Course Availability and College Enrollment: Evidence from Administrative Data and Enrollment Discontinuities

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Abstract

Community colleges serve close to half of the undergraduate students in the United States and tuition at two-year public colleges is mostly a public expenditure. We measure the effect of decreased course availability on grades, degree attainment, and transfer to four-year colleges using a regression discontinuity from course enrollment queues in oversubscribed courses. Using a panel from a large California community college and the National Student Clearinghouse we find that in the short run students substitute unavailable sections with other sections in other courses in the same subject area, if not in the same course. We find no significant effects on later school performance or transfer to other colleges, given the precision of our tests, however we cannot rule out economically significant effects.

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1 Introduction

The recent recession and deteriorating public support for state-funded higher education have placed enormous budgetary pressures on public colleges. California's two-year public schools in particular saw a sharp, per-student funding decrease of about 11 percent in 2009 due to the defeat of several budget proposals.[1] One of the primary impacts of cutbacks in funding is a reduction in the number of class offerings to students. At the same time, student enrollments in public schools have continued to increase in recent years,[2] further reducing the per-capita supply of classes. What are the effects of these reductions in the supply of publicly-subsidized college courses on student outcomes?

The answer to this question is theoretically ambiguous. Restricting the supply of classes will make it difficult for some students to enroll in the courses they need to graduate, increasing the time to degree and likelihood of dropping out of college. With subsidized public college tuition, there is no price mechanism to create an equilibrium whereby the price paid for an additional course would equal the marginal benefit of taking that course to the student. Instead, some students might choose to take the publicly-subsidized course even if the private marginal benefit is low. However, depending on the allocation mechanism, restricting supply might not have a large negative effect on educational outcomes for the marginal student being rationed. For example, if more motivated students can eventually enroll in a class by auditing it until a space opens, or students who value a class more highly can systematically be given priority access to classes, the system may still efficiently allocate spots to the "correct" students.

There is empirical evidence that when the burden on public institutions of higher education increases, there is a corresponding response from for-profit colleges.[3] The return, both private and social, to attending a for-profit two year college is arguably lower than at non-profit two years.

A better understanding of the impacts of restricting the supply of courses is especially important for community colleges.[4] About half of undergraduate education in the United States takes place at two-year colleges (U.S. Department of Education, 2014). Two-year colleges have increasingly moved from vocational education to preparing students

[1] www.insidehighered.com/news/2009/05/21/California

[2] Full-time equivalent enrollment at public colleges and universities increased by 17% from 2003 to 2012 while the US population grew by 8% in the same span.

[3] Cellini (2009) finds that for-profit colleges in California are more likely to enter markets when college bond referendums fail.

[4] Throughout the paper we will use the terms *two-year colleges public two-year colleges* and *community colleges* interchangeably. When referring to *private two-year* colleges we will note the distinction.

for four-year degrees.[5][6] In fact, in some states, including California, nearly half of all students attending a 4-year college previously attended a community college.[7] Transfers from community colleges to the California State University (CSU) system, for example, are projected to increase by 25 percent over the next decade (California Postsecondary Education Commission, 2010). There has also been a long term rise in the length of time students spend at two-year colleges.[8] Given the main source of funds for non-profit, two-year, post-secondary schools is state governments, they are particularly affected by budgetary pressures.[9] Tuition at community colleges is also very low relative to the cost of providing courses possibly making it more likely that marginal benefits fall below marginal costs for the marginal student. It is possible that the marginal student might want to take the publicly-subsidized course even if the private marginal benefit is low.

Identifying the effects of restricting the supply of classes on the marginal community college student is not straightforward. An exogenous constraint to offering classes at community colleges is needed, but is difficult to find. The random elimination or addition of classes at a community college is clearly not politically or fiscally feasible on a large enough scale to study educational impacts. Instead, we use a regression discontinuity design with data from a large community college in California to identify effects. Many community colleges in California cannot provide all of the classes to meet demand because of budgetary restrictions, limited classroom or lab space, or difficulties hiring faculty. Because of open enrollment policies, classes are rationed through registration priorities and waitlists instead of through selective admissions. Using discontinuities in waitlists and registration priority lists, we identify exogenous changes to the supply of sections of courses and courses to students. We form estimates of the educational effects of rationed course offerings by comparing students who were on either side of waiting list cutoffs, that is, the students who just missed being admitted to a section from the waiting list compared to nearly identical students who were just admitted to the same section. A unique and novel administrative dataset that records all course registration attempts and

[5]In the college we examine, the fraction of first-time non-foreign students entering in the fall term who declared an intent to transfer to a four-year college increased from 46% to 71% from 2003 to 2007. The proportion who declared an intent to obtain either a terminal two year degree (associates or vocational), certificate or license, update job skills, or prepare for a new career fell from 25% in fall 2003 to 11% in fall 2007.

[6]Gill and Leigh (2003) cite two traditional goals for community colleges. One is the "transfer function", and the other, and more recent is adult training services. Adult training services include vocational programs but also remedial education. However, for many students remedial education may be the first step in transferring to a four-year college.

[7]See U.S. Department of Education (2010); CCCCO (2009); Sengupta and Jepsen (2006).

[8]See, for example, Bound, Lovenheim, and Turner (2010, 2012).

[9]See Boswell (2000).

outcomes from the online enrollment system of the college allows us to reconstruct waitlist queues. We link these records to institutional transcript data on student course schedules and grades, long-term outcomes such as majors, retention and degree completion, and to enrollment at other institutions using files from the National Student Clearinghouse. Using these data we examine the short- and long-term educational effects of three exogenous changes restricting student class choices: i) not getting the section of the course chosen by the student in that term, ii) not getting the course chosen by the student in that term, and iii) restricting choice over which sections of courses and courses are available that term because of having a lower registration priority.

Our paper is related to a number of studies examining the returns to community college. Many studies (Grubb, 1993; Kane and Rouse, 1995; Hilmer, 2000; Gill and Leigh, 2003; and Light and Strayer, 2004) have followed the pioneering work of Heineman and Sussna (1977) who reported on the returns to a two-year degree relative to dropping out of a four year degree program by using data from a large urban centered community college. The main parameter of interest in this work is the labor market return to initially attending a two year college. Most notable is the work of Rouse (1995) which uses distance to closest community college as an instrument for two-year college attendance. A key question concerns heterogenous treatment effects. While two-year schools might have a positive effect for students who would have otherwise attained just a high school diploma, two year school may also "divert" students who would otherwise enroll at a four year college. Observational evidence (Hilmer, 2000) suggests that this may be a valid concern. Rouse finds that the causal effect of two year college attendance among students who where "diverted" is two-fold: a small negative effect on number of years of schooling, but no effect on the likelihood of completing a four year degree. Another important strand of the literature examines the effects of community college on displaced adult workers (Leigh and Gill, 1997; Jacobson, LaLonde, and Sullivan, 2005). These studies find that the returns for adults are the same as the returns for younger workers.

These studies examine the return to education for a given amount of schooling. This paper in turn examines whether the supply of education (as measured in available courses) is a factor in the amount of time taken to transfer or complete a degree and the probability of transferring or completing a two year degree. Previous studies that have examined this question have done so at an aggregate level by using, for example, variation in the size of the cohort of graduating high school seniors in an area[10]. They find that a secular decrease in college completion is caused by what type of school students attend but it is not caused by the student teacher ratio. The aggregate analysis does not allow deeper examination into other mechanisms but they conjecture that "crowding" i.e. queuing and course enrollment constraints may be an important determinant. We used detailed administrative data to

[10]See, for example, Bound and Turner (2006), Card and Lemieux (2001a, 2001b), and Fortin (2006).

examine the effect of this type of "crowding."

2 Institutional Background and Data

Tuition at two-year public/non-profit colleges is mostly a public expenditure.[11] Public schools offer lower than market tuition. More than half, 57%, of tuition is paid for with grants.[12] In addition, another twenty-two percent of tuition is paid for using publicly subsidized loans.

Nationally 79% of community college students expect to earn a BA, 46% are enrolled full-time, and 75% work while enrolled.[13]

Our sample comes from a panel of students who attended a large public two-year college from 2002 to 2007. Regular enrollment is 15,000 full-time equivalent students. The number of enrolled students is higher than this since many are not enrolled full-time. The college has three hundred full-time and six-hundred part-time unionized faculty. Classrooms are built with this enrollment cap in mind so deviations in enrollment far above 40 are rare. Online classes offered by the school, however, can be on the order of one-hundred students. Full-time tuition, including books and fees, is $2,000, larger than the corresponding figure reported for a nationally representative sample from the Begin Post-secondary Students Longitudinal Study of $1,269.[14]

The data contain three main parts. The first is a registration file with course grades, dates of attendance and degrees granted by the school. The second piece of data is enrollment information from other colleges and universities from the National Student Clearinghouse (NSC). Last are enrollment logs for all terms from 2002 to 2007. The school operates on a quarter system with three regular terms (winter, spring and fall), but like many other two-year schools it also offers courses during a summer term. The enrollment logs contain a record of each registration attempt during a term's registration period. This for example would be a period during the summer for enrollment in fall courses. An enrollment attempt is identified by student ID, time (with precision to the second), a particular section for a course, and an outcome. Outcomes can take one of four values: enrollment into the section, placement onto a wait list for that section, withdraw from the section, or

[11] In 1992, tuition accounted for ten percent of student expenditures at community colleges. In 1972 tuition accounted for 18% of student expenditures at community colleges. Authors' calculations from Bound, Lowenheim, and Turner (2012), Table 3, panel F.

[12] Based on Table 2 page 156 of Deming, Goldin and Katz (2012). Calculated from reported *net tuition minus grants* and *tuition*.

[13] As reported in Deming, Goldin and Katz (2012) Table 1. Based on summary statistics from the Beginning Postsecondary Students Longitudinal Study for 2003-2004 first-time beginning postsecondary students.

[14] Deming, Goldin, and Katz (2012), Table 2, page 156.

no change.

2.1 Course Enrollment

The online enrollment process we examine takes place before the term begins. It is governed by an automated system and students are given one of eight enrollment priority designation dates, upon which they are granted access to the registration system. A student searches for a desired section (e.g. MWF 9-10AM) of a desired course (e.g. Econ 101 Principles of Macroeconomics) and is told instructor for the particular section, where it meets, and how many seats are available. If there are no seats available, then the student is told how many students are on the wait list and how many spots are left. Waitlists are only allowed to reach 15 students per section.

Students are taken from the waitlist as currently enrolled students drop the section. When a spot is freed, the first waitlisted student is given 48 hours to enroll; if the student does not enroll, then the next student on the waitlist is given permission to enroll. After enrolling, students have two weeks to pay tuition for the section, if they do not pay within two weeks, they are dropped from the section. We limit our analysis to enrollment before the term starts. After this time, instructors may have some discretion with respect to who is granted enrollment in a section. Furthermore, enrollment is often conditional on section attendance.

2.2 Instrument Construction

In our first set of estimates we will use a regression discontinuity design based on a student's position on course waitlists. Here we will describe how we construct the running variable. It is important to note that the method we use accounts for the fact that a substantial number of students exit the wait list before the completion of the registration period. Attrition of this kind would otherwise result in selection at the threshold; those students who *barely* made it into the class were all students who did not drop themselves from the waitlist, but among students who *almost* made it into the class are students that exited the waitlist before the start of the term or before the last admission into the class.

We define RV_i, distance to the threshold for student i, as the minimum number of students, ahead of student i, who would have needed to drop the class section in order for student i to have successfully enrolled in the class section, had student i stayed on the wait list throughout the course of the pre-registration period.

Let us take a look at a hypothetical class section and describe the construction of this measure for three students (see Table 1). We can think of the distance to the threshold as a hypothetical "last wait list number". Suppose we are interested in student number 38 and

that Table 1 gives us the final set of events before the start of the term. In the previous period we can assume that 30 initial students, numbered 1 to 30, enrolled in this class without incident. Student 38 placed herself on the waitlist at 12:42PM on August 1st. At that time there were 35 students enrolled in the class and an additional 2 students on the wait list. We thus assign student 38 an initial wait list number of 3. This means that at least three people, of the 37 ahead of her (either enrolled in the class or on the wait list with an earlier entry time), must drop the class before she can successfully enroll. We further see that three students ahead of student 38 did in fact drop the class before the start of the semester. Thus student 38 is assigned a final wait list number of zero.

On the other hand student 39. Student 39 is assigned an initial wait list number of 4. Since three students ahead of student 39 dropped the class, student 39 is assigned a final wait list number of 1. Had student 39 stayed on the wait list she still would need one additional person to drop the class in order to successfully enroll.

Likewise, student number 40 is also assigned a final wait list number of 1. This student had an initial wait list number of 5, and 4 people ahead of her dropped the class before the start of the semester. Thus, at the start of the semester student 40 still needed one more person to drop before she could successfully enroll.

2.3 Sample Characteristics

Table 2 presents demographic information on race and national origin. Column one gives the number of observations of Americans broken down by race. Column two gives the percentage of each race group among non-Americans. The racial composition of the group has fewer African American and Hispanic students than samples of two-year college students from IPEDS and BPS. In our sample 3.87% of American students report being African American, while 10.9% of students in IPEDS and 14% of students in the BPS 2004-2009 samples are African-American. Relative to these samples American students at our school are slightly less Hispanic. Hispanics make up 13.38% of American students at our school while they comprise 15.7% and 15.9% of the IPEDS and BPS samples respectively. Asian Americans make up a plurality (42%) of U.S. students and a majority (65%) of international students at our school. Whites make up a quarter of American students and 13% of international students.

Given the substantial differences in racial composition it is worthwhile to compare other summary statistics against the IPEDS and BPS samples. Table 3 presents further summary statistics for the sample in our study. All three samples are 55% female. The sample has a higher mean age than the BPS sample, 25.97 compared to 24.4. A smaller fraction of students at have financial aid; 18%, relative to 74.9% reported having applied for aid in the BPS sample. Comparing educational goals, 33% of students in our sample

declared an intent of transferring to a four-year institution while 79.9% of community college students in the BPS sample say they expect to earn a BA.

3 Identification and Reduced-Form Evidence

In this section we start by laying out the assumptions in our regression discontinuity analysis, providing motivation for an instrumental variables model, and describing the local average treatment effect that is identified by our instrument. Next, we show that we have a strong first stage in our two-stage least-squares analysis. We proceed by conducting validity checks to ensure that there are no a priori discontinuities in baseline variables other than section enrollment, and that there is no sorting across waitlist position à la McCrary (2008). Last, we present reduced form evidence for our main results.

3.1 Identification

Consider a student who has placed herself on a section wait list. Let rv be her wait list number. Let $Y(1)$ be an educational outcome for her if she is admitted into the section, and let $Y(0)$ be the corresponding educational outcome for her if she is not admitted into the section. Denote the mean outcome for students with waitlist number rv had they all been admitted into their wait listed section as $E(Y(1)|RV = rv)$, similarly denote the mean outcome for students with waitlist number rv had they not been admitted into their waitlisted section as $E(Y(1)|RV = rv)$. Conditional on having waitlist number rv the effect of being admitted into the waitlisted section on the educational outcome Y is $E(Y(1) - Y(0)|RV = rv)$. Our identification strategy will allow us to measure the average effect for students on the cusp of being admitted from the waitlist, i.e. those students for whom $RV = 0$. Denote this local average treatment effect, LATE,

$$LATE \equiv E(Y(1) - Y(0)|RV = 0). \tag{1}$$

We measure this effect by estimating the four following quantities:

$$\lim_{rv\uparrow 0} E(X|RV = rv), \quad \lim_{rv\downarrow 0} E(X|RV = rv) \tag{2}$$

$$\lim_{rv\uparrow 0} E(Y|RV = rv), \quad \lim_{rv\downarrow 0} E(Y|RV = rv), \tag{3}$$

where X is an observed indicator for whether the student successfully enrolled in the waitlisted section and Y is the observed educational outcome. By definition $Y = Y(1) \iff X = 1$ so by conditional expectation we can write $E(Y|RV = rv) =$

$$E(Y(1)|RV = rv)P(X = 1|RV = rv) + E(Y(0)|RV = rv)P(X = 0|RV = rv). \tag{4}$$

Two necessary conditions are that there is a discontinuous jump in the likelihood of enrollment at the threshold, i.e. $\lim_{rv\uparrow 0} E(X|RV = rv) \neq \lim_{rv\downarrow 0} E(X|RV = rv)$. and that the functions $E(Y(j)|RV)$ are continuous at $RV = 0$ for $j = 1, 0$.

Define $p^j_\uparrow \equiv \lim_{RV\uparrow 0} P(X = j|RV)$ and $p^j_\downarrow \equiv \lim_{RV\downarrow 0} P(X = j|RV)$ for $j = 1, 0$. $E(Y(j)|RV)$ continuous at $RV = 0$ implies $\lim_{RV\uparrow 0} E(Y(j)|RV) = \lim_{RV\downarrow 0} E(Y(j)|RV) = E(Y(j)|RV = 0)$ for $j = 1, 0$.

$$\therefore \lim_{RV\uparrow 0} \{E(Y(j)|RV)P(X = j|RV)\} \quad = \quad E(Y(j)|RV = 0)p^j_\uparrow$$

and

$$\lim_{RV\downarrow 0} \{E(Y(j)|RV)P(X = j|RV)\} \quad = \quad E(Y(j)|RV = 0)p^j_\downarrow$$

for $j = 1, 0$.

Now consider $\lim_{RV\uparrow 0} E(Y|RV) - \lim_{RV\downarrow 0} E(Y|RV)$

$$= E(Y(1)|RV = 0)p^1_\uparrow + E(Y(0)|RV = 0)p^0_\uparrow - E(Y(1)|RV = 0)p^1_\downarrow - E(Y(0)|RV = 0)p^0_\downarrow$$

$$= E(Y(1) - Y(0)|RV = 0) * [p^1_\uparrow - p^1_\downarrow]$$

$$= LATE * [p^1_\uparrow - p^1_\downarrow]$$

In our regression discontinuity design we estimate the following system,

$$E(Y|RV, Z) = \pi^1_0 + \pi^1_1 Z + g^1(RV)$$

$$E(X|RV, Z) = \pi^2_0 + \pi^2_1 Z + g^2(RV)$$

where

$$\pi^1_1 = LATE * [p^1_\uparrow - p^1_\downarrow]$$

and

$$\pi^2_1 = p^1_\uparrow - p^1_\downarrow$$

We estimate the following instrumental variables model

$$E(Y|X, Z, W) = X\beta + W'\delta. \tag{5}$$

$$E(X|Z, W) = Z\pi_{12} + W\pi_{22}. \tag{6}$$

Here Z is an indicator for $RV < 1$, W contains continuous functions of the running variable and demographic variables that are correlated with our set of outcomes, X is an indicator for whether the student successfully enrolled in the waitlisted section, and Y is an outcome variable. The local average treatment effect if denoted β. The *exclusion restriction* is that conditional on W and X the best predictor of Y does not include Z.

3.1.1 What is the treatment and what is the local average treatment effect?

The treatment that we measure using the wait list discontinuity is the effect of admitting one additional student into a section *holding availability in all other sections fixed*. In an ideal experiment that estimates this parameter, only the supply one one section would be reduced. It may be that the response to a treatment where a large fraction of sections are eliminated is very different if reductions in other courses and sections raises the expected costs of substitution. A policy change that reduced overall course offerings would decrease the chances of students enrolling in their most preferred sections as well as the changes of enrolling in their second and third choices. The effects on student outcomes of such a change are likely to be substantially different than the effects measured in this paper. In the natural experiment that is the focus of this paper only the chances of enrollment in one section is affected.

Furthermore, the local effect that we measure is for individuals who have placed themselves on the course waitlist and who are on the margin of being admitted into the section during the pre-registration period. It is important to note that these students have placed themselves on wait lists where there is a substantial chance of not being admitted into the section. In the next section we will see that around the threshold the chances of failing to enroll in the section are between ten and twenty percent. There may a substantial portion of students who choose not to take this risk and who therefore do not place themselves on a waitlist. The effect of not enrolling in a section for these more risk averse students may be substantially different than the effect that we measure in the population of student that do place themselves on waitlists.

3.2 First Stage

Enrollment into a section is not completely determined by whether or not a student was allowed to enroll from the waitlist. Students who would have been admitted into the section may drop off the waitlist before the registration period ends. Likewise, students who are not admitted into the section before the end of the registration period may be admitted into the section after classes begin. Therefore our estimation will be based on a fuzzy RD design. Making the wait list cut-off produces a discontinuity in the probability of enrollment into the wait listed section but it does not completely determine enrollment. Figure 1 shows a 13.4% increase in enrollment associated with crossing the threshold from the right. Students on the right side of the red vertical line in this figure remained on the wait list at the start of the term. The running variable tells us how many other students were ahead of them on the waitlist at the start of the term. Students that remained on the wait list have a greater than 50% chance of enrolling in their desired section. This can happen from enrolling in the section after the start of the term. On the left side of the

figure we see that a small fraction of students that were admitted into the section did not enroll, or enrolled and later dropped the class.

The fuzzy RD design entails two-stage least-squares estimation using an indicator for being past the threshold of the running variable as an instrument. A concern with two stage least squares is that estimates can be biased toward the probability limit of ordinary least squares. Stock and Yogo (2005) develop a test for this bias based on the F statistic of the instrument in the first stage regression. Their "rule of thumb" cautions against using instruments when the first stage F statistic is below ten.

Table 4 shows that the first stage F statistics in three specifications are all much larger than ten. The three columns of the table regress enrollment in the chosen section on an instrument for being in the chosen section, the running variable, and the interaction of the two. The first column uses our largest sample, including individuals that are twenty spots from the threshold. The F statistic in this regression is over 270, much larger than ten. The second column reduces the sample by only including individuals that are within ten positions of the threshold. The F statistic of test of significance of the instrument in this regression remains large, with a value of over 175. The last column is for the most restrictive sample and includes only observations within five positions of the threshold. The F statistic falls to 80 but remains far above the Stock-Yogo rule of thumb, ten. This rules out a concern of weak instrument in our analysis.

3.3 Validity Checks

We conduct two validity checks. First we check for discontinuities in baseline covariates at the threshold, next we check whether there is, bunching, discontinuities in the probability density function, of the running variable at the threshold.

Figures 2 and 3 plot of the average values of eight covariates conditional on wait list position. Figure 2 plots the fraction of each race and the fraction international students along the running variable. The fraction white varies between 19 and 21.5%. The fraction Asian varies between 50 and 58%. While this fraction is decreasing as a function of the running variable it does not change discontinuously at the threshold. The fraction Hispanic varies between 8 and 12% and is steady around 11% as it crosses the threshold. The fraction of international students stays between five and nine percent and while there tends to be a higher fraction of international students on the left of the threshold this change is continuous.

Figure 3 examines mean age, fraction female, fraction of students with a high school degree or less and the average number of credits earned in the sample. While the conditional mean age varies smoothly with a general trend upward from 24 to 24.6 as the running variables goes from -5 to 5, the fraction female varies downward from around

.58 to .54 as the running variable goes from -5 to 5. Previous educational attainment as measured by the fraction of students with a high school education or less remains steady at around 70%. Cumulative course credits trend downward from an average of 37 to an average of 30 at the threshold and further right. One should note that the discontinuity in enrollment happens between 0 and 1 whereas the jump seem in panel d of Figure 3 occurs between -1 and 0.

3.4 No Sorting Across Wait List Position

Whereas differences in observable characteristics between individuals on either side of the threshold can be observed by examining the conditional distribution of each observable as it crosses the threshold, a similar examination of unobservable characteristics cannot be done. However, we can examine selection on unobservables due to sorting across the threshold (McCrary, 2008). Figure 4 is presented to examine differences in density across the threshold. Although the mechanical registration process restricts direct manipulation, differences in density can arise from manipulation of the running variable. Here we verify this restriction. It is rarely the case that individuals are indifferent between receiving treatment or not. In our case students would generally prefer to be enrolled in a selected section rather not.

If it *were* possible to manipulate the value of the running variable then there would be incentives to move to lower wait list values. Movement of this type would have higher payoff the closer a student is to crossing the threshold. We would then expect more of this movement to happen for students with positive but small values of the running variable. Movements of this type move mass from the positive side of the threshold to the negative side in the distribution of the running variable. Figure 4 shows no evidence of this being the case and validates the mechanical restrictions of the registration process.

3.5 Reduced Form Evidence

Before estimating our model more formally it will be helpful to examine the direct relationship between relative waitlist position and important outcomes. Figure 5 plots the mean number of courses in which students successfully enrolled during the concurrent term, excluding the section that produced the waitlist position. The figure shows that moving a student below the threshold is associated with a .126 increase in the number of other courses in which the student enrolled the concurrent term. Taken together with a first stage estimate of between .10 and .12 this implies that successfully enrolling in an additional course is associated with taking one fewer of the other courses available. Figure 6 shows that a similar sized jump is present when moving over the threshold for the

average number of other sections in the same subject. By additivity this implies that the effect of enrolling in fewer classes due to successfully enrolling in another is driven by substitution within classes in the same subject. Enrolling in an undesirable section may have implications beyond a change in the number of courses taken. Students may perform better when enrolled in a more desired class or when enrolled in the same class at a more desired time. Figure7 plots the fraction of students who enrolled in school the next term. This figure shows very little in the way of a jump at the point of discontinuity. This leads us to conclude that enrollment in a more desired class does not effect enrollment in school the subsequent term.

4 IV Results

Now we turn to estimation of the effects of enrollment on various outcomes. First we examine the effects on course enrollment within De Anza College. We will look at the number of enrolled courses the concurrent quarter and the number of enrolled courses the next quarter. Next we turn to the effects of enrollment on GPA and persistence. We will look at grade points averaged over all courses taken the concurrent semester and the average grade point focusing on courses in the same subject. One might think that enrolling in a more desirable section can lead to better preparation, more consistent attendance, or other factors that would influence academic performance. Last we turn to enrollment and attendance at other colleges. This analysis takes advantage of a match between our registration files from De Anza College and data from the National Student Clearinghouse. Here we test whether enrolling in a more desired course is associated with a higher probability of transferring to a four year college. Alternatively we also test the hypothesis that failing to enroll in a desired course increases the likelihood of seeking resources at another two-year college.

4.1 Course Enrollment

Tables 5 and 6 present our results for the effect on course enrollment within the same college. Table 5 presents local linear two stage least square results using three alternative bandwidth choices. Table 6 presents the same set of specifications using the optimal bandwidth selection and robust standard estimation procedure of Cattaneo, Calonico, and Titiunik (2014) (CCT from here on). Our main finding is that there is a robust and significant effect of successful enrollment on substitution away from other courses in the same subject during the concurrent term. Panel A, columns one, two, and three present TSLS estimates of this effect. From column one to column three we vary the bandwidth of our

local linear estimator from 20, to 10, to 5. We see that our measured coefficient on enrollment increases as we narrow the bandwidth used for estimation. Panel A, column one of Table 6 uses the CCT procedure to select optimal bandwidth for the regression. The point estimate given by the procedure is squarely in the middle of the three corresponding estimates presented in Table 5. All four estimates are lower than -1. In one case, column 3 panel A of Table 5 the estimate is significantly lower than -1. This might signal a quality/quantity trade-off in courses where students that fail to enroll in a highly desired course substitute with more than one less desired course. Columns four, five and six of panel A in Table 5 and column 2 of panel A of Table 6 present estimates of the effect of successful enrollment on enrollment in all subjects the concurrent term. Here even though the point estimates for one specification are significant at the 0.10 confidence level we do not see a consistent significant effect on total enrollment.

Panel B of Table 5 and panel B of table 6 examine the effects of enrollment on course selection the subsequent term. Columns one, two and three of panel B of table 5 and column 1 panel B of Table 6 examine the effect on taking courses in the same subject the next term. The point estimates in these regressions are each negative but none can rule out a coefficient of zero. Columns four, five and six of panel B of Table 5 and column two of panel B of Table 6 measure the effect on the total number of courses taken the subsequent school term. These regressions similarly show that there is little indication of inter temporal substitution of courses across school terms.

4.2 GPA and Persistence

Next we turn to estimates of the effect of enrollment on GPA and persistence. Table 7 presents results for three outcome measures. Column one examines the effect of successful enrollment on GPA for the current term[15]. We see a positive but statistically insignificant coefficient. Column two turns to an estimate of the effect on GPA for classes within the same subject. Again we see a slightly positive but more noisy coefficient estimate. Column three of Table 7 presents our estimate of the effect of course enrollment on an indicator for whether the student in seen in the same college one year later. This regression measures a negative but insignificant effect of later school enrollment.

4.3 Enrollment at 4-year and other 2-year colleges

Last we examine enrollment and attendance at other institutions. We first examine short term outcomes, where we look at where students are one year after the current term, then

[15]For simplicity from here on in this section we only present results for the CCT estimation procedure, results using alternative bandwidths are similar.

we look at longer term outcomes where we examine whether we see students at another institution within three years. Column one of Table 8 looks at the effect of enrolling in a college the next school term. Our estimate is that an additional successful enrollment into a desired course is associated with a six percentage point increase in the probability of attending a four year college the next term. However column two shows that successful enrollment is a desired course is associated with an eleven percentage point increase in the probability of attending a different two-year college. Neither of these estimate rule out no effect at all of successful enrollment into a desired course. Columns three and four of Table 8 examine longer term measures of these two outcome variables. Column three estimates that successfully enrolling in a course is associated with a fifteen percentage point increase in the probability of attending a four year school within three years. Column four gives a coefficient that indicates a negative effect of enrolling in a different two year school that is one one hundredth of a percentage point. Neither estimates are significantly different from zero. They are suggestive that the long term effect on four year college enrollment may be positive.

5 Subgroup Analysis and Robustness Checks

This section examines the robustness of our findings and whether there are differential effects by sub-group. The analysis looks at seven variables relating to course loads, gap, persistence and enrollment at other colleges. We make measurements for eleven subgroups based on gender, race, age, citizenship, and whether it is the student's first term in college. We check the robustness of findings by varying the number of control variables in the locally weighted regressions and by adding richer control variables. Table 9 presents our benchmark subgroup analysis. The regressions in this table estimate the following model

$$E(Y|RV, W) = \alpha + \delta' W + Z \cdot \beta + \sum_{j=1}^{3} \{\gamma_j^0 RV^j + \gamma_j^1 Z \cdot RV^j\}. \tag{7}$$

Table 9 presents estimates of the regression discontinuity estimates using two stage least squares and the method of bandwidth selection developed by Matias Cattaneo, Sebastian Calonico, and Rocio Titiunik (2013a 2013b)[16]. In simulations, CCT (2013), finds that confidence intervals have coverage that is almost correct. The estimates use a third order polynomial to approximate the underlying regression function, the expected outcome conditional on the running variable as a function of the running variable. In the CCT algorithm that selects bandwidth a fourth order polynomial is used to estimate bias due to

[16]Henceforth referred to as CCT.

functional form misspecification. The bandwidth selected using this method is usually between two and three.

Here we look at eleven outcome variables for the overall population and for eleven subgroups. Each entry in the table represents one estimate of the treatment effect for a separate two stage least squares local regression.

It looks like there may be an effect on GPA for males, and an effect on GPA for non-first-time students. There may be effect on whether you stayed in school 1yr for "young" students. There may be an effect on whether you enrolled in a four year college on first time students and on non-foreign students. There also seems to be an effect on whether you enrolled in another 2 year college on foreign students. There may be an effect on whether first-time students enrolled in a four year college the next major academic term. There may be an effect on whether foreign students enrolled in a two year college next term.

$$E(Y|RV, W) = \alpha + \delta' W + Z \cdot \beta + \sum_{j=1}^{3} \{\gamma_j^0 RV^j + \gamma_j^1 Z \cdot RV^j\} \tag{8}$$

The regressions in Table 10 allow for linear functions of the running variable with different slopes on either side of the threshold. They also control for cumulative course credits earned, cumulative number of courses taken, whether the semester is the student's first, whether the student received financial aid, gender, and whether the student declared an intention to obtain a vocational certificate or transfer to a four year college. Here we use a rectangular kernel with a bandwidth of five on either side of the threshold.

$$E(Y|RV, W) = \alpha + \delta' W + Z \cdot \beta + \gamma^0 RV + \gamma^1 Z \cdot RV \tag{9}$$

The regressions in Table 11 allow for linear functions of the running variable with different slopes on either side of the threshold. As before they control for cumulative course credits earned, cumulative number of courses taken, whether the semester is the student's first, whether the student received financial aid, gender, and whether the student declared an intention to obtain a vocational certificate or transfer to a four year college. They also control for race fixed effects, registration priority group fixed effects, term fixed effects and subject fixed effects. Here we use a rectangular kernel with a bandwidth of five on either side of the threshold.

There is a positive effect of taking classes on enrollment in a 4 yr college, the effect exists even with the addition of more extensive controls. It seems that it is driven by females and it is more pronounced for non-foreign students and older students.

The coefficient in the first row in the column titled "enrolled in 4 yr college" is the two stage least squares estimate of the effect of successfully enrolling in a wait listed section

on whether the student ever enrolls in a four year college. The estimated effect of 0.203 implies that missing a section is causality associated with a 20 percentage point drop in the likelihood of attending a four year college.

The first column has a coefficient of -1.34. This means that successfully enrolling in a desired section is associated with taking 1.3 fewer courses in the same subject. Perfect substitution would a coefficient of one and this estimate is not statistically significantly different than one. An estimate smaller than -1 would mean that each section not taken is replaced with more than one other course. This implies a marginal rate of substitution greater than one and implies that wait listed courses are more useful than the courses that replace them.

The regressions in 12 allow for linear functions of the running variable with different slopes on either side of the threshold. They also control for cumulative course credits earned, cumulative number of courses taken, whether the semester is the student's first, whether the student received financial aid, gender, and whether the student declared an intention to obtain a vocational certificate or transfer to a four year college. Here we use a rectangular kernel with a bandwidth of three on either side of the threshold.

6 Conclusions

Course availability at two year colleges is a potentially important factor in the acquisition of human capital. We examined the effect of course availability on later educational outcomes using a novel administrative data set and a regression discontinuity design based on oversubscription to college courses. We find a robust and substantial substitution effect. Specifically we find that successful enrollment into a desired course section is causes students to take fewer courses in that subject the concurrent term. We find some, but limited, evidence that students trade off quality for quantity when they successfully enroll in desired courses. That is, successfully enrolling in a desired course causes students to decrease the number of other courses in the same subject taken concurrently by more than one. Future work may seek to explore these outcomes in other settings or with larger samples. Of particular interest are the labor market outcomes of students who face course scarcity.

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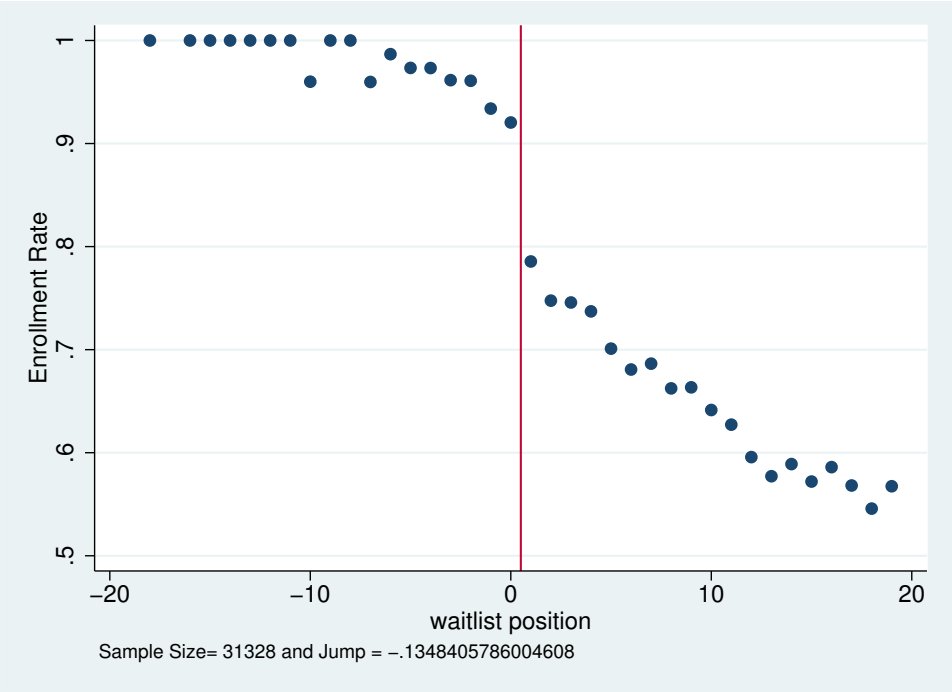
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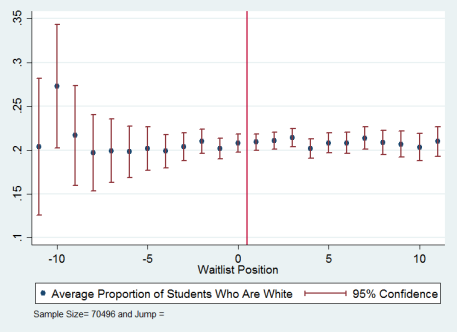
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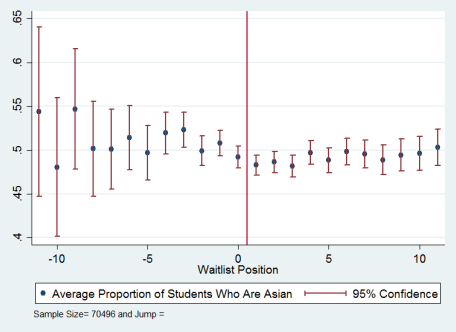
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Figure 1: First Stage: Mean Enrollment in waitlisted Section as a Function of Relative Wait List Position

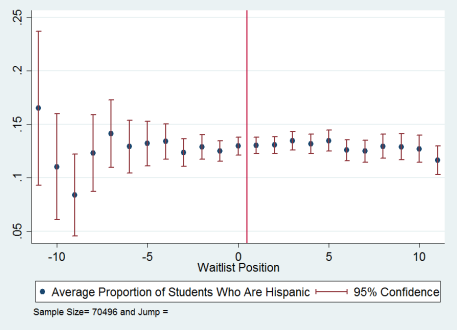




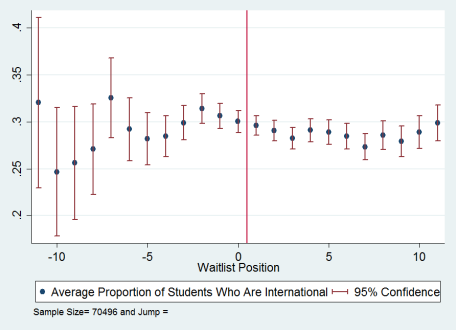
(a) White



(b) Asian

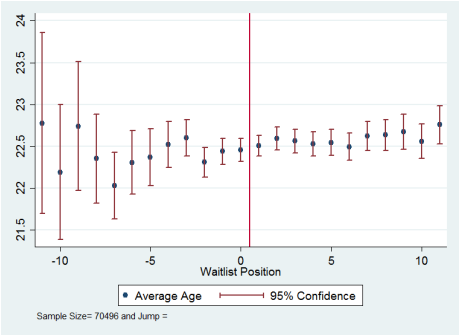


(c) Hispanic

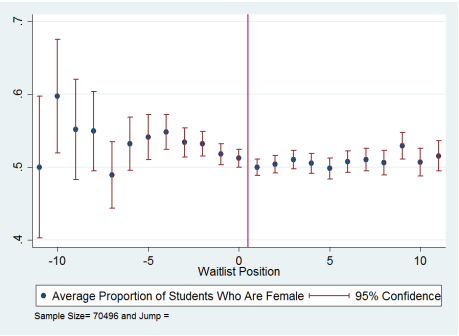


(d) International

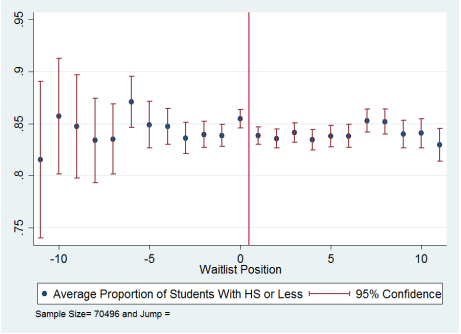
Figure 2: Smoothness on Covariates: Race and Citizenship Indicators



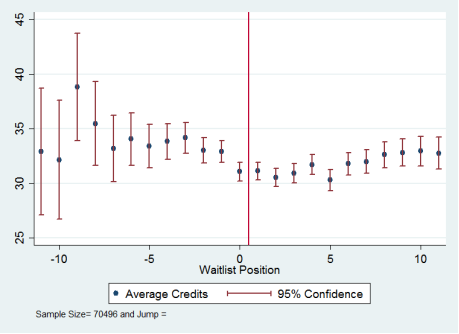
(a) Age



(b) Female



(c) HS or less



(d) Credits

Figure 3: Smoothness on Covariates: Age, Gender, a priori Education

Figure 4: No Sorting Across Waitlist Position

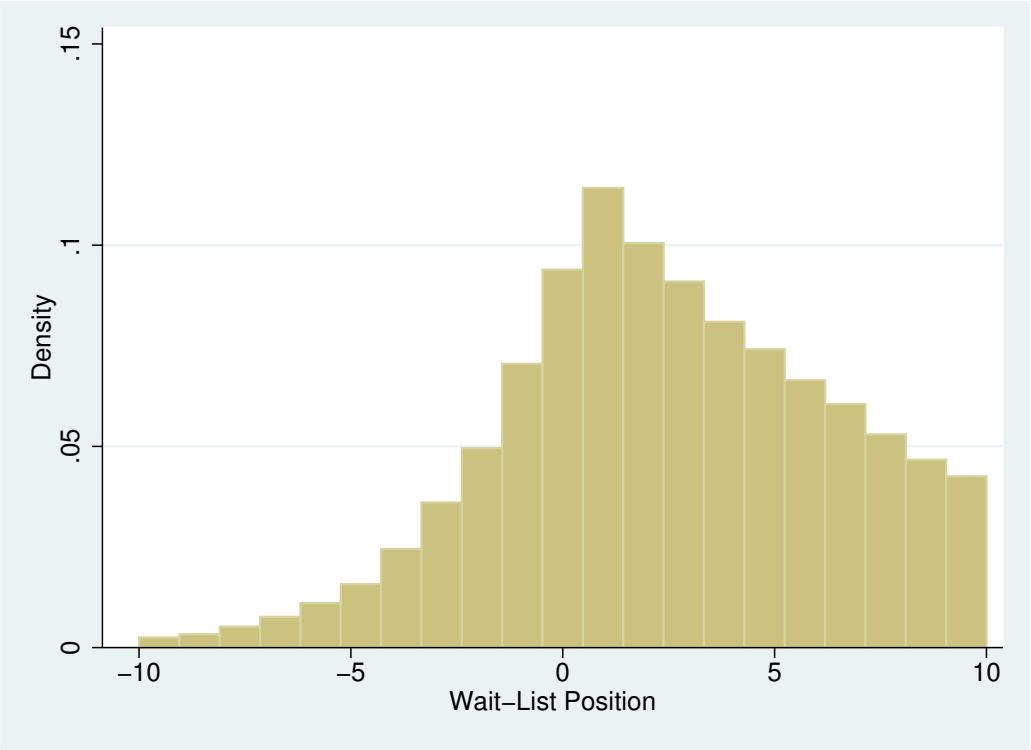


Figure 5: Enrollment in Other Sections, All Subjects, Concurrent Term

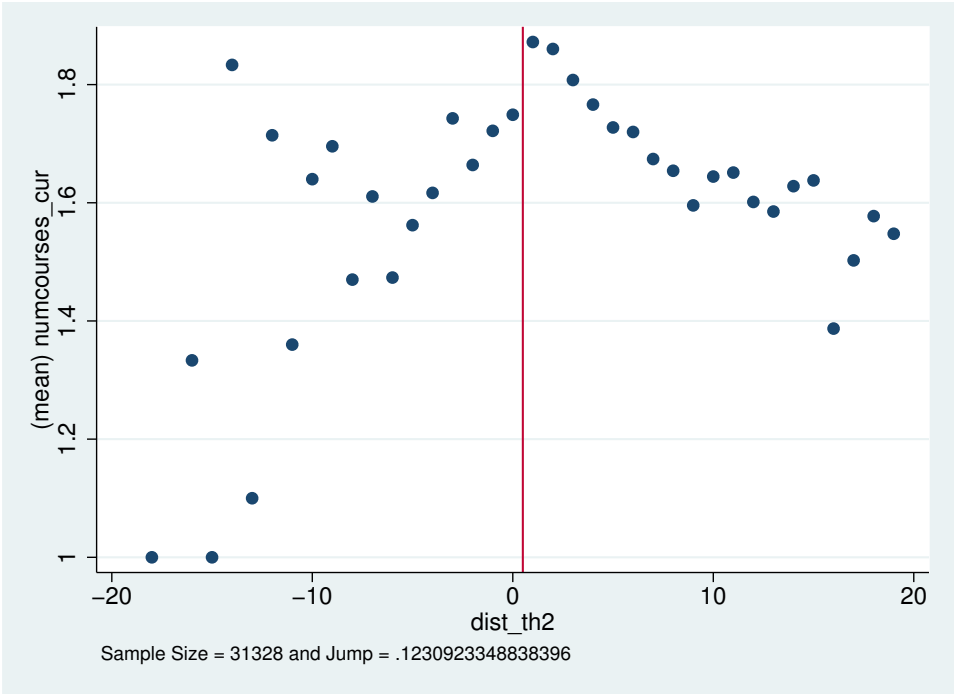


Figure 6: Enrollment in Other Sections in the Same Subject, Concurrent Term

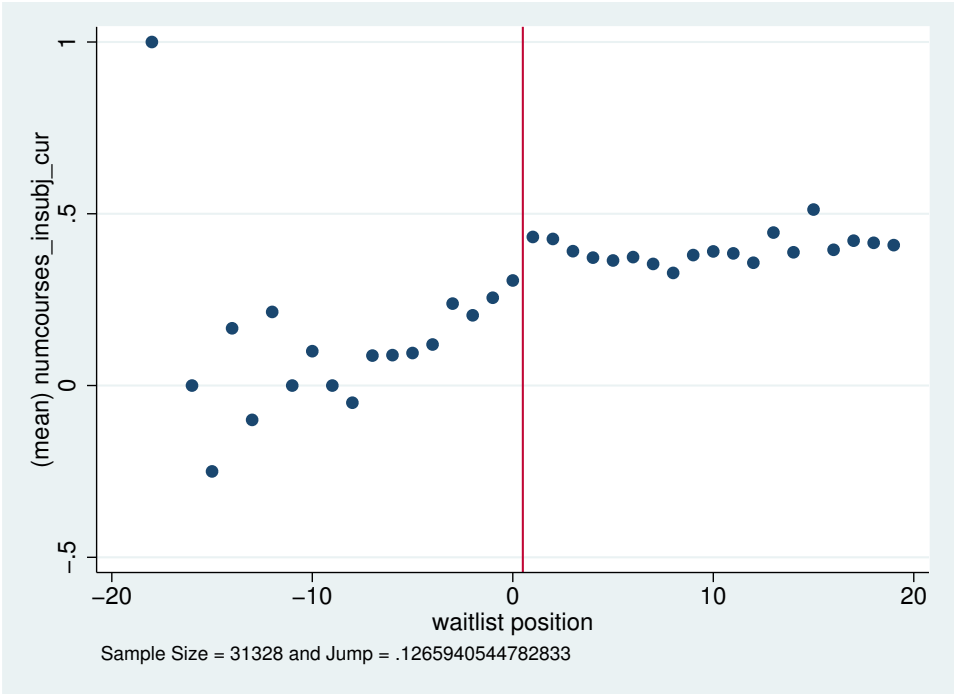


Figure 7: Stayed in School, 1 Year

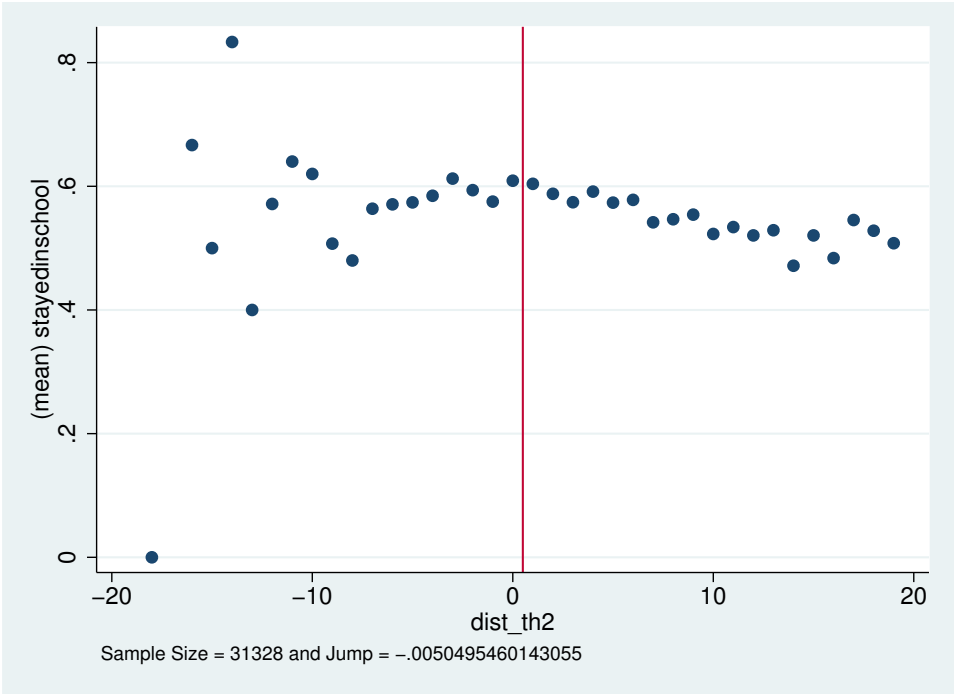


Table 1: Hypothetical Enrollment Log

student id	action	date/time
⋮	⋮	⋮
31	enroll	5:01:01 Aug 1, 2004
32	enroll	6:11:21 Aug 1, 2004
33	enroll	7:21:41 Aug 1, 2004
34	enroll	8:31:51 Aug 1, 2004
35	enroll	8:41:11 Aug 1, 2004
36	waitlist	8:51:31 Aug 1, 2004
37	waitlist	9:02:02 Aug 1, 2004
38	waitlist	11:22:12 Aug 1, 2004
39	waitlist	12:42:52 Aug 1, 2004
40	waitlist	13:32:22 Aug 1, 2004
41	waitlist	14:52:12 Aug 1, 2004
23	drop	11:32:43 Aug 14, 2004
36	enroll	11:45:32 Aug 14, 2004
13	drop	2:42:21 Aug 16, 2004
37	enroll	9:50:12 Aug 16, 2004
7	drop	5:45:33 Aug 20, 2004
38	enroll	2:01:37 Aug 21, 2004
39	drop	1:15:50 Aug 24, 2004

Table 2: Summary Statistics: Race

	U.S.		International	
	Count	Freq.	Count	Freq.
White	10,604	25.11	1,334	13.94
African-American	1,636	3.87	353	3.69
Hispanic	5,652	13.38	892	9.32
Asian	18,066	42.77	6,244	65.27
Native Am., Pac. Is., Other	1,226	2.9	185	1.93
Unknown	5,051	11.96	559	5.84
Total (n=51,802)	42,235	100	9,567	100

Table 3: Summary Statistics: Demographics

	Mean	Std. Dev.	Min	Max
Previous Enrollments	12.45	11.65	0	86
Cum. Course Hours	15.12	31.35	0	337
First Term	0.51	0.50	0	1
Financial Aid	0.18	0.39	0	1
Female	0.55	0.50	0	1
Age	25.97	8.53	18	50
Declared Certificate	0.03	0.18	0	1
Declared Transfer	0.33	0.47	0	1

Table 4: First Stage OLS Regressions

	(1) Enrollment	(2) Enrollment	(3) Enrollment
Z	0.118***	0.108***	0.101***
	(0.00714)	(0.00810)	(0.0112)
RV	-0.0113***	-0.0138***	-0.0129***
	(0.000521)	(0.00113)	(0.00343)
RVZ	0.00722***	0.00965***	0.00285
	(0.00194)	(0.00220)	(0.00509)
Constant	0.457***	0.467***	0.466***
	(0.00417)	(0.00558)	(0.00892)
Observations	51,802	41,940	27,365
R-squared	0.038	0.028	0.020
F	272.1	176.7	80.70

Table 5: TSLS Estimates of Effects on Course Enrollment

	Other Courses in Same Subject			Total Courses in All Subjects		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			Concurrent Term			
Enrolled	-1.022***	-1.582***	-2.023***	0.294*	-0.00513	-0.391
	(0.210)	(0.237)	(0.311)	(0.179)	(0.218)	(0.329)
RV	-0.00479*	-0.0214***	-0.0416***	-0.0215***	-0.0344***	-0.0546***
	(0.00287)	(0.00420)	(0.00898)	(0.00305)	(0.00553)	(0.0133)
RVZ	0.0296***	0.0448***	0.0695***	0.0347***	0.0463***	0.0582***
	(0.00373)	(0.00486)	(0.0118)	(0.00526)	(0.00644)	(0.0150)
Constant	1.467***	1.862***	2.178***	2.595***	2.766***	2.981***
	(0.139)	(0.159)	(0.211)	(0.0897)	(0.114)	(0.175)
R-squared	0.293	0.226	0.065	0.000	0.004	
Reduced Form p-val	0	0	0	0.0990	0.981	0.230
Panel B:			Subsequent Term			
Enrolled	-0.0149	-0.325	-0.230	0.129	0.240	0.560
	(0.277)	(0.374)	(0.499)	(0.297)	(0.367)	(0.538)
RV	0.00113	-0.0117	-0.00161	-0.0125**	-0.00945	0.00694
	(0.00523)	(0.0103)	(0.0200)	(0.00565)	(0.00942)	(0.0202)
RVZ	0.0150*	0.0269**	0.0103	0.0158**	0.0130	0.00662
	(0.00843)	(0.0110)	(0.0218)	(0.00736)	(0.00984)	(0.0215)
Constant	1.519***	1.702***	1.636***	2.926***	2.863***	2.691***
	(0.145)	(0.204)	(0.276)	(0.150)	(0.191)	(0.284)
R-squared	0.000					
Reduced Form p-val	0.957	0.374	0.640	0.664	0.510	0.285
Observations	51,429	41,631	27,193	51,429	41,631	27,193
Bandwidth(spots)	20	10	5	20	10	5

*** p<0.01, ** p<0.05, * p<0.1

Table 6: TSLS Estimates of Effects on Course Enrollment (CCT)

	Other Courses, Same Subject	Total Courses, All Subjects
	(1)	(2)
Panel A:	Concurrent Term	
RD_Estimate	-1.545***	0.123
	(0.334)	(0.421)
Observations	15,156	21,688
Panel B:	Subsequent Term	
RD_Estimate	-0.270	1.113
	(1.069)	(0.840)
Observations	4,462	12,108

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: TSLS Estimates of Effects on GPA and Persistence (CCT)

	(1) GPA overall cur. term	(2) GPA in subj. cur. term	(3) Enrolled 1 yr later
RD_Estimate	0.150	0.0696	-0.174
	(0.366)	(0.558)	(0.199)
Observations	18,317	10,993	16,049

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: TSLS Estimates of Effects on Four-Year College and Two-Year College Enrollment (CCT)

	Enrollment at other Colleges			
	(1)	(2)	(3)	(4)
	4 yr	other 2 yr	4 yr	other 2 yr
	nxt term	nxt term	nxt 3 yrs	nxt 3 yrs
RD_Estimate	0.0612	0.112	0.150	-0.000126
	(0.0852)	(0.0795)	(0.156)	(0.139)
Observations	16,049	16,049	16,049	16,049

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Cubic Local Polynomial Results, (CCT)

	Number of Enrolled Courses				ex-post GPA			Enrolled in College			
	all courses in subj	other courses	in same subj	all courses	overall	in subj	stayed in school 1yr	in 4-yr college	in other 2-yr college	in 4-yr college	in other 2-yr college
	in Current Term		Next Term					w/ in 3 yrs		Next Term	
overall	-1.769***	0.071	3.558	-0.0550	-0.306	-0.0486	-0.107	0.0805	0.0305	-0.0448	-0.0319
s.e.	(0.397)	(0.377)	(18.99)	(0.439)	(0.246)	(0.894)	(0.136)	(0.119)	(0.0948)	(0.0681)	(0.0504)
Obs.	23,138	34,004	9,298	23,132	22,033	17,507	34,127	34,127	34,127	34,127	34,127
Males	-1.180***	0.635	1.025	-0.101	-0.769**	0.383	0.151	-0.0974	-0.0312	-0.0485	-0.0477
s.e.	(0.454)	(0.417)	(1.492)	(0.482)	(0.389)	(1.239)	(0.154)	(0.135)	(0.106)	(0.0774)	(0.0572)
Obs.	11,321	16,632	4,670	11,391	14,919	8,557	16,682	16,682	16,682	16,682	16,682
Females	-2.429***	-0.931	1.582	0.0796	-0.280	-0.574	-0.525*	0.417	0.156	-0.0239	0.000487
s.e.	(0.803)	(0.842)	(1.598)	(0.913)	(0.691)	(1.366)	(0.303)	(0.258)	(0.192)	(0.132)	(0.0983)
Obs.	11,763	17,300	4,599	11,688	15,280	8,911	17,373	17,373	17,373	17,373	17,373
Non-First Time	-1.620***	0.232	1.725	-0.207	-0.587**	-0.573	-0.0530	-0.0229	0.0665	-0.108	-0.0604
s.e.	(0.621)	(0.419)	(4.716)	(0.476)	(0.292)	(1.897)	(0.151)	(0.136)	(0.105)	(0.0808)	(0.0560)
Obs.	18,956	27,591	7,676	18,722	17,835	14,640	27,692	27,692	27,692	27,692	27,692
Fist-Time	-1.926***	-0.573	0.406	0.725	0.652	0.372	-0.324	0.526*	-0.141	0.231*	0.0814
s.e.	(0.446)	(0.899)	(6.470)	(1.243)	(0.741)	(0.642)	(0.321)	(0.292)	(0.230)	(0.140)	(0.122)
Obs.	4,182	6,413	1,844	4,410	5,674	2,867	6,435	6,435	6,435	6,435	6,435
Non-foreign	-1.690***	0.0269	0.0757	0.0726	-0.236	1.188	-0.0819	-0.156	0.198*	-0.0547	0.0195
s.e.	(0.399)	(0.406)	(1.737)	(0.466)	(0.373)	(1.286)	(0.150)	(0.455)	(0.114)	(0.0747)	(0.0595)
Obs.	15,986	23,910	5,804	15,688	21,017	11,941	24,008	34,042	24,008	24,008	24,008
Foreign	-1.885**	0.156	0.719	-0.355	-1.593*	-2.308	-0.185	-0.102	-0.463**	-0.0170	-0.183*
s.e.	(0.870)	(0.860)	(0.619)	(1.065)	(0.920)	(1.881)	(0.298)	(0.262)	(0.233)	(0.154)	(0.106)
Obs.	7,152	10,094	2,874	7,444	9,248	5,566	10,119	10,119	10,119	10,119	10,119
Minority	4.483	0.0434	-1.274	0.849	-1.028	-0.401	-0.524	1.234	0.0781	0.188	-0.0459
s.e.	(6.973)	(1.990)	(2.903)	(1.306)	(2.376)	(1.142)	(0.761)	(0.960)	(0.638)	(0.267)	(0.278)
Obs.	3,874	5,821	1,537	3,664	4,913	3,223	5,847	5,847	7,134	5,847	5,847
Asian	-1.118***	0.397	0.635	-0.220	-0.619	-0.434	-0.100	-0.164	0.0278	-0.0570	-0.0231
s.e.	(0.367)	(0.514)	(1.364)	(0.614)	(0.431)	(0.776)	(0.187)	(0.175)	(0.131)	(0.0987)	(0.0662)
Obs.	11,497	16,678	5,145	12,125	15,188	8,838	16,721	16,721	16,721	16,721	16,721
White	-1.637***	-0.825	-0.0257	-0.246	-0.603	0.953	-0.101	0.162	-0.391	-0.0798	0.0297
s.e.	(0.559)	(0.630)	(1.505)	(0.631)	(0.662)	(1.147)	(0.687)	(0.193)	(0.541)	(0.118)	(0.0805)
Obs.	4,884	7,108	2,241	4,555	6,339	3,842	8,696	7,139	8,696	7,139	7,139
Old	-3.722	0.619	1.002	-0.292	-0.818	7.654	0.267	0.325	-0.0371	-0.170	-0.0207
s.e.	(3.731)	(0.885)	(1.501)	(0.966)	(0.659)	(30.53)	(0.292)	(0.255)	(0.192)	(0.159)	(0.107)
Obs.	7,920	11,481	3,963	6,680	9,782	7,414	11,544	11,544	11,544	11,544	11,544
Young	-1.475***	-0.191	1.140	0.0154	-0.413	0.155	-0.260*	-0.0193	0.0509	0.00273	-0.0361
s.e.	(0.306)	(0.397)	(2.455)	(0.479)	(0.402)	(0.795)	(0.156)	(0.136)	(0.109)	(0.0742)	(0.0565)
Obs.	15,218	22,523	7,138	16,452	20,483	11,501	22,583	22,583	22,583	22,583	22,583

This table presents estimates of the regression discontinuity design using two stage least squares and the method of bandwidth selection developed by CCT(2013a 2013b). The estimates use a third order polynomial to approximate the underlying regression function, the expected outcome conditional on the running variable as a function of the running variable. In the CCT algorithm that selects bandwidth a fourth order polynomial is used to estimate bias due to functional form misspecification. The bandwidth selected using this method is usually between two and three.

Table 10: Local Linear TSLS, with Control Variables (BW of 5)

	Number of Enrolled Courses				ex-post GPA			Enrolled in College			
	all courses in subj	other courses	in same subj	all courses	overall	in subj	stayed in school 1yr	in 4-yr col-lege	in other 2-yr col-lege	in 4-yr col-lege	in other 2-yr col-lege
	in Current Term		Next Term					w/ in 3 yrs		Next Term	
overall	-1.943***	0.138	0.0864	0.448	-0.0662	0.422	-0.0828	0.273**	0.0780	0.0576	0.00205
s.e.	(0.470)	(0.338)	(0.441)	(0.435)	(0.329)	(0.831)	(0.110)	(0.122)	(0.0941)	(0.0683)	(0.0496)
Obs.	27,403	40,442	8,930	27,363	36,058	20,881	40,593	40,593	40,593	40,593	40,593
Males	-1.852***	0.0719	-0.694	0.596	-0.292	0.0525	-0.0955	0.0658	0.0322	0.0171	-0.0483
s.e.	(0.564)	(0.416)	(0.720)	(0.557)	(0.420)	(1.026)	(0.140)	(0.149)	(0.120)	(0.0863)	(0.0653)
Obs.	13,375	19,719	4,461	13,420	17,685	10,156	19,781	19,781	19,781	19,781	19,781
Females	-2.044**	0.327	0.877	0.368	0.215	0.729	-0.0525	0.520**	0.137	0.0975	0.0616
s.e.	(0.798)	(0.568)	(0.736)	(0.695)	(0.541)	(1.407)	(0.177)	(0.220)	(0.154)	(0.112)	(0.0797)
Obs.	14,028	20,723	4,469	13,943	18,373	10,725	20,812	20,812	20,812	20,812	20,812
Non-First Time Students	-1.653	0.482	0.280	0.463	-0.329	0.989	-0.105	0.287*	0.0824	0.0189	-0.0284
s.e.	(1.043)	(0.432)	(0.614)	(0.535)	(0.419)	(1.974)	(0.143)	(0.158)	(0.117)	(0.0905)	(0.0616)
Obs.	22,326	32,611	7,301	21,991	29,122	17,339	32,733	32,733	32,733	32,733	32,733
Fist-Time Students	-2.079***	-0.679	-0.305	0.466	0.629	0.197	0.000616	0.229	0.0635	0.140*	0.0677
s.e.	(0.377)	(0.542)	(0.608)	(0.717)	(0.536)	(0.589)	(0.142)	(0.172)	(0.154)	(0.0831)	(0.0851)
Obs.	5,077	7,831	1,629	5,372	6,936	3,542	7,860	7,860	7,860	7,860	7,860
Non-foreign Students	-2.164***	-0.0154	-0.391	0.275	0.0409	1.592	-0.119	0.353**	0.152	0.0402	0.0365
s.e.	(0.544)	(0.408)	(0.856)	(0.502)	(0.394)	(1.142)	(0.138)	(0.156)	(0.124)	(0.0839)	(0.0658)
Obs.	18,945	28,486	5,585	18,586	25,080	14,225	28,607	28,607	28,607	28,607	28,607
Foreign Students	-1.696*	0.350	0.292	0.686	-0.399	-2.524	-0.0551	0.0911	-0.0601	0.0916	-0.0587
s.e.	(0.931)	(0.606)	(0.501)	(0.849)	(0.595)	(2.193)	(0.179)	(0.196)	(0.142)	(0.118)	(0.0716)
Obs.	8,458	11,956	3,345	8,777	10,978	6,656	11,986	11,986	11,986	11,986	11,986
Minority Students	-1.706***	0.812***	0.155	-0.698*	0.158	-0.549	-0.175*	0.0662	0.0609	-0.0145	-0.00835
s.e.	(0.661)	(0.305)	(0.281)	(0.424)	(0.303)	(2.686)	(0.0982)	(0.0845)	(0.0835)	(0.0458)	(0.0497)
Obs.	7,173	11,569	2,147	7,105	9,782	5,166	11,639	11,639	11,639	11,639	11,639
Asian Students	-1.227**	0.140	0.258	0.563	0.282	0.325	-0.0830	-0.0231	0.100	0.0435	0.0723
s.e.	(0.555)	(0.538)	(0.943)	(0.722)	(0.498)	(0.967)	(0.172)	(0.193)	(0.149)	(0.112)	(0.0765)
Obs.	13,683	19,982	4,934	14,407	18,182	10,587	20,031	20,031	20,031	20,031	20,031
White Students	-1.554***	-0.0181	0.534	0.829	-0.596	0.404	-0.370**	0.303*	0.151	-0.0121	0.0609
s.e.	(0.527)	(0.497)	(0.548)	(0.533)	(0.503)	(0.935)	(0.182)	(0.184)	(0.142)	(0.105)	(0.0759)
Obs.	5,729	8,343	1,658	5,336	7,454	4,513	8,377	8,377	8,377	8,377	8,377
Old Students	-4.058	0.284	0.445	0.0430	-0.277	3.184	0.485*	0.651**	-0.0210	0.0641	-0.0641
s.e.	(7.204)	(0.772)	(0.647)	(0.948)	(0.673)	(13.49)	(0.288)	(0.309)	(0.195)	(0.159)	(0.106)
Obs.	9,360	13,606	2,954	7,995	11,658	7,126	13,682	13,682	13,682	13,682	13,682
Young Students	-1.849***	0.126	-0.591	0.609	0.108	0.418	-0.245**	0.170	0.0997	0.0589	0.0211
s.e.	(0.363)	(0.360)	(0.648)	(0.484)	(0.372)	(0.720)	(0.120)	(0.133)	(0.107)	(0.0723)	(0.0561)
Obs.	18,043	26,836	5,976	19,368	24,400	13,755	26,911	26,911	26,911	26,911	26,911
Cum. Course credits	×	×	×	×	×	×	×	×	×	×	×
Cum. Enrolled courses	×	×	×	×	×	×	×	×	×	×	×
First Time Status	×	×	×	×	×	×	×	×	×	×	×
Received Fin. Aid	×	×	×	×	×	×	×	×	×	×	×
Female	×	×	×	×	×	×	×	×	×	×	×
Goal Voc. Cert.	×	×	×	×	×	×	×	×	×	×	×
Goal 4-yr Degree	×	×	×	×	×	×	×	×	×	×	×

These regressions allow for linear functions of the running variable with different slopes on either side of the threshold. They also control for cumulative course credits earned, cumulative number of courses taken, whether the semester is the student's first, whether the student received financial aid, gender, and whether the student declared an intention to obtain a vocational certificate or transfer to a four year college. Here we use a rectangular kernel with a bandwidth of five on either side of the threshold.

$$E(Y|RV, W) = \alpha + \delta' W + Z \cdot \beta + \gamma^0 RV + \gamma^1 Z \cdot RV$$

Table 11: Local Linear TSLS, with More Extensive Control Variables (BW of 5)

	Number of Enrolled Courses				ex-post GPA			Enrolled in College			
	all courses in subj	other courses	in same subj	all courses	overall	in subj	stayed in school 1yr	in 4-yr college	in other 2-yr college	in 4-yr college	in other 2-yr college
	in Current Term		Next Term					w/ in 3 yrs		Next Term	
overall	-1.340***	0.261	0.242	0.436	-0.0656	0.565	-0.0897	0.203*	0.0357	0.0662	-0.0117
s.e.	(0.337)	(0.343)	(0.342)	(0.449)	(0.344)	(0.825)	(0.101)	(0.122)	(0.0966)	(0.0703)	(0.0514)
Obs.	27,398	40,431	8,929	27,355	36,047	20,876	40,582	40,582	40,582	40,582	40,582
Males	-1.327***	0.174	0.0253	0.703	-0.292	0.235	-0.0213	-0.0358	-0.0235	0.00410	-0.0738
s.e.	(0.484)	(0.444)	(0.487)	(0.601)	(0.470)	(1.203)	(0.133)	(0.159)	(0.129)	(0.0930)	(0.0715)
Obs.	13,373	19,714	4,460	13,416	17,680	10,154	19,776	19,776	19,776	19,776	19,776
Females	-1.293***	0.436	0.457	0.207	0.233	0.550	-0.148	0.460**	0.117	0.143	0.0545
s.e.	(0.474)	(0.542)	(0.491)	(0.691)	(0.523)	(1.123)	(0.156)	(0.205)	(0.150)	(0.111)	(0.0777)
Obs.	14,025	20,717	4,469	13,939	18,367	10,722	20,806	20,806	20,806	20,806	20,806
Non-First Time Students	-0.583	0.552	0.530	0.450	-0.376	0.791	-0.0803	0.200	0.0279	0.0376	-0.0402
s.e.	(0.645)	(0.435)	(0.493)	(0.541)	(0.440)	(1.483)	(0.130)	(0.155)	(0.119)	(0.0923)	(0.0633)
Obs.	22,321	32,600	7,300	21,983	29,111	17,334	32,722	32,722	32,722	32,722	32,722
Fist-Time Students	-1.915***	-0.238	-0.442	0.379	0.835	0.765	-0.0929	0.184	0.0426	0.113	0.0730
s.e.	(0.345)	(0.506)	(0.442)	(0.767)	(0.562)	(0.745)	(0.127)	(0.169)	(0.156)	(0.0815)	(0.0861)
Obs.	5,077	7,831	1,629	5,372	6,936	3,542	7,860	7,860	7,860	7,860	7,860
Non-foreign Students	-1.389***	0.0774	0.525	0.210	0.0483	1.679	-0.129	0.284*	0.0977	0.0457	0.0198
s.e.	(0.359)	(0.412)	(0.686)	(0.522)	(0.417)	(1.093)	(0.126)	(0.154)	(0.125)	(0.0857)	(0.0674)
Obs.	18,942	28,478	5,584	18,581	25,072	14,222	28,599	28,599	28,599	28,599	28,599
Foreign Students	-1.360*	0.460	0.0781	0.899	-0.493	-2.600	-0.0439	0.0311	-0.0688	0.0930	-0.0572
s.e.	(0.731)	(0.627)	(0.423)	(0.933)	(0.605)	(2.083)	(0.167)	(0.203)	(0.150)	(0.124)	(0.0754)
Obs.	8,456	11,953	3,345	8,774	10,975	6,654	11,983	11,983	11,983	11,983	11,983
Minority Students	-1.373***	0.636**	0.0452	-0.511	0.189	-0.00798	0.0999	0.0550	0.0178	-0.00944	0.0167
s.e.	(0.323)	(0.271)	(0.229)	(0.363)	(0.273)	(1.067)	(0.0805)	(0.0778)	(0.0765)	(0.0422)	(0.0461)
Obs.	7,172	11,566	2,147	7,103	9,780	5,165	11,636	11,636	11,636	11,636	11,636
Asian Students	-1.030**	0.167	0.162	0.618	0.312	0.447	-0.113	-0.122	0.0803	0.0806	0.0710
s.e.	(0.433)	(0.582)	(0.619)	(0.852)	(0.557)	(0.999)	(0.164)	(0.211)	(0.163)	(0.123)	(0.0838)
Obs.	13,680	19,975	4,933	14,401	18,175	10,584	20,024	20,024	20,024	20,024	20,024
White Students	-1.341***	0.0994	0.631	0.740	-0.675	0.156	-0.373**	0.258	0.0803	-0.0291	0.0398
s.e.	(0.446)	(0.481)	(0.475)	(0.518)	(0.521)	(0.853)	(0.163)	(0.176)	(0.137)	(0.103)	(0.0738)
Obs.	5,728	8,341	1,658	5,336	7,452	4,512	8,375	8,375	8,375	8,375	8,375
Old Students	-0.426	0.636	0.414	0.124	-0.219	1.508	0.356	0.592**	-0.0725	0.0657	-0.0819
s.e.	(1.174)	(0.731)	(0.484)	(0.910)	(0.688)	(2.508)	(0.235)	(0.279)	(0.186)	(0.151)	(0.102)
Obs.	9,359	13,603	2,954	7,993	11,655	7,125	13,679	13,679	13,679	13,679	13,679
Young Students	-1.601***	0.146	0.0849	0.537	0.117	0.642	-0.238**	0.0933	0.0637	0.0854	0.00949
s.e.	(0.324)	(0.372)	(0.460)	(0.509)	(0.390)	(0.873)	(0.112)	(0.137)	(0.112)	(0.0763)	(0.0590)
Obs.	18,039	26,828	5,975	19,362	24,392	13,751	26,903	26,903	26,903	26,903	26,903
Race FEs	×	×	×	×	×	×	×	×	×	×	×
Reg. Priority Group FEs	×	×	×	×	×	×	×	×	×	×	×
Year × Term FEs	×	×	×	×	×	×	×	×	×	×	×
Subj. FEs	×	×	×	×	×	×	×	×	×	×	×

These regressions allow for linear functions of the running variable with different slopes on either side of the threshold. As before they control for cumulative course credits earned, cumulative number of courses taken, whether the semester is the student's first, whether the student received financial aid, gender, and whether the student declared an intention to obtain a vocational certificate or transfer to a four year college. They also control for race fixed effects, registration priority group fixed effects, term fixed effects and subject fixed effects. Here we use a rectangular kernel with a bandwidth of five on either side of the threshold.

Table 12: Local Linear TSLS, with More Extensive Control Variables (BW of 3)

	Number of Enrolled Courses				ex-post GPA			Enrolled in College			
	all courses in subj	other courses	in same subj	all courses	overall	in subj	stayed in school 1yr	in 4-yr col-lege	in other 2-yr col-lege	in 4-yr col-lege	in other 2-yr col-lege
	in Current Term		Next Term					w/ in 3 yrs		Next Term	
overall	-1.469**	0.283	2.648	0.460	-1.111	-0.0263	-0.193	0.392	-0.0437	0.0861	-0.0961
s.e.	(0.579)	(0.726)	(8.075)	(0.843)	(0.787)	(1.257)	(0.217)	(0.271)	(0.201)	(0.146)	(0.110)
Obs.	18,046	26,276	5,840	17,897	23,375	13,530	26,372	26,372	26,372	26,372	26,372
Males	-1.067*	0.685	-27.74	0.359	-1.357	-0.547	0.0224	-0.0248	-0.101	0.0259	-0.0768
s.e.	(0.645)	(0.745)	(1,026)	(0.789)	(0.894)	(1.812)	(0.217)	(0.259)	(0.212)	(0.151)	(0.116)
Obs.	8,840	12,850	2,955	8,773	11,520	6,613	12,891	12,891	12,891	12,891	12,891
Females	-2.048	-0.780	8.599	0.731	-0.847	0.414	-0.781	1.431	0.166	0.235	-0.144
s.e.	(1.437)	(2.078)	(36.20)	(2.879)	(1.760)	(2.071)	(0.773)	(1.199)	(0.522)	(0.398)	(0.286)
Obs.	9,206	13,426	2,885	9,124	11,855	6,917	13,481	13,481	13,481	13,481	13,481
Non-First Time Students	-1.321	0.643	-42.70	0.146	-1.995	0.0493	-0.0767	0.229	-0.0932	0.0493	-0.188
s.e.	(1.187)	(0.948)	(1,261)	(1.065)	(1.407)	(3.439)	(0.278)	(0.333)	(0.255)	(0.195)	(0.150)
Obs.	14,650	21,127	4,738	14,352	18,831	11,225	21,205	21,205	21,205	21,205	21,205
Fist-Time Students	-1.585***	-0.316	-2.381	0.929	0.385	0.528	-0.452	0.719	0.0373	0.155	0.134
s.e.	(0.520)	(1.065)	(5.395)	(1.387)	(0.889)	(0.908)	(0.319)	(0.460)	(0.319)	(0.175)	(0.181)
Obs.	3,396	5,149	1,102	3,545	4,544	2,305	5,167	5,167	5,167	5,167	5,167
Non-foreign Students	-1.526**	0.167	0.184	0.0895	-0.578	1.338	-0.226	0.598	0.231	0.0542	-0.00124
s.e.	(0.619)	(0.866)	(1.027)	(0.993)	(0.883)	(1.527)	(0.271)	(0.366)	(0.268)	(0.176)	(0.139)
Obs.	12,429	18,378	3,615	12,072	16,139	9,174	18,453	18,453	18,453	18,453	18,453
Foreign Students	-1.598	0.182	1.107	1.675	-2.536	-4.523	-0.110	-0.0872	-0.617	0.135	-0.274
s.e.	(1.353)	(1.400)	(1.091)	(2.052)	(2.102)	(5.185)	(0.378)	(0.455)	(0.455)	(0.278)	(0.215)
Obs.	5,617	7,898	2,225	5,825	7,236	4,356	7,919	7,919	7,919	7,919	7,919
Minority Students	-1.296***	0.679**	-0.0293	-0.569	0.227	-0.0645	-0.0822	0.0770	0.0146	-0.0222	-0.0334
s.e.	(0.332)	(0.279)	(0.236)	(0.392)	(0.283)	(1.096)	(0.0828)	(0.0800)	(0.0786)	(0.0435)	(0.0480)
Obs.	6,820	11,013	2,047	6,768	9,300	4,915	11,080	11,080	11,080	11,080	11,080
Asian Students	-0.621	0.174	1.078	0.403	-0.527	0.130	-0.390	0.164	-0.0877	0.225	-0.130
s.e.	(0.554)	(0.923)	(1.260)	(1.168)	(0.783)	(1.133)	(0.295)	(0.341)	(0.258)	(0.211)	(0.136)
Obs.	8,996	12,926	3,205	9,422	11,755	6,841	12,965	12,965	12,965	12,965	12,965
White Students	-2.315**	0.0505	-1.460	-0.124	-1.648	-0.989	-0.227	0.473	0.0383	-0.199	0.00674
s.e.	(1.088)	(0.943)	(3.053)	(0.801)	(1.229)	(1.536)	(0.294)	(0.369)	(0.261)	(0.212)	(0.142)
Obs.	3,784	5,440	1,086	3,473	4,833	2,935	5,464	5,464	5,464	5,464	5,464
Old Students	5.820	4.409	0.550	0.467	-4.924	-1.575	0.0641	0.886	0.373	-0.113	-0.466
s.e.	(65.07)	(5.816)	(0.861)	(2.422)	(6.449)	(7.490)	(0.804)	(1.233)	(0.784)	(0.572)	(0.596)
Obs.	6,084	8,808	1,883	5,134	7,512	4,593	8,855	8,855	8,855	8,855	8,855
Young Students	-1.214***	-0.359	-0.322	0.455	-0.246	0.210	-0.258	0.362	-0.120	0.186	-0.0205
s.e.	(0.391)	(0.676)	(1.142)	(0.912)	(0.685)	(0.965)	(0.202)	(0.261)	(0.200)	(0.141)	(0.105)
Obs.	11,962	17,468	3,957	12,763	15,863	8,937	17,517	17,517	17,517	17,517	17,517
Cum. Course credits	×	×	×	×	×	×	×	×	×	×	×
Cum. Enrolled courses	×	×	×	×	×	×	×	×	×	×	×
First Time Status	×	×	×	×	×	×	×	×	×	×	×
Received Fin. Aid	×	×	×	×	×	×	×	×	×	×	×
Female	×	×	×	×	×	×	×	×	×	×	×
Goal Voc. Cert.	×	×	×	×	×	×	×	×	×	×	×
Goal 4-yr Degree	×	×	×	×	×	×	×	×	×	×	×

These regressions allow for linear functions of the running variable with different slopes on either side of the threshold. They also control for cumulative course credits earned, cumulative number of courses taken, whether the semester is the student's first, whether the student received financial aid, gender, and whether the student declared an intention to obtain a vocational certificate or transfer to a four year college. Here we use a rectangular kernel with a bandwidth of three on either side of the threshold.