

THE EFFECT OF ATTENDING THE FLAGSHIP STATE UNIVERSITY ON EARNINGS: A DISCONTINUITY-BASED APPROACH

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Abstract—This paper examines the effect of attending the flagship state university on the earnings of 28 to 33 year olds by combining confidential admissions records from a large state university with earnings data collected through the state's unemployment insurance program. To distinguish the effect of attending the flagship state university from the effects of confounding factors correlated with the university's admission decision or the applicant's enrollment decision, I exploit a large discontinuity in the probability of enrollment at the admission cutoff. The results indicate that attending the most selective state university causes earnings to be approximately 20% higher for white men.

I. Introduction

WHILE there has been considerable study of the effect of educational attainment on earnings, less is known regarding the economic returns to college quality. This paper examines the economic returns to college quality in the context of attending the most selective public state university. It does so using an intuitive regression discontinuity design that compares the earnings of 28 to 33 year olds who were barely admitted to the flagship to those of individuals who were barely rejected.

Convincingly estimating the economic returns to college quality requires overcoming the selection bias arising from the fact that attendance at more selective universities is likely correlated with unobserved characteristics that themselves will affect future earnings. Such biases could arise for two reasons. First, bias could arise if certain student abilities or characteristics are observed by college admissions committees but not by the econometrician. Second, there could be bias if, conditional on all observable student and family characteristics and admission to the more selective university, the decision to attend that university is correlated with unobserved student or family characteristics that would themselves affect subsequent earnings.

Researchers have taken several approaches to answering this question and have done so primarily in the context of private colleges or universities generally rather than state flagship universities. Black and Smith (2004) describe problems that can arise for much of this literature that relies on the assumption of selection on observables. In attempts to overcome those problems, several other approaches have been applied. Dale and Krueger (2002) compare the earnings of students attending more selective colleges to those of students who were accepted at similarly selective col-

leges but chose to attend less selective institutions. They find that attending more selective colleges has a positive effect on earnings only for students from low-income families. Brewer, Eide, and Ehrenberg (1999) estimate the payoff by explicitly modeling high school students' choice of college type and find significant returns to attending an elite private institution for all students. Behrman, Rozenzweig, and Taubman (1996) identify the effect by comparing female twin pairs and find evidence of a positive payoff from attending Ph.D.-granting private universities with well-paid senior faculty. Using a similar approach, Lindahl and Regner (2005) use Swedish sibling data and show that cross-sectional estimates of the selective college wage premium are twice the within-family estimates.

This paper uses a different strategy in that it identifies the effect of school selectivity on earnings by comparing the earnings of those just below the cutoff for admission to the flagship state university to those of applicants who were barely above the cutoff for admission. To do so, I combined confidential administrative records from a large flagship state university with earnings records collected by the state through the unemployment insurance program. To put the selectivity of the flagship in context, the average SAT scores of the entering classes at the flagship over the period of time studied were between 1000 and 1100, approximately 65 to 90 points higher than the five next-most-selective public state universities.

The unique data set used allows this paper to make two primary contributions to the existing literature. First, by using the application data from a large flagship state university, this paper addresses the question of how college selectivity affects earnings in the context in which the public policy decision is made. Although determining the effect of attending an elite private college over a less selective one is interesting for several reasons, the public policy question is largely confined to the extent to which admission at flagship state universities affects subsequent earnings.

In addition, because this paper uses actual admissions data from the university, it can apply an intuitive discontinuity-based research design to detect whether there are economic returns to college quality. This is done by applying a regression discontinuity design like that used in several other recent papers (e.g., Jacob & Lefgren, 2004), though similar results are obtained when estimated using an approach similar to Angrist and Lavy (1999).¹ I report both intent-to-treat

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¹ These results are available on the author's Web site. The primary benefit of the more traditional instrumental variable approach is that the two-dimensional admission rule does not need to be collapsed into one dimension in order to implement it.

effects (i.e., the effect of admission at the flagship) and enrollment effect estimates, both of which capture local average treatment effects (Angrist, Imbens, & Rubin, 1996).

I find strong evidence of large positive discontinuities at the admission cutoff in the earnings of white men ten to fifteen years after high school graduation.² The estimates imply that enrolling at the flagship state university yields an economic return of approximately 20% higher earnings. This earnings premium is relative to those applicants who were barely rejected by the flagship in a state in which over 85% of college-going residents attend college in-state and in which there are seven less selective public state universities from which to choose. Finally, I find no evidence that admission to the flagship causes applicants to be more or less likely to participate in the in-state labor force ten to fifteen years later.

II. Data and Identification Strategy

A. Data Sources

The data used in this study are from two sources. The administrative data on admissions were acquired from a large flagship state university. As part of the agreement in acquiring the data, I agreed not to disclose the name of the institution. The university was able to retrieve the following information for every white student who applied for admission to the university from 1986 to 1989: social security number, sex, term for which the student was applying for admission, ACT score,³ SAT score, and whether the student subsequently enrolled. Finally, I also observed each student's high school GPA, a discrete (to the nearest tenth of a point) number recalculated by the university after excluding certain courses and adjusting for the different scales used by high schools.

These data were then sent directly from the university admissions office to a state office to which employers submit unemployment insurance tax reports. The provided social security numbers were used to match quarterly earnings records from 1998 through the second quarter of 2005 to the university records. All nominal wages were adjusted using the CPI so as to be measured in 2005 dollars.

Of the total number of white male applicants, approximately 2.5% were excluded from this analysis because they were missing an SAT score or GPA. An additional 19% of

the applicant pool were dropped because they missed the admission cutoff by more than 300 SAT points, exceeded it by more than 350 SAT points, or had grade point averages higher than 4.0 or lower than 2.0.⁴ Finally, 86 applicants were excluded because their applications were cancelled prior to the admission decision. The final sample contains data on 12,189 applicants, of whom 8,424 were observed with at least one year of earnings between ages 28 and 33.

One advantage of these earnings data is that they allow the examination of earnings well after nearly all applicants have completed their educations. The primary results in the paper are based on earnings observed ten to fifteen years after high school graduation, or when the applicants were between the ages of twenty-eight and thirty-three. These earnings are much more likely to be predictive of lifetime earnings than earnings observed for people in their early and mid-twenties who are still finishing their educations and sorting themselves in the job market. Another advantage of these administrative data is that they likely contain less measurement error relative to survey data. Finally, unemployment insurance coverage is mandatory in the state, with relatively few exemptions allowed.⁵

One drawback of using this data set is that an individual's earnings will not be observed if he has moved out of state. While this would be of particular concern if working in-state were endogenous to admission at the flagship state university, in results available on request, I find no evidence that this is the case.⁶

Perhaps the largest drawback of the data used in this paper is that no information is available on where rejected applicants ultimately attended college, though in section VI, I describe survey data and the characteristics and availability of alternative in-state public universities that provide some guidance as to the nature of the counterfactual.

Finally, while these data allow me to display strong evidence that the effect of attending the flagship state university on earnings is large, it is difficult to determine the

⁴ The results are similar when the entire sample of applicants observed with SAT scores and high school GPAs is used.

⁵ For example, employers must report their employment wages if their quarterly payroll exceeds \$1,500 or if it meets any number of other criteria. Employment not covered that could potentially be relevant for the individuals here includes employees of a church or religious association, unsalaried insurance agents, real estate agents, and barbers who are paid solely on commission and unsalaried speech, occupational, and physical therapists who contract with health agencies.

⁶ Specifically, I examine whether there is a discontinuity in the likelihood of being observed with annual earnings in each of the seventh through fifteenth years after high school graduation. Second, I examine whether there is a discontinuity in the likelihood that individuals are observed with at least one year of earnings from ages 28 to 33. I find nothing in either case. A related objection is that perhaps the most capable of the rejected applicants attend college out of state, while the least capable of the barely accepted applicants choose to attend college or work out of state. While I cannot completely rule out this type of selective, offsetting attrition, it is comforting to note that relatively few students from the state in question attend out-of-state universities. According to the 1998 Residence and Migration Survey, over 85% of college students originally from this state were enrolled in a college in their home state.

² I focus on the results for men due to the potential difficulty in interpreting the results for women during the ages examined. Specifically, while I find no evidence of a difference in the labor force participation rates of women at the admission cutoff, I do not observe the number of hours worked. Consequently, if women who enroll at the flagship are (say) more likely to work part time during the examined ages of 28 to 33, I may not observe a premium in earnings even if wages were higher. With that caveat in mind, in results available from my Web site, I show that while there is little evidence of an effect for white women overall, there is suggestive evidence of a positive effect for white women with strong attachment to the labor force.

³ ACT scores were converted to SAT score equivalents using a conversion table published by the university.

precise mechanism through which earnings are increased. This issue is explored further in section VI.

B. Earnings Measures

Earnings were calculated in the following way. For each applicant, I calculated the natural log of the sum of four quarters of consecutive real earnings in each of the tenth, eleventh, twelfth, thirteenth, fourteenth, and fifteenth years following high school graduation, or when the individuals were approximately twenty-eight to thirty-three years old.⁷ Consequently, only the earnings of those who had positive reported earnings for the four consecutive quarters were included in the calculations, although qualitatively and quantitatively similar estimates resulted when an average earnings figure was calculated on the basis of nonmissing quarterly earnings figures.⁸

Since no patterns in the estimates of the effect of enrolling at the flagship were evident over time when the earnings measures were examined year by year, I stacked the annual earnings figures from the tenth through fifteenth years together. This resulted in a data set that contained up to six observations of earnings for each applicant. To ensure that each applicant is included in the analysis only once while at the same time reducing noise as much as possible, I use a first-within-then-between estimator. That is, first I estimate the following:

$$\ln(\text{Earnings}) = \psi_{\text{Year}} + \varphi_{\text{Experience}} + \theta_{\text{cohort}} + \varepsilon, \quad (1)$$

where ψ_{Year} is a vector of year dummy variables, $\varphi_{\text{Experience}}$ is a vector of dummy variables for the number of years after high school graduation in which the earnings were observed, and θ_{cohort} is a vector of dummy variables controlling for the cohort in which the individual applied to the flagship (e.g., fall 1988). The residuals from this regression were then averaged for each applicant, with the resulting average residual earnings measure being used to implement

⁷ The advantage of examining earnings in this time period was identified by Mincer (1974), who showed that the return to schooling can be underestimated if earnings prior to the “year of overtaking” are used. Assuming that the cost of investment is constant over time, that year is equal to $(1 + 1/r)$ years after the completion of formal education, where r is the interest rate. Thus, assuming $r = 0.09$ and an applicant finishes schooling at age 22, the year of overtaking is $22 + 12.1 = 34.1$, approximately the age examined in this paper. This matters to the extent that attending the flagship university causes differences in postschooling investment. For example, to calculate earnings after 15 years for those who applied for admission in the fall of 1986, I used earnings received from the third quarter of 2001 through the second quarter of 2002. Similarly, for those who applied for admission in fall 1987, I used earnings received from the third quarter of 2002 through the second quarter of 2003, and so on.

⁸ That measure was calculated as four times the average quarterly earnings over the four quarters, treating the lack of any reported income in a quarter as a missing value rather than as a 0. This is due to the fact that for individuals who as 18 year olds were near the admission cutoff of the flagship university, missing values for an entire quarter are more likely due to out-of-state employment or full-time schooling than unemployment.

the result of the Frisch-Waugh-Lovell theorem in the primary earnings estimations.

C. Identification Strategy and Estimation

This paper uses the admission discontinuity to estimate the causal effect of enrollment at the state’s flagship university on earnings. This design will distinguish the effect of enrollment at the flagship university from other confounding factors so long as the determinants of earnings (e.g., motivation, parental support) are continuous at the admission cutoff. Under this assumption, any discontinuous jump in earnings at the admission cutoff is properly interpreted as the causal effect of admission to the flagship university on earnings.

This condition will fail in this context if either applicants or the university can manipulate the side of the cutoff on which applicants fall. For applicants, this would be a problem if those who would barely miss the cutoff were to retake the SAT until they surpassed the cutoff. In reality, such a scenario is unlikely for the simple reason that the admission rule was never published or revealed by the university and, in fact, was changed (albeit moderately) from year to year. Consequently, it is unlikely that the applicant would know, prior to applying, whether she was just above the cutoff or just below it. While the best way of testing this would be to look for discontinuities in other predetermined outcomes, in the absence of such data, one can examine the distribution of applicants near the cutoff. In results available on request, I find no evidence of such a dip in the distribution that would suggest applicants can manipulate where they are relative to the admission cutoff.

It is also unlikely that the university defined the admission cutoff at the point at which students with above-average unobservables lie just above the line, whereas students with below-average unobservables lie just below the line.⁹ In contrast, according to those familiar with the process at the time, the admission cutoffs were designed in order to achieve a target enrollment level.

One approach to estimate the discontinuity at the cutoff is to compare the earnings of those who barely were admitted to those who were barely rejected. However, this approach ignores all earnings information for those who are also close to the admission cutoff, making the estimate more susceptible to noise in the data.

An alternative approach is used here instead. I show the discontinuity in the outcome of interest graphically by controlling for a function of the one-dimensional admission

⁹ Note that this is different from the university’s choosing to admit a student whose qualifications leave him short of the admission cutoff (or choosing not to admit someone whose qualifications left him above the cutoff). As discussed later, the existence of these applicants (noncompliers) does not undermine the identification strategy. Rather, the estimates presented should be interpreted as the effect only for those whose admission or enrollment decisions were determined by the admission cutoff (the compliers).

score that determines admission, described later and referred to as the adjusted SAT score.¹⁰ Specifically, I use OLS regression to estimate the equation

$$\begin{aligned} \text{Outcome} = & \beta_0 + \beta_1(\text{AboveAdmissionCutoff}) \\ & + \gamma_1(h(\text{Adjusted SAT Score})) + \gamma_2\text{GPA} \\ & + \gamma_3(\text{SAT Score}) + \varepsilon, \end{aligned} \quad (2)$$

where $\text{AboveAdmissionCutoff} = 1$ if $(\text{Adjusted SAT Score}) \geq 0$, $h(\text{Adjusted SAT Score})$ is a polynomial function of the adjusted SAT score—a one-dimensional admission score described in detail in the next section. In this specification, since the discontinuity in the probability of enrollment is less than one, all estimates of β_1 need to be reweighted by the discontinuity in enrollment in order to calculate the effect of enrolling at the flagship state university.

Finally, in estimating the earnings equation, I regress the (averaged-by-individual) earnings residuals from equation (1) on the residuals from the independent variables in equation (2) that result from partialing out year, age and experience, and cohort effects. In doing so, I apply the result of the Frisch-Waugh-Lovell theorem to recover the coefficients of interest in equations (2).¹¹

III. The Admission Rule

A. The Nature of the Admission Rule

Over the time period in question, the admission cutoff was a two-dimensional nonlinear sliding scale that depended on both SAT score and high school GPA.¹² Consequently, for a given high school GPA, students were admitted if their SAT score met or exceeded the cutoff SAT score. Higher GPAs meant that lower SAT scores were required for admission.

In order to more easily observe the importance of the discontinuity in the probability of admission and enrollment (and ultimately earnings), it is helpful to convert the admission rule into a one-dimensional rule. To do so, I calculated an adjusted SAT score for each student by subtracting the SAT score required for admission, given the student's high school GPA, from each student's actual SAT score. This effectively captures the number of SAT points that student scored above or below the admission cutoff, given that student's high school GPA. For example, if an SAT score of 1300 were necessary for admission given a student's high school GPA of 2.0 and that student scored a 1350 on the SAT, that student was assigned a score of 50. As a result, all

students assigned scores of 0 or higher were predicted to be accepted to the flagship university.

B. Determining the Admission Rule

While those on the admissions committee recall the general form of the admission rule, the university did not keep records of the exact admission rules used. Consequently, the admission rules were estimated using the admissions data. In order to do that, the data were first partitioned by term of application (either summer or fall). The data were then partitioned further by high school GPA, after which the following equation was estimated for each subgroup using ordinary least squares:

$$\text{Acceptance} = \beta_0 + \beta_1(\text{SAT_Cutoff}) + \varepsilon$$

where Acceptance is a dummy variable equal to 1 if the student was accepted and SAT_Cutoff was a dummy variable equal to 1 if the SAT score was greater than or equal to a given SAT score. For example, the SAT cutoff for the fall of 1986 for white applicants with a high school GPA of 3.5 was determined by repeatedly estimating this equation for that subset of applicants using all possible SAT scores as the cutoff. The SAT score that resulted in the estimation with the highest R^2 was the cutoff that was then used.¹³ This process was repeated for all cohorts. For example, it was repeated for the fall of 1986 for white applicants with a high school GPA of 3.6, and then 3.7, and so on.

IV. Does the Admission Cutoff Predict the Enrollment Decisions of Applicants?

The first empirical question to address is whether the probability of enrollment at the university is discontinuous at the admission cutoff. The discontinuity in enrollment is shown graphically in figure 1, which takes the same form as others presented after it. It shows the probability of enrollment at the flagship (the outcome) on the vertical axis and the number of SAT points above or below the cutoff given the student's high school GPA on the horizontal axis. The open circles represent local averages. For example, at an adjusted SAT score of 0 (students who barely met the estimated admission cutoff), the open circle is the percentage of those applicants who enrolled at the flagship. Also shown is a fitted line from the regression of the outcome on a polynomial of adjusted SAT score.

As shown in figure 1, the discontinuity in enrollment is approximately 37 percentage points, the t -statistic of which was just over 10.¹⁴ Consequently, it is clear that being just above the admission cutoff causes a large and statistically significant increase in the probability of attending the flagship state university.

¹³ The "winning" R^2 was typically around 0.5.

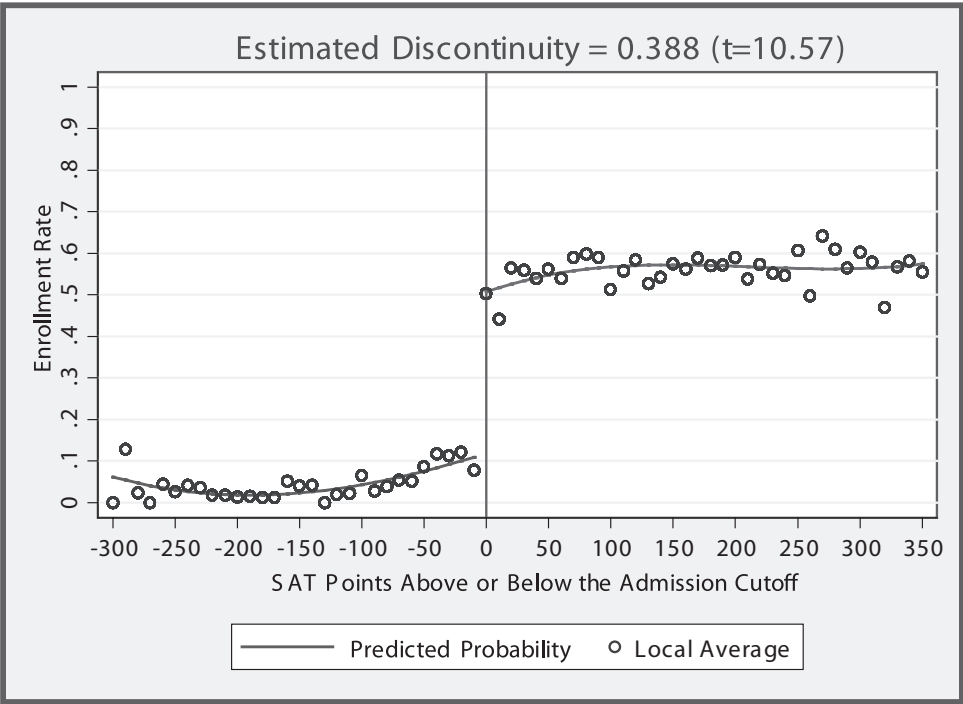
¹⁴ The discontinuity in admission is around 66 to 67 percentage points, depending on the specification.

¹⁰ An alternative approach that does not necessitate creating the one-dimensional score is to instrument for enrollment using a dummy variable equal to 1 if the individual is above the admission cutoff. This approach is similar to that of Angrist and Lavy (1999) and yields enrollment effects of around 15% to 20%. The results are available on the author's Web site.

¹¹ Due to the two-step process, standard errors were calculated by bootstrapping.

¹² At that time, students were not asked to write essays or include letters of recommendation.

FIGURE 1.—FRACTION ENROLLED AT THE FLAGSHIP STATE UNIVERSITY



V. Results

A. Earnings Discontinuities at the Admission Cutoff

To the extent that there are economic returns to attending the flagship state university, one should observe a

discontinuity in earnings at the admission cutoff. This is shown for white men in figure 2, which shows a regression of residual earnings on a cubic polynomial of adjusted SAT score. Table 1 shows the discontinuity estimates that result from varying functional form

FIGURE 2.—NATURAL LOG OF ANNUAL EARNINGS FOR WHITE MEN TEN TO FIFTEEN YEARS AFTER HIGH SCHOOL GRADUATION (FIT WITH A CUBIC POLYNOMIAL OF ADJUSTED SAT SCORE)

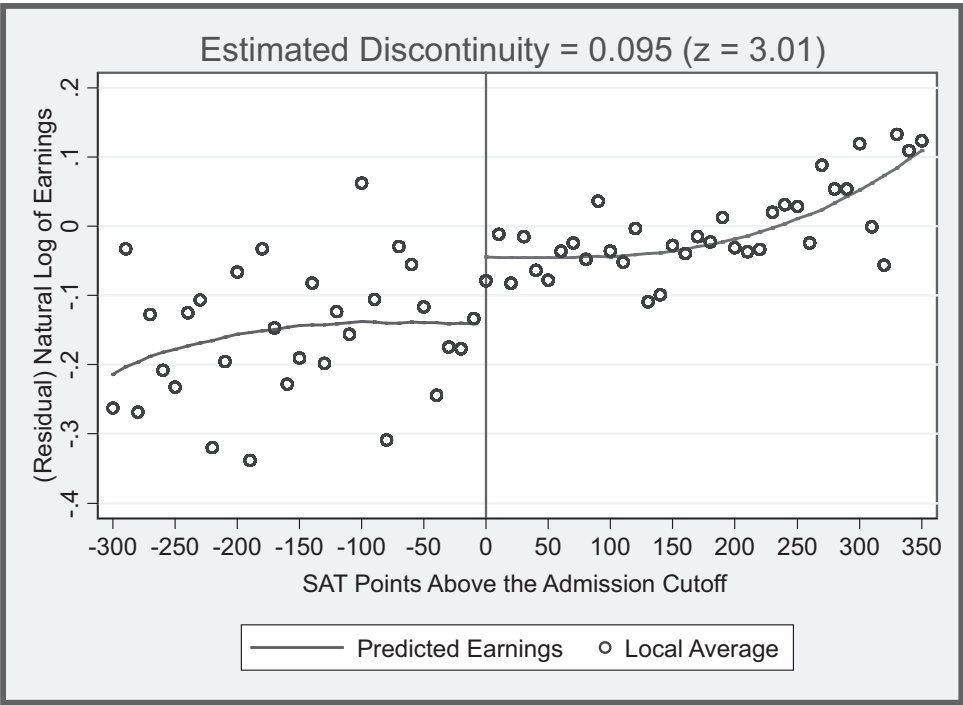


TABLE 1.—EARNINGS DISCONTINUITIES AND CORRESPONDING INTENT-TO-TREAT AND ENROLLMENT ESTIMATES FOR WHITE MEN

Regression Specification	Function of Adjusted SAT	Flexible Polynomial?	Additional Controls	Discontinuity	Treatment Effect	
				Estimated Earnings Discontinuity	Intent-to-Treat Effect	Enrollment Effect
(1) Plotted in Figure 2	Cubic	No	No	0.095*** (0.032) [0.003]	0.135*** (0.046) [0.004]	0.223*** (0.079) [0.005]
(2)	Cubic	No	Yes	0.092*** (0.033) [0.005]	0.131*** (0.048) [0.006]	0.216*** (0.081) [0.008]
(3)	Quadratic	Yes	Yes	0.111** (0.045) [0.014]	0.170** (0.073) [0.019]	0.281** (0.121) [0.021]
(4) (includes only applicants within 200 points of cutoff)	Quadratic	No	Yes	0.081** (0.038) [0.034]	0.116** (0.056) [0.038]	0.192** (0.094) [0.041]
(5) (includes only applicants within 100 points of cutoff)	Linear	No	Yes	0.074** (0.038) [0.050]	0.110* (0.058) [0.060]	0.181* (0.099) [0.067]

Notes: Bootstrapped standard errors are in parentheses; *p*-values are given in brackets. Additional controls include (residual) SAT score and (residual) high school GPA. “Flexible polynomial” indicates whether the estimated coefficients of the adjusted SAT score polynomial were allowed to differ on each side of the admission cutoff. *, **, and ***: statistical significance at the 10%, 5%, and 1% levels, respectively. Intent-to-treat and enrollment effects are estimated using two-stage least squares.

assumptions¹⁵ and bandwidths, the results of which reveal statistically significant earnings discontinuities on the order of 7.4% to 11.1%. In general, the estimates are larger as higher-order and more flexible polynomials are used. Finally, in results available on request, I find suggestive but inconclusive evidence that the earnings premium is larger for students who have high school GPAs above the median given their adjusted SAT score, term, and year. This suggests that students who, say, tend to work harder in the classroom but test lower experience either high human capital accumulation or a larger signaling effect due to attending the flagship.¹⁶

It is comforting to note that the estimates are not affected in a meaningful way by the inclusion of control variables such as actual SAT score and high school GPA, consistent with the identifying assumption of the regression disconti-

nuity design. In addition, the results do not appear to be driven by earnings outliers; the discontinuity using median regression fit with an inflexible cubic yields a statistically significant discontinuity estimate of 10.0%.

B. Intent-to-Treat and Enrollment Effects

In order to calculate the effect of admission and enrollment on earnings, the earnings discontinuities reported in the previous section must be reweighted by the enrollment (or admission) discontinuities due to the fuzzy discontinuities in admission and enrollment. I do this using two-stage least squares,¹⁷ for which the first-stage *F*-statistics are over 300. The results are shown in the last two columns of table 1 and suggest intent-to-treat (admission) effects ranging from 11.0% to 17.0% and enrollment effects of at least 18.1%.

VI. Interpretation

Like other papers (e.g., Jacob & Lefgren, 2004), the approach here uses a fuzzy discontinuity design due to the fact that the estimated discontinuities in admission and enrollment rates are less than 1. While this does not invalidate the research design, it does change the interpretation of the estimates. The estimates presented here are local average treatment effects and should be interpreted as the causal effect only for those applicants on the margin whose enrollment decisions were determined by the admission guideline. While clearly not everyone’s enrollment decision was determined by this admission cutoff, as shown in figure 1 it

¹⁵ Due to concern for potential specification error of the type described by Card and Lee (2006) that arises in estimating the cutoff assuming that the specification errors are independent but unequal, the standard errors were computed by bootstrapping using the adjusted SAT score as the resampling cluster. In addition, I ran goodness-of-fit tests as outlined in Card and Lee. While the null hypothesis that the polynomial is not too restrictive is not rejected for any of the polynomials in rows 1 to 3, the test statistic does indicate that the flexible quadratic is “better” than the inflexible quadratic (not shown) and the cubic is (very slightly) “better” than the flexible quadratic. Furthermore, while more flexible specifications for the restricted samples used in rows 4 and 5 are slightly preferred to those shown, allowing for higher-order and more flexible polynomials there yields even larger estimates than those shown. For example, allowing a flexible linear function in specification 5 increases the estimated discontinuity from 7.4% to 10.8%. The upshot is that to the extent there is specification error, it appears less likely for specifications yielding larger estimates.

¹⁶ Specifically, in equation (2), I add a dummy variable indicating if the applicant’s high school GPA was higher than the median for men with the same adjusted SAT score who applied in the same term and year. I also interact that dummy variable with the dummy variable indicating if the applicant was above the admission cutoff. The earnings discontinuity for students with above-median GPAs ranges from 3.2 to 7.8 percentage points higher than those for students with below-median GPAs for the five specifications shown in table 1, though only two of the differences are statistically significant at the 10% level.

¹⁷ For a rigorous discussion of the relationship between discontinuity estimates and 2SLS estimates, see Hahn, Todd, and Van der Klaauw (2001).

TABLE 2.—EARNINGS OF MALE APPLICANTS IN THE FOUR YEARS FOLLOWING HIGH SCHOOL AND THE EFFECT OF ADMISSION AND ENROLLMENT AT THE FLAGSHIP ON EARNING MORE THAN \$20,000 ANNUALLY DURING THE COLLEGE YEARS

Time Period	Summary Statistics				Admission and Enrollment Effects	
	Average Earnings	% Applicants in Labor Force	Average Earnings for Applicants in Labor Force for All 4 Quarters	% Sample Earning above \$20,000 per year	Effect of Admission on the Probability of Earning More Than \$20,000	Effect of Enrollment on the Probability of Earning More Than \$20,000
First year after high school	\$2,300 { \$3,545 }	63.9% { 48.0% }	\$7,403 { \$4,840 }	0.3% { 5.6% }	−0.003* (0.002)	−0.003*** (0.001)
Second year after high school	\$3,040 { \$4,709 }	63.4% { 48.0% }	\$9,132 { \$5,905 }	1.0% { 10.0% }	0.002 (0.003)	−0.005*** (0.002)
Third year after high school	\$3,845 { \$6,156 }	62.7% { 48.4% }	\$10,848 { \$8,174 }	2.5% { 15.7% }	−0.014*** (0.004)	−0.012*** (0.003)
Fourth year after high school	\$5,206 { \$8,091 }	65.2% { 47.6% }	\$13,403 { \$10,548 }	5.4% { 22.6% }	−0.003 (0.007)	−0.012** (0.004)

Note: The sample includes 15,321 white male applicants to the 1992–93 through 1995–96 academic years. Standard deviations are in brackets; robust standard errors are in parentheses. All specifications include a cubic of high school GPA and SAT score and indicators for the year-term in which the individuals applied. *, **, ***; statistical significance at the 10%, 5%, and 1% levels, respectively.

was clearly a determining factor for a significant proportion of applicants.¹⁸

The premium of 20% that accrues to men as a result of attending the flagship state university is larger than the size of the effect of one additional year of schooling, estimated at around 9% (Ashenfelter & Rouse, 1998). It is thus natural to wonder about the mechanism through which enrollment at the flagship increased earnings.

One question is whether barely rejected applicants attended college elsewhere. In answering that question, although I do not directly observe the enrollment decisions of those who do not enroll at the flagship (including those who fell just short of admission to the flagship), there certainly were several opportunities for those applicants to attend college elsewhere. Specifically, there were seven alternative in-state public universities, all of which charged tuition that was within 3% of that charged by the flagship. Second, while an ideal test would be to observe if there is a discontinuity in full-time labor force participation at the admission cutoff during the years in which the applicants would be in college, there are no earnings data available prior to the fourth quarter of 1992, and even then the data do not contain information on hours worked. Consequently, I examine whether cohorts who applied from 1992 to 1995 were more likely to earn at least \$20,000 a year in the four years following high school graduation if they were accepted to or enrolled at the flagship, conditional on high

school GPA and SAT score.¹⁹ As shown in table 2, fewer than 1% of applicants earn more than \$20,000 a year in the two years following high school graduation, increasing slightly to 2.5% and 5.4% in the third and fourth years, respectively. The estimated effects of admission and enrollment are negative and mostly statistically significant, though all are small and suggest that marginal rejection causes fewer than 1 in 100 applicants to meet the earnings threshold of \$20,000 a year. This is at least suggestive that those applicants are primarily attending college. Finally, given that this analysis relies on selection on observables to identify admission and enrollment effects, it is likely that these estimates overstate the negative effect of attending the flagship on earnings during the four years after high school.

Conditional on attending college, it is likely that the applicants attended college in-state: According to the national 1998 Residence and Migration Survey, over 85% of surveyed college students from the state in question were enrolled in their home state. Given that likelihood, the two primary potential explanations of the earnings premium due to attending the flagship state university are increased human capital formation and signaling.²⁰ With respect to the human capital explanation, the first possibility is that the flagship spends more on a full-time-equivalent basis than do the next-best-alternative state universities. The relative spending of the alternative state universities is shown in table 3. While the five next-most-selective alternative universities spend on average 6% less than the flagship, it

¹⁸ Several factors likely caused the fuzziness of the admission and enrollment discontinuities. First, a handful of high schools had reputations for giving lower grades than average. Second, as with any other Division 1 school, exceptions to the admission policy were made on the basis of nonacademic issues. In addition, some degree of error was possible in either the decision-making process or the reporting process, as well as potentially in the estimation of the rule in those cells in which there were relatively few observations. Even more significant, in any given term, the university aimed to enroll a certain number of students. Given uncertainty about yields, the university would often change the admission rule slightly during the process to accept more or fewer students, thereby introducing noise into the admission rule. Finally, there was an appeals process for rejected applicants, though it affected relatively few students and was described by one admissions officer on the committee at the time as “very noisy.”

¹⁹ The basis for using \$20,000 is that according to the 1996 CPS March Supplement, white men ages 20 to 22 in the flagship state who were not enrolled in college earned an average of \$19,633 (adjusted to 2005 dollars, as in the earnings data) in the prior year. Those who were enrolled in college reported earning just under \$9,000. Consequently, this is a test of whether individuals report earning more than average for an unenrolled individual or just over twice what the typical college enrollee reports making.

²⁰ The return might also be higher if the flagship were located in a more favorable labor market. In this state, however, the flagship university is in the least urban location of all the state universities.

TABLE 3.—SELECTIVITY AND EDUCATIONAL INPUTS OF THE IN-STATE PUBLIC UNIVERSITIES

In-State Selectivity Rank	Per Student Spending/Flagship's per Student Spending	SAT Points below Flagship for Entering Freshmen Class
1	100%	—
2	104	65
3	92	83
4	84	87
5	94	90
6	94	90
7	73	106
8	94	147

seems unlikely that this relatively small difference in spending could cause the large flagship earnings premium for men.

In contrast, it is more difficult to distinguish between other human capital accumulation explanations and signaling. For example, while table 3 shows that the next five most selective state universities have incoming freshmen classes with SAT scores between 65 and 100 points lower than the flagship's freshmen class, that fact is consistent with both human capital and signaling explanations of the premium.²¹

VII. Conclusion

In this paper, I identify the causal effect of attending the flagship state university by using a regression discontinuity design that compares the earnings of those who were just admitted by the flagship to the earnings of those who just missed the admission cutoff. I do so by combining confidential student applicant records from a large flagship state university with earnings data collected by the state through

²¹ One might expect that if the premium were to work primarily through signaling, it may well decline with the age. However, while no evidence of this was found, the ages studied (28 to 33) provide a relatively short time span with which to perform this test.

the unemployment insurance program. The results indicate that attending the flagship state university increases the earnings of 28- to 33-year-old white men by approximately 20%, which suggests significant economic returns to college quality, at least in the context of the most selective public state university.

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