The Effect of Attending the Flagship State University on Earnings: A Regression Discontinuity Approach

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Abstract

This paper examines the effect of attending the flagship state university on the earnings of 28 -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 and/or the applicant's enrollment decision, a regression discontinuity approach is used along with a conventional instrumental variable approach. The results indicate that attending the most selective state university causes earnings to be at least 10% higher for white men, an effect that is considerably higher than ordinary least squares estimates. However, I find no effect on earnings for white women generally and only weak evidence of a positive effect for white women with strong attachment to the labor force.

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1. Introduction

The question of the extent to which attending a more selective college or university affects subsequent earnings is of interest for several reasons. In addition to being of significance to the students and parents making decisions regarding whether to incur costs associated with either getting admitted and/or attending a more selective university, it is also tied to the question regarding peer effects in education—whether attending college with more highly qualified students increases learning and ultimately productivity in the labor force. In addition, states may well be interested in the extent to which attending the flagship state university benefits students in helping them decide how large the flagship university should become. Finally, the question of how attending the state flagship university affects the earnings of various student subgroups may also be relevant to the affirmative action debate in light of the reduction or elimination of such policies in various state university systems.

The empirical difficulty in estimating the effect of university selectivity on earnings is due to 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, bias could arise 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. For example, if the student chooses to attend the more selective university because she is more motivated than an observationally equivalent student who chose to attend a less selective school, the effect

of selectivity on earnings will be overstated. On the other hand, if the student chooses to attend the more selective university because she received more need-based financial aid at the selective school relative to an observationally equivalent student who chose to attend a less selective school, the effect of selectivity on earnings will likely be understated.

Researchers have taken several approaches to answering this question, doing 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." Several other approaches have also been applied, however. Dale and Krueger (2002) compare the earnings students attending more selective colleges to those of students who were accepted at similarly selective colleges but who chose to attend less selective colleges. 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 for attending Ph.D.-granting private universities with well-paid senior faculty; 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 utilizes a different identification 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.

The unique data set used allows this paper to make two primary contributions to the existing literature. First, by utilizing 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. Indeed, 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 the subsequent earnings of various subgroups.

Second, because this paper uses actual admissions data from the university, it can apply a regression discontinuity design to detect whether or not there is a discontinuity in earnings at the point of the admission cutoff. Doing so overcomes any biases that might arise due to the correlation of the decision to enroll at the flagship university with other unobserved factors that may themselves affect earnings so long as the assumptions for the regression discontinuity design are met. This approach yields two types of estimates, both of which are local average treatment effects. First, I estimate an "intent-to-treat" effect—that is, the effect of admission to the flagship state university. Second, I estimate the effect of enrolling at the flagship, which, following Angrist, Imbens, and Rubin (1996) requires the additional assumption that admission only affects earnings through

enrollment at the flagship.[1] As a comparison to the regression discontinuity estimates, I also estimate the effect of attending the flagship using both a more conventional two-stage least squares instrumental variable approach as well as ordinary least squares.

Using this approach, I find strong evidence of positive discontinuities in the earnings of white men 10-15 years after high school graduation. These positive earnings discontinuities and conventional instrumental variable estimates imply that enrolling at the flagship state university causes earnings to be at least 10% higher that they would be otherwise. These sets of estimates are at least three times larger than the enrollment effects estimated using ordinary least squares. However, I find no effect for white women generally and only very weak evidence of a positive effect for white women with strong attachment to the labor force. Finally, I find no evidence that admission to the flagship causes applicants to be more or less likely to be observed in the in-state labor force 10 – 15 years later.

2. Data

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 involved. The university was able to retrieve the following information for every student that applied for admission to the university from 1986 – 1989: Social Security Number, race, sex, term for which the student was applying for admission, ACT score, SAT math score, SAT verbal score, whether or not the student subsequently enrolled, year of birth, and whether or not the student subsequently graduated from the university. Finally, I also

[1] This rules out such things as applicants leveraging their acceptance at the flagship to secure admission at other universities.

observe 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 different scales used by high schools.

These data were then sent to a state office to which employers submit Unemployment Insurance tax reports. Using the provided Social Security Numbers, quarterly earnings records from 1998 through the second quarter of 2005 were matched to the university records. All nominal wages were adjusted using the CPI so as to be measured in 2005 dollars.

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 10 to 15 years after high school graduation—or when the applicants are between the ages of 28 and 33 years old. These earnings are much more likely to be predictive of lifetime earnings than are 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. A limitation, however, relates to the fact that an individual's earnings will not be observed if he or she is employed in a job not covered by the UI system or has moved out of state. The latter of these may be a particularly significant concern to the extent that working in-state is endogenous to whether or not the student was admitted to the flagship state university. Fortunately, the data also enable me to examine if there is a discontinuity in whether or not an applicant is observed with earnings.

There were 31,080 white high school graduates who applied for admission in the summer or fall of the 1986-87, 1987-88, 1988-89, or 1989-90 school years. Of those, there were 5,318 for whom I did not observe either a high school GPA or an ACT or SAT score. In addition, 1,816 applicants were excluded because their high school GPA was lower than 2.0 or higher than 4.0. One hundred seventy-eight more applicants were excluded because they cancelled their application prior to the admission decision while 886 applicants were excluded because they did not meet the minimum GPA required for admission in that term/year.[2] Finally, 1,830 applicants were excluded because they exceeded the admission cutoff by more than 300 SAT points or missed it by more than 350 SAT points.[3] Consequently, the final data set contains observations on 21,052 applicants.

3. Identification Strategy

This paper uses a regression discontinuity design to estimate the causal effect of admission at a state's flagship university on earnings. This design will distinguish the effect of admission to the flagship university from other confounding factors so long as the unobservable determinants of earnings (e.g., motivation, parental support, etc.) are continuous at the admission cutoff. As long as this condition holds, 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

[2] In some of the years, a 2.0 was the minimum GPA required. Later on, however, this minimum was increased and there was no SAT score observed that would ensure admission for someone with that GPA.
[3] The admission cutoff is discussed in Section 4. In addition, as one would expect, the exclusion of these students in the tails of the distribution does not affect the results reported later in the paper in a meaningful way.

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 from year to year. Consequently, it is very unlikely that the applicant would know, prior to applying, whether or not she was just above the cutoff or just below it.

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.[4] 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.

Applying a regression discontinuity design in this context is somewhat different from the standard application, however. The reason is that the admission cutoff rule is two dimensional rather than one dimensional since it depends on both the SAT score and the high school GPA and took the form of a nonlinear sliding scale. To address this issue and convert the two-dimensional sliding scale rule into a one-dimensional rule, an adjusted SAT score was created for each student. This was done by subtracting the SAT score required for admission, given the student's high school GPA, from each student's actual SAT score. For example, if an SAT score of 1300 were necessary for admission given a student's high school GPA 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. In case students with similar

[4] Note that this is different from the university 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 (e.g., the compliers).

adjusted SAT scores have different earnings potentials, actual SAT score and high school GPA were included as controls in some specifications when estimating the earnings effects.

One approach to estimate the discontinuity at the cutoff is to compare the earnings of those who barely were admitted (e.g., those with adjusted SAT scores of 0 or 10) to those who were barely rejected (e.g., those which adjusted SAT scores of -20 or -10). However, if earnings increase with adjusted SAT scores as is likely the case, this will overstate the effect of admission to the flagship university on earnings. In addition, 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.

The alternative approach is to estimate an equation for the outcome as a function of the adjusted SAT score. Specifically, I estimate the following equation using least squares regression:

$$Outcome_i = \beta_0 + \beta_1 X + \beta_2(AboveAdmissionCutoff_i) + \gamma(f(Adjusted\ SAT\ Score_i)) + \varepsilon_i$$

where $AboveAdmissionCutoff_i = 1$ if $(Adjusted\ SAT\ Score_i) \geq 0$, $f(Adjusted\ SAT\ Score_i)$ is a flexible polynomial function of the adjusted SAT score, and X is a vector of control variables. I then estimate this equation using various functions $f(\cdot)$.

4. The Admission Rule

4.1 Determining the Admission Rule

Because the admissions records are nearly 20 years old, the university did not have records of the exact rules used to determine admissions. During the time in question, however, admissions decisions were made using a discrete sliding scale of high school GPA (as adjusted by the university to account for course content and differences in high

school GPA scales) and SAT score.[5] That is, for a given high school GPA, the student was admitted if her SAT score met or exceeded the cutoff SAT score. Higher high school GPAs implied lower minimum SAT scores were necessary for admission. For example, to compensate for a high school GPA that was one tenth of a point lower, a student may have to have an SAT score that is 20 points higher.[6]

In order to estimate the admission cutoff, the data were first partitioned by race and term of application (either summer or fall). The data were then partitioned further by high school GPA, after which the following equation was estimated using ordinary least squares:

$$Acceptance = \beta_0 + \beta_1(SAT_Cutoff) + \epsilon$$

where *Acceptance* is a dummy variable equal to one if the student was accepted and *SAT_Cutoff* was a dummy variable equal to one 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 men with a high school GPA of 3.5 was determined by estimating this equation separately using all possible SAT scores (e.g., from 800 to 1600) as the cutoff. The SAT score that resulted in the estimation with the highest R^2 was the cutoff that was then used.[7] This process was repeated for all cohorts. For example, it was repeated for the fall of 1986 for white men with a high school GPA of 3.6, and then 3.7, etc.

[5] For some students, ACT scores were used instead. In those cases, I converted these to equivalent SAT scores using the university formula.
[6] To assure the confidentiality of the university that provided the data, I cannot reveal the admission standards as I estimated them. However, I can note that the tradeoff between SAT score and high school GPA was nonlinear.
[7] The "winning" R^2 was typically around 0.5.

4.2 Does the Admission Cutoff Predict the Admission and Enrollment Decisions of Applicants?

After estimating the admission cutoff, the obvious question is whether or not the probability of acceptance and enrollment at the university is discontinuous at the admission cutoff. The discontinuity in admission can be seen in Figure 1, which shows the probability of being accepted (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. This figure takes the same form as others presented after it. The open circles represent local averages. For example, at an adjusted SAT score of zero (i.e., for students who barely met the estimated admission cutoff), the open circle is the percentage of those applicants who were accepted at the flagship.

The estimates of the admission discontinuity shown in Figure 1 are reported in Table 1. Row (1) reports estimates of the discontinuity without controlling for either a function of the adjusted SAT score or the actual SAT score and high school GPA. In contrast, the regression equations that yielded the discontinuity estimates reported in rows (2) – (4) included polynomials of order 3 or higher in the adjusted SAT score, all of which were "flexible" in that they allowed for a different polynomial on either side of the admission cutoff. For example, the equation estimated in row (2) includes the variables (Adjusted SAT Score), $(\text{Adjusted SAT Score})^2$, and $(\text{Adjusted SAT Score})^3$ as well as those same variables interacted with a dummy variable equal to one if the applicant was predicted to have been admitted by the flagship (i.e., those for whom the adjusted SAT score ≥ 0). Specifications (2) and (4) also control for each applicant's actual SAT score and adjusted High School GPA.

The results are consistent in showing that there is a large and statistically significant discontinuity in the likelihood of acceptance at the admission cutoff on the order of 68 – 69 percentage points. For example, in the preferred specification in row (4) that allows for fourth-order polynomials of adjusted SAT score on both sides of the admission cutoff, the estimated discontinuity is 68.6 percentage points, which is statistically significant at all conventional levels. Furthermore, a look at the underlying data in Figure 1 shows that this discontinuity is not simply the consequence of an incorrect functional form.

Since any effect of admission to the flagship state university is most plausibly due to enrollment at the university, it is worth investigating the extent to which the admission cutoff predicts enrollment. This is shown graphically in Figure 2; estimates from various specifications are reported in Table 2. Both the graph showing the underlying data and the tabulated estimates suggest that the discontinuity in enrollment is approximately 40 percentage points and is not a perverse consequence of an incorrect functional form.

4.3 Potential Causes of the "Fuzziness" of the Estimated Admission Discontinuity

The fact that this estimated discontinuity is less than one means that this design utilizes a "fuzzy" discontinuity. Several factors may be responsible for this, all of which must be considered when interpreting the results on earnings. First, a handful of high schools had reputations for giving lower grades than average. While this was not taken into account by the university in calculating adjusted high school grade point averages, it was something taken into account during the admissions process. In addition, as in any process with human involvement, errors in either the decision-making process or the reporting process almost certainly exist to some degree. Even more significantly, 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.[8] In addition, there was an appeals process for rejected applicants. However, it affected relatively few students and was described by one admissions officer on the committee at the time as "very noisy". Finally, and perhaps most importantly, given the presence of any noise in the admission process such as that caused by the factors mentioned above, the estimation of the rule is itself probably characterized by some degree of error, especially for cells in which there were relatively few observations.

It is important to point out, however, that these factors described above do not invalidate the regression discontinuity design (or the more general instrumental variable design). Rather, the estimates produced are local average treatment effects and should be interpreted as the causal effect only for those whose admission decision (or enrollment decision) was determined by the admission guideline (i.e., the compliers). Consequently, while the effect is properly interpreted as the causal effect of admission (or enrollment) for the student on the margin—the student whom the state university policy is mostly likely to affect—the extent to which the effect generalizes to students on different parts of the distribution is an open question. Similarly, the effects reported in this paper cannot be interpreted as the causal effect on earnings for those students whose high school was among the handful that had a reputation for giving lower-than-average grades. However, the discontinuity shown in Figure 1 shows that the admission guideline was the defining factor in admissions for the majority of applicants near the cutoff. Further confirmation

[8] According to one admission official who served in the late 1980's, the university aimed to err on the side of a policy that was too low at first, then toughened it slightly more students chose to attend than expected. In this way the university didn't reject applicants who it would have accepted had the university processed the application somewhat more quickly.

that high school GPA and SAT score were the two primary considerations in the admission decision comes from the fact that high school graduates were not required to submit essays or letters of recommendation as part of the application.

Due to the fuzziness of the regression discontinuity design, all earnings discontinuity estimates must be rescaled by the estimated admission (enrollment) discontinuity in order to determine the appropriate intent-to-treat (enrollment) effect.

5. Applicants with Missing Earnings Data

As discussed earlier, one drawback to using these state-level earnings data is that some applicants may not be observed with positive earnings 10 - 15 years after high school graduation because they moved to a different state. For example, I observe earnings in all 4 quarters in the 12^{th} year after high school graduation for approximately 46% of the applicant pool. To the extent that the probability of working in-state is endogenous to admission and attendance at the flagship university, the discontinuity estimates may be biased. The data allow me to examine the extent to which this is the case by comparing the probability of observing positive earnings for applicants who were barely accepted to that of observing the earnings of applicants who were barely rejected. Furthermore, I do so separately for men and women due to the differences in their respective labor force participation rates. I also do so using two definitions of being observed with positive earnings. Specifically, I examine whether there is a discontinuity in the likelihood of being observed with 4 consecutive quarters of positive earnings in each of the 7^{th} through 15^{th} years after high school graduation as well as in the likelihood of being observed with any positive quarter of earnings in that year.

Table 2 shows the resulting discontinuity estimates for each of the 7^{th} through 15^{th} year following high school graduation. Overall, there is no evidence that the admission

cutoff determines whether an individual will be observed with in-state earnings in any year from ages 25 to 33. For white men, only 1 of 18 estimates is statistically significant at the 10% level, fewer than one would expect to see by chance alone. For white women, 1 of 18 estimates is statistically significant at the 5% level, which is approximately what one would expect on the basis of chance alone. While the underlying graphs of the data yielding these estimates are not reported, they are available upon request and provide visual evidence of the conclusion of Table 2.

The bottom line is that it seems very unlikely that any jump in earnings at the admission cutoff would be due to differences in the in-state labor force participation rates rather than to attending the flagship state university.

6. Earnings Discontinuities at the Admission Cutoff

6.1 White Men

For each applicant, I calculated the natural log of the sum of four quarters of consecutive real earnings in each of the 10^{th}, 11^{th}, 12^{th}, 13^{th}, 14^{th}, and 15^{th} years following high school graduation, or when the individuals were approximately 28 to 33 years old. [9] Consequently, only the earnings of those who had positive reported earnings for the 4 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 non-missing quarterly earnings figures. [10]

[9] The advantage of examining earnings in this time period was shown 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, which is approximately the age examined in this paper. This matters to the extent that attending the flagship university causes differences in post-schooling investment.

[10] That measure was calculated as 4 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 zero. This is due to the fact that

For example, to calculate the earnings after 15 years for those who applied for admission in the fall of 1986, I used earnings received from the 3rd quarter of 2001 through the 2nd quarter of 2002. Similarly, for those who applied for admission in the fall of 1987, I used earnings received from the 3rd quarter of 2002 through the 2nd quarter of 2003, and so on. Finally, given that the post-graduation within-state labor force participation rates of applicants does not appear to be influenced by the admission cutoff, to increase precision I pooled the annual earnings figures in the 10th through 15th years together.

The underlying earnings data for white men are shown in Figures 3 and 4, while the regression discontinuity estimates resulting from various specifications are shown in Table 3. In the first row (graphed in Figure 3), controls include a quadratic function of adjusted SAT score that is not flexible (i.e., it does not include a polynomial interacted with the admission cutoff predictor that allows the coefficients on the polynomial to vary on either side of the cutoff). In the second row, an inflexible quadratic is used along with other controls including indicator variables for the term/year of application and the year after high school graduation, actual SAT score, and high school GPA. The third row estimates (those graphed in Figures 4 - 6) control for an inflexible cubic of adjusted SAT score and no controls, while the fourth row estimates control for an inflexible cubic and the full set of control variables. Finally, the fifth specification controls for a flexible quadratic of adjusted SAT score while the sixth specification limits the sample to those students within 100 SAT points of the admission cutoff and controls for adjusted SAT

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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 things like out-of-state employment than unemployment.

score in a linear way, forcing the slope of the line to be the same on each side of the admission cutoff.

The resulting regression discontinuity estimates range from 5% - 12%. While those on the smaller end of that range that result from the quadratic specification of adjusted SAT score (see rows 1 and 2 and Figure 3) are not quite statistically significant at the 10% level, all of the others are significant at the 5% level. In general, allowing for more flexibility in the polynomial of adjusted SAT score—both by allowing for higher-order polynomials and for flexibility on either side of the admission cutoff—leads to higher effects on earnings.

It is also important to note that the estimates are not affected in a meaningful way by the inclusion of control variables such as actual SAT score, high school GPA, and indicator variables for the year after high school graduation. This lends support to the assumption that the other determinants of future earnings capacity are continuous at the admission cutoff and that individuals just above the admission cutoff are not significantly more or less likely to be observed with earnings at older ages than are individuals who were just below the admission cutoff.

6.2 White Women

The earnings data for women are shown in Figure 5, while the discontinuity estimates from the various specifications are shown in Table 3. Only one of the six estimates is statistically significant at conventional levels, and even a casual glance at the underlying data in Figure 5 provides strong evidence that there is no earnings discontinuity at the admission cutoff for white women.

One may be concerned that while there may not be an effect for women generally, there may be an effect for women with strong attachment to the labor force.

Consequently, the last column of Table 3 restricts the sample to those white women who are observed with 24 consecutive quarters of positive earnings from age 28 – 33 while Figure 6 displays the underlying data. The estimates range from a statistically insignificant 5.4% to statistically significant 12.4%, though visual inspection of the underlying data in Figure 6 does not yield clear evidence of a positive effect, at least to me. Consequently, while the data and estimates are suggestive that there may be a positive effect of attending the flagship state university for women with strong attachment to the labor force, they are far from conclusive.

7. Summary of Intent-to-Treat and Enrollment Effects

7.1 White Men

Before calculating the intent-to-treat (i.e., admission) and enrollment effects implied by the previously discussed regression discontinuity estimates, I estimate the effects using ordinary least squares in order to have a benchmark with which to compare those estimates and to determine the size and direction of the bias. These estimates, which are reported in rows 1 and 2 of Table 4, yield intent-to-treat effects around 5.5% and enrollment effects of just over 3%, both of which are statistically significant at the 5% level.

One can also estimate intent-to-treat and enrollment effects using a more conventional instrumental variable strategy in which one controls directly for functions of actual SAT score and high school GPA rather than converting both into a one-dimensional measure like adjusted SAT score. One could then instrument for either admission or enrollment using the indicator variable for whether the student was above the admission cutoff. Results from this analysis, as estimated using two-stage least squares (2SLS), are reported in rows 4 and 5. The estimates imply an intent-to-treat

effect of 7 – 8% and an enrollment effect of 12 – 14%; all four estimates are statistically significant at the 10% level.

As discussed earlier, in the presence of the "fuzzy" discontinuity in admission and enrollment, the regression discontinuity estimates must be scaled up in order to estimate treatment effects. The resulting treatment effects and the appropriate standard errors can be estimated using 2SLS. The results from these estimations, all of which control for some function of adjusted SAT score, are shown in rows 6 – 9, the last of which includes only individuals who were within 100 points of the admission cutoff.

The resulting intent-to-treat estimates vary from 6.5% (p=0.159) to more than 17% while the enrollment effect estimates range from 10.8% (p=0.160) to nearly 30%.

It is interesting to note that the enrollment effects from both traditional IV estimates and the regression discontinuity estimates are considerably higher than the OLS estimates. Given that the OLS estimates are from regressions that control for the two determinants of the admission decision, this suggests that selection in enrollment conditional on admission causes bias. While one might initially expect that the OLS estimates would be larger than the regression discontinuity and conventional IV estimates, there are plausible explanations for this finding given the context. First, the admitted applicants who choose to enroll at the flagship may well be those who receive more generous need-based financial aid packages than do observationally equivalent applicants who choose to enroll elsewhere, biasing the OLS estimates downward. Second, high school graduates at the top of the distribution may well choose to enroll at a private college or more selective out-of-state university rather than the state flagship university. If those students who choose to do so are those with the largest family

resources to support the students' further education and careers—a likely possibility—then this would also bias the OLS estimates downward.

7.2 White Women

Corresponding estimates for white women with strong attachment to the labor force are also presented in Table 4. Neither OLS nor conventional 2SLS instrumental variable estimates are statistically significant at conventional levels, though all are positive. While the scaled up regression discontinuity intent-to-treat and enrollment effects are large and statistically significant, the underlying data shown in Figure 6 provide less-than-compelling visual evidence that this is not due to a functional form misspecification. Finally, although the estimates for all white women are unreported and available upon request, there is no statistically significant evidence of either a positive or a negative effect, consistent with the graph of the underlying data shown in Figure 5.

8. Conclusion

In this paper, I identify the causal effect of attending the flagship state university by utilizing a regression discontinuity design that compares the earnings of those who were just accepted 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 to earnings data collected by the state through the Unemployment Insurance Program. Evidence is presented showing that there are large, statistically significant discontinuities in both the probability of admission and the probability of enrollment at the admission cutoff, enabling the estimation of both intent-to-treat effects and enrollment effects on the earnings of 28 – 33 year-olds.

Both conventional instrumental variable results estimated with 2SLS and the treatment effects arising from the regression discontinuity design provide strong evidence

of a positive effect of attending the state flagship university for white men. Using the regression discontinuity design, the estimates suggest that the intent-to-treat effect on the earnings of white men is at least 6.5% while the effect of enrolling at the state flagship university is at least 10.8%, with most estimates significantly higher. Conventional 2SLS instrumental variable estimates yield intent-to-treat effects of just over 7% and enrollment effects around 13%. Both traditional IV estimates and regression discontinuity estimates are considerably higher than OLS estimates.

In contrast, I find very little evidence that enrolling at the state flagship university affects the earnings of white women, and I find only very weak evidence of a positive effect for white women with strong attachment to the labor force.

The results provide evidence that being accepted by and attending the flagship state university does cause an increase in subsequent earnings, at least for men. Consequently, the higher earnings that result from attending the flagship state university may justify, at least to some extent, costs (i.e., SAT preparation courses) undertaken by some students and parents to gain admission to and/or to attend the top university in the state. Perhaps more importantly, although this paper did not (yet) examine the effect of attending the flagship university on minority applicants, the results for whites may yield insight nonetheless into the potential consequences of the elimination of affirmative action and the subsequent reduction in enrollment rates for minorities at the top state schools. To the extent that the effect for minorities is similar to that of white applicants, one may well expect that the earnings of minorities may fall as a result of the elimination of affirmative action in the admissions of flagship state universities.

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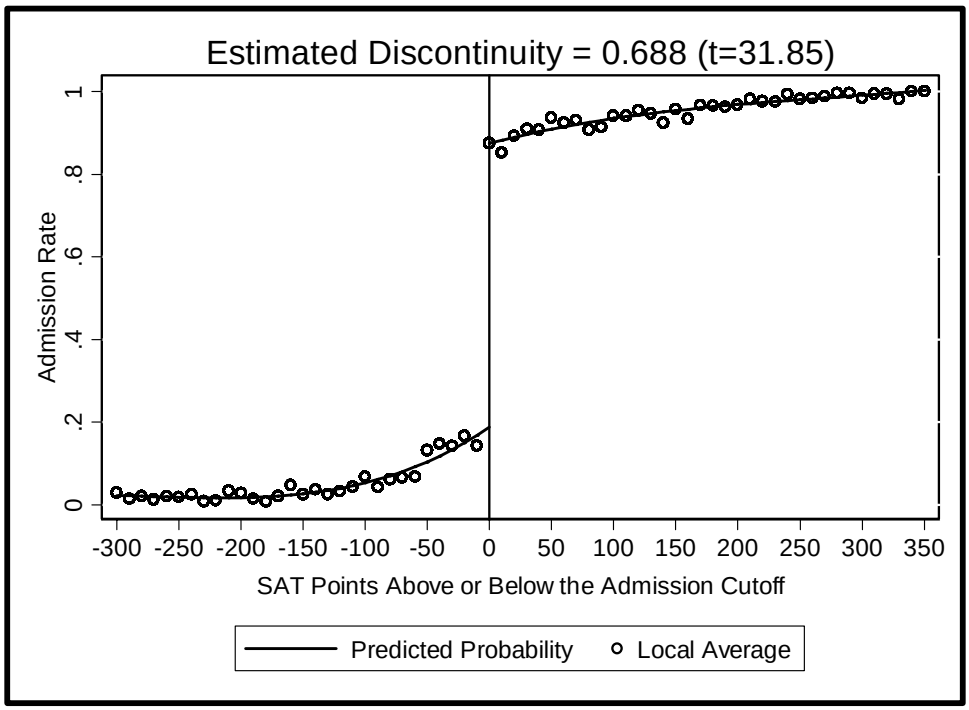


Figure 1: Fraction Admitted to the Flagship State University

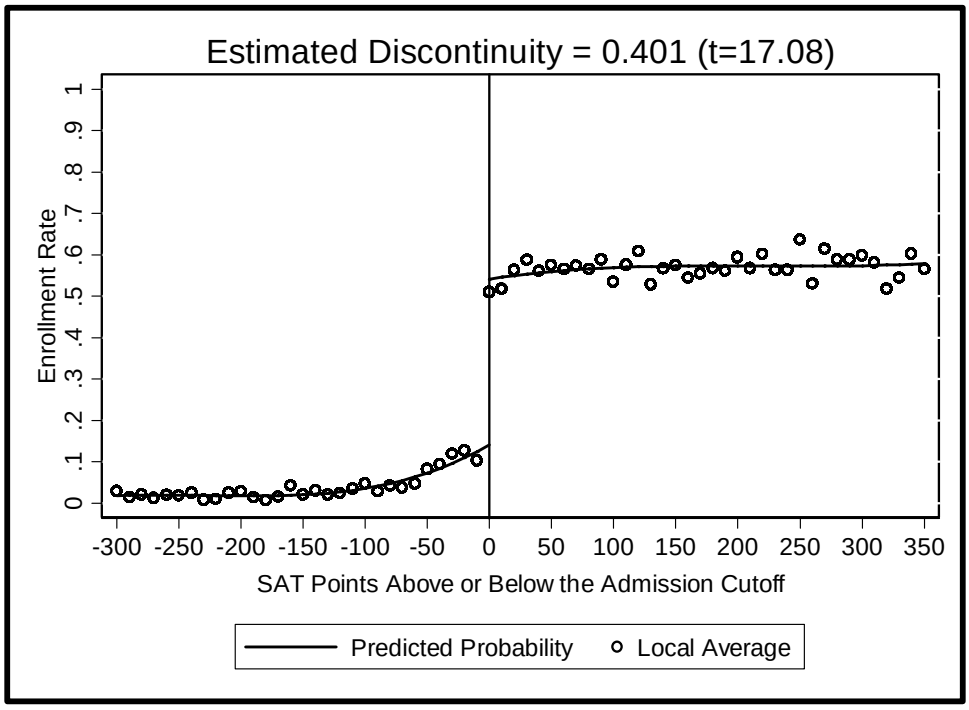


Figure 2: Fraction Enrolled at the Flagship State University

Table 1: Regression Discontinuity Estimates for the Admission Rate of White Applicants

Regression Specification	Function of Adjusted SAT	Flexible Polynomial?	Controls	Estimated Admission Discontinuity	Estimated Enrollment Discontinuity
(1)	none	-	No	**0.876** **(0.013)** **[0.000]**	**0.518** **(0.009)** **[0.000]**
(2)	quadratic	Yes	No	**0.704** **(0.014)** **[0.000]**	**0.420** **(0.016)** **[0.000]**
(3)	quadratic	Yes	Yes	**0.702** **(0.014)** **[0.000]**	**0.419** **(0.014)** **[0.000]**
(4) (Plotted in Figures 1 & 2)	cubic	Yes	No	**0.688** **(0.022)** **[0.000]**	**0.401** **(0.023)** **[0.000]**
(5)	cubic	Yes	Yes	**0.685** **(0.021)** **[0.000]**	**0.405** **(0.021)** **[0.000]**

Notes: Robust standard errors are in parentheses; p-values are given in brackets. Controls include a dummy variable for each year/term of application as well as actual SAT score and 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. Estimates in bold are statistically significant at the 10% level.

Table 2: Regression Discontinuity Estimates for the Probability that White Applicants Are Observed with Positive Earnings in the 7^{th} – 15^{th} Years Following High School Graduation

Year After High School Graduation	Men		Women	
	Earnings Measure		Earnings Measure	
	4 Consecutive Qtrs.	Any Earnings	4 Consecutive Qtrs.	Any Earnings
7th	-0.027	-0.020	-0.035	-0.008
	(0.031)	(0.027)	(0.030)	(0.036)
	[0.376]	[0.459]	[0.253]	[0.834]
8th	0.014	-0.018	-0.008	-0.007
	(0.028)	(0.020)	(0.025)	(0.023)
	[0.620]	[0.377]	[0.764]	[0.758]
9th	0.051	-0.011	-0.126	**-0.067**
	(0.038)	(0.026)	(0.023)	**(0.030)**
	[0.186]	[0.683]	[0.587]	**[0.028]**
10th	0.006	-0.010	-0.027	-0.024
	(0.021)	(0.024)	(0.055)	(0.031)
	[0.762]	[0.666]	[0.625]	[0.446]
11th	-0.019	-0.034	-0.028	-0.023
	(0.0240)	(0.021)	(0.066)	(0.060)
	[0.426]	[0.105]	[0.671]	[0.708]
12th	-0.042	-0.017	-0.024	0.003
	(0.025)	(0.021)	(0.060)	(0.043)
	[0.104]	[0.424]	[0.688]	[0.936]
13th	0.034	0.019	-0.063	-0.036
	(0.026)	(0.027)	(0.043)	(0.043)
	[0.193]	[0.479]	[0.148]	[0.411]
14th	**0.048**	0.022	-0.028	-0.018
	(0.025)	(0.033)	(0.034)	(0.047)
	[0.061]	[0.502]	[0.416]	[0.703]
15th	0.036	0.037	0.025	-0.004
	(0.037)	(0.028)	(0.030)	(0.044)
	[0.328]	[0.189]	[0.402]	[0.933]

Notes: Robust standard errors clustered at the adjusted SAT score level are in parentheses; p-values are in brackets. Estimates in bold are statistically significant at the 10% level. All estimates are from regressions controlling for a cubic of adjusted SAT score.

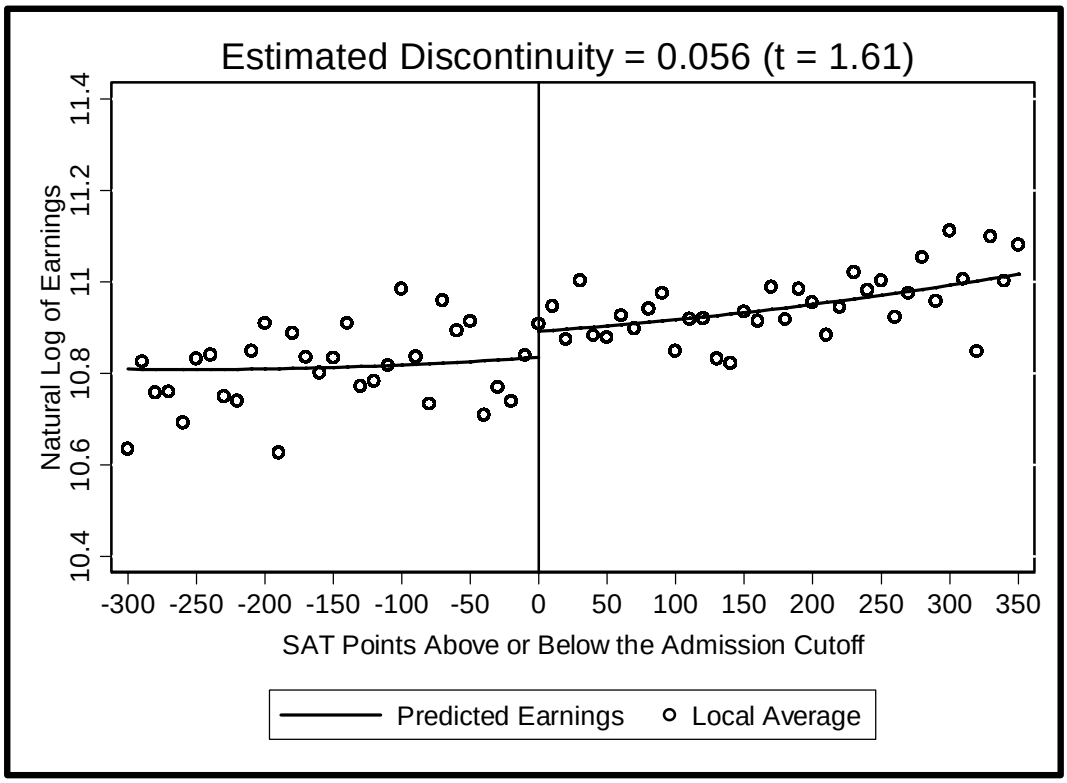


Figure 3: The Natural Log of Annual Earnings for White Men 10 - 15 Years after High School Graduation (Fitted with a quadratic polynomial of adjusted SAT score)

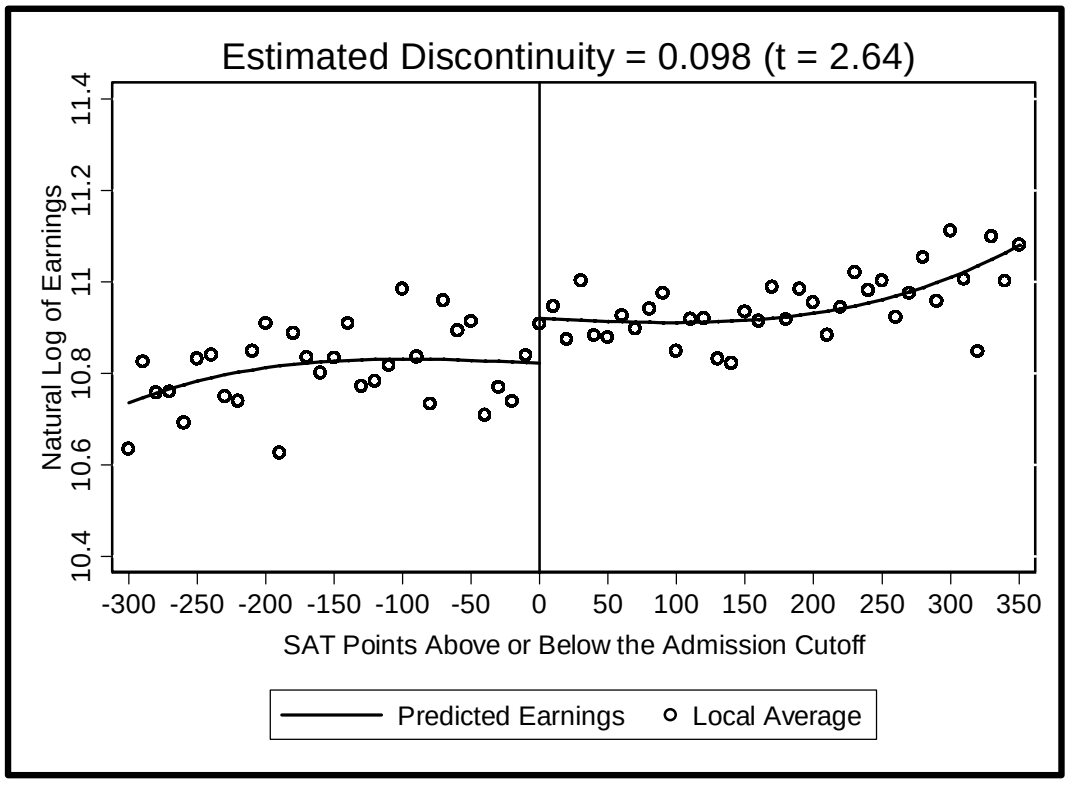


Figure 4: The Natural Log of Annual Earnings for White Men 10 - 15 Years after High School Graduation (Fitted with a cubic polynomial of adjusted SAT score)

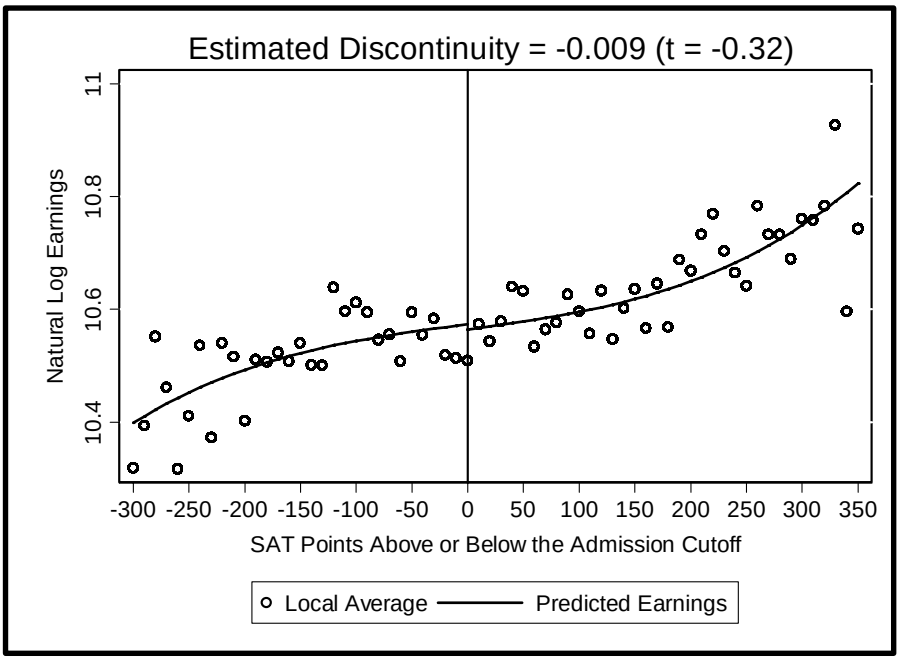


Figure 5: The Natural Log of Annual Earnings for White Women 10 – 15 Years after High School Graduation

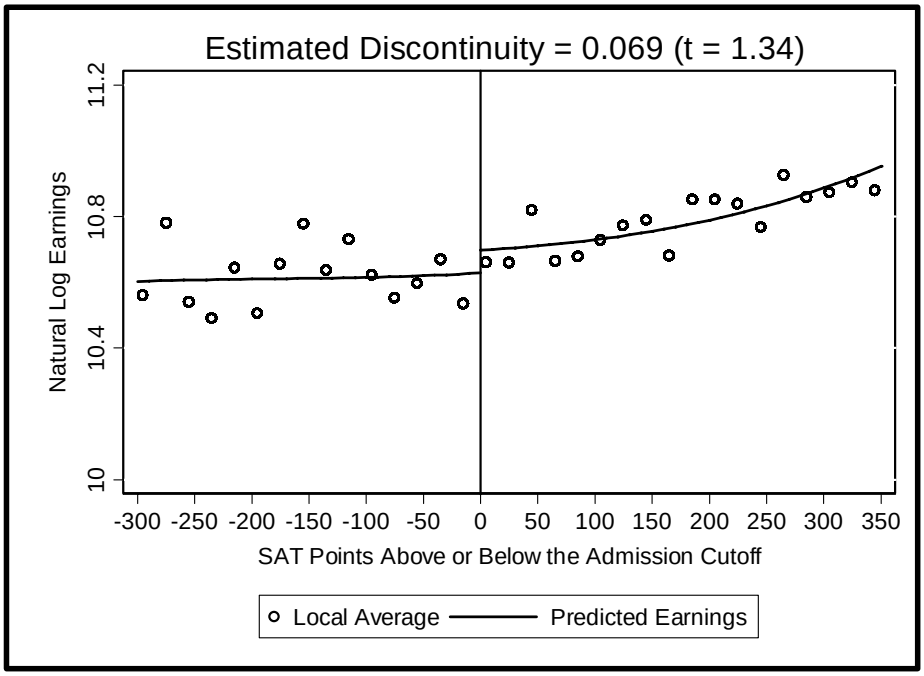


Figure 6: The Natural Log of Annual Earnings for White Women with Strong Attachment to the Labor Force 10 – 15 Years after High School Graduation

Table 3: Summary of Regression Discontinuity Estimates for White Applicants

				Men	Women	Women with Strong Attachment to Labor Force
Regression Specification	**Function of Adjusted SAT**	**Flexible Polynomial?**	**Additional Controls**	**Estimated Earnings Discontinuity**	**Estimated Earnings Discontinuity**	**Estimated Earnings Discontinuity**
(1)	quadratic	No	No	0.056	**-0.043**	0.054
Plotted in Figure 3				(0.035)	**(0.025)**	(0.044)
				[0.111]	**[0.097]**	[0.223]
(2)	quadratic	No	Yes	0.051	-0.042	0.056
				(0.035)	(0.025)	(0.045)
				[0.154]	[0.105]	[0.219]
(3)	cubic	No	No	**0.098**	-0.009	0.069
Plotted in Figures 4 -6				**(0.037)**	(0.030)	(0.052)
				[0.010]	[0.752]	[0.186]
(4)	cubic	No	Yes	**0.085**	-0.011	0.070
				(0.038)	(0.029)	(0.053)
				[0.027]	[0.695]	[0.190]
(5)	quadratic	Yes	Yes	**0.123**	0.025	**0.124**
				(0.045)	(0.025)	**(0.050)**
				[0.007]	[0.326]	**[0.016]**
(6)	linear	No	Yes	**0.118**	0.015	**0.118**
(Includes only applicants				**(0.049)**	(0.033)	**(0.055)**
within 100 points of cutoff)				**[0.025]**	[0.651]	**[0.044]**

Notes: Robust standard errors clustered at the adjusted SAT score level are in parentheses; p-values are given in brackets. All specifications include indicators for each year after HS graduation in which earnings are observed. Additional controls include actual SAT score, high school GPA and indicators for each year/term of application. "Flexible Polynomial" indicates whether the estimated coefficients of the adjusted SAT score polynomial were allowed to differ on each side of the admission cutoff. Estimates in bold are statistically significant at the 10% level.

Table 4: Intent-to-Treat and Enrollment Effects for White Applicants

						Men		Women with Strong Attachment to the Labor Force	
Row	**Estimation Type**	**Sample**	**Function of Adjusted SAT**	**Function of SAT and GPA**	**Flexible Polynomial?**	**Intent-to-Treat**	**Enrollment Effect**	**Intent-to-Treat**	**Enrollment Effect**
1	OLS	Full	none	quadratic	No	**0.058**	**0.034**	0.019	0.024
						(0.024)	**(0.016)**	(.042)	(0.027)
						[0.016]	**[0.029]**	[0.659]	[0.374]
2	OLS	Full	none	cubic	No	**0.053**	**0.032**	0.017	0.024
						(0.025)	**(0.016)**	(0.043)	(0.027)
						[0.033]	**[0.041]**	[0.699]	[0.385]
4	2SLS (Conventional IV)	Full	none	quadratic	No	**0.079**	**0.136**	0.052	0.092
						(0.036)	**(0.061)**	(0.053)	(0.093)
						[0.027]	**[0.027]**	[0.322]	[0.323]
5	2SLS (Conventional IV)	Full	none	cubic	No	**0.071**	**0.122**	0.052	0.093
						(0.037)	**(0.064)**	(0.054)	(0.096)
						[0.057]	**[0.057]**	[0.334]	[0.335]
6	2SLS (Regression Discontinuity)	Full	quadratic	linear	No	0.065	0.108	0.067	0.110
						(0.046)	(0.076)	(0.054)	(0.092)
						[0.159]	[0.160]	[0.223]	[0.234]
7	2SLS (Regression Discontinuity)	Full	cubic	linear	No	**0.113**	**0.189**	0.086	0.142
						(0.045)	**(0.076)**	(0.068)	(0.113)
						[0.013]	**[0.013]**	[0.206]	[0.209]
8	2SLS (Regression Discontinuity)	Full	quadratic	linear	Yes	**0.174**	**0.290**	**0.153**	**0.248**
						(0.066)	**(0.112)**	**(0.063)**	**(0.111)**
						[0.010]	**[0.011]**	**[0.018]**	**[0.028]**
7	2SLS (Regression Discontinuity)	Only applicants within 100 points of admission cutoff	linear	linear	No	**0.164**	**0.282**	**0.143**	**0.204**
						(0.070)	**(0.120)**	**(0.068)**	**(0.106)**
						[0.029]	**[0.029]**	**[0.049]**	**[0.069]**

Notes: Robust standard errors are in parentheses; p-values are in brackets. All specifications control for indicators for each year after HS graduation in which earnings are observed and for each year/term of application. Standard errors for specifications not including adjusted SAT scores are clustered at the student level; the others are clustered at the adjusted SAT score level. "Flexible Polynomial" indicates whether the estimated coefficients of the adjusted SAT score polynomial were allowed to differ on each side of the admission cutoff. Estimates in bold are statistically significant at the 10% level.