossers give up about 0.38 years of CC attendance for each additional year of SUS attendance, and 0.52 FTE terms of CC attendance for each FTE term of SUS attendance. The ratio of CC to SUS terms is larger in absolute value than the ratio of CC to SUS years because threshold-crossing students often attend SUS part time. Despite reduced CC attendance, there is no evidence that threshold crossing reduces students' likelihood of receiving a two-year degree or vocational certificate. Students above the threshold are no less likely to express the intent to attend an out-of-state or in-state private college than students just below the threshold.

Table 4: Effects on academic outcomes

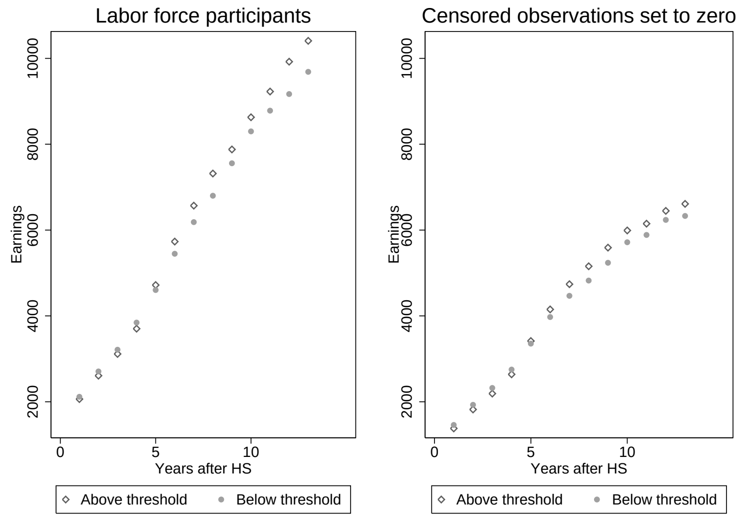
Dep. Var.	Main	Controls	BW=0.5	BW=0.15	Loc. Lin.
A. Admissions and attendance					
Admitted to FIU	0.234***	0.233***	0.246***	0.282***	0.205***
	(0.021)	(0.018)	(0.022)	(0.023)	(0.016)
Attend FIU	0.104***	0.105***	0.112***	0.0980**	0.088**
	(0.025)	(0.026)	(0.029)	(0.040)	(0.027)
Attend SUS	0.119***	0.118***	0.126***	0.125**	0.104***
	(0.021)	(0.023)	(0.025)	(0.037)	(0.023)
Years SUS	0.457**	0.463**	0.492**	0.495**	0.420*
	(0.089)	(0.094)	(0.097)	(0.114)	(0.103)
SUS FTE terms	0.644*	0.643*	0.698*	0.650*	0.622
	(0.179)	(0.192)	(0.190)	(0.185)	(0.207)
B. SUS Graduation					
Within 4 years	-0.007	-0.008	-0.008	-0.009	-0.005
	(0.007)	(0.007)	(0.007)	(0.009)	(0.008)
Within 5 years	0.002	0.001	0.008	-0.002	0.007
	(0.018)	(0.019)	(0.018)	(0.021)	(0.021)
Within 6 years	0.057	0.057	0.056	0.044	0.069
	(0.022)	(0.022)	(0.026)	(0.022)	(0.024)
C. Other academic outcomes					
Years CC	-0.172*	-0.171*	-0.222**	-0.199**	-0.164*
	(0.053)	(0.051)	(0.067)	(0.055)	(0.061)
CC FTE terms	-0.338***	-0.327**	-0.394**	-0.412**	-0.300**
	(0.081)	(0.081)	(0.103)	(0.101)	(0.095)
AA within 6 years	-0.009	-0.005	0.005	-0.006	-0.001
	(0.021)	(0.020)	(0.021)	(0.021)	(0.025)
VC within 6 years	-0.007	-0.007	-0.006	-0.009	-0.006
	(0.006)	(0.006)	(0.005)	(0.003)	(0.006)
Survey: Out of state college	0.006	0.007	0.007	0.008	0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Survey: In-state private	-0.012	-0.014	-0.010	-0.021	-0.009
	(0.009)	(0.009)	(0.009)	(0.014)	(0.011)
N	6542	6542	9659	3294	6542

Significance: ***: 1% **: 5% *10%. Standard errors are clustered within grade bins. p-values are calculated using a clustered wild bootstrap-t procedure described in section 3 and Appendix B. SUS and CC attendance and degree variables are computed using schooling data from the first six years after students leave high school. Out-of-state college and in-state-private college variables are taken from surveys administered in the senior year of high school.

5.3 Earnings effects

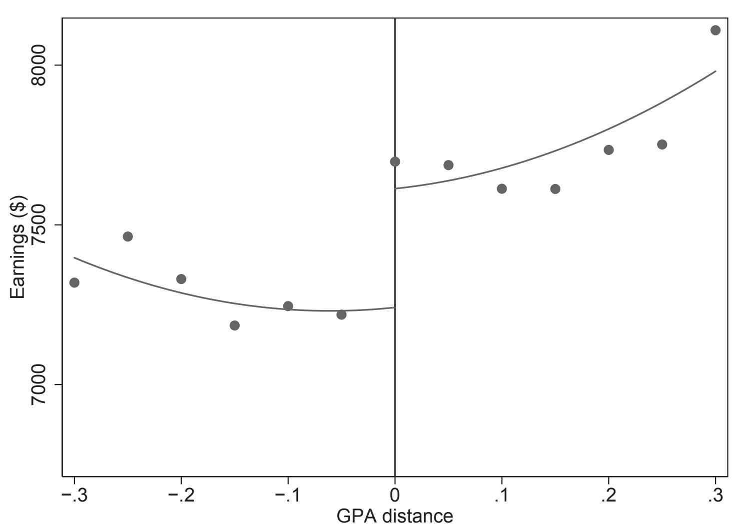
Before turning to regression discontinuity estimates of earnings effects, it is informative to consider how earnings change over time for students above and below the admissions threshold. The left panel of Figure 7 displays mean quarterly earnings by year since high school completion for students above and below the threshold with uncensored earnings reports. For the first four years following high school, below-threshold students earn about $100 to $150 more than above-threshold students, who, as we have seen, are more likely to be enrolled in an SUS institution during that period. Earnings for above-threshold students surpass those for below-threshold students in year five following high school completion. The gap between above- and below-threshold earnings remains fairly steady thereafter at $300 to $500, though there is some suggestion of a widening in years 12 and 13. The right panel presents earnings profiles in which censored quarterly observations are set to zero. The curves for above- and below-threshold students cross here as well, confirming that the pattern is not the result of differential selection into the Florida labor force either before or after completion of postsecondary education. Evidence from earnings profiles thus suggests that a) threshold-crossing is associated with early earnings losses and later earnings gains, but that b) the gains are larger than the losses. I return to this point when discussing internal rates of return in section 5.4.

Figure 7: Quarterly earnings by years since high school completion



Quarterly dollar earnings by years since high school completion and status relative to admissions threshold. Quarterly earnings are averaged within each year category in the sample of marginal students. Left panel includes only uncensored (positive) observations. Right panel sets censored observations to zero. Both graphs drop means from 14 years following high school completion, which are estimated noisily.

Figure 8: Quarterly earnings by distance from GPA cutoff



Lines are fitted values based on Main specification. Dots, shown every 0.05 grade points, are rolling averages of values within 0.05 grade points on either side that have the same value of the threshold-crossing dummy.

Figure 8 shows the effect of threshold crossing on quarterly earnings, measured in 2005 dollars. Threshold crossing raises mean quarterly earnings by $372. This is a 5.1 percent gain over expected earnings just below the threshold, which are equal to $7,241. Table 5 presents estimates of reduced form earnings effects, as well as instrumental variables estimates that scale earnings effects by changes in FIU admission status, years of SUS attendance, and BA degree receipt. Earnings rise across the threshold by $1,593 per FIU admission, $815 per additional year of SUS attendance, and $6547 per additional BA recipient. These are equal to 22 percent, 11 percent, and 90 percent of below-threshold earnings, respectively. Note that the IV exclusion restriction likely only holds for the admissions results. This is because threshold-crossing increases SUS attendance and graduation rates, but simultaneously reduces community college attendance. That is, the estimated effects are net of any earnings losses from forgone community college attendance, and do not correspond to the effect one would obtain by manipulating SUS attendance while holding constant other investments in human capital. If the earnings effects of community college are positive in this population, these IV estimates represent a lower bound on the effect of SUS attendance in this population.[18] In contrast, the offer of admission is an exogenous action on the part of the institution and is not jointly determined with other schooling choices.

These earnings effects are large, but not implausibly so. My IV estimate of the effect of a year of SUS attendance on earnings is equal to 11 percent of below-threshold earnings. Card (1999) presents OLS estimates of Mincer earnings regressions in CPS data and finds a return of 14.2 percent for men and 16.5 percent for women per year of education. Another informative comparison is with Hoekstra (2009). Hoekstra estimates that the earnings effect of the marginal admission to a flagship state university campus is between 11 and 17 percent. Since students at the margin of flagship campus admission likely attend other universities if they are not admitted, Hoekstra's estimates largely reflect the effect of improved quality of university-level education. My estimates of the earnings effects of the marginal admission range from 22 to 27 percent of mean below-threshold earnings. This comparison suggests that, for the marginally qualified student, the earnings gains from attending a less-selective university rather than a community college are larger than the earnings gains from attending a more-selective university rather than a less-selective university. That between-institution-type variation in earnings effects might be larger than within-institution-type variation seems plausible.

[18]Evidence on the effects of community college attendance on earnings is mixed. Kane and Rouse (1995) find that earnings effects of four-year and two-year college credits are similar, while Reynolds (2012) finds evidence that attending two-year college has a negative impact on earnings. See Reynolds for a review of the literature.

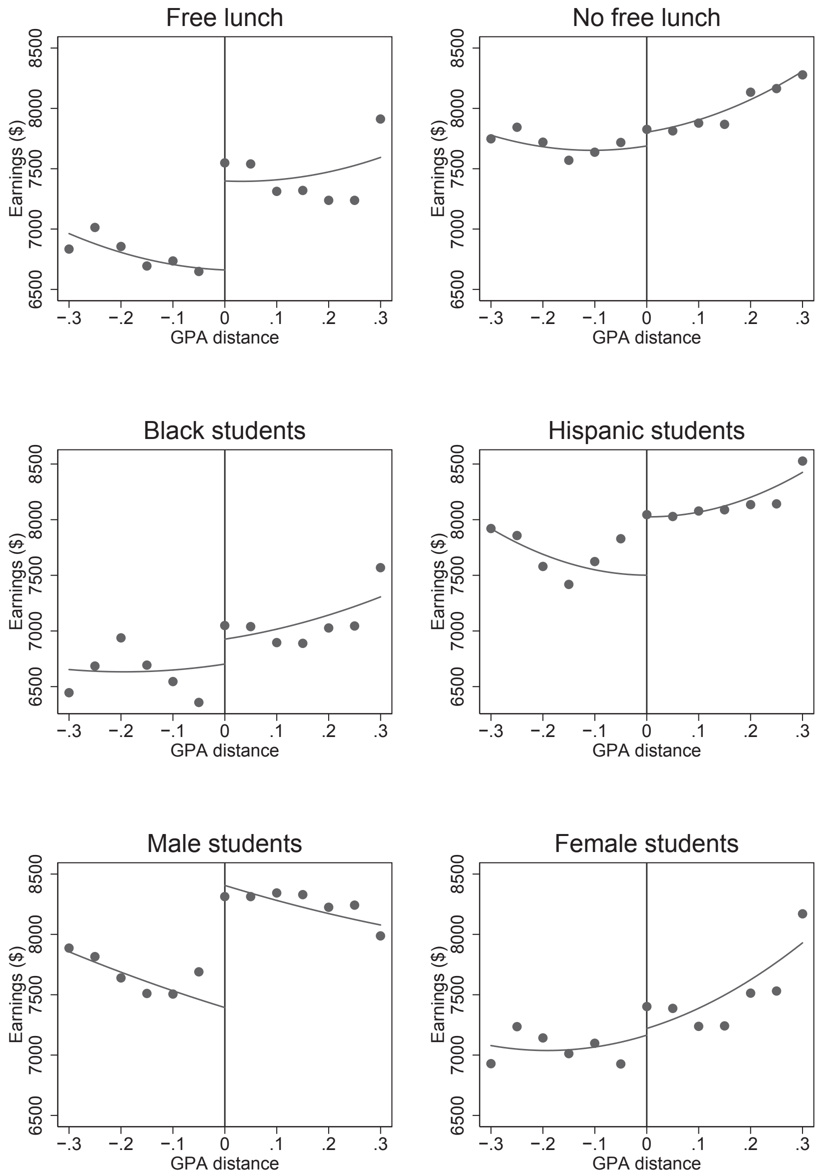
Table 5: Earnings effects 8 to 14 years after high school completion

	Main	Controls	BW=0.5	BW=0.15	Loc. Lin.
Reduced form estimates					
Above cutoff	372*	366**	409**	479**	410**
	(141)	(130)	(154)	(198)	(147)
Instrumental variables estimates					
FIU admission	1593*	1575**	1665**	1700**	2001*
	(604)	(584)	(645)	(621)	(696)
Years of SUS attendance	815**	792**	833**	966***	977**
	(276)	(262)	(271)	(305)	(306)
BA degree	6547*	6442*	7366*	10769	5958**
	(2496)	(2411)	(2998)	(5726)	(2024)
N	6542	6542	9659	3294	6542

Significance: ***: 1% **: 5% *:10%. Standard errors are clustered within grade bins. p-values are calculated using a clustered wild bootstrap-t procedure described in section 3 and Appendix B. The dependent variable in each regression is average quarterly earnings in 2005 dollars.

Population estimates of earnings effects mask substantial heterogeneity across types of students. Figure 9 shows reduced form estimates of earnings effects by race, gender, and free lunch status. Differences by gender and free lunch status are stark. For men, earnings rise by more than $1,000 across the threshold, while earnings for women barely change. For free lunch students, earnings rise by over $700 across the threshold, compared to about $100 for non-free lunch students. These differences are significant at the ten percent level. Estimated effects are somewhat larger for Hispanic students than for black students, although the difference is not significant and the discontinuity is not as visually clear for Hispanics.

Figure 9: Heterogeneity in earnings effects



Lines are fitted values based on Main specification. Dots, shown every 0.05 grade points, are rolling averages of values within 0.05 grade points on either side that have the same value of the threshold-crossing dummy.

To better understand the sources of differences in earnings effects, the panel A of Table 6 presents estimates of changes in educational outcomes across the cutoff for different groups of students. Effects are estimated using the main specification (second-degree polynomial, bandwidth of 0.3 grade points). Given the large differences in earnings effects, the degree of similarity in educational outcomes for men and women is surprising: gains in admissions, enrollment, and years of SUS attendance are similar for the two groups. Threshold crossing does raise graduation rates more for men than for women (8.1 percentage points vs. 4.3 percentage points). It also appears to reduce community college attendance more for men than for women (26.1 percentage points vs. 12.9 percentage points). However, neither difference is significant at conventional levels. It appears that men realize larger per-admission earnings gains despite limited evidence of disproportionate increases in academic success. This is consistent with a story in which per-unit returns to changes in educational attainment induced by threshold crossing are larger for men than women in this sample.

Free lunch students are somewhat more likely to be admitted than non-free lunch students (27.4 percentage points vs. 21.2 percentage points). However, estimated effects of threshold-crossing on years of SUS attendance and graduation are similar for the two groups. The most notable difference is that free lunch students give up fewer years of community college attendance than non-free lunch students (0.010 years vs. 0.336 years), though again this difference is not significant at conventional levels. This suggests that free lunch students may realize large earnings gains because threshold-crossing has a larger effect on their overall level of schooling. But, as with the gender comparison, it is also possible that free lunch students simply realize larger per-unit returns to changes in the quantity and type of educational attainment induced by threshold crossing.

Table 6: Heterogeneous effects in educational outcomes and earnings

Sample:	Black	Hispanic	Male	Female	FR lunch	No FR lunch
A. Educational outcomes						
FIU admit	0.276***	0.233***	0.242***	0.230***	0.274***	0.212***
	(0.031)	(0.032)	(0.040)	(0.017)	(0.037)	(0.023)
Attend SUS	0.140*	0.118***	0.102**	0.129***	0.140***	0.111*
	(0.040)	(0.040)	(0.027)	(0.027)	(0.037)	(0.032)
Years SUS	0.463	0.394**	0.477**	0.436**	0.477***	0.474*
	(0.178)	(0.108)	(0.149)	(0.110)	(0.104)	(0.128)
BA in 6 yrs.	0.055	0.063	0.081	0.043	0.054	0.063
	(0.033)	0.021	(0.034)	(0.018)	(0.026)	(0.024)
Years CC	-0.245	-0.098	-0.261	-0.129	0.010	-0.336
	(0.128)	(0.102)	(0.143)	(0.076)	(0.116)	(0.111)
AA in 6 yrs	-0.047	0.029	0.011	-0.020	-0.006	-0.012
	(0.026)	(0.047)	(0.027)	(0.023)	(0.038)	(0.024)
B. Earnings Regressions						
Reduced form	224	524*	1012**	56	737**	114
	(227)	(224)	(230)	(211)	(171)	(199)
IV: Admit	811	2255*	4191*	244	2695***	539
	(792)	(914)	(1324)	(916)	(521)	(940)
N	2123	3148	2261	4281	2989	3553

Significance: ***: 1% **: 5% *:10%. Standard errors are clustered within grade bins. p-values are calculated using a clustered wild bootstrap-t procedure described in section 3 and Appendix B. The dependent variable in each regression is average earnings in 2005 dollars. All estimates are computed using the main specification defined above.

5.4 The private and social returns to the marginal admission

The previous section showed that college admission leads to large post-college earnings gains for academically marginal students. However, the marginal admission also pushed students to spend more time obtaining postsecondary education, and to do so at institutions that are more costly to both the student and the taxpayer (i.e., state universities as opposed to community colleges). When deciding whether it is socially beneficial to admit more students on this margin, or privately beneficial for admitted students to accept admissions offers, one critical question is whether the earnings benefits of admission outweigh the increased cost.

To answer this question, I combine direct estimates of the earnings losses attributable to increased schooling with institution-specific data on private and social direct costs. The cost data come from the IPEDS, as processed by the Delta Cost Project.[19] Within institution by year cells, I define the per-student-year social direct cost as the average educational expenditure per full time student. I define private costs as the average tuition payment per full time student, net of federal, state, local, and institutional financial aid. This measure includes student fees. I compute the annual costs of public postsecondary attendance for each student in my analysis sample based on the number of terms in an academic year that students attended different institutions.[20] This cost variable is limited in the sense that it cannot account for variation in financial aid packages across students. Nor can it account for differences in the marginal cost of educating different types of students. For instance, it may be more costly to educate low-ability students if they require more academic support. What it will do effectively is capture differences in social and private direct costs that are driven by differences in average tuition and expenditures across institutions, which is highly relevant here.

Panel A of Table 7 presents descriptive statistics on direct costs and IV estimates of the effects of admissions on tuition costs and educational expenditures. Both state universities and community colleges are heavily subsidized. Students in the sample who enroll in the state university system spend an average of $1,166 per term and incur education-related expenditures of $4,904. Students who enroll in community college spend an average of $199 per term on tuition, but incur $4,308 in education-related expenditures. Over the six years following high school completion, students in the sample spend an average of $4,560 on tuition at state universities and under $600 dollars on tuition at community colleges. They incur education-related expenditures of $19,372 and $13,022, respectively. FIU admission raises tuition payments to SUS institutions by $3,327, and educational expenditures at SUS institutions by $11,913. It reduces private payments to community colleges by only $348, but reduces educational expenditures by $6,199. Though some of these estimates are imprecise, the picture that emerges here is one in which the marginal admission substantially raises the private and public costs of SUS attendance, but in which much of the social cost is offset by reduced public expenditures on community college attendance.

Panel B of Table 7 presents descriptive statistics and IV estimates of the effects of admissions on labor market outcomes between one and seven years after high school completion. I treat censored earnings values as zeros in this analysis to allow for exten-

[19]Housed at the NCES, the Delta Cost Project uses the IPEDS to create a longitudinal dataset of postsecondary revenues and expenditures. See Delta Cost Project (2012a).

[20]See Appendix C for a detailed description of variable construction and Table A4 for descriptive statistics on average annual tuition and educational expenditures at several SUS and CC institutions.

sive margin effects of college admission. Dropping censored values reduces estimates of indirect costs. Students in the analysis sample have non-zero earnings in 73 percent of quarters over the period, and earn an average of $4,380 in those quarters. On average, they earn a total of $94,368 over the entire period, or around $13,000 per year. FIU admission leads to imprecisely estimated but seemingly modest reductions in both intensive and extensive margins of labor force participation. Conditional on employment, admitted students earn $200 less per quarter than non-admitted students, and are about 5 percentage points less likely to have any earnings. These effects yield total earnings losses of just over $12,000 per admission. None of these estimates are statistically significant.

Table 7: Direct and indirect costs of admission in the analysis sample

A. Tuition and educational expenses		Descriptive statistics		Admissions effects	
Source	Cost type	Per term	6-year total	Effect	SE
SUS	Private cost	1166	4560	3327*	(930)
	Expenditure	4904	19372	11913	(4608)
CC	Private cost	199	568	-348	(207)
	Expenditure	4308	13022	-6199**	(1664)
Sum: SUS and CC	Private cost		5128	2979*	(873)
	Expenditure		32394	5713	(3995)

B. Labor market outcomes 1-7 years after HS	Descriptive Statistics	Admissions effects	
Outcome	Sample mean	Effect	SE
Mean quarterly earnings	4380	-200	(322)
Frac. quarters employed	0.73	-0.047	(0.034)
Total earnings	94368	-12294	(7380)

N=6542. Panel A: Private costs are tuition costs to student. Expenditures are total educational expenditures.'Per term' costs are means for students enrolled in the stated institution type within the six years following high school completion. '6-year totals' are the sum over term costs for each individual. Panel B: Mean quarterly earnings calculated using only uncensored observations. Total earnings sums over years 1-7, setting censored observations to zero. Significance: ***: 1% **: 5% *:10%. Standard errors are clustered within grade bins.p-values are calculated using a clustered wild bootstrap-t procedure described in section 3 and Appendix B.

A back-of-the-envelope calculation of the internal rate of return (IRR) to the marginal admission helps synthesize estimates of cost discontinuities with estimates of longer-run earnings effects from section 5.3. To the simplify the calculation I make three assumptions about the time path of cost and earnings effects. First, I assume that the differences in total direct costs for admitted relative to non-admitted students are incurred evenly over years one through four and over years five through six following high school completion. I estimate separate direct cost effects for these two periods. The goal is to capture in a parsimonious way the narrowing gap in postsecondary enrollment between above- and below-threshold students more than four years after high school completion, as shown in Figure A2. Second, I assume that forgone earnings effects are incurred evenly over years one through four and over years five through seven following high school completion. This captures the shift from earnings losses over the former period to small earnings gains over the latter period, as shown in Figure 7. Third, I assume that, beginning in the eighth year after high school completion, the quarterly per-admission gains in earnings reported in Table 5 accrue to students in each quarter that they work, and that students in this sample work in two-thirds of total quarters, as reported in Table 2 for the sample of marginal students.

I present two IRR calculations. The first considers only earnings outcomes within the support of my data; i.e., within the first fourteen years following high school completion. The second considers earnings outcomes through 47 years after high school, or approximately age 65. I present both calculations to provide a sense of what can be said about IRRs using only observed outcomes, and also of the size of IRRs we would expect if effects persist over the life cycle. I focus on IRRs in the population as a whole because I do not have data on heterogeneity in financial aid packages and student support costs across demographic groups. This calculation should be interpreted with caution given that cost data are approximate and cost effects are imprecisely estimated.

Table 8 presents results from IRR calculations. The first column shows estimate of private IRRs, the second column shows estimates of social IRRs, and the third column shows estimates of social IRRs that incorporate Feldstein's (1999) estimate of the deadweight loss of taxation at 30 percent into estimates of direct costs.[21] One might think of column two as representing the sum of private costs and budgeted costs to the government, and column three as representing total costs to society. Panel A displays the present discounted values (PDVs) of different categories of costs and benefits at an approximate market interest rate of $r = 0.06$. At this interest rate, students realize a private return of just over \$2,000 through fourteen years after high school completion, while the investment roughly breaks even from a social perspective. The private IRR is about eight percent, while the social IRRs are about six percent. Through 47 years after

[21]The net tax burden associated with subsidies to education will be reduced if admitted students are less likely to receive other government benefits later in life. Here, I abstract from possible reductions in the receipt of other benefits. This choice will push estimates of social IRRs downward.

high school, students realize net private returns of just under thirty thousand dollars, and government and society realize returns of about $27,000. The private IRR is 15 percent, compared to a social IRR of 14 percent. The takeaway here is that by fourteen years after high school completion the private beneficiary of the marginal admission has already more than broken even. If effects persist through all or even part of students' remaining working lives, both private and social returns will be quite large.

Table 8: Internal rate of return to the marginal admission

	Private	Social	Social (incl. DWL)
A. PDV of costs and benefits at r=0.06			
Direct costs	2493	4565	5187
Indirect costs	11093	11093	11093
Benefits through 14 year after HS	15853	15853	15853
Net return through 14 years after HS	2267	195	-427
Benefits through 47 years after HS	42729	42729	42729
Net return through 47 years after HS	29143	27071	26449
B. Internal rates of return			
Through 14 years after HS	0.0822	0.0618	0.0561
Through 47 years after HS	0.1516	0.1389	0.1355

Columns differ by treatment of direct costs. 'Private' column includes tuition net of aid. 'Government' column includes per-student education-related expenditures. 'Social (incl. DWL)' column multiplies estimated government direct costs net of private payments by 1.3, an estimate of the deadweight loss of taxation from Feldstein (1999).

An important caveat is that these IRRs capture the returns to admissions for students on the margin. Reducing the grade cutoff enough to have a measurable effect on overall rates of college attendance and graduation could have negative effects that are not captured here. The addition of many marginal students could reduce the quality of education for all students, either by stretching institutional resources or by reducing the positive spillover effects from higher-achieving peers. Even if the quality of education were to remain the same, increasing the supply of college graduates in the labor force could reduce wages for this skill group (Heckman, Lochner and Taber 1998).

5.5 Additional robustness tests

The results presented here are robust to adjustments that take into account heaping in the running variable, and to changes in the earnings measure. To address the concern that heaping in the running variable could lead to biased estimates even when the RD design passes standard balance tests, I follow two approaches recommended in Barreca et al. (2011b). The first is to estimate a 'donut' regression discontinuity that drops earnings observations precisely at the cutoff value, the location of the largest data heap. The second approach is to control flexibly for heterogeneity related to heaping by allowing for separate intercepts and trends in heaped data. Panel A of Table A5 presents results obtained by implementing these modifications in the main specification. Precision is reduced in some specifications, which is to be expected given that these specifications (respectively) use less data and estimate additional parameters. But point estimates tend to rise slightly in absolute value.

Panels B and C of Table A5 show estimates of reduced form earnings effects given different topcoding values for earnings and different timeframes for earnings measurement. Core estimates topcode earnings at the 99th percentile within each cohort; lowering this value to the 98th percentile or raising it to the 99.5th have little impact on estimated earnings effects. Core estimates use earnings observations between eight and fourteen years after high school completion. Focusing on years eight to ten results in somewhat larger effects, while focusing on years eleven through fourteen produces smaller and less precisely estimated effects. This lack of precision is to be expected given that the longer-run earnings analysis necessarily drops the 1999-2001 cohorts. I cannot reject the hypothesis that short term and long term effects are the same.

6 Discussion

In this paper, I use a regression discontinuity design to show that the earnings gains associated with the marginal four-year college admission are quite large. Students just above an admissions cutoff in high school grades earn an average of $372 more per quarter than students just below the cutoff. This corresponds to an increase of $1,593 for each marginal admission, equal to 22 percent of below-threshold expected earnings. Students at the margin of admission realize these gains despite the fact that their mean SAT scores are nearly 200 points below the mean SAT scores for college-bound students nationally. The effects of the marginal admission on earnings are largest for male students and for free lunch recipients.

Both the private and social internal rates of return to the marginal admission appear to be well above market interest rates. This is because the marginal admission has relatively small costs in terms of forgone early-career earnings for marginal students, and because increases in the direct costs of state university attendance for admitted students are partially offset by decreases in the costs of community college attendance. I therefore interpret my findings as evidence that admissions-based supply constraints on seats in four-year college bind in the sense that they prevent students from making investments with high private and social returns. Expanding supply along this margin would likely be welfare improving provided it did not result in a substantial reduction in returns for infra-marginal students, through, say, a drop in per-student resources or the dilution of positive peer effects.

The effects of the marginal admission on earnings are largest for male students and for free lunch recipients. Interestingly, these are groups of students who are relatively unlikely to attend college. In 2000, men made up 44 percent of US college students, and students from families with bottom-quintile incomes were 30 percentage points less likely to attend college than students from families with top-quintile incomes (NCES 2011, Table 198 and NCES 2012, Table 210.5). There are a number of possible explanations for this combination of low attendance rates and high returns at the margin in these groups. One is that, conditional on determinants of the returns to postsecondary schooling, male and low-income students may tend to invest less in educational production while in high school. This could be because these students face credit constraints, have more trouble focusing on school, or are unaware of the returns to higher education.[22] The students in these groups who do make it to the admissions margin tend to realize high returns. This is a topic for future work.

One reason to be cautious in interpreting these results is that they are based on students applying to a single university in Florida, and may not apply to other students or other universities. It is worth noting, however, that the university studied here is relatively comparable to public institutions across the state and the nation in terms of both student quality and student outcomes. At minimum, it played an important role in state policy over the period in question through its status as the public university with the most academically forgiving admissions standards. The relevance of this study for US policy thus depends in large part on the extent to which results from Florida can be extrapolated to other states.

[22]Each of these possibilities has been the subject of substantial research. For instance, Tyler (2003) finds that work while in high school reduces math achievement. Fortin, Oreopoulos and Phipps (2012) find that the female-male gap in high school performance is related to lower educational expectations and greater frequency of misbehavior for boys. Goldin, Kuziemko and Katz (2006) attribute the long-run increase in college attendance for women to changes in expected returns to college and to developmental differences between boys and girls.

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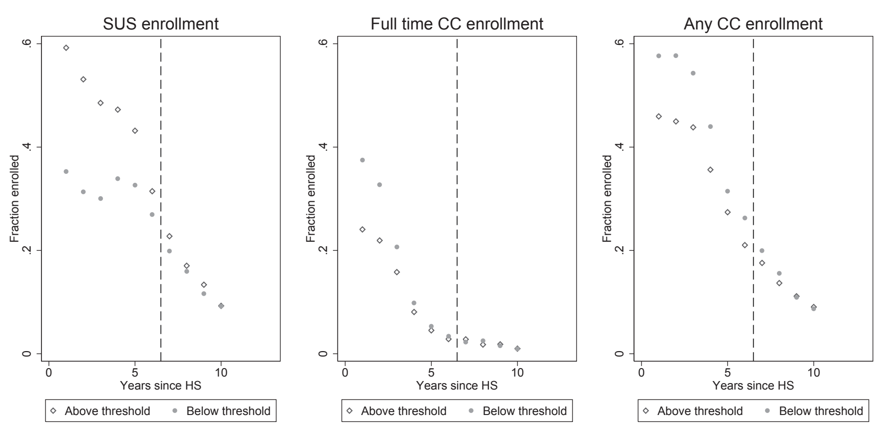
Appendix

A Additional tables and figures

Figure A-1: Postsecondary enrollment by years since high school



Mean enrollment by status relative to cutoff and years since high school completion. Sample: marginal students. Points to the left of the dashed lines are included in measures of educational attainment used in the paper.

Table A-1: Florida International University Admissions and Enrollment statistics, AY 2000-2001

Enrollment		Academics	
Total	23591	SATM: 25th ptile	510
Men	10283	SATM: 75th ptile	590
Women	13308	SATV: 25th ptile	510
PT	9546	SATV: 75th ptile	590
FT	14045	HS GPA	3.46
Black	3390	Grad. rate	0.49
Hispanic	12975		
Applications		**Costs**	
Total Apps	5891	In-State tuition	2242
Total Acc.	3176	Out-of-state tuition	9580
Total Enroll	2563	Room+Fees	4398
		Pct. Rec. FA	0.40
		Avg. FA value	5163

Source: FIU Common Data Set submissions 2000-2001 and 2007-2008 (Florida International University Office of Planning and Institutional Research 2012). Enrollment data refers to degree-seeking students only. Academic characteristics are for degree-seeking first-time-enrollee freshmen. Six year graduation rates are computed for Fall 2001 entering cohort; graduation rates for the Fall 1999 entering cohort were 0.48. Applications data is for Fall 2000 entrants. Tuition and financial aid are reported in nominal terms. The percentage of students receiving aid includes only full-time undergraduates receiving need-based aid.

Table A-2: Florida SUS admissions rules

SAT	Required GPA	Fraction of marg. applicants
1140	2.0	0.00
1110	2.1	0.00
1090	2.2	0.00
1060	2.3	0.00
1030	2.4	0.01
1010	2.5	0.01
1000	2.6	0.01
990	2.7	0.01
980	2.8	0.02
970	2.9	0.02
$<$970	3.0	0.73
Did not take	3.0	0.19

Source: Florida Administrative Rule 6C-6.002. Sample: Marginal applicants are defined as all FIU applicants with FIU-computed GPAs within 0.3 grade points of their individual-specific cutoff GPA, computed using SAT scores. N=6,542.

Table A-3: Common applicant GPA comparisons

	FIU	UCF	UF	USF	UNF	FAU	FSU
FIU	3.3	3.32	3.58	3.29	3.17	3.25	3.4
	3.3	3.16	3.17	3.12	2.96	3.15	3.19
	24690	4310	4852	3538	741	3689	5618
UCF		3.36	3.58	3.24	3.16	3.14	3.36
		3.36	3.27	3.24	3.14	3.21	3.32
		26009	9877	8586	2159	3573	11223
UF			3.47	3.26	3.14	3.12	3.31
			3.47	3.56	3.39	3.46	3.58
			30239	7052	1282	2194	13329
USF				3.28	3.07	3.04	3.32
				3.28	3.06	3.13	3.28
				25563	1889	2872	7950
UNF					3.2	2.96	3.19
					3.2	3.04	3.17
					4542	910	1862
FAU						3.16	3.23
						3.16	3.12
						10849	2912
FSU							3.42
							3.42
							27680

Note: Table displays mean GPAs for same-year cross-applicants to institutions listed in the row and column. Within each cell, the first row is the mean GPA for cross-applicants at the row institution, the second the mean GPA at the column institution, and the third the number of cross-applicants. College names are as follows. FIU: Florida International University. UCF: University of Central Florida. UF: University of Florida. USF: University of Southern Florida. UNF: University of Northern Florida. FAU: Florida Atlantic University. FSU: Florida State University.

Table A-4: Direct costs of college attendance

	FTE enrollment	Educational expenses	Gross tuition	Net of inst. aid	Net of all aid
FIU	22716	8997	3792	3443	2044
FSU	29949	10020	3846	2000	1459
FAU	14311	12925	3510	1047	142
UF	41543	14885	3859	3072	2392
Mean all SUS	26237	11756	3546	2532	1720
Miami-Dade CC	25323	10251	3231	2772	176
Broward CC	12747	8220	3114	2896	1481
Palm Beach CC	8390	9128	2801	2639	1812
Mean all CC	10679	8688	2523	2298	900

Institution-level costs from 2000. Source: Delta Cost Project, based on IPEDS data. Rows define specific institutions or institution types. FTE enrollment is fall full-time equivalent enrollment. Educational expenses are total education-related expenses divided by FTE enrollment. This variable is used to compute social costs in main text. Gross tuition is total tuition revenue per FTE enrollment. Tuition net of institutional aid is tuition revenue net of institutional aid divided by FTE enrollment. Tuition net of all aid is tuition revenue net of federal, state, local, and institutional aid, divided by FTE enrollment. This variable is used to compute private costs in main text.

Table A-5: Robustness of core results to heaping and topcoding

	A. Robustness to controls for heaping		
	Main	Drop cutoff heap	Trends in heaps
FIU admit	0.234***	0.219***	0.241***
	(0.021)	(0.027)	(0.026)
Attend SUS	0.119***	0.149***	0.131***
	(0.021)	(0.017)	(0.109)
Years SUS	0.457**	0.502***	0.494***
	(0.089)	(0.109)	(0.071)
BA in 6 yrs	0.057	0.062	0.065*
	(0.022)	(0.030)	(0.017)
Years CC	-0.172*	-0.194**	-0.180**
	(0.053)	(0.065)	(0.049)
AA in 6 yrs	-0.009	-0.021	-0.007
	(0.021)	(0.026)	(0.019)
Earnings	372*	400	402
	(141)	(227)	(163)
N	6542	5626	6542

	B. Robustness to topcoding procedures		
	Main	98th %tile	99.5 %tile
Earnings	372*	346*	380**
	(141)	(142)	(143)
N	6542	6542	6542

	C. Timeframe of earnings grains		
	Main	Years 8-10	Years 11-14
Earnings	372*	403*	154
	(141)	(160)	(228)
N	6542	6477	2421

Significance: ***: 1% **: 5% *:10%. Standard errors are clustered within grade bins. Significance stars are calculated using a clustered wild bootstrap-t procedure described in section 3 and Appendix B. Estimated coefficients on threshold crossing are reported in all rows. All estimates are computed using the main specification defined above. Panel A: Column 'Main' reproduces results from the main text. Column 'Drop cutoff heap' drops observations with grades equal to the cutoff value. Column 'Trends in heaps' controls for a dummy equal to one for heaped values and an interaction between that dummy and quadratic in distance from the cutoff. Panel B reports reduced form earnings results, topcoding at the indicated percentile of the within-cohort earnings distribution. Panel C reports results from the main reduced form specification that restrict earnings observations to the listed years since high school completion.

B Inference procedures

Inference in RD estimation is based on a clustered wild bootstrap-t procedure, clustering within each GPA bin (i.e., each one hundredth of a grade point). As shown in Cameron et al. (2008), the clustered wild bootstrap-t performs well when there are relatively few clusters, while inference using analytic cluster-robust standard errors tends to overreject. This is a concern in this application, because a large proportion of observations are concentrated at relatively few points in the grade distribution, particularly in the samples used for estimation at narrower bandwidths. This heaping is visible in Figure 1. In the specifications with a window width of 0.3 grade points, there are 46 clusters, and the seven largest account for 68 percent of observations. In the specifications with window width of 0.5, there are are 75 clusters, with the eleven largest accounting for 64 percent of observations. And in the specifications with window width of 0.15, there are 24 clusters, with the largest 3 accounting for 68 percent of the data.

When implementing clustered wild bootstrap, I follow the recommendations of Cameron et al. in that I a) use Rademacher weights and b) impose the null hypothesis when computing regression residuals. To implement the wild bootstrap in instrumental variables specifications, I use the wild restricted efficient residual bootstrap developed in Davidson and MacKinnon (2010). As in Cameron et al., I account for clustering by assigning the Rademacher weights at the cluster level. I use 1,999 bootstrap replications, and conduct hypothesis tests using equal-tail p-values. In the text, I present both analytic cluster-robust standard errors and the bootstrapped p-values. This presentation follows Busso, Gregory and Kline (2013). As expected, bootstrapped inference is often more conservative than inference based on the analytic cluster-robust standard error estimates.

C Data description

C.1 Overview

I obtained this dataset through agreements with the Florida Department of Education and the College Board. I have data on seven cohorts of students (12th graders in 1996, 1997, 1999, 2000, 2001, 2002, and 2004, where years refer to the spring of the academic year) from fifteen counties (Dade, Broward, Hillsborough, Orange, Polk, Santa Rosa, Charlotte, Putnam, Martin, Highlands, Calhoun, Jefferson, Gulf, Franklin, and Hamilton). These counties were selected based on size and geographic and socioeconomic diversity and do not form a random sample of counties in the state. The sample includes four of the largest 20 school districts in the US.[23] I did not have access data from other cohorts or counties when conducting this analysis.

I track each cohort of 12th graders backward through the 1996 school year and forward through the 2008-2009 school year. SUS data includes application records for all 11 state university campuses. I link the administrative educational data to SAT test records provided by the College Board and to Florida UI earnings records. For all cohorts except the 2004 cohort, I have access only to students' most recent SAT test records. For the 2004 cohort, I have access to students' SAT score histories. The UI data includes earnings (not hours or wages) for workers employed in Florida. Earnings data runs from 1995 through the first quarter of 2010.

C.2 Construction of key variables

In this section I describe the construction of key variables used in my analysis.

1. Education variables.

    (a) Admissions: Admissions GPAs are reported by SUS campuses as part of their application records. Admissions outcomes are also included in this data. Students apply to specific year-term-campus combinations. I code 12th grade students as having applied to FIU if they apply for admission to any term of the following academic year. I code 12th grade students as having been admitted to FIU if they are admitted or provisionally admitted to any term of that year. For students who apply to FIU multiple times within the same year and have different FIU GPAs, I take the GPA associated with their first application. I assign students' cutoff GPAs based on their SAT scores (see below). Approximately 20 percent of marginal students do not take the SAT; I assign these students a grade cutoff of 3.0 based on the observation that this is the cutoff facing 90 percent of SAT takers (see Table A2). My results are robust to excluding these students.

    (b) SAT scores: I use most recent combined verbal and math scores as my SAT score variable, because I do not have access to score histories for cohorts used in the earnings analysis.

    (c) SUS and CC attendance. I count a student as attending a state university in a given academic year if they enroll in any state university at any point in that

[23] In the 1999-2000 school year, Dade was the fifth largest district, Broward the sixth, Hillsborough the 13th, and Orange the 16th. In addition, Polk was the 37th largest. See Young (2001), Appendix A.

academic year. To create a count of total years of SUS or CC attendance, I sum year-specific enrollment variables over the first six years after high school for each student. To count terms of SUS attendance, I aggregate total SUS credits within student-year-term cells, and code terms as half terms if students take less than 12 credits and full terms if they take 12 or more credits. I then sum over all terms over the first six years after high school. To count terms of CC attendance, I use a part-time/full time designator provided by the FLDOE; full time is defined as 12 or more credits. I count terms as full time if students are enrolled full time at any community college and part time if they are enrolled in a community college but not full time. I count summer terms as part-time terms. I then take a sum of total terms over the first six years after high school.

2. Demographic variables.

   (a) Race and gender: these variables are provided in a demographic file accompanying the educational records.

   (b) Free lunch status: Free lunch status may vary by enrollment year and term. I code a student as a free/reduced lunch recipient if he is ever reported as eligible.

3. Earnings variables. Earnings records from UI tax reports are reported at the job-quarter-individual level. I sum earnings in each quarter, deflate to 2005 dollars using the quarterly PCE, and take a within-person average over all observations between the fall of the eighth academic year following the year of college application and the first quarter of 2010. UI wage reports cover employers with quarterly payrolls of $1,500 or more in a calendar year, or that have one or more employees for any portion of a day during twenty weeks in a calendar year. However, some types of earnings are not reported. These include informal sector earnings, self-employment earnings, and earnings from active-duty military service. One reporting exemption that may be important for computing earnings very early in the career covers services for universities by enrolled students. If above-threshold students are more likely to provide these kinds of services than below-threshold students, it may lead me to overstate forgone early-career earnings in cost-benefit calculations.

4. Cost data. I use cost data assembled from the 1987-2010 IPEDS as part of the Delta Cost Project and maintained by the National Center for Educational Statistics. See

Delta Cost Project (2012a). I compute per-student educational expenditures used in social cost calculations as total annual institutional spending on direct educational costs (including instruction, student services, and shares of academic support and maintenance) divided by fall FTE enrollment. The relevant Delta Cost Project variables are 'eandr' and 'fte_count'. I compute per-student net tuition used in private direct cost calculations as total annual institutional tuition revenue (net of Pell, federal, state, and local grants) divided by fall FTE enrollment. This includes grant aid that may be used to offset non-tuition expenditures such as room and board, so tuition values are slightly negative in a few cases. Including grant aid targeted at non-tuition expenditures like room and board seems reasonable in this application because students receive these subsidies only if they enroll in college, but have to pay for living expenses regardless of enrollment. The relevant Delta Cost project variables are 'net_student_tuition' and 'fte_count'. See Delta Cost Project (2012b).