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The Returns to College Admission for Academically Marginal Students

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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% between 8 and 14 years after high school completion. These gains outstrip the costs of college attendance, and they are largest for male students and free-lunch recipients.

I. Motivation

The college wage premium has risen dramatically over the past 30 years. In 1980, college graduates earned roughly 50% more than high school graduates; by 2008, they earned 97% more.[1] A series of influential papers (e.g., Katz and Murphy 1992; Goldin and Katz 2008; Acemoglu and Autor 2011)

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Contact the author at seth.zimmerman@yale.edu. Information concerning access to the data used in this article is available as supplementary material online.

[1] The source is Acemoglu and Autor (2011). Estimates adjust for changes in demographic composition.

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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, 297) estimate that between 1980 and 2005, the demand for college graduates increased by about 3.5% per year, while the relative supply of college graduates increased by only 2% per year. The net result was growth in the college wage premium at the rate of 0.9% 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, as well as 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 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 article asks whether relaxing supply constraints through reductions in admissions standards at 4-year colleges would allow students to make investments with high private and social returns. I combine a rich data set on

[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 (2013).

[3] See Cameron and Taber (2004), Belley and Lochner (2007), Stinebrickner and Stinebrickner (2008), 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 noncognitive development.

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

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 (grade point average) cutoff for admission to the Florida State University System (SUS) to estimate the returns to 4-year college admission for students at the margin of admission to any SUS campus. I focus my analysis on Florida International University (FIU), a SUS campus that was especially generous in the way it computed the GPAs used for admissions during the period in question and that 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 a SUS campus for an additional 3.8 years, and they graduate at rates similar to those in the broader student population. Threshold-crossing produces a $372 gain in quarterly earnings between 8 and 14 years after high school completion, corresponding to a $1,593 increase in quarterly earnings per marginal admission. This is equal to 22% 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 Integrated Postsecondary Education Data System (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 article 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 4-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 article. The average

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

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 United States. Such distinctions are important because, as discussed in Card (1999), Carneiro, Heckman, and Vytlacil (2011), and Meghir and Rivkin (2011), credible use of instrumental variables (IV) 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 article 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 4-year college are partially offset by decreases in expenditures on community college. This analysis draws on a match between college attendance microdata and panel data on institution-specific per-student tuition receipts (net of financial aid) and total educational expenditures. With the exception of Öckert (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 article proceeds as follows. Section II describes the policy environment that gives rise to the admissions cutoff, Section III describes my econometric strategy, and Section IV describes the academic and labor market data I use in my analysis. In Section V, I present my core regression discontinuity results and estimates of internal rates of return. Section VI concludes.

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

II. 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 20–25 thousand first-time-in-college freshmen each year. The middle 50% 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 article 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 6-year graduation rate for FIU students in the 2001–2 entering class was 49%, close to the 55% national graduation rate for students entering 4-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.

Although 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 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 stu-

[7] For freshmen enrollment in 2000–2001, see State University System of Florida Board of Governors (SUSBOG 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 the National Center for Education Statistics 2010, table 341.

[9] As noted in table A2, 19% 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.

dents admitted through profile assessment is limited to 10% of total system wide admissions.[10]

Although 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.

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 data set.[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 FSU. Of the 69.3% 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% of marginal FIU students whose grades fell below the cutoff, only one in 77 (0.4%) surpassed the FSU cutoff. Panel C presents parallel results for admissions. Of the 69.8% of marginal students who were admitted to FIU, one-ninth (7.9%) were also admitted to FSU. But of the 30.2% 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.

III. Econometric Strategy

I recover estimates of the earnings effects of the marginal college admission using a fuzzy regression discontinuity (FRD) design that com-

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

[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.

Table 1
FIU and FSU Admissions GPAs for Joint Applicants

	A. GPAs for Joint Applicants	
	Mean	SD
High school GPA	2.98	.39
Florida International University (FIU) GPA	3.40	.50
Florida State University (FSU) GPA	3.19	.62
	B. Status Relative to Grade Cutoffs	
	FSU=1	FSU=0
FIU=1	.231	.462
FIU=0	.004	.303
	C. Admissions Outcomes	
	FSU=1	FSU=0
FIU=1	.079	.619
FIU=0	.012	.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. High school GPAs are unweighted cumulative GPAs provided by high schools. The FIU and FSU GPAs are university computed and are 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.

pares 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 data set) 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 postcollege earnings for individual i, g_i be the distance between the grades for individual i and the cutoff he or she 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 8 and 14 years after high school completion (roughly ages 26–32) 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 bachelor's 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 V.A, 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.

IV. Data

I use data on six cohorts of public high school twelfth-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 Florida 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 these 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 twelfth-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 earnings effects for in-state labor market participants. However, censoring

[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 twelfth-grade cohort, but I exclude them from this analysis because I observe their earnings at most 5 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 4 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.

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 V.A 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 V.B, 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 8 and 14 years after high school (generally between the ages of 26 and 32) as the outcome variable of interest. Focusing on outcomes 8 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 V.C, 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 8 and 10 and between 11 and 14 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 community college (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 6 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 during the year in question. I count terms of SUS and CC enrollment by summing full-

[14] Estimates of the effects of threshold crossing on SUS outcomes through 7 years following high graduation are available upon request, and these show that extending the analysis time frame does not meaningfully affect estimated discontinuities.

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 twelfth-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 they are 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. Fifty-one percent of marginal applicants attend a SUS institution, and 50% attend a community college in the year following the twelfth-grade year, compared to 9% who express the intent to attend a private college in Florida or any college outside of Florida.[15] Finally, 80% 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 V.A.

V. Results

A. Robustness of the Regression Discontinuity Design

There are two major concerns about this research design. The first, standard in the regression discontinuity (RD) 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 out-migration.

[15] Students may attend both a 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 Applicant Sample	Marginal Applicant Sample	Labor Force Sample
White	.40	.18	.15	.15
Black	.27	.26	.33	.32
Hispanic	.28	.50	.47	.48
Male	.48	.37	.36	.35
Free or reduced-price lunch	.40	.43	.46	.46
High school GPA	2.63	2.92	2.72	2.72
SAT	NA	943	841	839
Attend SUS school next year	.16	.59	.51	.51
Attend community college next year	.31	.37	.50	.51
Survey: attend non-Florida college	.06	.04	.03	.03
Survey: attend Florida private college	.04	.08	.06	.06
In labor force sample	.68	.78	.80	1.00
Fraction quarters with earnings observations	.53	.64	.67	.83
N	351,198	24,690	8,147	6,542

NOTE.—FIU = Florida International University; SUS = Florida State University System. Sample means for selected student populations. FIU applicant sample refers to all FIU applicants. Marginal applicant sample refers to marginal FIU applicants. Labor force sample refers to marginal FIU applicants for whom outcome period earnings data are available. Fraction quarters with earnings observations is the fraction of quarters during the outcome period with uncensored (positive) earnings observations.

There are a 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 upward. Alternatively, if high-earning above-threshold students are more likely to take out-of-state jobs, this would bias my estimates downward.

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

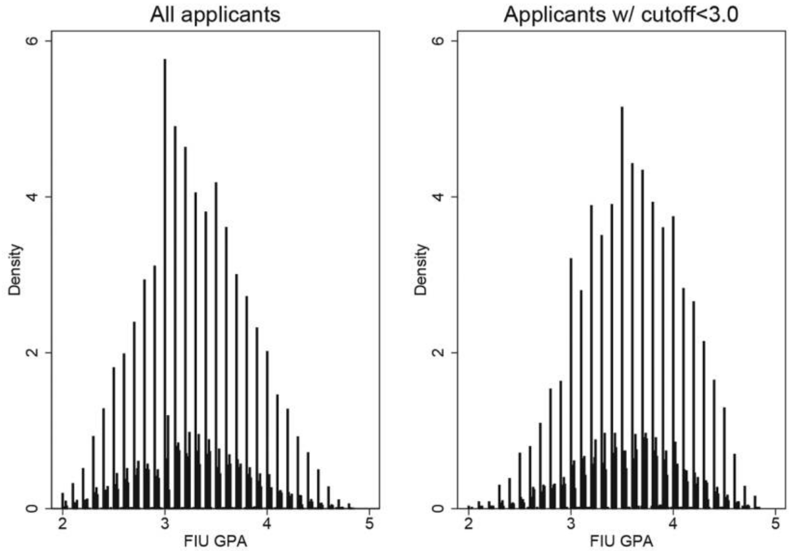

FIG. 1.—Histograms of admissions GPAs of all sample students and of sample students with cutoffs of less than 3.0. Students with grades below 2.0 are dropped. Separate columns are shown for each GPA bin; bin width is .01 grade points.

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 1% 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 noncutoff points (e.g., at a GPA of 3.5) suggest the latter story may be important.

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 1% 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,

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

where $f(g)$ 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's 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, each density ratio is continuous around the cutoff value.

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

[16] To see this, consider some binary variable $X \in \{0, 1\}$. Then substituting for $f(g|X = 1)$ using Bayes's 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.

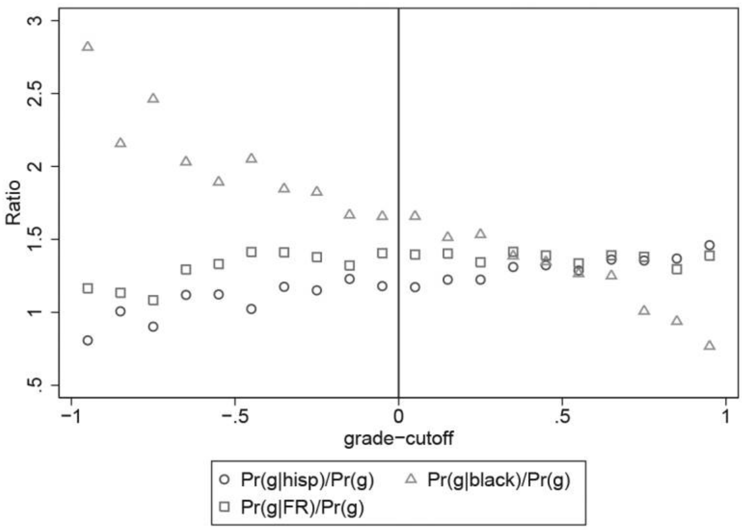

FIG. 2.—Ratios of conditional to unconditional grade densities by distance relative to the admissions cutoff for three different conditioning groups: Hispanic students, black students, and students who have free or reduced-price lunch. Densities are computed with bins with a width of .1 grade points.

number of 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 .3 grade 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 it adds controls for gender, race, free-lunch status, and twelfth-grade cohort. The "BW=.15" specification uses observations within .15 grade points above and below the cutoff and allows for a linear trend in distance from the cutoff. The "BW=.5" specification uses observations within .5 grade points on either

[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.

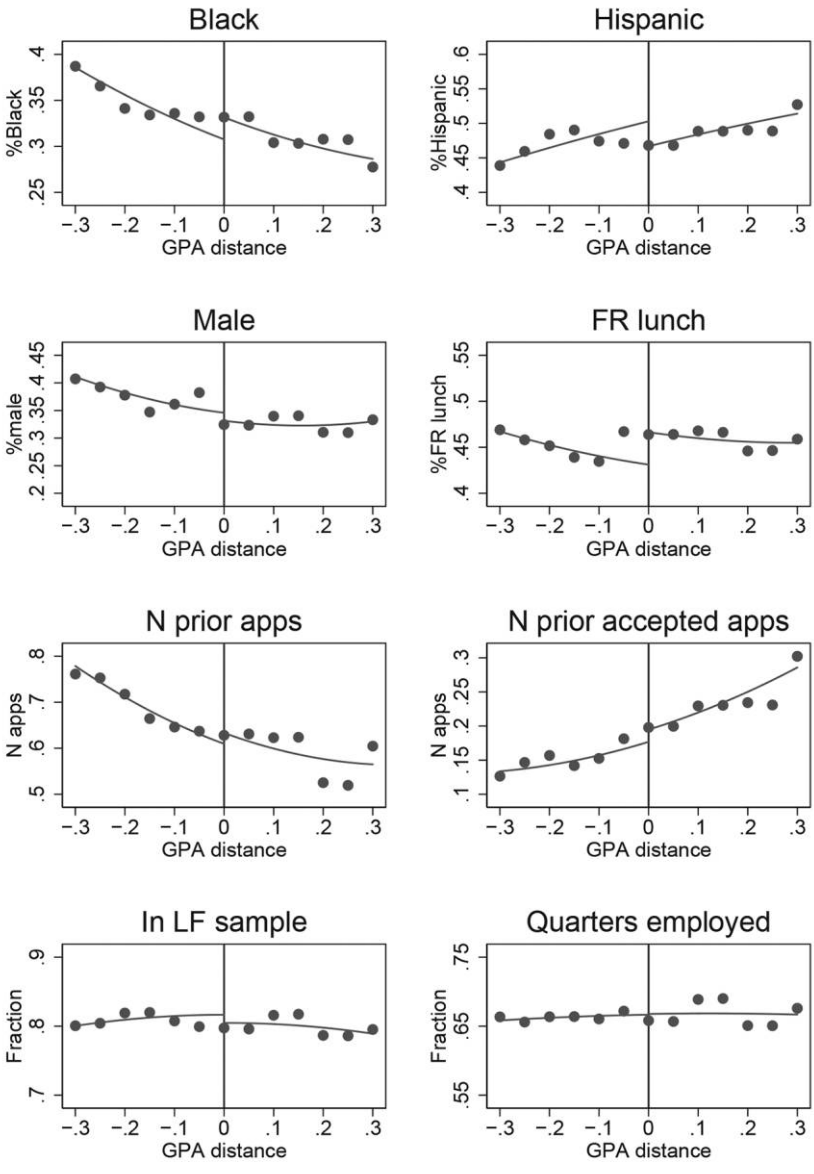

FIG. 3.—Covariate balance and employment effects. Means of demographic variables and labor force participation by distance relative to the cutoff. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.

Table 3
Validity of the Regression Discontinuity Design

Dependent Variable	Main	Controls	BW=.5	BW=.15	Local Linear
A. Student characteristics:					
Black	.024		.017	.027	.027
	(.018)		(.020)	(.019)	(.022)
Hispanic	−.036*		−.018	−.022	−.038*
	(.021)		(.022)	(.024)	(.022)
Free or reduced-price lunch	.035		.036	.018	.037
	(.024)		(.025)	(.026)	(.028)
Male	−.015		−.020	−.054**	−.007
	(.017)		(.019)	(.020)	(.018)
Index	6.2		19.8	20.2	1.9
	(31.3)		(31.4)	(40.6)	(35.6)
N	6,542		9,659	3,294	6,542
B. Other SUS applications:					
Acceptances	.018	.016	.013	.007	.022
	(.023)	(.022)	(.025)	(.025)	(.026)
Total applications	.024	.015	−.002	−.013	.034
	(.042)	(.037)	(.045)	(.050)	(.044)
N	6,542	6,542	9,659	3,294	6,542
C. Labor force participation:					
In labor force sample	−.012	−.017	−.021*	−.018	−.013
	(.012)	(.013)	(.014)	(.017)	(.013)
Fraction of quarters in labor force	.001	.000	−.010	−.029	.002
	(.015)	(.016)	(.015)	(.011)	(.015)
N	8,147	8,147	12,085	4,083	8,147

NOTE.—SUS = Florida State University System. Standard errors are clustered within grade bins. The *p*-values are calculated using a clustered wild bootstrap-*t* procedure described in Sec. III and app. 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, 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 8–14 years after high school. "In labor force sample" is a dummy equal to one if a marginal applicant shows up later in the earnings sample. "Fraction of quarters in labor force" is equal to the proportion of noncensored quarterly observations for each student. The "BW=.15" specification uses observations within .15 grade points above and below the cutoff and allows for a linear trend in distance from the cutoff. The "BW=.5" specification uses observations within .5 grade points on either side of the cutoff and allows for a quartic polynomial in distance from the cutoff. The "Local Linear" specification is identical to the main specification, but it allows for linear slope terms in distance from the cutoff that differ above and below the threshold.
* Significant at the 10% level.
** Significant at the 5% level.

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 it 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 III 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 30 hypothesis tests in panels A and B of table 3, three reject the null at the 10% level. Nor do I find evidence of differential selection into postcollege 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 of 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 V.B below.

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, et al. (2011) 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, Guldi, et al. (2011) and Barreca, Lindo, and Waddell (2011) 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 V.E.

B. 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 postcollege 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 to 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 a 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

Table 4
Effects on Academic Outcomes

Dependent Variable	Main	Controls	BW=.5	BW=.15	Local Linear
A. Admissions and attendance:					
Admitted to FIU	.234***	.233***	.246***	.282***	.205***
	(.021)	(.018)	(.022)	(.023)	(.016)
Attend FIU	.104***	.105***	.112***	.0980**	.088**
	(.025)	(.026)	(.029)	(.040)	(.027)
Attend SUS	.119***	.118***	.126***	.125**	.104***
	(.021)	(.023)	(.025)	(.037)	(.023)
Years SUS	.457**	.463**	.492**	.495**	.420*
	(.089)	(.094)	(.097)	(.114)	(.103)
SUS FTE terms	.644*	.643*	.698*	.650*	.622
	(.179)	(.192)	(.190)	(.185)	(.207)
B. SUS graduation:					
Within 4 years	−.007	−.008	−.008	−.009	−.005
	(.007)	(.007)	(.007)	(.009)	(.008)
Within 5 years	.002	.001	.008	−.002	.007
	(.018)	(.019)	(.018)	(.021)	(.021)
Within 6 years	.057	.057	.056	.044	.069
	(.022)	(.022)	(.026)	(.022)	(.024)
C. Other academic outcomes:					
Years community college	−.172*	−.171*	−.222**	−.199**	−.164*
	(.053)	(.051)	(.067)	(.055)	(.061)
Community college FTE terms	−.338***	−.327**	−.394**	−.412**	−.300**
	(.081)	(.081)	(.103)	(.101)	(.095)
Associate's degree within 6 years	−.009	−.005	.005	−.006	−.001
	(.021)	(.020)	(.021)	(.021)	(.025)
Vocational certificate within 6 years	−.007	−.007	−.006	−.009	−.006
	(.006)	(.006)	(.005)	(.003)	(.006)
Survey: out-of-state college	.006	.007	.007	.008	.005
	(.007)	(.007)	(.007)	(.007)	(.007)
Survey: in-state private college	−.012	−.014	−.010	−.021	−.009
	(.009)	(.009)	(.009)	(.014)	(.011)
N	6,542	6,542	9,659	3,294	6,542

NOTE.—FIU = Florida International University; SUS = Florida State University System; FTE = full-time equivalent. Standard errors are clustered within grade bins. The *p*-values are calculated using a clustered wild bootstrap-*t* procedure described in Sec. III and app. B. The SUS and community college attendance and degree variables are computed using schooling data from the first 6 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. The "BW=.15" specification uses observations within .15 grade points above and below the cutoff and allows for a linear trend in distance from the cutoff. The "BW=.5" specification uses observations within the .5 grade points on either side of the cutoff and allows for a quartic polynomial in distance from the cutoff. The "Local Linear" specification is identical to the main specification, but it allows for linear slope terms in distance from the cutoff that differ above and below the threshold.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

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

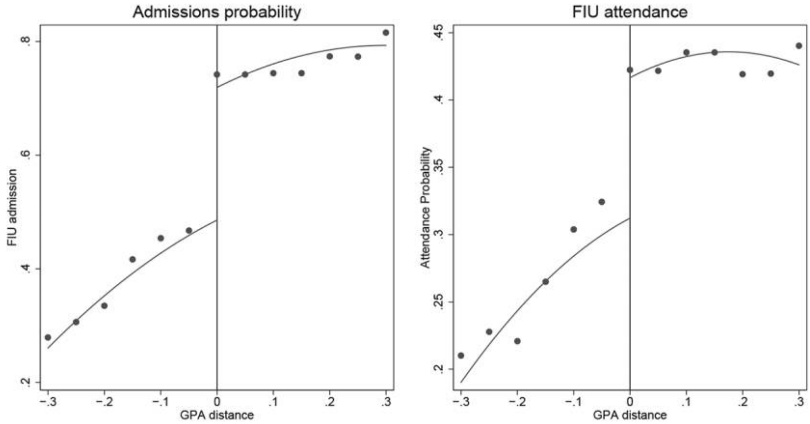

FIG. 4.—Admissions and FIU attendance. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.

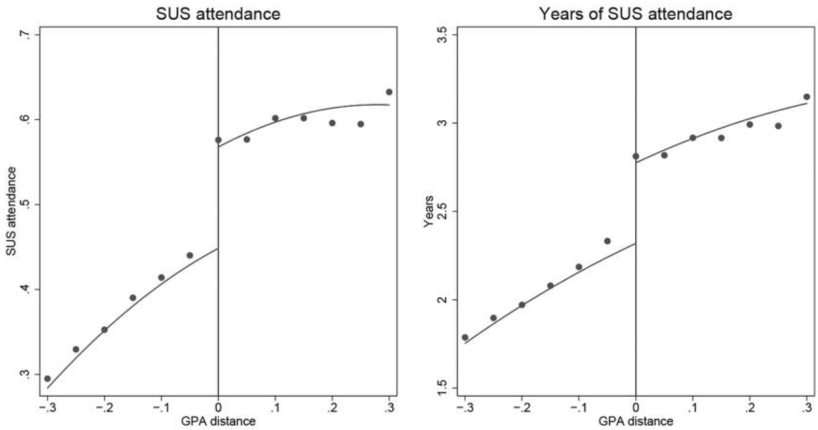

FIG. 5.—SUS attendance and persistence. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.

the case, the effect on overall SUS attendance (i.e., at attendance any campus) would be less than the effect on FIU attendance.

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

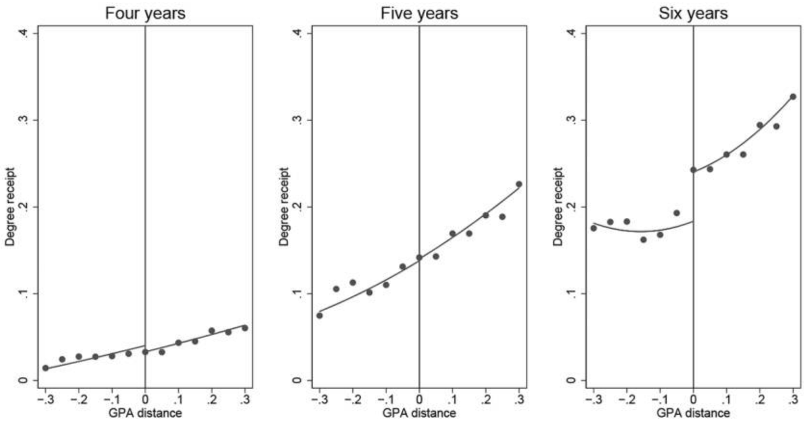

FIG. 6.—SUS BA receipt by years elapsed since high school. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.

figure 6 and panel B of table 4, threshold-crossing has no effect on the probability that students will have graduated from college by 4 or 5 years after high school. However, by 6 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 6-year graduation rate of 48%, statistically indistinguishable from the 49% 6-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 full-time-equivalent (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 2-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.

C. 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

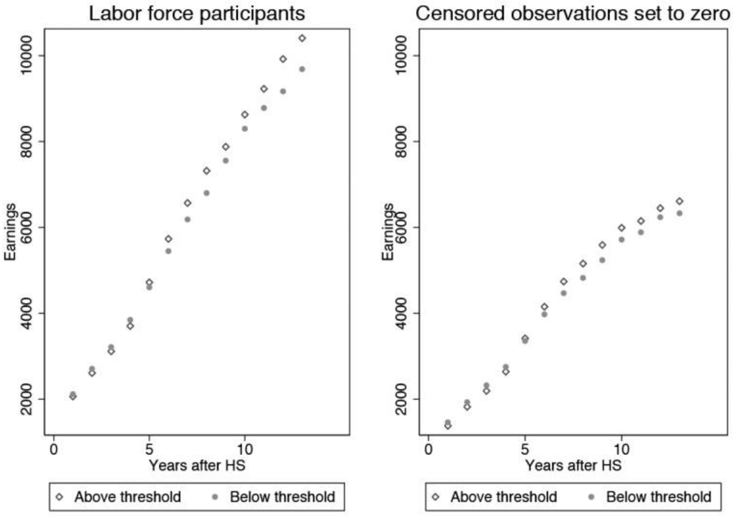

FIG. 7.—Quarterly dollar earnings by years since high school completion and status relative to admissions threshold. Quarterly earnings are averaged within each year category in the sample of marginal students. Left panel includes only uncensored (positive) observations. Right panel sets censored observations to zero. Both graphs drop means from 14 years following high school completion, which are estimated noisily.

students above and below the threshold with uncensored earnings reports. For the first 4 years following high school, below-threshold students earn about \$100–\$150 more than above-threshold students, who, as we have seen, are more likely to be enrolled in a SUS institution during that period. Earnings for above-threshold students surpass those for below-threshold students in year 5 following high school completion. The gap between above- and below-threshold earnings remains fairly steady thereafter at \$300–\$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 V.D.

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% 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 IV estimates that scale earnings effects by

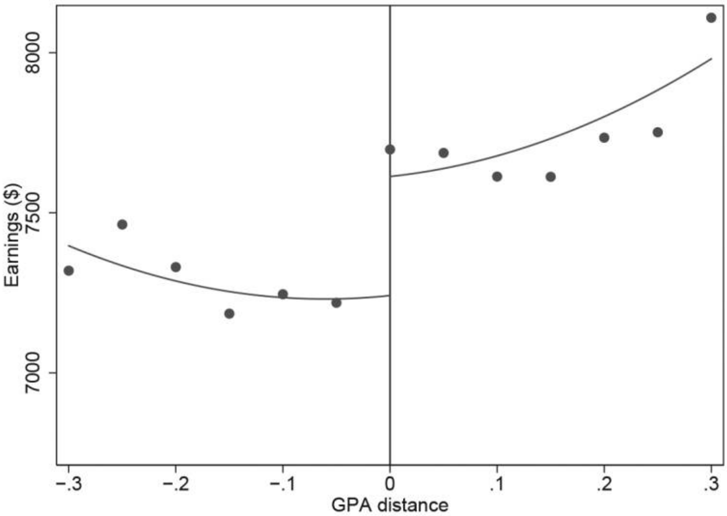

FIG. 8.—Quarterly earnings by distance from GPA cutoff. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.

Table 5
Earnings Effects 8–14 Years after High School Completion

	Main	Controls	BW=.5	BW=.15	Local Linear
Reduced-form estimates:					
Above cutoff	372*	366**	409**	479**	410**
	(141)	(130)	(154)	(198)	(147)
Instrumental variables estimates:					
FIU admission	1,593*	1,575**	1,665**	1,700**	2,001*
	(604)	(584)	(645)	(621)	(696)
Years of SUS attendance	815**	792**	833**	966***	977**
	(276)	(262)	(271)	(305)	(306)
BA degree	6,547*	6,442*	7,366*	10,769	5,958**
	(2,496)	(2,411)	(2,998)	(5,726)	(2,024)
N	6,542	6,542	9,659	3,294	6,542

NOTE.—FIU = Florida International University; SUS = State University System; BA = bachelor's degree. Standard errors are clustered within grade bins. The p-values are calculated using a clustered wild bootstrap-t procedure described in Sec. III and app. B. The dependent variable in each regression is average quarterly earmings in 2005 dollars. The "BW=.15" specification uses observations within .15 grade points above and below the cutoff and allows for a linear trend in distance from the cutoff. The "BW=.5" specification uses observations within the .5 grade points on either side of the cutoff and allows for a quartic polynomial in distance from the cutoff. The "Local Linear" specification is identical to the main specification, but it allows for linear slope terms in distance from the cutoff that differ above and below the threshold.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

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 $6,547 per additional BA recipient. These are equal to 22%, 11%, and 90% 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 it simultaneously reduces CC attendance. That is, the estimated effects are net of any earnings losses from forgone community college attendance, and they 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% of below-threshold earnings. Card (1999) presents OLS estimates of Mincer earnings regressions in Current Population Survey data and finds a return of 14.2% for men and 16.5% 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%. 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% 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.

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 10% level. Estimated effects are somewhat larger for Hispanic students than for black

[18] Evidence on the effects of community college attendance on earnings is mixed. Kane and Rouse (1995) find that earnings effects of 4-year and 2-year college credits are similar, while Reynolds (2012) finds evidence that attending 2-year college has a negative impact on earnings. See Reynolds for a review of the literature.

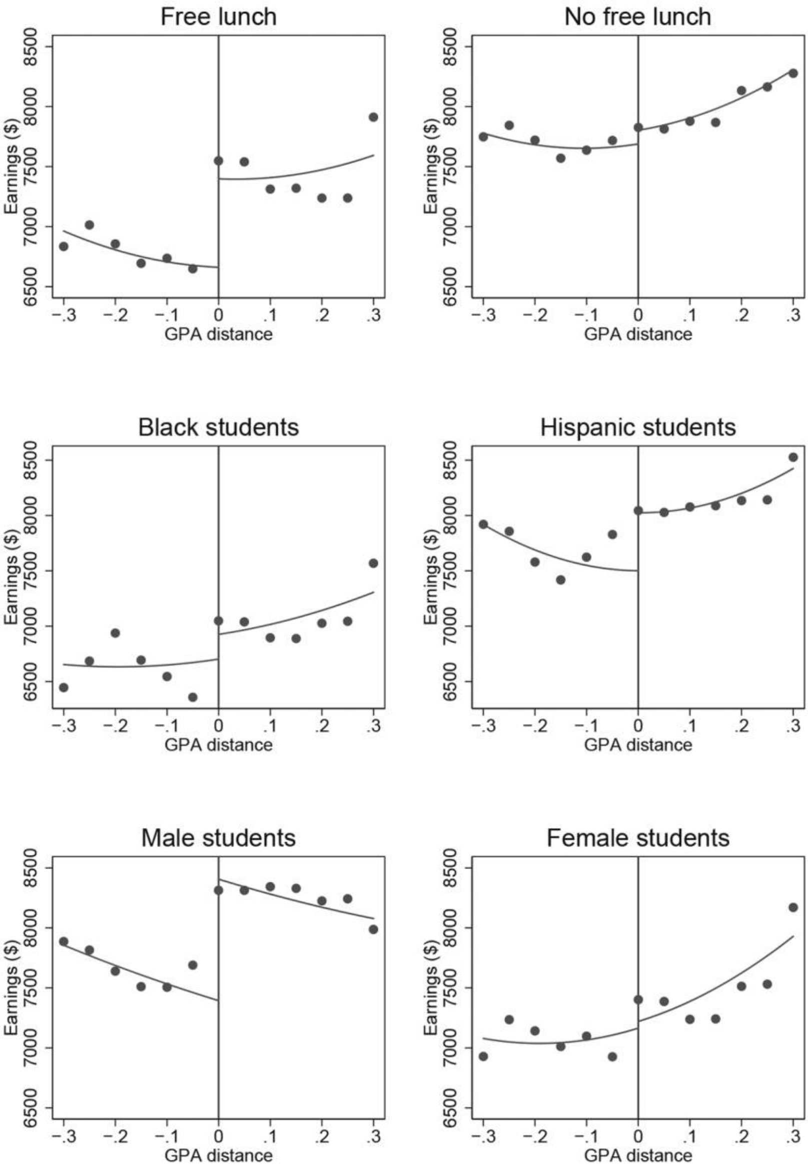

FIG. 9.—Heterogeneity in earnings effects. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.

students, although the difference is not significant and the discontinuity is not as visually clear for Hispanics.

To better understand the sources of differences in earnings effects, 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 ac-

Table 6
Heterogeneous Effects in Educational Outcomes and Earnings

	Sample					
	Black	Hispanic	Male	Female	Free or Reduced-Price Lunch	No Free or Reduced-Price Lunch
A. Educational outcomes:						
FIU admitted	.276***	.233***	.242***	.230***	.274***	.212***
	(.031)	(.032)	(.040)	(.017)	(.037)	(.023)
Attend SUS	.140*	.118***	.102**	.129***	.140***	.111*
	(.040)	(.040)	(.027)	(.027)	(.037)	(.032)
Years SUS	.463	.394**	.477**	.436**	.477***	.474*
	(.178)	(.108)	(.149)	(.110)	(.104)	(.128)
BA in 6 years	.055	.063	.081	.043	.054	.063
	(.033)	.021	(.034)	(.018)	(.026)	(.024)
Years CC	−.245	−.098	−.261	−.129	.010	−.336
	(.128)	(.102)	(.143)	(.076)	(.116)	(.111)
AA in 6 years	−.047	.029	.011	−.020	−.006	−.012
	(.026)	(.047)	(.027)	(.023)	(.038)	(.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	2,123	3,148	2,261	4,281	2,989	3,553

NOTE.—FIU = Florida International University; SUS = Florida State University System; BA = bachelor's degree; CC = community college; AA = associate's degree; IV = instrumental variable. Standard errors are clustered within grade pins. The *p*-values are calculated using a clustered wild bootstrap-*t* procedure described in Sec. III and app. B. All estimates are computed using the main specification above.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

ademic 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.

D. 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 (Integrated Postsecondary Education Data System), 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 fi-

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

[20] See app. 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.

nancial 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 they incur $4,308 in education-related expenditures. Over the 6 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 edu-

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 ($)	1,166	4,560	3,327*	930
Expenditure ($)	4,904	19,372	11,913	4,608
CC:				
Private cost ($)	199	568	−348	207
Expenditure ($)	4,308	13,022	−6,199**	1,664
Sum: SUS and CC:				
Private cost ($)		5,128	2,979*	873
Expenditure ($)		32,394	5,713	3,995

	B. Labor Market Outcomes 1–7 Years after High School		
	Descriptive Statistics	Admissions Effects	
Outcome	Sample Mean	Effect	SE
Mean quarterly earnings ($)	4,380	−200	322
Fraction quarters employed	.73	−.047	.034
Total earnings ($)	94,368	−12,294	7,380

NOTE.—N = 6,542. SUS = Florida State University System; CC = community college. 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 6 years following high school completion. The "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: standard errors are clustered within grade bins. The p-values are calculated using a clustered wild bootstrap-t procedure described in Sec. III and app. B.
* Significant at the 10% level.
** Significant at the 5% level.

cational expenditures at SUS institutions by $11,913. It reduces private payments to community colleges by only $348 but reduces educational expenditures by $6,199. Although 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 1 and 7 years after high school completion. I treat censored earnings values as zeros in this analysis to allow for extensive margin effects of college admission. Dropping censored values reduces estimates of indirect costs. Students in the analysis sample have nonzero earnings in 73% of quarters over the period, and they 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 nonadmitted students, and they 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.

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 V.C. To 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 nonadmitted students are incurred evenly over years 1–4 and over years 5–6 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 4 years after high school completion, as shown in figure A1. Second, I assume that forgone earnings effects are incurred evenly over years 1–4 and over years 5–7 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, that is, within the first 14 years following high school completion. The second considers earnings outcomes through 47 years after high school, or approximately age 65. I present both

Table 8
Internal Rate of Return to the Marginal Admission

	Private	Social	Social (Including Deadweight Loss)
A. Present discounted values of costs and benefits at $r = .06$ ($):			
Direct costs	2,493	4,565	5,187
Indirect costs	11,093	11,093	11,093
Benefits through 14 years after high school	15,853	15,853	15,853
Net return through 14 years after high school	2,267	195	−427
Benefits through 47 years after high school	42,729	42,729	42,729
Net return through 47 years after high school	29,143	27,071	26,449
B. Internal rates of return ($):			
Through 14 years after high school	.0822	.0618	.0561
Through 47 years after high school	.1516	.1389	.1355

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

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 estimates 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% into estimates of direct costs.[21] One might think of column 2 as representing the sum of private costs and budgeted costs to the government and column 3 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 14 years after high school completion, while the investment roughly breaks even from a social perspective. The private IRR is about 8%, while the social IRRs are about 6%. Through 47 years after high school, students realize net private returns of just under

[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.

$30,000, and government and society realize returns of about $27,000. The private IRR is 15%, compared to a social IRR of 14%. The takeaway here is that by 14 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.

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).

E. 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, Lindo, et al. (2011). 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 time frames 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 8 and 14 years after high school completion. Focusing on years 8–10 results in somewhat larger effects, while focusing on years 11–14 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.

VI. Discussion

In this article, I use a regression discontinuity design to show that the earnings gains associated with the marginal 4-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% 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 4-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 inframarginal 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% 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; 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]

[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.

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 they 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.

Appendix A
Additional Tables and Figures

Table A1
Florida International University Admissions and Enrollment Statistics, Academic Year 2000–2001

Enrollment		Academics	
Total	23,591	SAT mathematics: 25th percentile	510
Men	10,283	SAT mathematics: 75th percentile	590
Women	13,308	SAT verbal: 25th percentile	510
Part-time	9,546	SAT verbal: 75th percentile	590
Full-time	14,045	High school grade point average	3.46
Black	3,390	Graduation rate	.49
Hispanic	12,975		
Applications		**Costs ($)**	
Total applications	5,891	In-state tuition	2,242
Total acceptances	3,176	Out-of-state tuition	9,580
Total enrollments	2,563	Room + fees	4,398
		Fraction receiving financial aid	.40
		Average financial aid value ($)	5,163

Source.—Florida International University Common Data Set Submissions, 2000–2001 and 2007–8 (Florida International University Office of Planning and Institutional Research 2012).

Note.—Enrollment data refers to degree-seeking students only. Academic characteristics are for degree-seeking, first-time-enrollee freshmen. The 6-year graduation rates are computed for the fall 2001 entering cohort; graduation rates for the fall 1999 entering cohort were 0.48. Applications data are for fall 2000 entrants. Tuition and financial aid are reported in nominal terms. The fraction of students receiving aid includes only full-time undergraduates receiving need-based aid.

Table A2
Florida State University System Admissions Rules

SAT	Required GPA	Fraction of Marginal Applicants
1140	2.0	.00
1110	2.1	.00
1090	2.2	.00
1060	2.3	.00
1030	2.4	.01
1010	2.5	.01
1000	2.6	.01
990	2.7	.01
980	2.8	.02
970	2.9	.02
< 970	3.0	.73
Did not take	3.0	.19

SOURCE.—Florida Administrative Rule 6C-6.002.

NOTE.—Sample: marginal applicants are defined as all FIU applicants with FIU-computed GPAs within .3 grade points of their individual-specific cutoff GPA, computed using SAT scores. $N = 6{,}542$.

Table A3
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
	24,690	4,310	4,852	3,538	741	3,689	5,618
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
		26,009	9,877	8,586	2,159	3,573	11,223
UF			3.47	3.26	3.14	3.12	3.31
			3.47	3.56	3.39	3.46	3.58
			30,239	7,052	1,282	2,194	13,329
USF				3.28	3.07	3.04	3.32
				3.28	3.06	3.13	3.28
				25,563	1,889	2,872	7,950
UNF					3.2	2.96	3.19
					3.2	3.04	3.17
					4,542	910	1,862
FAU						3.16	3.23
						3.16	3.12
						10,849	2,912
FSU							3.42
							3.42
							27,680

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 is the mean GPA at the column institution, and the third is 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 A4
Direct Costs of College Attendance ($)

	FTE Enrollment	Educational Expenses	Gross Tuition	Tuition Net of Institutional Aid	Tuition Net of All Aid
FIU	22,716	8,997	3,792	3,443	2,044
FSU	29,949	10,020	3,846	2,000	1459
FAU	14,311	12,925	3,510	1,047	142
UF	41,543	14,885	3,859	3,072	2,392
Mean all SUS	26,237	11,756	3,546	2,532	1,720
Miami-Dade CC	25,323	10,251	3,231	2,772	176
Broward CC	12,747	8,220	3,114	2,896	1,481
Palm Beach CC	8,390	9,128	2,801	2,639	1,812
Mean all CC	10,679	8,688	2,523	2,298	900

Source.—Delta Cost Project, based on IPEDS data.
Note.—Institution-level costs are from 2000. Rows define specific institutions or institution types. FIU = Florida International University; FAU: Florida Atlantic University; FSU: Florida State University; UF = University of Florida; SUS = Florida State University System; CC = community college. 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 the 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 the main text.

Table A5
Robustness of Core Results to Heaping and Topcoding

	A. Robustness to Controls for Heaping		
	Main	Drop Cutoff Heap	Trends in Heaps
FIU admit	.234***	.219***	.241***
	(.021)	(.027)	(.026)
Attend SUS	.119***	.149***	.131***
	(.021)	(.017)	.017
Years SUS	.457**	.502***	.494***
	(.089)	(.109)	(.071)
BA in 6 years	.057	.062	.065*
	(.022)	(.030)	(.017)
Years CC	−.172*	−.194**	−.180**
	(.053)	(.065)	(.049)
AA in 6 years	−.009	−.021	−.007
	(.021)	(.026)	(.019)
Earnings	372*	400	402
	(141)	(227)	(163)
N	6,542	5,626	6,542
	B. Robustness to Topcoding Procedures		
	Main	98th Percentile	99.5 Percentile
Earnings	372*	346*	380**
	(141)	(142)	(143)
N	6,542	6,542	6,542

Table A5 (*Continued*)

	C. Time Frame of Earnings Gains		
	Main	Years 8–10	Years 11–14
Earnings	372*	403*	154
	(141)	(160)	(228)
N	6,542	6,477	2,421

NOTE.—Standard errors (in parentheses) are clustered within grade bins. Significance is calculated using a clustered wild bootstrap-t procedure described in Sec. III and app. B. Estimated coefficients on threshold crossing are reported in all rows. All estimates are computed using the main specification defined above. Panel A: "Main" reproduces results from the main text. "Drop Cutoff Heap" drops observations with grades equal to the cutoff value. "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 restricts earnings observations to the listed years since high school completion.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

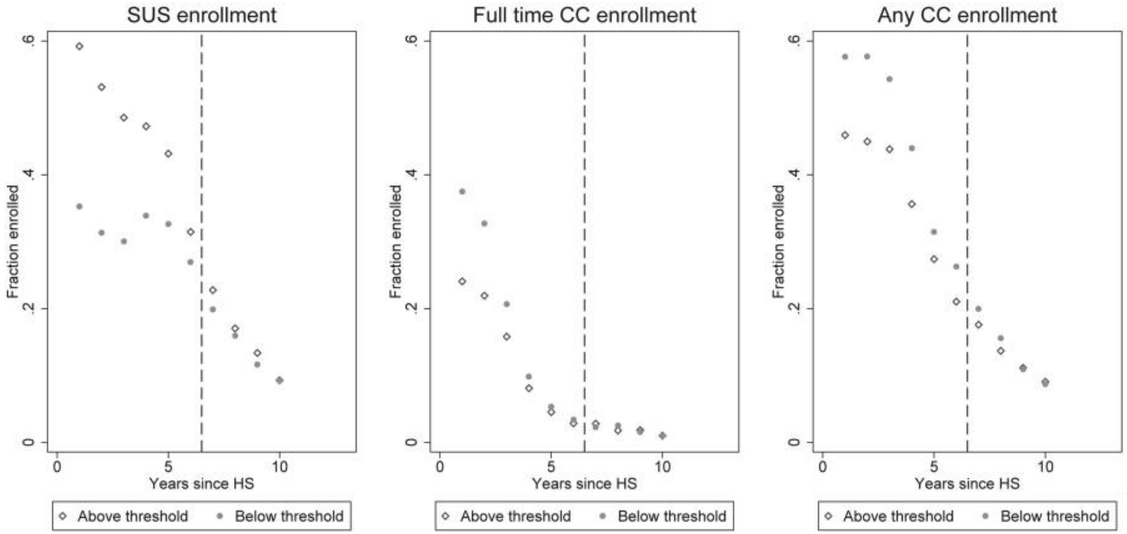

FIG. A1.—Postsecondary enrollment by years since high school. Mean enrollment by status relative to cutoff and years since high school completion. Sample: marginal students. Points to the left of the dashed lines are included in measures of educational attainment used in this article.

Appendix 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 .3 grade points, there are 46 clusters, and the seven largest account for 68% of observations. In the specifications with window width of .5, there are 75 clusters, with the 11 largest accounting for 64% of observations. And in the specifications with window width of .15, there are 24 clusters, with the largest three accounting for 68% 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. (2008), 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.

Appendix C

Data Description

A. Overview

I obtained this data set through agreements with the Florida Department of Education and the College Board. I have data on seven cohorts of students (twelfth-graders in 1996, 1997, 1999, 2000, 2001, 2002, and 2004, where years refer to the spring of the academic year) from 15 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

United States.[23] I did not have access data from other cohorts or counties when conducting this analysis.

I track each cohort of twelfth-graders backward through the 1996 school year and forward through the 2008–9 school year. The Florida State University System data include 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 include earnings (not hours or wages) for workers employed in Florida. Earnings data run from 1995 through the first quarter of 2010.

B. Construction of Key Variables

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

Education Variables

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 twelfth-grade students as having applied to FIU if they apply for admission to any term of the following academic year. I code twelfth-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% 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% of SAT takers (see table A2). My results are robust to excluding these students.

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.

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 academic year. To create a count of total years of SUS or CC attendance, I sum year-specific enrollment variables over the first 6 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

[23] In the 1999–2000 school year, Dade was the fifth largest district, Broward the sixth, Hillsborough the thirteenth, and Orange the sixteenth. In addition, Polk was the thirty-seventh largest. See Young (2001, app. A)

terms if students take fewer than 12 credits and full terms if they take 12 or more credits. I then sum over all terms over the first 6 years after high school. To count terms of CC attendance, I use a part-time/full-time designator provided by the Florida Department of Education; 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 6 years after high school.

Demographic Variables

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

Free-lunch status: Free-lunch status may vary by enrollment year and term. I code a student as a free-/reduced-price-lunch recipient if he or she is ever reported as eligible.

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. The 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 20 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.

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 nontuition expenditures such as room and board, so tuition values are slightly negative in a few cases. Including grant aid targeted at nontuition expenditures like room and board seems reasonable in this application because students receive these subsidies only if they enroll in college, but they 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).

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