The Effect of Course Shutouts on Community College Students: Evidence from Waitlist Cutoffs*

Silvia Robles†

Max A. Gross‡

Robert Fairlie§

Thomas Barrios¶

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Abstract

Community colleges serve over half of undergraduates in California, while being funded at less than half of the per-student rate of the more selective University of California system. The impact of funding disparities in higher education on student success is unclear, particularly the mechanisms through which such resource effects could operate. This paper measures the effect of course shutouts, a popular explanation of why resources matter, at De Anza Community College in California. Using reconstructed waitlist queues from detailed registration data, we compare students who missed the admission cutoff for a course section to those who made the cutoff, in a small neighborhood around the course admission threshold. Estimates from a fuzzy regression discontinuity analysis show that students who miss a waitlist cutoff are 3.7 percentage points more likely to take zero courses that term. There is also evidence that students substitute for the waitlisted course by transferring to another two-year school shortly after. These results document the importance of structural differences between four-year schools and two year, non-selective institutions.

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†Ford School of Public Policy, University of Michigan; *srobles@gmail.com*

‡Department of Economics, University of Michigan; *maxgross@umich.edu*

§Department of Economics, University of California, Santa Cruz; *rfairlie@ucsc.edu*

¶TrueCar, Inc; *tbarrios@umich.edu*

1 Introduction

According to the latest Digest of Education Statistics,[1] 34% of undergraduate students in the U.S., and 53% of undergraduates in California are enrolled in a public, two-year college. Given the large fraction of students who experience higher education through a community college, it is important to quantify how structural differences between two-year and four-year colleges can help or hinder students in pursuit of their education and workforce goals. One of the most obvious differences between public two-year colleges and four-year universities, particularly selective ones, is a large disparity in resources. For example, California's 2017-18 state budget accorded \$32,368 in core funding per full-time equivalent student for the University of California (UC) system, and \$13,021 in the California Community College (CCC) system. While intuition dictates that funding UC students at more than twice the level of CCC students should make a difference in any education production function, exactly how an extra dollar per student could influence outcomes such as credit accumulation or time to degree is poorly understood.

Anecdotally, overburdened community college budgets are often associated with course overcrowding, a claim supported in the popular press. When a college faces budgetary difficulties, it may reduce its course offerings or the number of sections per course, and more students may find themselves stuck on waiting lists, unable to enroll in the courses they need in order to complete a degree or transfer to a four-year. This hypothesis appears in the academic literature as well, where some papers have measured the impacts of aggregate variation in resources per student. For example, Bound and Turner (2007) looks at changes in the size of incoming high school graduating cohorts to estimate the effect of fewer per-student resources, and finds that it is associated with lower bachelor's completion rates. More recently, Deming and Walters (2017) examines changes in state budget appropriations and finds they cause lower college enrollment as well as lower bachelor's completion rates. Both papers cite oversubscribed courses as a possible mechanism that can explain the relationship

[1] National Center for Education Statistics, 2017 Tables and Figures, Table 304.80.

between funding and enrollment and completion. Despite the prevalence of the course-shutout hypothesis, few studies have tried to measure the impact of being rationed out of a course.

This paper provides the first evidence, to our knowledge, of the impact of being shut out of an oversubscribed course section in a community college setting, using detailed administrative data from De Anza Community College in California. The analysis constructs waitlist queues from registration attempt records and links them to transcript data containing student course schedules, grades and degrees, as well as to the National Student Clearinghouse (NSC) in order to measure the impact of course waiting lists on future course-taking, enrollment, and transfers to other postsecondary institutions.

Specifically, the analysis uses a fuzzy regression discontinuity design (FRD) to compare students who signed up for a course-section waiting list and narrowly missed or made the admission cutoff. To understand the intuition behind the FRD, consider a section for an introductory English composition course. Suppose the section has a waiting list with two people on it. By the end of the registration period, if one formerly enrolled student decided to drop out, then the first person on the waitlist would have the opportunity to enroll in her desired section, while the second person on the list would not. Since neither waitlisted student can reliably predict how many seats will open up, the cutoff, or the waitlist number below which a student does not get an opportunity to enroll, is difficult to manipulate and introduces exogenous variation.

The paper studies traditional community college students with a stated interest in either earning their associates degree or transferring to a four-year college. The reduced form results show that missing a waitlist cutoff significantly increases the probability of sitting out the term by 2.4 percentage points. That is, students who miss a cutoff are more likely to enroll in zero courses in the concurrent term, a phenomenon we call same-semester drop-out. This represents a 36% increase relative to the same-semester dropout rate of 6.6% among students who do not miss a waitlist cutoff.

Using the the waitlist cutoff as an instrument for being rationed out of a section during the registration period, the 2SLS estimates show that being shut out of a course during the registration period leads to a 3.7 percentage point increase in same-semester dropout. This effect is relative to the same-semester dropout rate among control compliers, where control compliers are those who enroll in their desired section by the end of the registration period, precisely because they did not miss the waitlist cutoff. About 6.4% of control compliers sit out the semester. Therefore, being shutout of a section leads to a 58% increase in same-semester dropout for compliers.

The rise in same-semester dropout is accompanied by a 2.4 percentage point fall in full-time enrollment and a 1.3 point fall in part-time enrollment, though these estimates are not significant.

In addition, missing a waitlist cutoff increases transfers to other two-year colleges within the next two years by 3.6 percentage points, possibly indicating that students attempt to substitute for the course they were shut out of. There is no detectable evidence of an effect on persistence at De Anza in the next semester, or an average effect on completion rates for associates degrees, certificates, or bachelors degrees within five years. However, when students are split into subgroups by ethnic categories the analysis finds divergent impacts. Asian students respond to rationing by transferring to a four year college sooner than they would have otherwise, while transfers to two-year schools are highest among underrepresented minorities. This leads to a corresponding increase in bachelors degree completion rates within five years of the waitlist for Asian students and a lag in completion rates for White students relative to the counterfactual. Ethnicity is most likely a proxy for other unobservable skills and advantages (or lack thereof) in navigating the higher education system, and illustrates the heterogeneity in how the CCC system is used.

Taken together, the evidence of large enrollment impacts from a relatively small friction, such as missing the waitlist cutoff for one section, demonstrates the potential for oversubscribed courses to meaningfully alter a student's trajectory. This can be taken as a proof of concept for the negative effect of course scarcity on educational attainment, one of the main hypotheses for how resources can affect higher education outcomes. Although waiting lists are frequently mentioned as a negative result of budget shortfalls, relatively little information on the scale and scope of the problem exists. The documented impacts of course shutouts at De Anza offer a reason to examine this question system-wide.

1.1 Related Literature

Several studies have used aggregate data to measure the impact of college resources. For example, Bound and Turner (2007) uses variation in the size of graduating high school cohorts to estimate the effect of decreases in per capita funding, finding a commensurate drop in bachelors degree attainment. Fortin (2006) uses variation in cohort sizes, state appropriations, and tuition to estimate impacts on college enrollment (and ultimately the college wage premium). More recently, Deming and Walters (2017), estimates the effect of large changes in state budgets on enrollment and degree attainment. The paper finds a decline in the number of bachelors degrees driven by a decrease in persistence among students who were already enrolled prior to the budget cut, rather than decreases in matriculation rates. In addition, Deming and Walters (2017) detect decreases in student support spending on services such as tutoring and mentoring.

Notwithstanding the work using aggregate data, there is limited causal evidence on specific pathways through which college budgets could affect degree attainment. Some studies have implied how resources could matter by evaluating resource-intensive interventions such as financial incentives (Barrow et al., 2014), tutoring, mentoring (Betting and Baker, 2014), or full-service wrap-around programs such as the CUNY ASAP experiment(Scrivener et al., 2015).[2]

To the best of our knowledge, there are two other papers which attempt to identify the causal effect of course scarcity. Kurlaender et al. (2014) uses randomly assigned registration times as an instrument for course availability and finds that scarcity does not influence time

[2]ASAP provided community college students with a comprehensive package of interventions, one of which was a higher course registration priority.

to degree.[3] The paper uses registration time as an instrument for the frequency with which a student is "shut out" from enrolling in an oversubscribed course. While it finds a meaningful relationship between "shutouts" and time to degree using OLS, there is no detectable effect in an IV framework. The setting for Kurlaender et al. (2014) is UC Davis, a selective four year college where the average undergraduate SAT score is 1180.

The other paper, Neering (N.D.), uses a similar registration priority instrument in one of the California State universities.[4] The paper finds evidence that Cal State students use summer school to avoid negative long-term impacts of course overcrowding.

There are at least four reasons why course scarcity might have a different effect on community college students relative to students at four year colleges in the UC and Cal State systems. On the demand side, community colleges have open enrollment policies, unlike selective four-year schools that can reject applicants in order to manage course demand. Second, tuition is much lower at community colleges, which reduces the barrier to entry and also fuels demand.

On the supply side, community colleges are particularly reliant on funding from state governments, which are affected by budgetary pressures. Together, these factors make community colleges susceptible to large, unexpected swings in enrollment and funding. For example, enrollment in community colleges increased by over 8% between 2008 and 2009 during the Great Recession while enrollment in four year colleges increased by less than 1% (Dunbar, Hossler, and Shapiro, 2011). California's two year public schools in particular saw a sharp, per-student funding decrease of about 11% in 2009 due to the defeat of several budget proposals.[5] Finally, at De Anza Community College in particular, section enrollment is capped at 40 students with few exceptions, while class sizes at a UC or Cal State school may be allowed to expand more readily. The potential for sectoral and geographic heterogeneity

[3]In practice, the registration times are randomly assigned within registration priority blocks. The blocks are determined by credit accumulation.

[4]Priority is assigned in a quasi-random fashion based on the first three letters of a student's last name, which introduces exogenous variation in the scarcity a student faces.

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

leave a gap in the current understanding of the effect of course capacity constraints.

This paper also contributes to the literature on course registration behavior. Registration attempt data has rarely been used for descriptive analysis, let alone causal inference. Gurantz (2015) presents a review of other papers using registration attempt data and finds that they are few and far between. The paper also shows that it is not uncommon for community college students to register for classes well after their designated time, perhaps as a result of a weaker commitment to their education or a consequence of the difficulty of navigating the registration process. Understanding the reasons why students delay registration is especially important if course scarcity impacts student outcomes, as delays affect the degree to which students experience scarcity. This paper presents an innovative method for circumventing the selection bias in registration time which may prove useful in future work with similar data.

Finally, findings from this study can speak to the documented downward trend in bachelor's degree completion rates conditional on some college, and the upward trend in time to degree, even as there has been an overall increase in the number of students attending post-secondary institutions (Bound and Turner, 2007; Bound, Lovenheim, and Turner, 2010, 2012). These phenomena have been concentrated among students enrolling in non-selective two-year and four-year schools, and the literature has suggested disparities in resources per student between selective and non-selective schools as a possible explanation.

2 Historical and Institutional Background

The study uses administrative data from De Anza Community College, a large two year college which is part of the California Community College system, the largest higher education system in the United States (see Fairlie, Hoffmann, and Oreopoulos, 2014, for more details). The college has an average total enrollment of approximately 23,000 students per year and costs about $3000 per year for a full time student. Yearly tuition is higher than the average two year school ($1,269), yet is much lower than public four year colleges ($9,230) (Deming, Goldin, and Katz, 2012, Table 2, page 156). The college operates on a quarter system, although the summer term is optional.

De Anza offers a particularly useful setting for examining the impact of course shutouts. For one, community colleges are an important sector of the higher education landscape in California and nationally. In California, nearly half of all students attending a four year college previously attended a community college.[6] Furthermore, transfers from California community colleges to the California State University (CSU) system were projected to increase by 25% from 2010 to 2020 (Wilson, Newell, and Fuller, 2010). Thus, two year schools are an increasingly vital step in the production of labor market skills.

Most pertinent to this study, De Anza is a likely setting for observing course scarcity due to non-selective admissions, low tuition, small class sizes, and the budgetary pressures of the recession. The data includes the years during the Great Recession, when California community colleges decreased the size of their staff by 8% due to budget shortfalls (Bohn, Reyes, and Johnson, 2013). According to the Public Policy Institute of California, 88% of senior community college administrators surveyed in 2012 agreed that funding reductions were harmful for maintaining course offerings (Bohn, Reyes, and Johnson, 2013).

Meanwhile, like all community colleges in California, De Anza has an open enrollment policy; anyone with a high school diploma or equivalent is automatically admitted. Not all open enrollment settings will automatically lead to scarcity. A college could respond to scarcity in realtime by creating additional sections if they observe excess demand during the registration period. However, both empirical evidence and anecdotal evidence from De Anza administrators offer little support for this type of dynamic course creation. There were no sections in the data where the first student enrolled a few days after a different section of the same course filled up. In addition, the marginal cost of adding a section is non-trivial. According to De Anza's salary schedule, most instructors are paid between $7,500 and $9,000

[6] See U.S. Department of Education (2017); CCCCO; and Sengupta and Jepsen (2006).

to teach an additional section. This figure does not factor in any costs or constraints from classroom space or equipment, any increase in fringe benefit costs, or the difficulty of hiring in a part of the state with consistently lower-than-average unemployment rates. The actual marginal cost is likely more expensive.

2.1 Data Sources

This study benefits from access to community college institutional records and data from the NSC. Data from the college includes registration attempt logs, student demographic characteristics and student-level transcript records. Students in the sample enrolled at the school between the fall quarter of 2002 and the spring quarter of 2010. Students are linked to their transcripts which record grades and credits for every course offered by the college during the sample period. In addition, internal data on associates degrees, vocational degrees, and certificates are available through the summer of 2010.

Especially important for the analysis, detailed logs document each registration attempt during a term's registration period. An enrollment attempt is identified by student ID, time (with precision to the second) and course section. For each attempt, the logs report an outcome that can take one of four values: enrolled in the section, placed on a waitlist, dropped from the section, or no change. The difficulty of obtaining data of this nature has prohibited most analyses of course scarcity on a micro level.

Students are also matched to the NSC through summer of 2016, which records enrollment at most postsecondary institutions in the United States. The NSC also provides limited data on degrees earned, supplementing administrative records from De Anza. While enrollment information from the NSC is comprehensive, the degree data has less coverage. Thus, the analysis focuses on enrollment patterns and two-year degrees, as it is restricted in what it can say about bachelors degree attainment.

2.2 Section Enrollment

The online registration process takes place one or two months before the term begins. It is governed by an automated system and students are given one of seven enrollment priority designation dates, upon which they are granted access to the registration system. Registration priority is primarily determined by credit accumulation, although some students are assigned special priority if they are an athlete, a veteran, or are involved with the Extended Opportunities Programs and Services (a service for at-risk students). The registration priority assignment rules should generate discontinuous changes in the time that students sign up for courses, independently of any waitlist effects, therefore all analysis is done within registration priority (and special student) categories.

When a given student searches for a desired section (eg. MWF 9-10AM) of a desired course (eg. ECON 101 Principles of Microeconomics), she is informed of the location, instructor and the available number of seats for that particular section. Students can sign up for a maximum of 21.5 credits at one time, or about 7 courses. If there are no seats available, the system displays the number of other students on the waitlist.

There are a few rules governing the waitlist process. Students on a waitlist for one section of a course are not allowed to register for the waitlist of other sections of the same course, and cannot register for sections of other courses that meet at the same time. According to current policies, if a seat opens up in a section during the registration period, waitlisted students are automatically enrolled in the section. While archived records of the waitlist policy are available going back to 2008, anecdotes about the policy before 2008 suggest that when students on the waitlist were notified of an opening, they were given 24 hours to enroll. If they did not enroll in 24 hours, then the next student on the waitlist could claim the spot. Results are robust to restricting the analysis to attempts between 2008-2010.

The analysis focuses on registration attempts before the term begins. After the term begins, instructors have more discretion over enrollment and often make enrollment conditional on attendance. The first stage estimates the impact of missing a waitlist cutoff on enrollment in the waitlisted section. Enrollment here is defined as being enrolled on the last day of the registration period, prior to the start of classes. Many of the outcomes concern enrollment patterns as well. For these, enrollment is defined as ever being enrolled according to transcript records.

2.3 Sample Characteristics

Students are part of the sample if they registered for a course during the registration period between Fall 2002 and Spring 2010. Community colleges serve a wide variety of people, including students hoping to transfer to four year schools, those completing a vocational degree, and those taking a recreational course. Therefore, the analysis imposes additional restrictions to be more comparable to previous studies on course scarcity. The sample is limited to students who fit the profile of a "traditional" community college student. That is, a student attempting to get a two year associates degree or transfer to a four year institution, and for whom enrolling in a bachelors program in a four-year institution could be considered a reasonable substitute. Upon enrolling, students are asked to declare their educational goal or intention. Appendix Table A1 lists all of the categories a student can choose from in declaring their intention. The sample includes all students who declare an intention to transfer to four year schools, earn a two year degree, or who are undecided (options A, B, C, or M in the table). Finally, the estimates exclude registration attempts in the optional summer term.[7]

In addition, the analysis focuses on the first waitlist a student is ever signed up for in order to avoid dynamic RD issues. Indeed, the analysis is explicitly testing the hypothesis that missing a waitlist cutoff influences whether a student appears in a subsequent semester. However, results are robust to including all waitlists and clustering standard errors at the student level.

Table 1 reports summary statistics at the section, student and registration attempt

[7]The summer term lasts between 6 and 8 weeks depending on the course. The other terms are about 3 months long. Far fewer students enroll during the summer term.

levels. Column (1) of the top panel shows that just under half of all sections were ever oversubscribed. This statistic masks differences across subject areas. 68% of all sections in science, technology, engineering, and math (STEM) courses are oversubscribed during the registration period, compared with 50% of arts & humanities sections, 60% of social science courses, and only 30% of sections for vocational courses.[8] For classes that were oversubscribed, the average waiting list had about nine students still on it at the end of the registration period. Column (2) of Panel A shows the subject breakdown for all course sections included in the analysis (which by definition all have waiting lists). Courses are included if a traditional student is waitlisted, and if it is that student's first waitlist. 32% of sections included in the analysis were in STEM fields, 28% where in arts and humanities, 13% were social science courses, and 27% were vocational courses. Average waitlist lengths at the end of the registration period for sections in the analysis were slightly lower, at 7.97 students.

Panel B shows descriptive statistics for students in the analysis compared to the U.S. average. In total, the sample contains registration attempts from 4,258 unique students. Column (1) reports student-weighted demographics for all two-year colleges in the United States from the Integrated Postsecondary Education Data System (IPEDS). Column (2) contains information for all students who ever enrolled or attempted to enroll at De Anza community college during the sample period, while Column (3) reports the characteristics for students included in the analysis sample. De Anza serves slightly more women than men, though the ratio is not higher than the national average. The ethnic breakdown reflects the demographics of the Bay Area: in Column (2), 46% of students are Asian and 29% are White, while Black and Hispanic students make up only 21% of the student body. Relative to the national average, De Anza students are less likely to be underrepresented minorities and less likely to receive financial aid.

Column (3) shows the analysis sample is more likely to receive financial aid and is younger.

[8]These statistics are calculated before imposing any sample restrictions. STEM definitions come from the National Science Foundation.

Students in Column (3) take an average of 3.43 courses in the first observed term relative to the population average of 1.57. Finally, on average, in-sample students appear on 1.12 waiting lists during the advanced registration period in their first term. This does not reflect how many times each student's waitlist position is within the bandwidth of the FRD analysis, but is an upper bound on the number of times a student may be in the analysis. Among all De Anza students who attempt to register during the advanced registration period, the average number of waitlists in the first observed term is 1.36.

De Anza students as a whole are thus more likely to sign up for a waitlist but enroll in fewer courses. This is consistent with the restrictions on students' educational goals, which select students with an intention to transfer or earn a two-year degree. Students who did not declare this interest are probably less attached students or students taking recreational courses. They may perceive the opportunity cost of signing up for a waitlist as lower, and therefore sign up for more waitlists, while taking fewer courses overall.

3 Empirical Strategy

The analysis employs a fuzzy regression discontinuity design using reconstructed waiting list queues as a running variable. While the decision to sign up for a waiting list is clearly endogenous, it is difficult to anticipate how many spots will open for any given section, and therefore how deep into the queue offers will be extended. This makes the cutoff difficult to manipulate.

3.1 Construction of the Running Variable

Conceptually, the running variable represents the number of spots that would have needed to open up in order for a student to have the opportunity to enroll during the registration period, assuming she never dropped out of the queue. It must encapsulate several features of real-life waitlist behavior, such as the possibility of two people having the same position in the same waitlist at different points in time, or the possibility that a student signs up and then drops out of the queue. It's even possible for somebody to sign up chronologically later than somebody else, but have a smaller initial waitlist position. For example, person A could sign up in position ten, then five people could drop out, and person B would then be in position six if she signed up. To account for these features, students are assigned a running variable if they sign up for the queue, and remain in the analysis even if they drop out of the queue later. The ultimate running variable position is a function of the initial waiting list position, and the number of people who have exited the section or section waiting list.

Figure 1 shows a hypothetical enrollment log to illustrate the running variable construction. The first column, P_i is a student identifier that represents the chronological order in which students initially sign up for any section or section waiting list. A student who enrolls in a section without ever having been on a waiting list also has a position P_i. However, X_i, the initial waitlist position, is only defined for students who enter a waiting list queue. In Figure 1, $X_{42} = 1$, as student 42 is first on the waitlist when she signs up and similarly, $X_{43} = 2$ and $X_{44} = 3$. Importantly, the initial waitlist position is *not* the same as the running variable. Rather, the running variable for student i also involves the number of students who registered before student i and dropped out after student i (as long as it was during the registration period).

The number of students who sign up for the section before student i and drop out of the section (or section waiting list) after student i is denoted by D_i. In Figure 1, both student 7 and student 22 enrolled before students 42, 43, and 44, and dropped after these students entered the waitlist, therefore D_{42}, D_{43} and D_{44} all equal two. Although student 38 also dropped out of the queue, this occurred before students 42, 43, and 44 signed up for the waitlist and therefore student 38 has no effect on D_{42}, D_{43} or D_{44}. Essentially, D_i counts the types of drops that would move a student up on the waitlist or create a spot for her in the section.

The running variable, RV_i is defined as the difference between one's initial waitlist position and the number of drops, D_i,

$$RV_i = X_i - D_i. \tag{1}$$

Students with a strictly positive running variable would not have had the opportunity to enroll in the section during the registration period. Students with running variables less than or equal to zero would have had an opportunity to enroll, conditional on staying in the queue. A student can only influence her own running variable by signing up, not by dropping out. For example, although student 44 eventually dropped off of the waitlist, she still received a running variable. This paper compares the outcomes of students who just made the waitlist cutoff, that is students with $RV_i = 0$, to those who just missed it, or students with $RV_i = 1$.

This running variable construction is preferred to other possible definitions because it preserves the order in which students sign up for the waitlist. For example, suppose student A signs up and observes a waiting list that is two people long (he is in third position), and student B signs up the next day, but in the interim two people have dropped out of the class. Student B would be in the second position, but student B's running variable as defined above could not be smaller than student A's. A running variable based on the time that students sign up would also have this order preserving feature, however, the construction of a cutoff time is not obvious. Appendix B tests the robustness of the results to a time-based running variable; the findings remain similar.

Of course, students continue to enroll and drop after the registration period ends. The analysis does not include these attempts because there is a larger role for instructor discretion once the quarter begins. There is imperfect compliance since students can drop out of the queue, and instructors can admit students after the registration period ends without regard to the waitlist position. Therefore, students with $RV \leq 0$ might not actually enroll in the section, and those with $RV > 0$ might eventually enroll. Thus, estimates use a fuzzy RD design as opposed to a sharp RD.

3.2 Estimation

Consider a student who placed herself on a waitlist. $NotEnroll_{ist}$ indicates the treatment, and is one if the student does not enroll in her desired section s in term t during the registration period, and zero otherwise. This is our measure of rationing. Let $Y_i(NotEnroll_{ist} = 1)$ be her educational outcome if she does not enroll in her preferred section and $Y(NotEnroll_{ist} = 0)$ be her educational outcome if she does. The analysis estimates $\mathbb{E}[Y(NotEnroll_{ist} = 1) - Y(NotEnroll_{ist} = 0) \mid RV_{ist} = 1]$. This is interpreted as the local average treatment effect (LATE) for compliers, that is, students who are rationed out of a section if they miss the cutoff, or are induced to enroll if they make the cutoff. It is important to consider the type of student represented by a complier in this scenario.

To estimate the LATE, we use a two stage least squares regression for students within one position of the waitlist cutoff. That is, for student i in section s and term t with $RV_{ist} \in [0, 1]$:

$$NotEnroll_{ist} = \alpha_0 + \alpha_1 MissWL_{ist} + \mathbf{X_{ist}}'\Gamma + \delta_t + \zeta_{ist} \tag{2}$$

$$Y_{ist} = \beta_0 + \beta_1 Not\hat{E}nroll_{ist} + \mathbf{X_{ist}}'\Pi + \delta_t + \epsilon_{ist} \tag{3}$$

where $Not\hat{E}nroll_{ist}$ represents the student's predicted probability of not enrolling in the section according to equation 2. Enrollment for the first-stage equation is measured on the last day of the advanced registration period, prior to the start of classes. RV_{ist} is the running variable, and $MissWL_{ist}$ is an indicator equal to one if $RV_{ist} = 1$ and equal to zero otherwise. $\mathbf{X_{ist}}$ is a vector of covariates including gender, race, ethnicity, US citizenship status, age, financial aid receipt, registration priority, special admit or special program status, an indicator for whether the course is a recreational course, an indicator for having the lowest registration priority, and indicators for missing covariates. The δ_t represent a vector of term by year fixed effects and ζ_{ist} and ϵ_{ist} are error terms.

The estimates rely on local randomization assumptions to identify the causal effect of not enrolling in a desired section due to oversubscription, for compliers (for a detailed description of local randomization see Cattaneo, Titiunik, and Vasquez-Bare, 2017; Cattaneo, Idrobo, and Titiunik, Forthcoming). Essentially, local randomization assumes that within one position on either side of the waitlist cutoff, the running variable is unrelated to potential outcomes. That is, assignment of the running variable is "as-if random," and there is no selection into treatment. The full set of assumptions include

1. *Fixed Potential Outcomes.* Potential outcomes are non-random and fixed for students within one position the cutoff.

2. *Known randomization mechanism.* The distribution of the treatment assignment vector is known for those within one position of the cutoff.

3. *Unconfoundedness.* Whether students end up directly on the right or left of the cutoff does not depend on potential outcomes.

4. *Exclusion Restriction.* Within one position of the cutoff, the running variable influences outcomes only through treatment, not directly.

5. *SUTVA.* Locally, within one position of the cutoff, each student's potential outcomes only depend on his or her own treatment assignment, and not anybody else's.

6. *Monotonicity.* Within one position of the cutoff, missing the cutoff does not cause any students to be more likely to enroll than they otherwise would have been, and making the cutoff does not cause any students to be less likely to enroll.

These assumptions are the same as the assumptions for an instrumental variables strategy, with the exception that they only hold locally, for observations in the narrowest window around the waitlist cutoff. Local randomization is most appropriate for settings with extremely discrete running variables, as opposed to the more commonly used RD assumptions involving continuity of the regression function (this would require a continuous running variable).

For more discussion on local randomization versus assumptions based on continuity of the conditional regression functions of the potential outcomes given the running variable see Cattaneo, Idrobo, and Titiunik (Forthcoming).

Assumption one and two define what is meant by random. Assumption one means that a student's potential outcomes are fixed and inherent to her.[9]

Assumption three is the key to local randomization and has some testable implications. Any manipulation of a student's own running variable would violate this assumption. However, a student's running variable is dependent on the number of other students who drop the section, and is likely out of his or her control. An example of a violation of the assumption is if a student is more likely to sign up for the waitlist because she knows that a friend is planning to drop. This seems unlikely, particularly for incoming students who don't know many people (the majority of the sample). Section 3.3 formally tests for manipulation around the cutoff by looking at covariate balance.

Assumption four, the exclusion restriction, is generally not needed in RD studies that rely on continuity of the conditional regression function, and indeed, it would be unreasonable to assume that there is no direct relationship between the running variable and the potential outcomes for all values of the running variable. Clearly, somebody who signed up for a section very early in the registration period is different from somebody who signed up very late. However, it is more plausible that there is no difference, on average, between people within one waitlist position of each other.

The stable unit treatment value assumption (SUTVA) is standard in estimating LATE using an instrumental variable, though of course it's possible that there are spillovers from other students. Again, one mitigating factor for these possible spillovers is that most students are first-time enrollees and likely do not know each other well. The monotonicity assumption is also standard. Since signing up for a waitlist has a cost (students are barred from signing

[9]There is a formulation of the local randomization assumptions for potential outcomes that are random variables as well, but it would not change anything in the mechanics of estimating the LATE parameter (Cattaneo, Titiunik, and Vasquez-Bare, 2017).

up for any other section at the same time or for the same course), it is implausible that being high enough on the waitlist to gain admission would cause a student to be less likely to sign up for a course than they otherwise would have been. Being more likely to sign up for a course because one missed the waitlist cutoff is also intuitively unlikely, though not testable.

Equations (2) and (3) are estimated using a two-stage least squares regression. Lee and Card (2008) suggest clustering standard errors by the value of the running variable when the running variable is discrete. However, Kolesar and Rothe (2016) point out that confidence intervals constructed in this way have poor coverage when the number of clusters is small, which is the case in this analysis. Therefore, only the usual heteroskedasticity robust standard errors are used, unless otherwise noted (e.g. examining a sample where a student appears on multiple waitlists, in which case standard errors are clustered at the student-level).

3.3 Validity Checks

One can test for manipulation of the running variable by checking for smoothness in the density of the running variable at the cutoff. Figure 2 shows the density of the running variable. Table 2 reports p-values from formal tests for smoothness using a McCrary-like test specifically designed for discrete running variables, and introduced in Frandsen (2017). The results show no evidence of manipulation. An important assumption of the Frandsen (2017) test is that the second order finite difference of the running variable's probability mass function (pmf) is bounded at zero, with the bound represented by k. Intuitively, k represents the amount of curvature or nonlinearity in the pmf of the running variable that would still be compatible with no manipulation. The choice of k is left to the researcher, but a natural maximum is the amount of curvature in a discretized normal distribution that is roughly as discrete as the observed distribution of the running variable (call this the "rule of thumb" maximum). The density test fails to reject the null of no manipulation for values of k that are much smaller than the rule of thumb maximum of 0.73.

Another testable implication of the FRD assumptions is that predetermined characteristics should be balanced across the waitlist cutoff. Figure 3 plots the average, conditional on the running variable, for four student characteristics: an indicator for whether the student is female, Asian, international, or whether the student has ever received financial aid. The size of the dots represents the relative number of observations. The analysis focuses on the students with a running variable of zero or one (the last person to make the waitlist cutoff and the first person to miss the cutoff, respectively). Upon visual inspection, the averages of the characteristics in Figure 3 appear to be similar across the cutoff. Appendix C includes similar pictures for the other covariates used in the analysis, and these look broadly similar.

Table 3 reports the results of linear regressions testing for imbalance across the waitlist cutoff in student characteristics. Age is significantly different across the threshold at the five percent level, though the difference is small in magnitude (less than five months). Two characteristics are different at the ten percent level: the share Hispanic and the share missing gender information. However, these difference are small in magnitude and not meaningful in an economic sense. The covariates are not jointly significant, with a joint F-test yielding a p-value of 0.16.

4 Course Scarcity and Student Outcomes

4.1 First Stage Estimates

The first stage estimates can be easily seen in registration discontinuities at the cutoff. Figure 4a shows a discontinuity at the waitlist cutoff for enrollment in the waitlisted section. About 64.1% of students who do not miss the waitlist cutoff enroll in their desired section. In accordance with the definition of the running variable, students who miss the waitlist cutoff are not able to enroll during the advanced registration period. Figure 4b shows the enrollment rates for courses in which a student has been waitlisted for one section. Due to the rules about only being able to enroll in one waitlist per course, the first stage looks almost identical. In theory, somebody on the left of the cutoff could have switched sections within the same course. This does not appear to happen often, as 65.4% of students who do not miss a cutoff ultimately enroll in the waitlisted course, an increase of 1.3 percentage points relative to those who enroll in the waitlisted section.

It is important to verify that the first stage effect of missing a waitlist cutoff is large enough to avoid a weak instruments problem. Table 4 examines sensitivity of the first stage to the inclusion of covariates for both enrollment in the desired section (Panel A) and the desired course (Panel B), and reports F-statistics. The F-statistics are all greater than 3500 regardless of whether covariates are included and whether examining enrollment in the waitlisted section or course. Students who miss the waitlist are between 64.1 and 64.4 percentage points less likely to enroll in their desired section than those who just make it. The barrier to entry for a section translates into a barrier at the course level. In Panel B, students are between 64.5 and 64.8 percentage points less likely to enroll in their desired course after missing the waitlist cutoff.

Although estimates of the first stage for section enrollment and course enrollment are qualitatively similar, all further analysis uses the section enrollment as the endogenous variable of interest, as it is most directly influenced by the waitlist cutoff.

4.2 Reduced Form and IV Estimates

The main outcomes of interest are enrollment in the concurrent term and enrollment in other two and four-year schools within two, three, four, and five years of the waitlisted term. Figures 5 and 6 plot the residuals of the main outcome variables, conditioned on the observable, pre-determined characteristics, and binned by values of the running variable. Figure 5 is the visual representation of the reduced form effects of missing a waitlist cutoff on whether students enroll in zero, one to two, or three or more courses in the waitlisted term. Enrolling in zero courses can be thought of as same-term drop-out (though the student may appear again in a later term). Enrollment in one or two courses would be like enrolling part-time, while three or more courses is roughly full-time enrollment. There is less than a 2.5 percentage point jump in same-semester dropout, less than a percentage point drop in part-time enrollment, and a 1.8 point drop in full-time enrollment. The discontinuity in same-semester dropout is more prominent.

Figure 6 shows the reduced form impact on whether the student transfers to other two-year schools within two, three, and four years of being on a waitlist. There is 1.8 percentage point rise in the share of students who transfer within two years to another two-year school for those who missed the waitlist cutoff. The difference in transfers to other two-year schools on either side of the cutoff gets smaller, after three and four years. While reduced form effects of one or two percentage points may seem small, these translate to meaningfully large effects relative to the control means. For example, only 10.6% of students transfer to another two year within two years.

Table 5 presents formal estimates of the LATE of being shut out of a course on enrollment patterns in the concurrent semester. The endogenous variable is being rationed out of a course during the advanced registration period, and the instrument is missing waitlist cutoff. Columns (1), (2), and (3) report the effect of begin shut out on whether a student enrolls in zero, one to two, or three or more courses, respectively. All results control for the full vector of covariates and use a bandwidth of one.

The main results show students are 3.7 percentage points more likely to "drop out" in the waitlisted term; that is, to take no course at all that term (this is not permanent dropout, students may appear on-campus in later terms). The rise in same-semester drop-out is accompanied by a 2.4 percentage point, non-significant decrease in the probability of enrolling in three or more classes (a full course load), relative to a control complier mean of 68%. There is also a non-significant 1.3 percentage point decrease in the likelihood of enrolling in one to two courses that term relative to a control complier mean of 26%. These results cannot distinguish between a cascading effect (such as somebody who would otherwise have taken three courses dropping down to two and somebody who would have taken two, dropping down to one, and so on) and a more dramatic shift from a plan to take a full course load to taking no courses, or some combination of these two options.

The estimated increase in same-semester dropout is an increase of 58% relative to the same-semester dropout rate of the control compliers, which is 6.4%. There are no detectable subgroup differences in enrollment patterns after missing a waitlist cutoff.

Table 6 shows the effect of course shutouts on transfer rates and degree completion for associates degrees, certificates, and bachelors degrees. There is a large positive effect of 3.6 percentage points on the transfer rate to other two years within two years of missing the waitlist cutoff. This is relative to a control complier mean of 11%, which means the transfer rate increases by 33%. The point estimates for transfers to other two year schools within three, four, and five years are also relatively large (2.6, 2.5, and 2.6 percentage points, respectively), but not significant. It suggests the effect attenuates but might not entirely dissipate over time. There are no detectable effects on transfers to four year schools or on the share who earn associates degrees or certificates from De Anza, or bachelors degrees up to five years out.

In general, the three most frequent recipients of De Anza's transfer students are: Foothills College, Evergreen Valley College, and San Jose City College. These are roughly 15 minutes, 30 minutes, and 18 minutes from De Anza by car, respectively. Foothills college in particular, is almost seamlessly integrated, with cross-registration between De Anza and Foothills being common and easy to do because it uses the same registration system. However, when estimating the treatment effect on attending each of these alternative schools separately (see Appendix A2), there is a statistically significant increase in enrollment at Evergreen, San Jose City College, and all other two-year schools, but not at Foothills. It's likely that students consider classes at Foothills as part of the initial choice set when they are registering, and not as a back-up option after the fact.

4.3 Subgroup Analysis

This section reports results by subgroup categories, including differential impacts by gender, ethnicity, popularity of the course, and course subject. The latter two categories are meant to test the idea that all courses are not equally important to a student's labor market goals. The demographic breakdowns are proxies for student vulnerability or disadvantage. The ethnic categories in particular are not taken to have theoretical meaning in their own right, but are rather meant to serve as rough correlates of unobservable characteristics such as the human capital of a student's social network or other barriers to human capital accumulation.

Appendix Tables A3 to A6 show the differential effects on course enrollment in the concurrent term for all subgroups. There are no detectable differential impacts on enrollment patterns by demographic subgroups. In some cases the null is quite precisely estimated. For example, the difference in same semester dropout between men and women is not significant with a p-value of 0.99.

There is more evidence that the type of course may be important for enrollment patterns. To gauge the popularity of the course, we tallied enrollment requests for all courses and picked the top five most requested with the rationale that more popular courses are likely to be important pre-requisites for common majors or for transfer. The top five include three introductory writing courses, a government course, and a psychology course. The point estimates for same semester drop-out are larger for the top five most popular classes, although these are not significantly different. In addition, being rationed out of a top five class seems to lead students to either drop out or increase their enrollment to full time, with a significantly larger drop in part-time enrollment. Waitlists for other classes cause relatively larger decreases in full-time enrollment instead. This suggests that students enrolled in the most popular classes are relatively less attached to college. Differences in impact by subject matter, however, are minimal.

Interesting dynamics emerge in transfer and degree completion by ethnic categories.

Appendix Tables A7 and A8 report results on transfer rates to other colleges by ethnicity, where students are partitioned into three groups: Asian, White, and underrepresented minority (URM). The URM category consists of Black, Hispanic, Native American, or multi-racial students, or students who do not fit into any other category. The point estimates are plotted in Figures 9a and 9b.

Figures 9a and 9b show a divergence in transfer responses. Although all students show a positive uptick in transfer rates to other two year schools within two years of the waitlist, the point estimates are highest for URM students in Figure 9a. For these students, transfers to two year schools persist at a high level three, four, and five years out. Meanwhile, other students do not transfer to two year schools at an appreciably high rate.

In contrast, Asian students are more likely to transfer to a four year school in response to being rationed out of a course, as shown in Figure 9b, while URM students become increasingly less likely to transfer to a four year school as time goes on. With Asian students accelerating their transfer to a four-year school, there should be a corresponding uptick in bachelors degree completion for Asian students. Indeed, Figure 10 shows a positive effect of rationing on bachelors degree completion among Asian students, especially at the five year mark. There is no impact on bachelors degree completion for URM students, although the control complier mean for this group is near zero for the first four years after the waitlist, and still quite low at about 6% in the fifth year out. Finally, there is evidence that bachelors degree attainment among White students is hampered by course rationing. Being rationed out of a course reduces bachelors degree completion within five years by over 50% for White students, relative to a control complier mean of 13%. The estimates plotted in Figure 10 can be found in Appendix Table A9.

4.4 Sensitivity Analysis

This section shows results for a standard validity check in FRD analysis, whether there are treatment effects at placebo thresholds.[10] This section also reports the sensitivity of results to different sample restrictions.

Figure 7 plots the coefficients for the two outcomes affected by course shutouts, estimated for nine different waitlist thresholds. The outcomes are: took zero courses in the waitlisted term, and transferred to another two year within two years. Appendix Table A11 reports the corresponding point estimates and standard errors represented in the figure. The true cutoff represents the last student on the waitlist who received an offer of admission to the section. For each placebo cutoff j, students with $RV_{ist} = j$ behave as the control group and are compared to students directly to the right, with $RV_{ist} = j + 1$. The difference in outcomes at any cutoff $j \neq 0$ should not be significantly different from zero, which is the case.

Table A10 shows the LATE of a course shutout on selected outcomes using different samples of students. Results are robust to alternative sample restrictions. Column (1) includes all students, regardless of which initial intention they declared, and all waitlists. This examines whether estimates are sensitive to conditioning on students' initial declared intentions (listed in Appendix Table A1). Column (2) restricts the sample only to students who declared an intention to transfer to a four year. Column (3) demonstrates that results are robust to only including terms after 2008, when documentation on enrollment rules is available (see section 2.2 for a discussion of the issue).

The estimates on taking zero courses in the waitlisted term are still positive and significant, though the magnitudes are smaller in the first two columns, and larger in the last. The decrease in the share taking a full course load, however, are all larger. The samples include

[10] An FRD that relied on continuity assumptions might also check for sensitivity to bandwidth choices and controlling for different polynomials of the running variable. However, with local randomization, the identification is only valid within one position around the cutoff, so bandwidth and functions of the running variable are not relevant.

many more continuing and returning students in Column (1) and (2), and students who had other goals upon enrolling in community college (such as recreation) in Column (1); these students appear slightly less likely to sit out the term. The impact on same-semester drop-out is potentially stronger post 2007, which could be related to more overcrowding during those years. Overall, the magnitudes are quite similar. The main difference is that the sign flips on the share enrolled part-time (taking one or two courses) in columns (1) and (2). This indicates a larger role for shifting from full to part-time enrollment among some students rather than sitting out the term.

4.5 Complier Densities

This section estimates outcome densities for treated and untreated compliers in order to better understand how enrollment patterns change. Following Abdulkadiroglu, Pathak, and Walters (2018), we estimate kernel densities of the form

$$\frac{1}{h}K\left(\frac{Y_{ist}-y}{h}\right) \times NotEnroll_{ist} = \tau_y NotEnroll_{ist} + \mathbf{X}_i'\lambda_y + v_{iy} \tag{4}$$

where $Y_i(0)$ and $Y_i(1)$ are potential outcomes, and failing to enroll in the desired course section (being shut out) is the treatment. We use a Gaussian kernel for $K(u)$, and Silverman's rule of thumb for h, the bandwidth (Silverman, 1986). The instrument for treatment is missing the waitlist cutoff. The 2SLS estimate of τ_y is a consistent estimate of the density of $Y_{ist}(1)$, evaluated at y. Likewise, by substituting $Enroll_{ist} = 1 - NotEnroll_{ist}$ in equation (4), the equivalent of the 2SLS coefficient, τ_y, is a consistent estimate of the density of $Y_{ist}(0)$ evaluated at y. Densities are evaluated on a grid of 100 points. For more examples and discussion of estimating complier densities, see Angrist et al. (2016); Walters (Forthcoming).

Figure 8 (a) shows the complier densities for the number of courses a student is ever enrolled in according to transcripts from the waitlisted term. Figure 8 (b) shows the densities for the time it takes students to earn an associate's degree, certificate, or bachelor's degree.

Students who do not earn a degree within five years are coded as receiving a degree in six or more. The red dashed line represents the density for compliers who miss the cutoff; these students are shut out of their desired section. The blue solid line shows the estimated density for compliers who do not miss a cutoff; these students represent the counterfactual, business as usual for students who are not rationed out of the section they want. They are enrolled during the advanced registration period. There is a shift to the left in the distribution of the number of courses a student takes for students who get shut out of a course, though a small minority does seem to respond by taking even more courses, perhaps to compensate. A heterogeneous response would make it more difficult to detect an average impact on the share of students taking a full course-load, which is demonstrated by the vertical lines representing average number of courses. These are basically superimposed.

The plot for time to degree reveals that very few compliers earn any type of degree. While the average differences are too small to detect, the potential outcome densities do reveal more more nuance. There is slightly less mass at four years, and slightly more mass at five and six for students shut out of a course, which means a small share of compliers may take longer to earn a degree or not earn a degree after being shut out of a course. While the magnitudes are small and not statistically detectable, this is suggestive that further investigation is necessary on long-term outcomes.

5 Conclusion

This paper studies the effect of course scarcity in a setting with open enrollment, during a time when California had especially high enrollment and budget shortfalls. The analysis measures course scarcity by using cutoffs in waitlist queues which discontinuously change the probability of enrolling in a desired section. Comparing students that just miss the waitlist cutoff to those who just make it, we find that students who are not able to enroll in their preferred section due to oversubscription are 3.7 percentage points less likely to take any courses that term. At the same time, missing a waitlist cutoff causes a corresponding 3.6 percentage point increase in the share of students who transfer to other two-year schools. This could signal substitution behavior to try to earn the credits associated with the waitlisted course. These effects are large relative to the control complier means. 6.4% of control compliers dropped out in the waitlisted semester, and 11% transferred to another two year within two years. Therefore, the results represent a 58% increase in same-semester dropout and a 33% increase in transfers to other two-year colleges.

The 2SLS results show that a course shutout induces an increase in enrollment at two common recipients of De Anza transfers: Evergreen Valley College, and San Jose City College. According to the U.S. Department of Education's College Scorecard website, De Anza community college costs less, has a higher graduation rate, and students who attend De Anza earn higher average salaries after attending than attendees at the other two colleges. Online ranking services such as NICHE and Wallethub, also consistently rank De Anza above the other two common substitutes. Both by observable characteristics of the schools, and by revealed preference, it is likely that students are worse off from having to substitute for the courses they need at these common alternatives.

While there are no average impacts of course rationing on transfers to four year schools or bachelors degree attainment, there is evidence of diverging impacts by ethnicity. For Asian students, facing rationing leads to an accelerated rate of transfer to a four year college. URM students are more likely to continue in other two-year schools and if anything, become less likely to transfer to a four year as time goes on. White students seem to delay their transfer to a four year. These patterns show up again in bachelors degree completion, with Asian students reacting to rationing by earning a bachelor's degree sooner than they otherwise would have, and White students earning their degree later. URM students are earning degrees at such a low rate within five years of the waitlist that they exhibit a floor-effect (they can't do any worse). While these subgroup differences are puzzling, anecdotal evidence suggests that there are potentially two streams of students using the community college as a vehicle to access four-year schools.

The first type of student can't access a four-year initially, and uses the community college to build their skills in a stepping-stone fashion. This represents the traditional picture of how community colleges are thought to function. However, there could be a group of very positively selected students who actually could have enrolled in a four-year school initially, but instead choose to start in a two-year setting. This could be because they can complete their core courses at a lower tuition rate, or because it may be less competitive to access a selective University of California campus by transferring from a two-year rather than applying directly out of high school. Whatever the case, a positively selected student who faces rationing may become frustrated with the resource-constraints of a two-year setting and abandon his initial plans to start in a community college, leading him to transfer to a four-year sooner. We postulate that ethnicity serves as a proxy for students' ability to navigate the higher education system and allows the analysis to identify these types of effects. Finding these different responses is consistent with prior literature that worries about diverting students from selective four-year schools to two-year schools or less selective four-year schools by heavily subsidizing these options (see for example Cohodes and Goodman, 2014).

The results of our analysis contrast with the findings in Kurlaender et al. (2014), which did not document any detectable effects of course shutouts. One possible reason for this contrast is the setting of a community college with open enrollment versus a relatively selective private four-year university. This also suggests that the possible effects of course scarcity may be even more pronounced than what can be measured in this study. Although the community college being studied has open enrollment, students at De Anza seem positively selected along sociodemographic characteristics relative to the national average for students at two-year schools. Scarcity effects could be more prevalent among community college students in other parts of the country with a student population that is lower income or otherwise at-risk. Underfunded community colleges are not unique to California; 46 states spent less per-student in 2016 than they did before the 2008 recession (Mitchell, Leachman, and Masterson, 2016). In light of sustained decreases in per-student funding for public colleges, future work should continue to explore the effects of course scarcity at the institution level.

In addition, we estimate the effect of missing a waitlist cutoff, *holding availability in all other sections fixed*. This could be considered a small friction, and the response to a scenario in which a large fraction of sections are eliminated at once may be very different and presumably more severe. Likewise, students likely face more than one waitlist during their studies. This paper presents a lower bound on the cumulative impact of missing multiple waitlists. The evidence of short-term behavior change is at least consistent with Bound, Lovenheim, and Turner (2010) and Deming and Walters (2017), which find aggregate impacts of decreases in funding per student. However, more work is needed to explore downstream impacts, and to shed light on how persistent rationing can affect students.

In summary, this paper provides evidence of the impact of course shutouts on educational attainment, a mechanism that was previously untestable due to data limitations. It also introduces a new method for leveraging registration logs, a data resource that has been underused to perform causal inference. Finally, we continue the work of documenting and quantifying the importance of structural differences between four year schools and two year, non-selective institutions, which disproportionately serve low-income students and students of color. In the face of unequal access to educational resources, it is more important than ever to understand the exact processes through which these disparities can lead to diverging outcomes, in order to create effective solutions.

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Table 1: Summary Statistics

Panel A: Section-level statistics

	All Sections (1)	Analysis Sections (2)
% with a WL	0.49	1.00
% STEM with WL	0.68	0.32
% Arts/Humanities with WL	0.50	0.28
% Social Sciences with WL	0.60	0.13
% Vocational with WL	0.30	0.27
WL Length	8.98	7.97
WL Length (SD)	9.15	7.04
Observations	29,614	3,499

Panel B: Student-level and registration attempt-level statistics

	US 2-year Public Colleges (1)	All DeAnza (2)	Analysis Sample (3)
Female	0.56	0.52	0.50
White	0.48	0.29	0.24
Black	0.13	0.05	0.05
Hispanic	0.23	0.15	0.15
Asian	0.06	0.46	0.43
Ever Receives Aid	0.75	0.17	0.32
Age 18-24	0.55	0.56	0.77
Age 25-64	0.35	0.4 0	0.20
# Courses, first term		1.57	3.43
# Waitlists, first term		1.36	1.12
Observations	6,284,462	189,173	4,258

Notes: Panel A presents section-level statistics for De Anza Community College between 2002 and 2010. Column (1) reports the average share of sections with waitlists, by subject, before sample restrictions. For all sections in the analysis, column (2) reports the share in each subject. By design, all sections in the analysis have a waitlist. In Panel B, column (1) describes student characteristics at all two-year colleges in the US, column (2) shows characteristics for De Anza students, and column (3) reports statistics for the students in the analysis (sample restrictions are detailed in Section 2.3). The STEM definition follows the National Science Foundation. Professional and business courses (listed in Section 2.3) are included in the vocational category. Waitlist length measures how many students remain on the waitlist at the end of the registration period for oversubscribed sections. A student is counted as receiving aid if they received it at any time in the sample period, and is calculated using using first-time, full-time undergrads for the 2-year public colleges. The number of waitlists is the total that a student signed up for during the advanced registration period in the student's first observed term. The number of courses is the number a student was enrolled in after the drop date in the first observed term. Data for all two-year public colleges in the US comes from IPEDS for Fall 2015, except for financial aid receipt which is from the 2014-2015 school year. The IPEDS data is weighted by enrollment to create student-level means.

Table 2: Frandsen Manipulation Test When Running Variable is Discrete

Nonlinearity Parameter (k) (1)	P-value (2)
0.015	0.051
0.020	0.070
0.040	0.231
0.060	0.502
0.080	0.764
0.100	0.924
0.120	0.983
0.140	0.998
0.160	1.000

Notes: This table presents results from the manipulation test proposed in (Frandsen, 2017). The parameter k, which is chosen by the researcher, represents the "maximal degree of nonlinearity in the probability mass function that is still considered to be compatible with no manipulation" (Frandsen, 2017). Column (1) reports tested values of k, which were chosen to be between zero and 0.73 (the rule of thumb maximum for a discretized normal pdf). Column (2) reports the p-value of a test of the null hypothesis that no manipulation occurred.

Table 3: Testing for Balance in Pre-determined Student Characteristics at the Cutoff

	Coefficient (1)	Standard Error (2)	P-Value (3)
White	-0.02	(0.013)	0.16
Asian	-0.00	(0.015)	0.90
Hispanic	0.02	(0.011)	0.06
Black	0.00	(0.007)	0.73
Missing Race	-0.01	(0.008)	0.28
Female	-0.02	(0.015)	0.30
Missing Female	0.00	(0.001)	0.08
Age	0.41	(0.207)	0.05
Missing Age	-0.00	(0.000)	0.32
International	0.00	(0.013)	0.97
Received Financial Aid	-0.01	(0.014)	0.40
Missing Financial Aid Receipt	0.00	(0.002)	0.60
First Time Student	0.00	(0.004)	0.80
Missing Class Standing	0.00	(0.002)	0.41
Lowest Registration Priority	0.00	(0.003)	0.45
Joint p-value			0.16
Observations (N_l/N_r)	1,977	2,281	

Notes: Each row reports results from a linear regression of the dependent variable on an indicator for missing a waitlist cutoff, for students within one position of the cutoff. The first column shows coefficients, the second column shows the robust standard error, and the third column shows the p-value. The p-value in the last row is from a chi-squared test of whether the differences in each characteristic are jointly significant.

Table 4: Effect of Missing the Cutoff on Enrollment in Waitlisted Section and Course

	(1)	(2)
Panel A: Section Enrollment		
Missed WL Cutoff	-0.641***	-0.644***
	(0.011)	(0.011)
R-squared	0.489	0.493
F-Statistic	3526	3594
Controls	N	Y
Control Mean	0.641	
Panel B: Course Enrollment		
Missed WL Cutoff	-0.645***	-0.648***
	(0.011)	(0.011)
R-squared	0.485	0.489
F-Stat	3516	3563
Controls	N	Y
Control Mean	0.654	
Observations (N_l/N_r)	1,977/2,281	

Notes: Results are from a linear regression where the dependent variable is enrollment in the waitlisted section in Panel A and enrollment in the waitlisted course in Panel B. All students are within one running variable position from the cutoff. The first column does not include controls while the second controls for race/ethnicity, gender, age, citizenship, financial aid receipt, class status (first time, continuing, returning), special student status, registration priority fixed effects, term and year fixed effects, and indicators for missing variables. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table 5: Effect of Missing the Cutoff on Enrollment in the Waitlisted Term

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
2SLS	0.037***	-0.013	-0.024	-0.017
	(0.012)	(0.019)	(0.020)	(0.021)
Reduced Form	0.024***	-0.009	-0.015	-0.011
	(0.008)	(0.012)	(0.013)	(0.013)
CCM	0.064	0.26	0.68	0.69
Observations (N_l/N_r)	1,977	2,281		

Notes: This table shows results from a 2SLS regression as in equation 3. The outcome is an indicator for whether the student took no courses in the concurrent term in Column (1), took one or two courses in Column (2), or took three or more courses in Column (3). A course is counted if the student is enrolled after the add/drop date. The outcome in column (4) is an indicator for whether the student enrolls in any classes the following major term. The standard errors are in parentheses, with the control complier means (CCM) and the reduced form displayed below. Controls include gender, race, indicators for first-time student and returning student status, US citizenship, age, special program status, financial aid receipt, an indicator for lowest registration priority, and term by year fixed effects. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table 6: Effect of Missing a Waitlist Cutoff on Transfers and Degrees

Outcome	Within 1 Year (1)	Within 2 Years (2)	Within 3 Years (3)	Within 4 Years (4)	Within 5 Years (5)
Transfer Other Two-Year	0.009	0.036**	0.026	0.025	0.026
	(0.011)	(0.015)	(0.017)	(0.019)	(0.020)
CCM	[0.06]	[0.11]	[0.15]	[0.19]	[0.22]
Reduced Form	0.006	0.023**	0.017	0.016	0.017
	(0.007)	(0.010)	(0.011)	(0.012)	(0.013)
Transfer Four-Year	0.001	0.011	0.005	0.013	0.021
	(0.008)	(0.012)	(0.017)	(0.019)	(0.020)
CCM	[0.03]	[0.06]	[0.14]	[0.19]	[0.22]
Reduced Form	0.000	0.007	0.003	0.008	0.013
	(0.005)	(0.008)	(0.011)	(0.012)	(0.013)
Certificate/ Associates	0.002	-0.003	-0.011	-0.017	-0.012
	(0.004)	(0.009)	(0.013)	(0.015)	(0.015)
CCM	[0.01]	[0.03]	[0.08]	[0.11]	[0.12]
Reduced Form	0.002	-0.002	-0.007	-0.011	-0.007
	(0.003)	(0.006)	(0.008)	(0.009)	(0.010)
Bachelors	0.004	0.002	0.007	-0.002	-0.001
	(0.003)	(0.003)	(0.006)	(0.010)	(0.014)
CCM	[0.00]	[0.01]	[0.01]	[0.04]	[0.09]
Reduced Form	0.003	0.001	0.004	-0.001	-0.000
	(0.002)	(0.002)	(0.004)	(0.007)	(0.009)
Observations (N_l/N_r)	1,977	2,281			

Notes: This table shows the coefficient on predicted enrollment from a 2SLS, local linear regression as in equation 3 with a bandwidth of one. Each coefficient comes from a different regression. Column 1 shows effects within one year of the waitlisted term, column 2 shows effects within two years, etc. We show the control complier means in square brackets below the standard errors. All columns include controls for gender, race, ethnicity, indicators for returning and continuing status, US citizenship, age, prior GPA, cumulative credits, special program status, financial aid receipt as well as registration priority fixed effects and term by year fixed effects. Standard errors are robust to heteroskedasticity and stars represent significance at the 10%, 5% and 1% level.

P_i	action	date	time	X_i	D_i	RV_i
⋮	⋮	⋮	⋮	⋮	⋮	⋮
36	enroll	Aug 1, 2004	11:00:00	-	-	-
37	enroll	Aug 1, 2004	12:00:00	-	-	-
38	enroll	Aug 1, 2004	13:00:00	-	-	-
39	enroll	Aug 1, 2004	14:00:00	-	-	-
40	enroll	Aug 1, 2004	15:00:00	-	-	-
38	drop	Aug 2, 2004	8:00:00	-	-	-
41	enroll	Aug 2, 2004	10:00:00	-	-	-
42	waitlist	Aug 2, 2004	12:00:00	1	2	-1
43	waitlist	Aug 2, 2004	13:00:00	2	2	0
44	waitlist	Aug 2, 2004	14:00:00	3	2	1
7	drop	Aug 3, 2004	20:00:00	-	-	-
42	enroll	Aug 3, 2004	21:00:00	-	-	-
22	drop	Aug 4, 2004	9:00:00	-	-	-
43	enroll	Aug 4, 2004	11:00:00	-	-	-
44	drop	Aug 4, 2004	15:00:00	-	-	-
45	waitlist	Aug 4, 2004	17:00:00	1	0	1

Figure 1: A hypothetical enrollment log. P_i is a student identifier, X_i is the initial waitlist position, D_i counts the number of students who signed up before student i signed up for the waitlist, and dropped after student i (as long as it was during the registration period). $RV_i = X_i - D_i$ is student i's running variable.

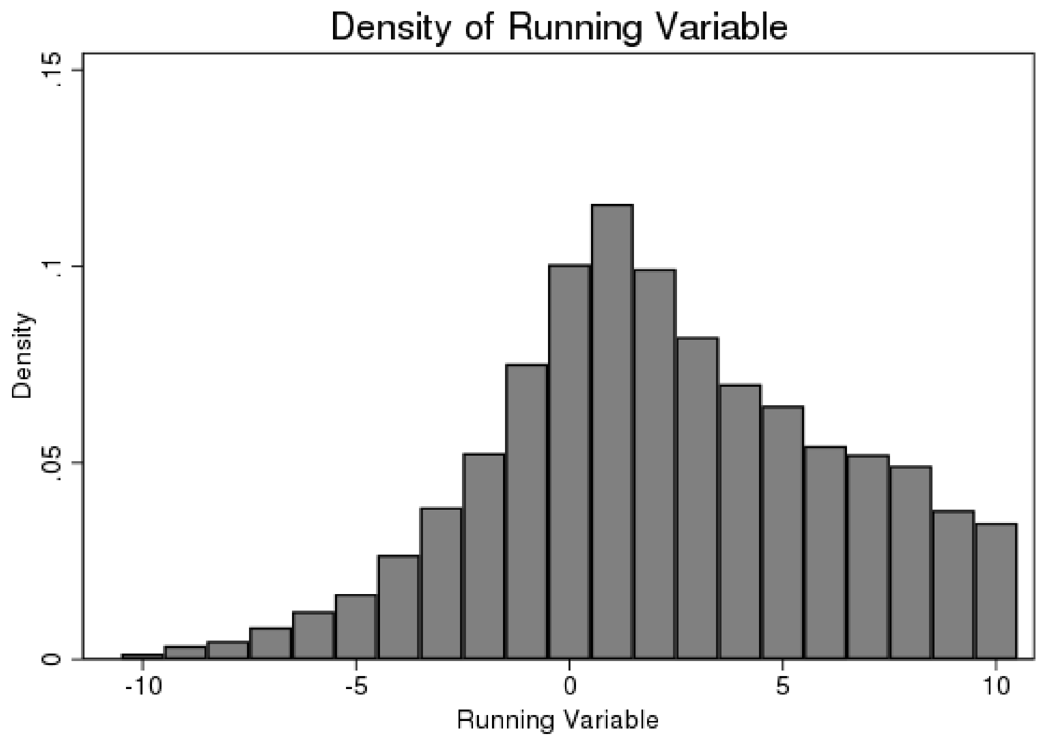
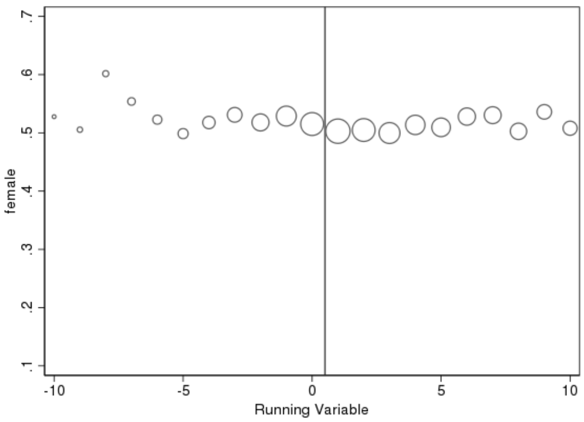
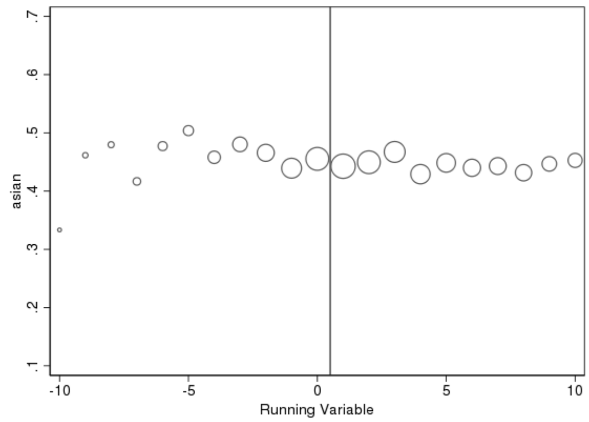


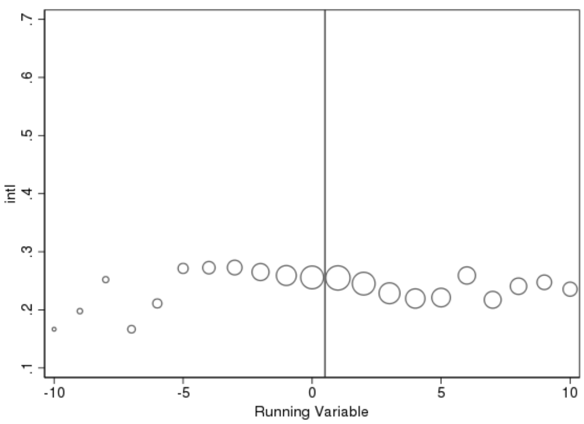
Figure 2: Density of the Running Variable



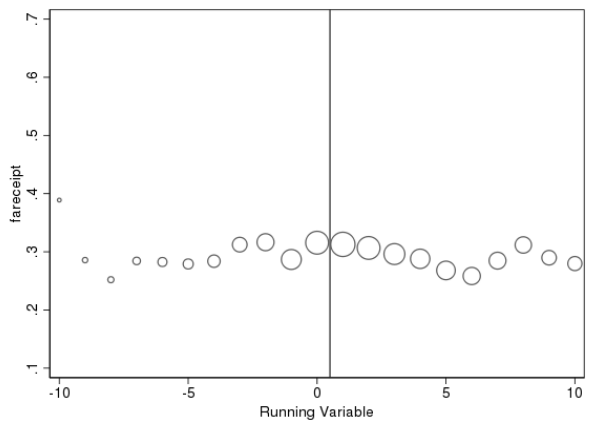
(a) Female



(b) Asian

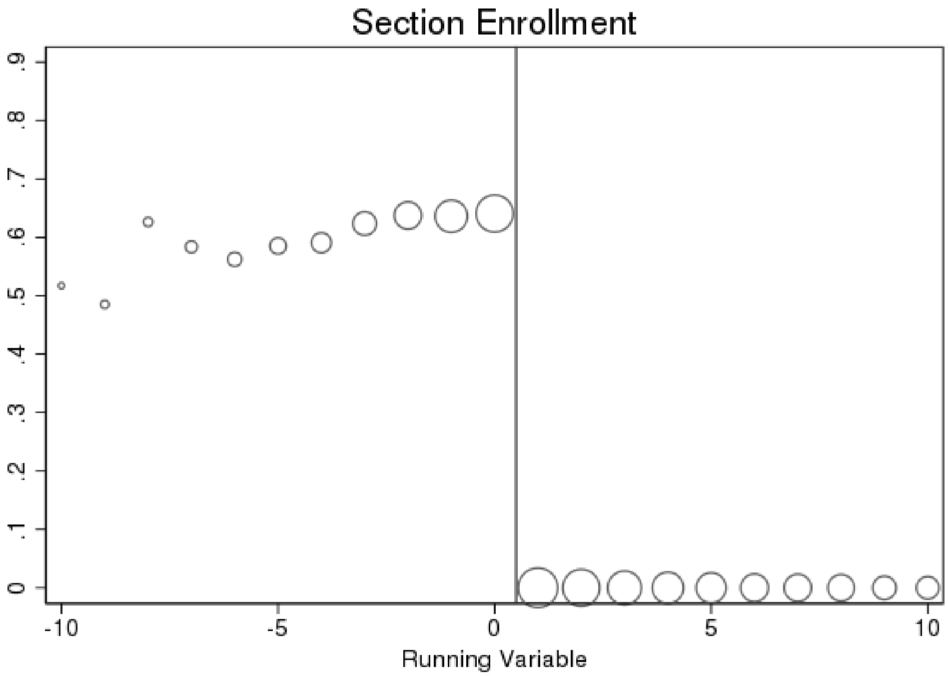


(c) International

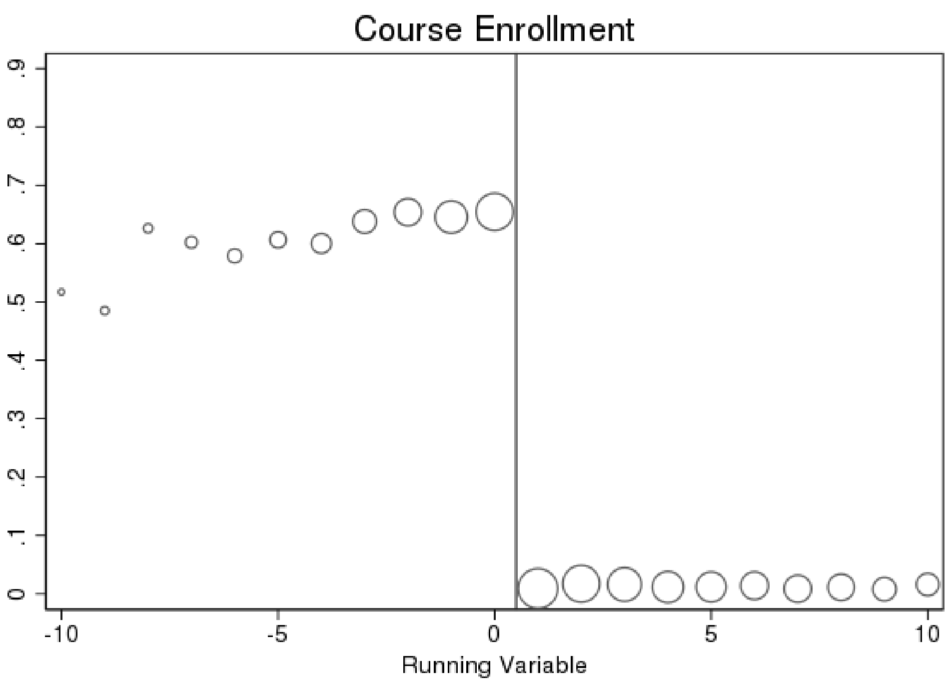


(d) Received Financial Aid

Figure 3: Covariate Smoothness. Each dot represents the mean of the covariate, binned by the value of the running variable. The size of the dot reflects the number of observations in each bin.

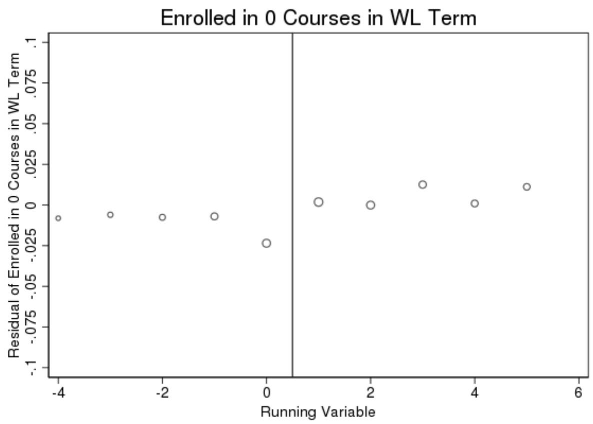


(a) Jump = -0.641

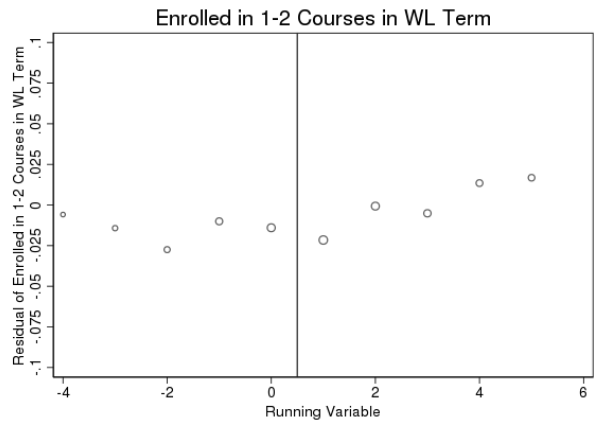


(b) Jump = -0.645

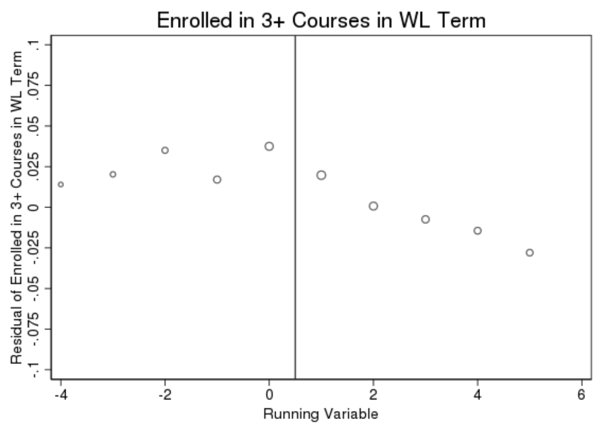
Figure 4: First Stage. Each dot represents enrollment binned by the value of the running variable. The size of the dot reflects the number of observations in each bin.



(a) Jump = 0.025

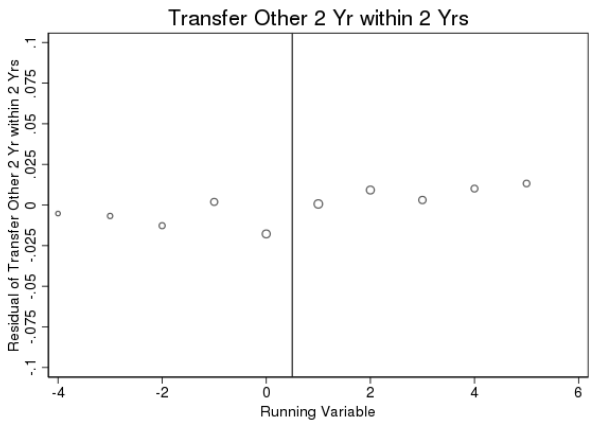


(b) Jump = -0.007

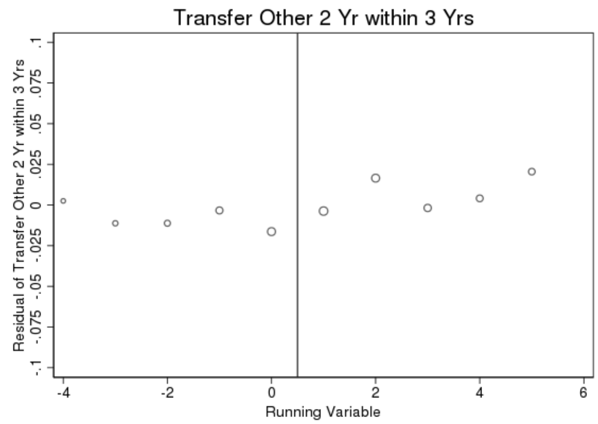


(c) Jump = -0.018

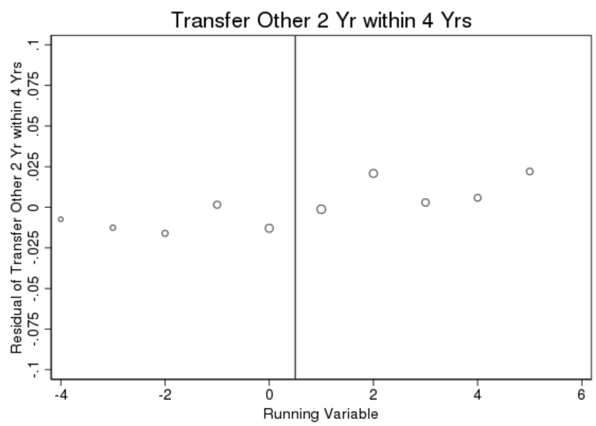
Figure 5: Enrollment in Waitlisted Term Each dot represents the average residual of the outcome, conditioned on the observable characteristics in Table 3, and binned by the value of the running variable. The size of the dot reflects the number of observations in each bin.



(a) Jump = 0.018

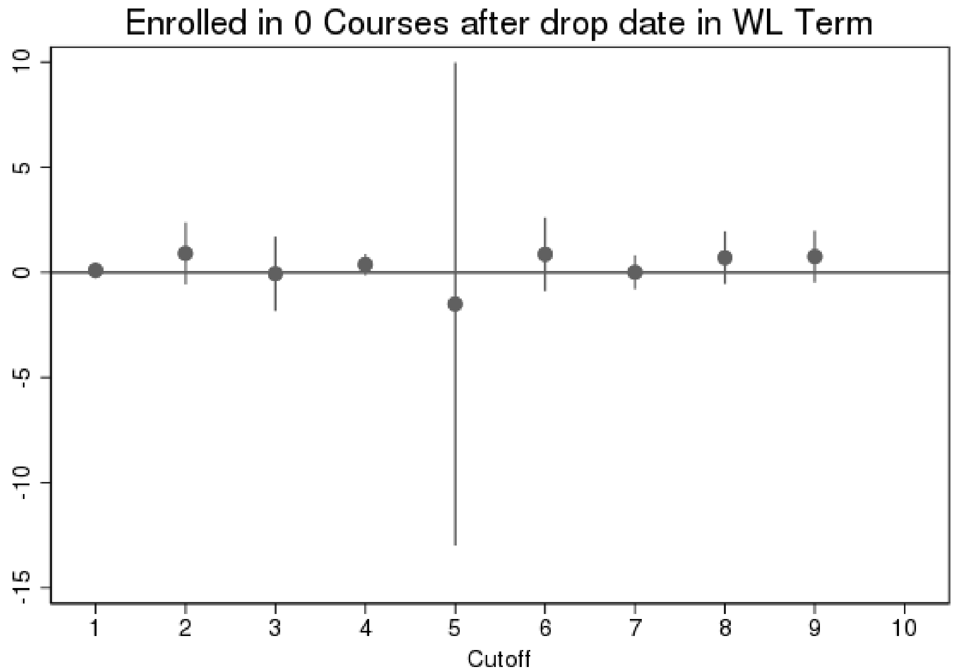


(b) Jump = 0.013

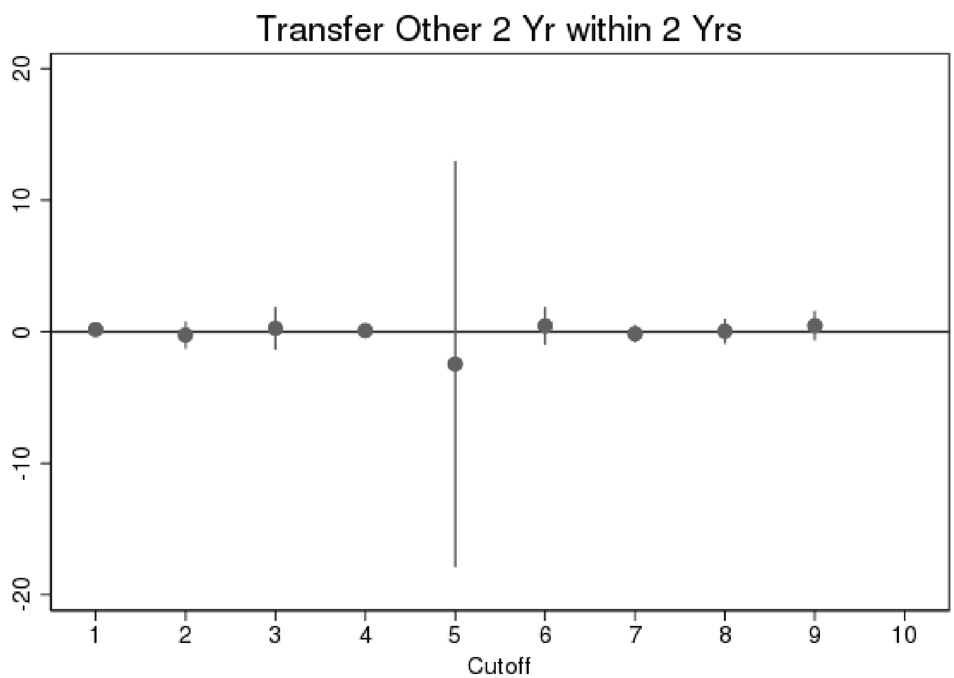


(c) Jump = 0.012

Figure 6: Substitution Toward Other Two-Year Schools Each dot represents the residual of the outcome, conditioned on the observable characteristics in Table 3, and binned by the value of the running variable. The size of the dot reflects the number of observations in each bin.

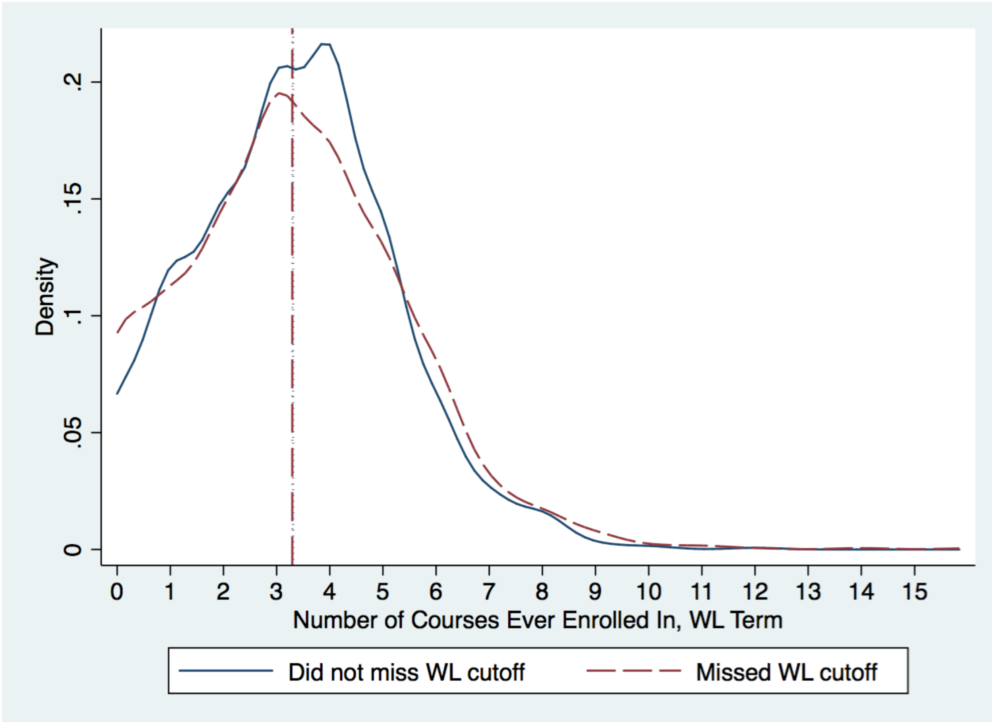


(a) Enrolled in Zero Courses After Drop Date in Waitlisted Term

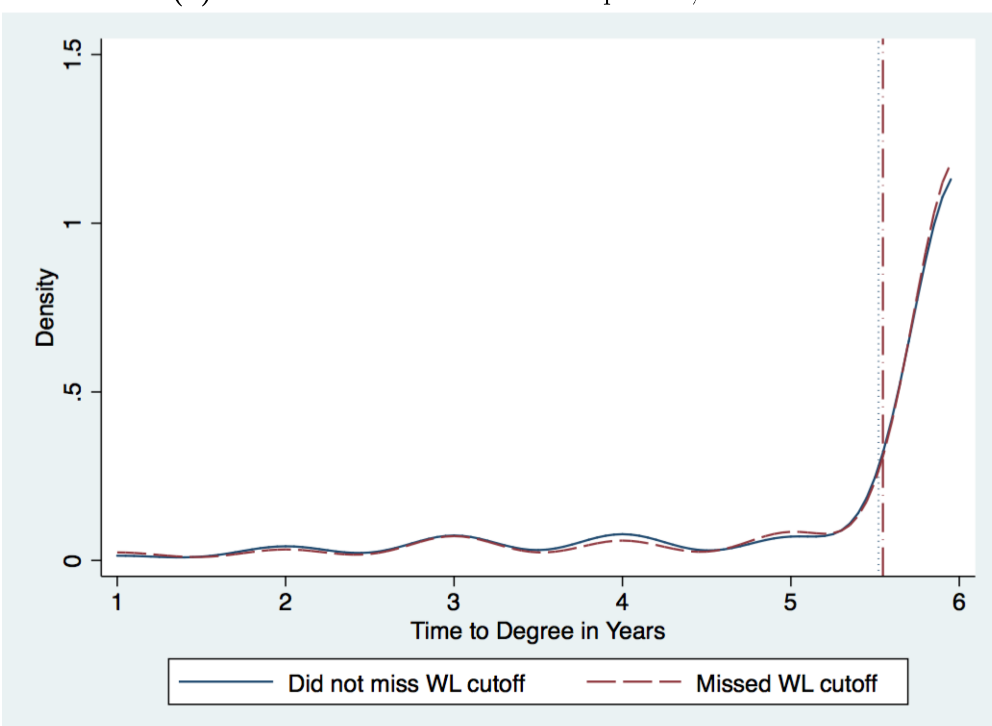


(b) Transfer to Other Two Year School within Two Years

Figure 7: This figure plots point estimates and confidence intervals for 2SLS estimates of the effect of being shut out of a course, instrumented by being on the right of the placebo cutoff. Each value on the x-axis represents the value of the placebo cutoff being tested.

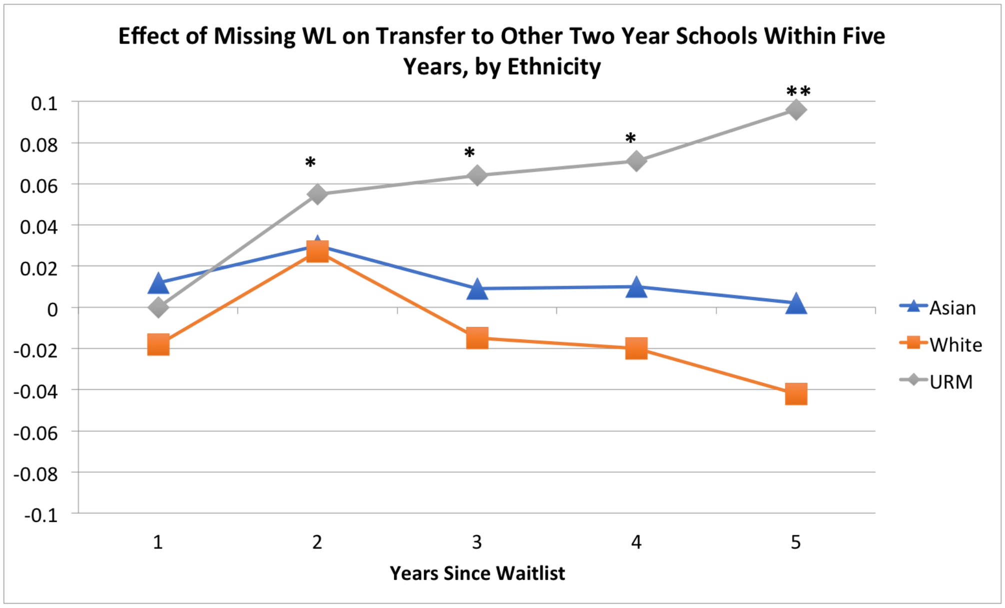


(a) Number of Courses After Drop Date, WL Term

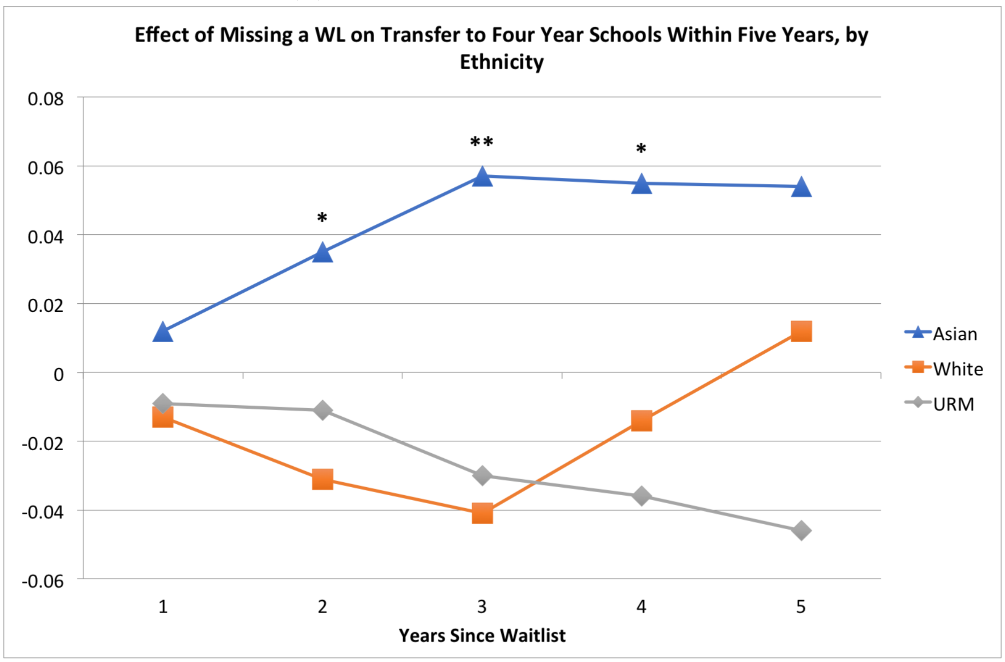


(b) Time to Degree (Associates or Certificate)

Figure 8: This figure plots estimates of the potential outcome densities for treated and untreated compliers. Treated compliers missed the waitlist cutoff and did not enroll in their desired section, and untreated compliers did not miss the cutoff and therefore enrolled in their desired section.



(a) Transfer to Two Year Schools



(b) Transfer to Four Year Schools

Figure 9: This figure shows 2SLS point estimates of the impact of being rationed out of a class section during the pre-registration period, estimated separately for ethnic subgroups. Subfigure 9a shows the effect of rationing on transfer rates to other two-year schools within one to five years of the waitlist. Subfigure 9b shows the effect of rationing on transfer rates to four-year schools within one to five years of the waitlist. Stars indicate that the estimate is statistically different from zero at the ten, five, or one percent level (represented by one, two, or three stars, respectively). The URM category consists of Black, Hispanic, Native American, or multi-racial students, or students who do not fit into any other category.

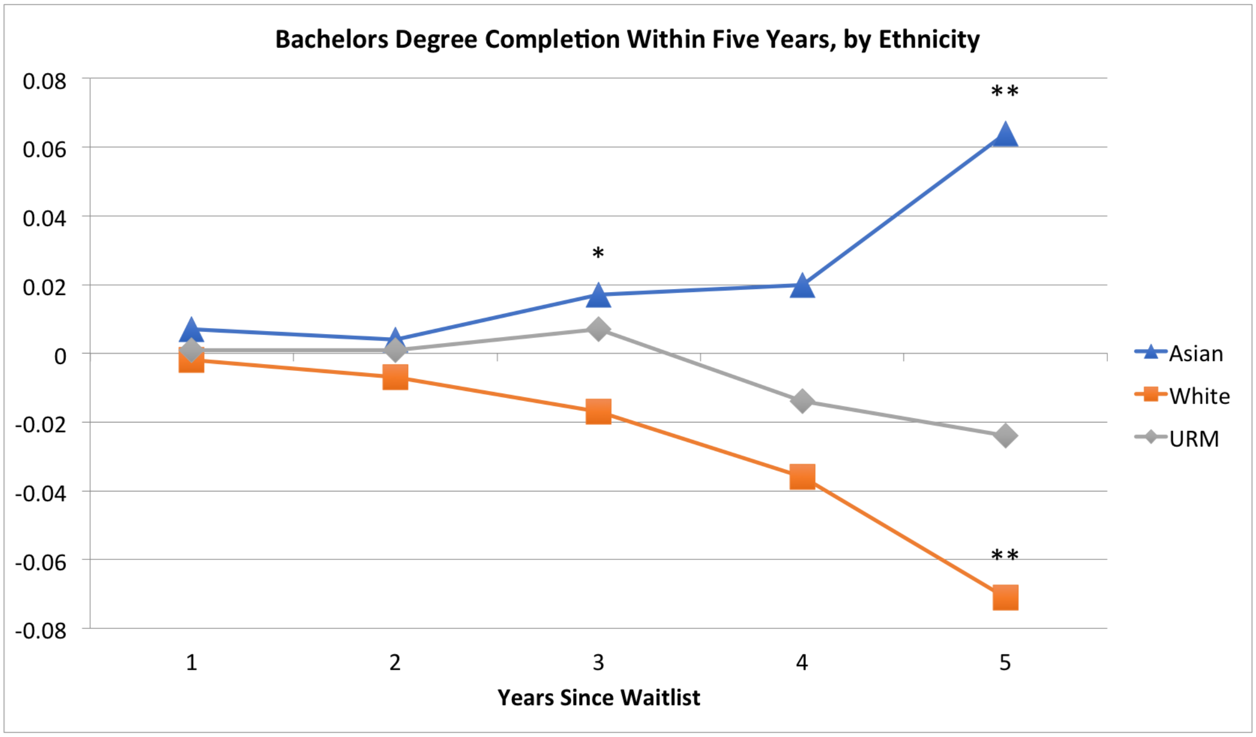


Figure 10: This figure shows 2SLS point estimates of the impact of being rationed out of a class section during the pre-registration period on bachelors degree completion within one to five years of the waitlist, estimated separately for ethnic subgroups. Stars indicate that the estimate is statistically different from zero at the ten, five, or one percent level (represented by one, two, or three stars, respectively). The URM category consists of Black, Hispanic, Native American, or multi-racial students, or students who do not fit into any other category.

A Appendix Tables

Table A1: Student Initial Education Goal

Included in Sample	Code	Description
Yes	A	Obtain an associate degree and transfer to a 4-year institution
Yes	B	Transfer to a 4-year institution without an associate degree
Yes	C	Obtain a two year associate's degree without transfer
	D	Obtain a two year vocational degree without transfer
	E	Earn a vocational certificate without transfer
	F	Discover/formulate career interests, plans, goals
	G	Prepare for a new career (acquire job skills)
	H	Advance in current job/career (update job skills)
	I	Maintain certificate or license (e.g., Nursing, Real Estate)
	J	Educational development (intellectual, cultural); often recreational course-takers
	K	Improve basic skills in English, reading, or math
	L	Complete credits for high school diploma or GED; often high school students
Yes	M	Undecided on goal
	N	To move from noncredit coursework to credit course work
	O	4 year college student taking courses to meet 4 year college requirement
	X	Uncollected/unreported
	Y	Not Applicable

Notes: At application, students are asked to indicate their initial educational goal from the above list. The sample is restricted to "traditional" community college students who might consider a bachelors degree at a four-year institution a reasonable substitute to their current program.

Table A2: Transfer within Two Years to Other Two Year, by Institution

Outcome	Foothills (1)	Evergreen Valley (2)	San Jose City (3)	Other 2-Year (4)
2SLS	-0.008	0.013**	0.014**	0.017**
	(0.010)	(0.006)	(0.006)	(0.008)
CCM	0.05	0.01	0.01	0.03

Notes: This table reports 2SLS coefficients of the effect of being shut out of a course on transfer rates to different two-year colleges within two years of missing the waitlist cutoff. See Table 5 for details of the estimation. Stars represent significance at the 10%, 5% and 1% level.

Table A3: Effect of Missing the Cutoff on Enrollment in the Waitlisted Term, by Ethnicity

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
Asian	0.028	-0.014	-0.014	-0.021
	(0.019)	(0.028)	(0.030)	(0.031)
CCM Asian	0.065	0.231	0.705	0.750
N Asian (N_l/N_r)	1,148/1,036			
White	0.044	0.005	-0.049	-0.042
	(0.027)	(0.041)	(0.043)	(0.046)
CCM White	0.071	0.290	0.639	0.645
N White (N_l/N_r)	593/ 542			
URM	0.013	-0.009	-0.003	0.037
	(0.022)	(0.035)	(0.038)	(0.042)
CCM URM	0.064	0.260	0.676	0.642
N URM (N_l/N_r)	665/525			
P-value White=Asian	0.62	0.70	0.50	0.71
P-value URM=Asian	0.61	0.93	0.83	0.27
P-value URM=White	0.40	0.86	0.50	0.51

Notes: This table shows results from a 2SLS regression as in Table 5, but estimated separately by ethnic categories. The underrepresented minority category includes students who self-identify as Black, Hispanic, Native-American, mixed-race, or other. The outcome is an indicator for whether the student took no courses in the concurrent term in Column (1), took one or two courses in Column (2), or took three or more courses in Column (3). A course is counted if the student is enrolled after the add/drop date. The outcome in column (4) is an indicator for whether the student enrolls in any classes the following major term. The standard errors are in parentheses, with the control complier means (CCM) and observations per ethnic category displayed below. Standard errors are robust to heteroskedasticity. P-values test for the difference in point estimates between ethnic groups. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A4: Effect of Missing the Cutoff on Enrollment in the Waitlisted Term, by Gender

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
Male	0.047**	0.002	-0.049	-0.024
	(0.018)	(0.028)	(0.031)	(0.032)
CCM Male	0.062	0.235	0.703	0.689
N Male (N_l/N_r)	1,100/1,281			
Female	0.047**	-0.014	-0.033	-0.040
	(0.018)	(0.028)	(0.031)	(0.031)
CCM Female	0.065	0.285	0.650	0.687
N Female (N_l/N_r)	1,172/ 1,304			
P-value Male=Female	0.99	0.69	0.73	0.73

Notes: This table shows results from a 2SLS regression as in Table 5, but estimated separately by gender. The outcome is an indicator for whether the student took no courses in the concurrent term in Column (1), took one or two courses in Column (2), or took three or more courses in Column (3). A course is counted if the student is enrolled after the add/drop date. The outcome in column (4) is an indicator for whether the student enrolls in any classes the following major term. The standard errors are in parentheses, with the control complier means (CCM) and observations per ethnic category displayed below. Standard errors are robust to heteroskedasticity. P-values test for the difference in point estimates between genders. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A5: Effect of Missing the Cutoff on Enrollment in the Waitlisted Term, by Course Subject

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
STEM	-0.004	0.015	-0.012	-0.011
	(0.021)	(0.030)	(0.034)	(0.034)
CCM STEM	0.081	0.214	0.705	0.733
N STEM (N_l/N_r)	757/856			
Arts/Humanities	0.043**	-0.042	-0.002	0.013
	(0.022)	(0.034)	(0.036)	(0.039)
CCM Arts/Hum.	0.065	0.274	0.661	0.640
N Arts/Hum. (N_l/N_r)	616/748			
Social Studies	0.043	-0.058	0.015	-0.070
	(0.034)	(0.054)	(0.058)	(0.062)
CCM Soc. Stud.	0.045	0.284	0.671	0.650
N Soc. Stud. (N_l/N_r)	300/309			
Other	0.053**	0.024	-0.077*	-0.018
	(0.024)	(0.040)	(0.041)	(0.042)
CCM Other	0.055	0.303	0.641	0.682
N Other (N_l/N_r)	602/680			
P-value STEM=Arts/Hum	0.27	0.50	0.97	0.46
P-value STEM=Soc. Stud.	0.81	0.14	0.21	0.98
P-value STEM=Other	0.47	0.64	0.37	0.60
P-value Arts/Hum = Soc. Stud	0.56	0.33	0.20	0.61
P-value Arts/Hum= Other	0.76	0.30	0.41	0.86
P-value Soc. Stud.=Other	0.75	0.08	0.06	0.72

Notes: This table shows results from a 2SLS regression as in Table 5, but estimated separately by the subject of the course a student is waitlisted in. The outcome is an indicator for whether the student took no courses in the concurrent term in Column (1), took one or two courses in Column (2), or took three or more courses in Column (3). A course is counted if the student is enrolled after the add/drop date. The outcome in column (4) is an indicator for whether the student enrolls in any classes the following major term. The standard errors are in parentheses, with the control complier means (CCM) and observations per ethnic category displayed below. Standard errors are robust to heteroskedasticity. P-values test for the difference in point estimates between genders. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A6: Effect of Missing the Cutoff on Enrollment in the Waitlisted Term, Top 5 Most Popular Courses

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
Top 5	0.086***	-0.119**	0.033	-0.009
	(0.033)	(0.048)	(0.054)	(0.066)
CCM Top 5	0.021	0.229	0.751	0.742
N Top 5 (N_l/N_r)	170/209			
Other Courses	0.043**	0.008	-0.052**	-0.043*
	(0.014)	(0.021)	(0.023)	(0.023)
CCM Other	0.068	0.262	0.670	0.687
N Other (N_l/N_r)	1,807/2,072			
P-value Top 5= Other	0.23	0.02	0.15	0.63

Notes: This table shows results from a 2SLS regression as in Table 5, but estimated separately separately for courses among the top 5 most often requested. The outcome is an indicator for whether the student took no courses in the concurrent term in Column (1), took one or two courses in Column (2), or took three or more courses in Column (3). A course is counted if the student is enrolled after the add/drop date. The outcome in column (4) is an indicator for whether the student enrolls in any classes the following major term. The standard errors are in parentheses, with the control complier means (CCM) and observations per category displayed below. Standard errors are robust to heteroskedasticity. P-values test for the difference in point estimates by course popularity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A7: Effect of Missing a Waitlist Cutoff on Transfers to Two-Year Schools, by Ethnicity

	Within 1 Year (1)	Within 2 Years (2)	Within 3 Years (3)	Within 4 Years (4)	Within 5 Years (5)
Asian	0.012	0.030	0.009	0.010	0.002
	(0.016)	(0.022)	(0.027)	(0.029)	(0.031)
CCM	0.045	0.086	0.142	0.179	0.212
White	-0.018	0.027	-0.015	-0.020	-0.042
	(0.026)	(0.033)	(0.038)	(0.041)	(0.043)
CCM	0.074	0.120	0.177	0.215	0.259
URM	0.028	0.055*	0.064*	0.071*	0.096**
	(0.023)	(0.030)	(0.034)	(0.037)	(0.039)
CCM	0.552	0.508	0.206	0.194	0.058
P-value Asian=White	0.34	0.94	0.60	0.54	0.40
P-value Asian=URM	0.55	0.51	0.21	0.19	0.06
P-value White=URM	0.18	0.44	0.11	0.08	0.02

Notes: This table shows the coefficient on predicted enrollment from a 2SLS, local linear regression as in equation 3 with a bandwidth of one, estimated separately by ethnic category. Column 1 shows effects of rationing on transfer to other two year schools within one year of the waitlisted term, column 2 shows effects within two years, etc. Control complier means in are below the standard errors. All columns include controls for gender, ethnicity, indicators for returning and continuing status, US citizenship, age, prior GPA, cumulative credits, special program status, financial aid receipt as well as registration priority fixed effects and term by year fixed effects, interacted with ethnicity. Standard errors are robust to heteroskedasticity and stars represent significance at the 10%, 5% and 1% level.

Table A8: Effect of Missing a Waitlist Cutoff on Transfers to Four-Year Schools, by Ethnicity

	Within 1 Year (1)	Within 2 Years (2)	Within 3 Years (3)	Within 4 Years (4)	Within 5 Years (5)
Asian	0.013	0.035*	0.057**	0.055*	0.054
	(0.010)	(0.020)	(0.029)	(0.032)	(0.033)
CCM	0.020	0.062	0.140	0.202	0.232
White	-0.016	-0.031	-0.041	-0.014	0.012
	(0.017)	(0.027)	(0.036)	(0.040)	(0.043)
CCM	0.042	0.087	0.173	0.210	0.249
URM	-0.008	-0.011	-0.030	-0.036	-0.046
	(0.012)	(0.019)	(0.026)	(0.032)	(0.034)
CCM	0.020	0.036	0.093	0.139	0.164
P-value Asian=White	0.14	0.03	0.02	0.13	0.34
P-value Asian=URM	0.18	0.10	0.03	0.04	0.04
P-value White=URM	0.70	0.42	0.69	0.78	0.40

Notes: This table shows the coefficient on predicted enrollment from a 2SLS, local linear regression as in equation 3 with a bandwidth of one, estimated separately by ethnic category. Column 1 shows effects of rationing on transfer to four year schools within one year of the waitlisted term, column 2 shows effects within two years, etc. Control complier means in are below the standard errors. All columns include controls for gender, ethnicity, indicators for returning and continuing status, US citizenship, age, prior GPA, cumulative credits, special program status, financial aid receipt as well as registration priority fixed effects and term by year fixed effects, interacted with ethnicity. Standard errors are robust to heteroskedasticity and stars represent significance at the 10%, 5% and 1% level.

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Table A9: Effect of Missing a Waitlist Cutoff on Bachelors Degree Completion, by Ethnicity

	Within 1 Year (1)	Within 2 Years (2)	Within 3 Years (3)	Within 4 Years (4)	Within 5 Years (5)
Asian	0.007	0.004	0.016*	0.020	0.063**
	(0.004)	(0.006)	(0.009)	(0.018)	(0.025)
CCM	0.002	0.005	0.011	0.040	0.091
White	-0.002	-0.007	-0.016	-0.035	-0.070**
	(0.007)	(0.009)	(0.015)	(0.023)	(0.031)
CCM	0.005	0.012	0.031	0.071	0.131
URM	0.003	0.003	0.008	-0.012	-0.021
	(0.003)	(0.003)	(0.006)	(0.016)	(0.020)
CCM	0.000	0.000	0.002	0.026	0.057
P-value Asian=White	0.30	0.27	0.06	0.06	0.00
P-value Asian=URM	0.42	0.78	0.46	0.19	0.01
P-value White=URM	0.55	0.28	0.13	0.40	0.19

Notes: This table shows the coefficient on predicted enrollment from a 2SLS, local linear regression as in equation 3 with a bandwidth of one, estimated separately by ethnic category. Column 1 shows effects of rationing on bachelors degree completion within one year of the waitlisted term, column 2 shows effects within two years, etc. Control complier means in are below the standard errors. All columns include controls for gender, ethnicity, indicators for returning and continuing status, US citizenship, age, prior GPA, cumulative credits, special program status, financial aid receipt as well as registration priority fixed effects and term by year fixed effects, each interacted with ethnicity. Standard errors are robust to heteroskedasticity and stars represent significance at the 10%, 5% and 1% level.

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Table A10: Robustness to Sample Restrictions

Outcome	No Restrictions (1)	Intend to Transfer (2)	Post 2007 (3)
Enrolled in 0 Courses	0.025***	0.016***	0.045***
	(0.003)	(0.004)	(0.005)
Enrolled in 1-2 Courses	0.013***	0.019***	-0.024
	(0.005)	(0.006)	(0.037)
Enrolled in 3+ Courses	-0.038***	-0.035***	-0.021
	(0.005)	(0.007)	(0.039)
Observations (N_l/N_r)	30,329/37,103	17,338/ 20,873	637/692

Notes: This table shows the coefficient from a 2SLS regression as in equation 3. Column (1) includes all students, and all waitlists. Column (2) includes only students who declared an intention to transfer to a four year school upon enrolling at De Anza, and all waitlists. Column (3) restricts the sample to observations after 2007, as well as the restrictions used in the main analysis. Controls include the full vector in Table 1 as well as registration priority fixed effects and term by year fixed effects. Standard errors are clustered at the student level when more than one observation per student is used, and are robust to heteroskedasticity otherwise, and stars represent significance at the 10%, 5% and 1% level.

Table A11: Effect of Missing a Placebo Cutoff

Cutoff	Enrolled in Zero Courses, Concurrent Term (1)	Transfer to Other 2 Year, Within 2 Years (2)	Observations (N_l/N_r) (3)
1	0.10	0.16	2,593/ 2,260
	(0.192)	(0.193)	
2	0.90	-0.26	2,260/1,881
	(0.752)	(0.535)	
3	-0.07	0.25	1,881/1,608
	(0.900)	(0.831)	
4	0.37	0.08	1,608/1,474
	(0.261)	(0.237)	
5	-1.50	-2.46	1,474/1,250
	(5.86)	(7.89)	
6	0.86	0.45	1,250/1,201
	(0.895)	(0.735)	
7	0.01	-0.16	1,201/1,126
	(0.413)	(0.351)	
8	0.703	0.035	1,126/873
	(0.641)	(0.484)	
9	0.75	0.46	873/815
	(0.631)	(0.571)	

Notes: This table shows the coefficient from a 2SLS regression where the instrument is whether a student has a running variable equal to the cutoff value plus one. For each row, the sample includes only students with running variable equal to the cutoff value and one plus the cutoff. The outcome in column (1) is an indicator for being enrolled in zero courses after drop date in the waitlisted term. The outcome in column (2) is an indicator for being enrolled in another two-year school within two years. Standard errors are robust to heteroskedasticity and regressions control for the full vector of covariates in Table 1.

B Time Running Variable

The analysis uses a highly discrete running variable, which necessitates local randomization assumptions. Alternatively, the running variable can be framed as a continuous measure if it is redefined in terms of registration time. The discrete running variable used in the main analysis is the "position RV" while this new continuous version is the "time RV."

Consider the time of day that each waitlisted student made her registration attempt. The time when the student with a position RV equal to zero signed up for the waitlist creates a cutoff in registration time. Students who signed up to the waitlist before this time could enroll in the section during the registration period (ie. had a negative position RV) while those who signed up after could not (ie. had a positive position RV). Therefore, the time RV is the amount of time, in hours, between when a student signed up for the waitlist and when the student with a position RV of zero registered. In this sense, the analysis compares students who missed the waitlist cutoff to those who just made it, within a window of hours around the cutoff time.[11]

Figure B1 shows the density of the time RV. Note that there is a large spike at zero. This is a mechanical result due to the definition of the time RV. There is not a natural way to set the cutoff, therefore a position of zero is defined using the position RV from the main analysis. This forces many students to be at or near the cutoff artificially. For this reason, the density fails the manipulation test proposed in McCrary (2008) as well as the more recently proposed test in Cattaneo, Jansson, and Ma (2017). However, there is little chance that the density is a result of systematic manipulation rather than an artifact of the variable definition. The main argument for identification is that since the time RV, like the position RV, depends on the number of other students who drop, students cannot easily

[11] There are 2 edge cases in which it is not possible to compute a time RV for waitlisted students in a section. First, if enough previously enrolled students drop during the registration period such that everyone who signed up for the waitlist is able to get a seat, then there is no student with a position RV equal to zero. Second, if no previously enrolled students drop such that nobody who signed up to the waitlist is able to get a seat during the registration period, then there is also no student with a position RV equal to zero. The analysis drops these attempts, which amount to just over 10% of the registration attempts in the sample.

control it.

Figure B2 plots section and course enrollment rates at the end of the advanced registration period binned by values of the running variable. There is a clearly visible jump in enrollment to the left of the cutoff. Table B1 shows formal estimates of the first stage and confirms that there is a discontinuity in the probability of section enrollment. Students who miss the waitlist cutoff are 81 percentage points more likely to be shut out of their desired section during the advanced registration period, and similarly unlikely to enroll in their desired course during advanced registration. These discontinuities are larger than those in the main analysis, which were 64 and 65 percentage points, respectively.

Table B2 shows the estimates of the LATE on enrollment patterns in the concurrent term. The direction of the results is similar. There is a 3.6 percentage point increase in the likelihood of taking no courses in the waitlisted term. The analysis cannot detect a change in the share of students who enroll part-time, or full-time, though the magnitudes of these fall. These results almost perfectly line up with the main specification; not be'ing able to enroll in a desired section leads to same-semester drop-out.

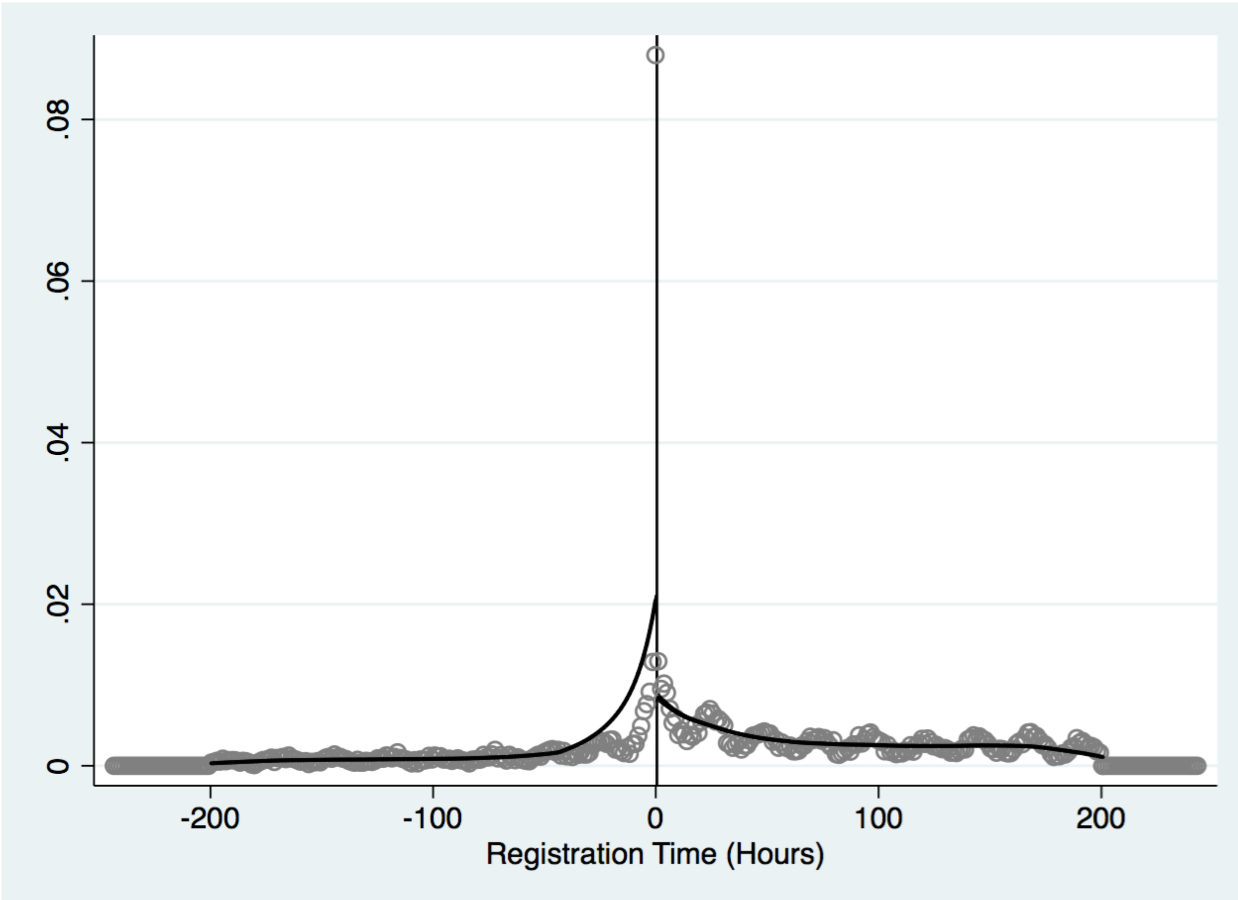
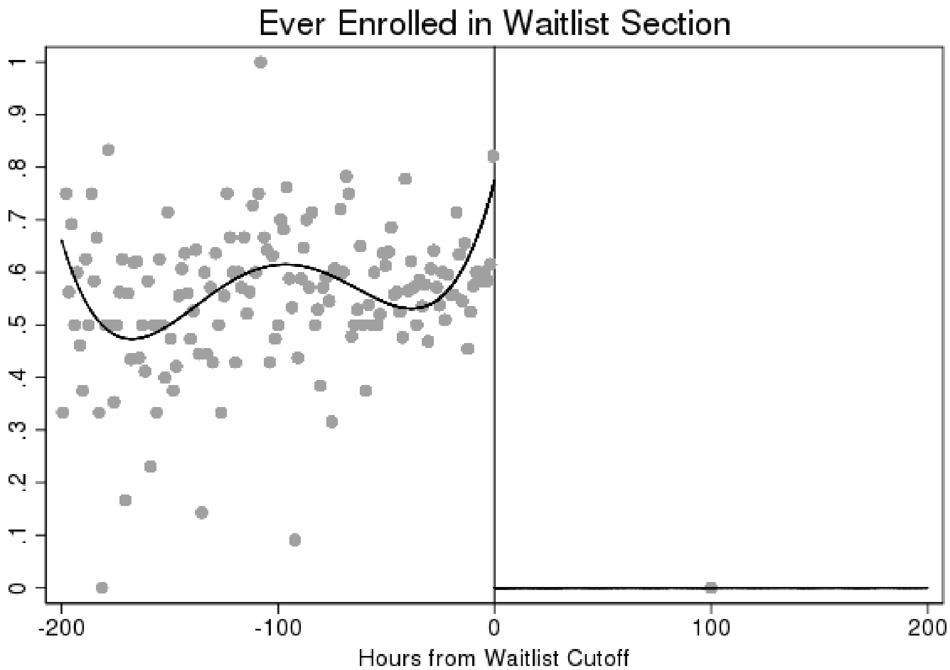
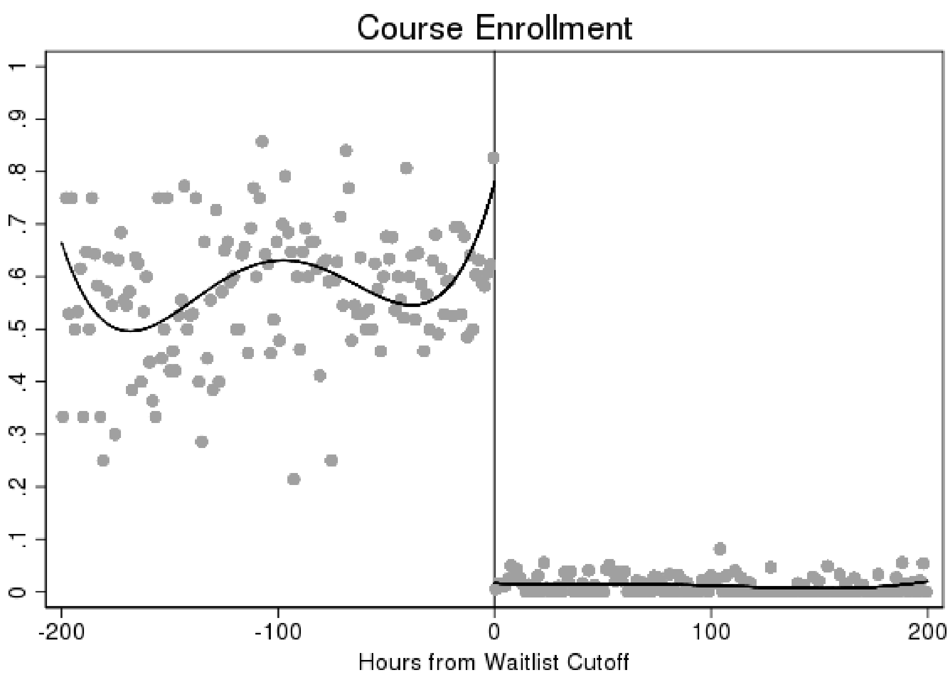


Figure B1: Density of the Time Running Variable



(a) Jump = -0.21



(b) Jump = -0.15

Figure B2: Time Running Variable First Stage. Each dot represents enrollment binned by the value of the time running variable.

Table B1: Time RV First Stage

	Not Enroll, Section		Not Enroll, Course	
	(1)	(2)	(3)	(4)
Miss WL	0.813***	0.812***	0.814***	0.813***
	(0.009)	(0.009)	(0.010)	(0.010)
Observations (N_l/N_r)	2,481/1,446	2,480/1,445	2,461/1,409	2,461/1,409
Controls	N	Y	N	Y
BW	13.76	13.73	13.01	13.00

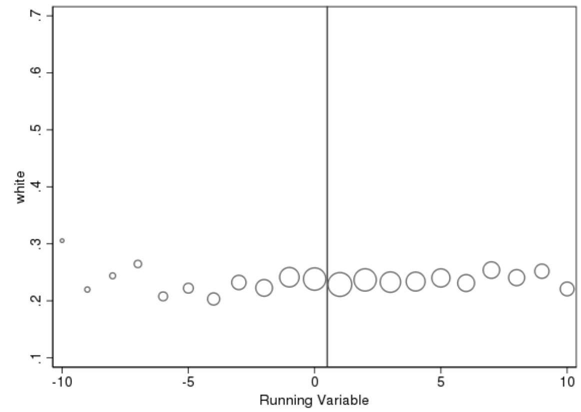
Notes: Results are from a local linear regression where the endogenous variable is not enrolling in the waitlisted section or course. Bandwidths are calculated using the CCT method. Controls include gender, ethnicity, indicators for returning and first-time student status, US citizenship, age, special program status, financial aid receipt as well as registration priority fixed effects and term by year fixed effects. Stars represent significance at the 10%, 5% and 1% level.

Table B2: Effects of Missing a Time Waitlist Cutoff on Enrollment in Waitlisted Term

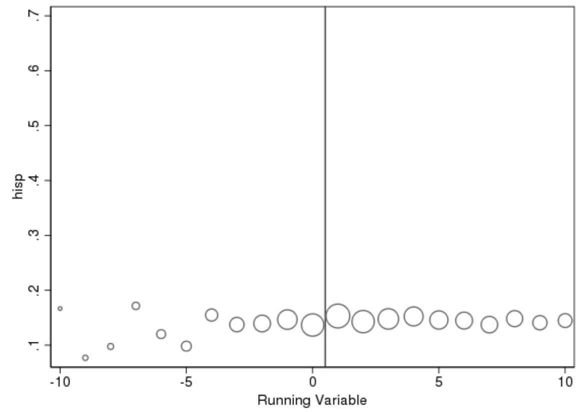
	Enrolled in 0 Courses	Enrolled in 1-2 Courses	Enrolled in 3+ Courses
	(1)	(2)	(3)
Shut Out	0.036***	-0.009	-0.017
	(0.013)	(0.019)	(0.021)
Observations (N_l/N_r)	3,325/3,099	3,761/4,082	3,280/3,015
CCT BW	42.71	65.75	40.69

Notes: Estimates are from local linear regression with triangular kernel where the running variable is hours between when the student signed up to the waitlist and when the student with a position running variable of zero signed up. The outcome is an indicator for the number of courses that a student was ever enrolled in during the waitlisted term (zero, one or two, and three or more). The standard errors are bias corrected and the CCT optimal bandwidth displayed below, as in Cattaneo, Calonico, and Titiunik (2014). The window of included observations is equal to the cutoff, plus or minus the bandwidth. Controls include gender, race, ethnicity, indicators for returning and continuing status, US citizenship, age, special program status, financial aid receipt as well as registration priority fixed effects and term by year fixed effects. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

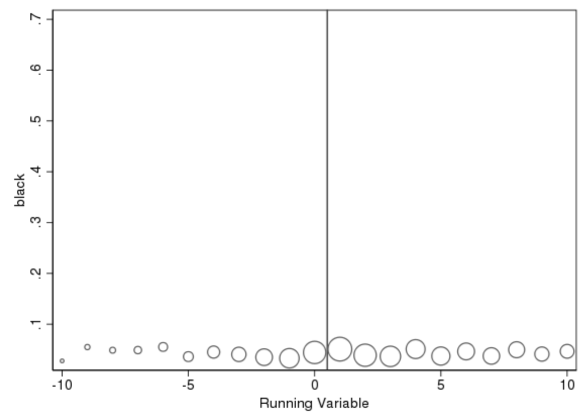
C Covariate Smoothness



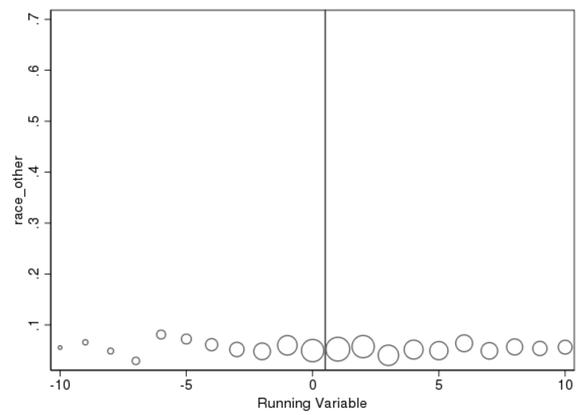
(a) White



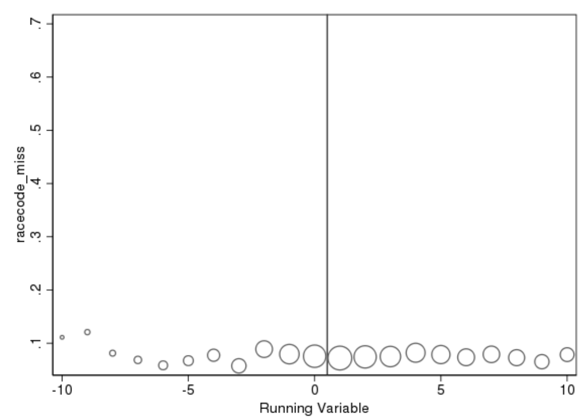
(b) Hispanic



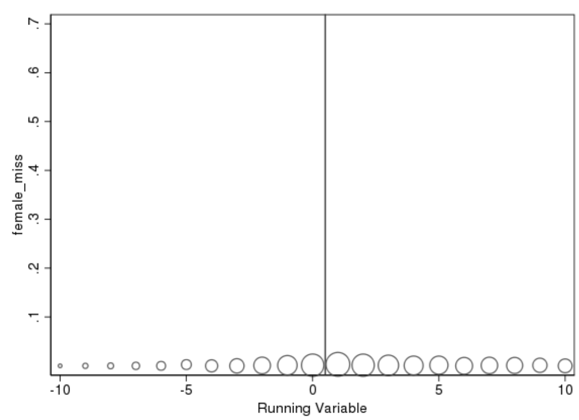
(c) Black



(d) Other Race

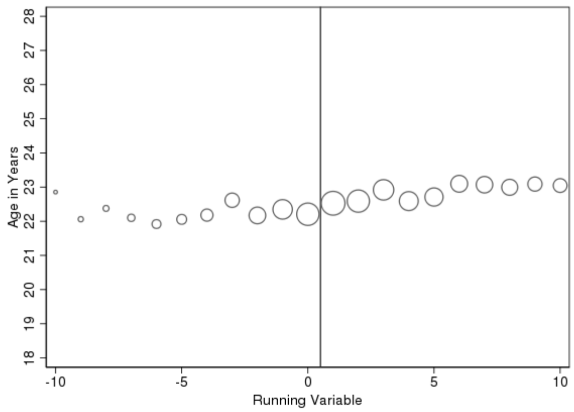


(e) Missing Race Information

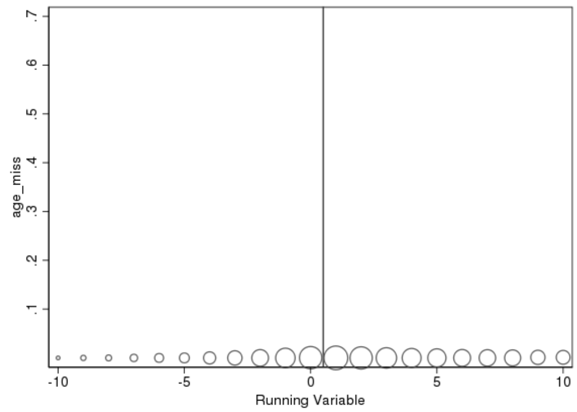


(f) Missing Gender Information

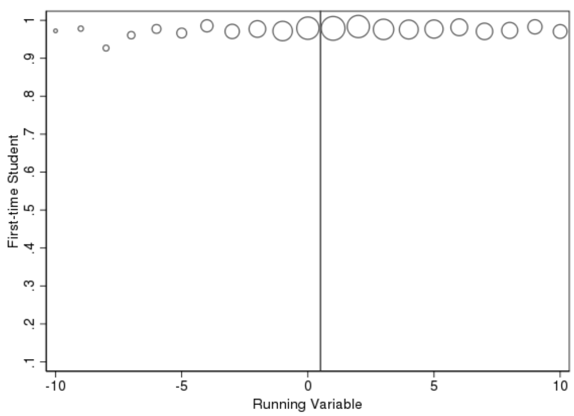
Figure C1: Covariate Smoothness. Each dot represents the mean of the covariate, conditioned on the value of the running variable. The size of the dot reflects the number of observations in each bin.



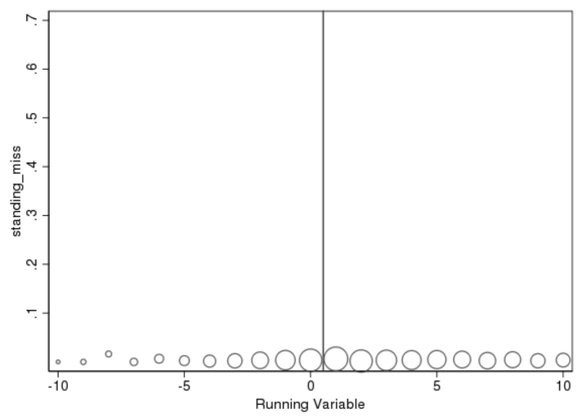
(a) Age in Years



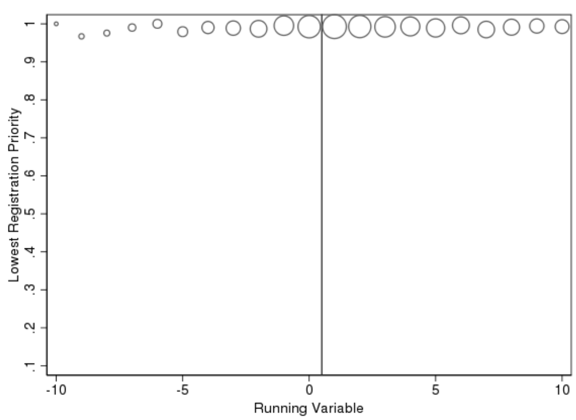
(b) Missing Age Information



(c) First-Time Student Status



(d) Missing Class Standing Information



(e) Lowest Registration Priority

Figure C2: Covariate Smoothness. Each dot represents the residual of the covariate, conditioned on the institutional features described in Table 3 and binned by the value of the running variable. The size of the dot reflects the number of observations in each bin.