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

Silvia Robles*[1], Max Gross[1], Robert W. Fairlie[2] and Thomas Barrios[3]

[1]University of Michigan
[2]University of California, Santa Cruz and NBER
[3]TrueCar, Inc

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Abstract

One frequently cited yet understudied channel through which money matters for college students is course availability- colleges may respond to budgetary pressure by reducing course offerings. Open admissions policies, binding class size constraints, and heavy reliance on state funding may make this channel especially salient at community colleges, which enroll 47% of U.S. undergraduates in public colleges and 55% of underrepresented minority students. We use administrative course registration data from a large community college in California to test this mechanism. By exploiting discontinuities in course admissions created by waitlists, we find that students stuck on a waitlist and shut out of a course section were 25% more likely to take zero courses that term relative to a baseline of 10%. Shutouts also increased transfer rates to nearby, but potentially lower quality, two-year colleges. These results document that course availability- even through a relatively small friction- can interrupt and distort community college students' educational trajectories.

*Corresponding author: Silvia Robles, *srobles@umich.edu*
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1 Introduction

State funding for higher education in the United States has dramatically declined over the past decade. Budget cuts following the Great Recession have persisted even ten years later-funding for public two and four-year colleges in 2017 was $9 billion less than its pre-recession level in 2008 (Mitchell, Leachman, and Masterson, 2016). Despite some evidence that money matters for college students, the mechanisms through which such resource effects operate are decidedly less clear. Anecdotally, overburdened college budgets are often associated with course overcrowding. When a college faces budgetary pressure, it may reduce course offerings or the number of sections per course. More students may find themselves unable to enroll in the courses they need to complete a degree. This hypothesis appears in the academic literature as well. Both Bound and Turner (2007) and Deming and Walters (2017) cite oversubscribed courses as a likely mechanism to explain the relationship between funding and college student outcomes, yet do not test it directly.

Credibly identifying the impact of limited course availability is challenging. Doing so requires detailed course registration data as a means to determine what classes students wish to take. It also requires exogenous variation in who is rationed out of a course. Using newly available data, this paper provides some of the only causal evidence on the impact of being shut out of a college course and the first estimates of its impacts among community college students. Community colleges enrolled 47% of all U.S. undergraduate students in public colleges and 55% of underrepresented minority students (Snyder, de Brey, and Dillow, 2018, Table 306.50). Their open enrollment policies, binding class size constraints, lower tuition rates, and heavy reliance on state funding may make course scarcity especially salient relative to four-year colleges.

We use novel administrative course registration data from a large community college in California to construct waitlist queues for each course. We link these to transcript data containing student course schedules, grades and degrees, as well as to the National Student Clearinghouse (NSC). The analysis measures the discontinuous impact of being stuck on a waitlist and unable to enroll in one's desired course on a student's current and future course-taking and degree completion, including transfers to other postsecondary institutions.

Our study is the first to use waitlist queues and admissions cutoffs as discontinuous breaks to determine who is able to or unable to enroll in a course. To understand the intuition behind this design, consider a section for an introductory English composition course. Suppose the section had a waitlist with two people on it, and before the end of the registration period, just one formerly enrolled student decided to drop out. This would give the first person on the waitlist the opportunity to enroll in her desired section, but not the second person. The admissions cutoff- the waitlist number below which a student does not get an opportunity to enroll- is very difficult to manipulate because waitlisted students can not reliably predict how many seats will open up. This introduces exogenous variation in who is able to take their desired courses. Our new approach leveraging waitlists for causal inference can be applied broadly in many other contexts.

The analysis primarily takes a local randomization approach to regression discontinuity analysis in order to compare students who signed up for a course-section waitlist and just missed or made the admission cutoff. Unlike a continuity-based framework, the local randomization approach explicitly treats observations in a narrow window around the cutoff like a randomized experiment. Cattaneo, Idrobo, and Titiunik (Forthcoming) argue that in settings with a very discrete running variable- those where there are only a few mass points around the cutoff- local randomization is the preferred estimation strategy.

Comparing students that just miss the waitlist cutoff to those who just make it, we find that students who were not able to enroll in their preferred section due to oversubscription were more likely to sit out the term altogether. Specifically, the reduced form results show that being stuck on a waitlist increased the probability of enrolling in zero courses by 1.6 percentage points, a phenomenon we call same-term drop-out. This represents a 16% increase relative to the 10.1% same-term drop-out rate among students who just got off of the waitlist.

Using the the waitlist cutoff as an instrument for being rationed out of a section, the

2SLS estimates show that being shut out of a course led to a 2.6 percentage point increase in same-term dropout. This is a 25% increase relative to the 10.4% same-term dropout rate among control compliers, those who enrolled in their desired section by the end of the registration period precisely because they got off of the waitlist. The estimated effects are robust to alternative sample definitions and different design decisions, including a continuity-based regression discontinuity analysis.

The rise in same-term dropout was driven by students waitlisted for especially popular courses, which serve as prerequisites for a variety of other courses and can be easily transferred for credit at any public four-year college in California. While students were no more or less likely to transfer to another two or four-year school within one year of being stuck on a waitlist, they were 34% more likely to transfer to other two-year schools within two years of missing the waitlist cutoff. In particular, students tend to substitute to nearby two-year schools within 30 minutes of driving distance. These nearby schools have lower degree completion rates and their students have lower average salaries, both of which indicate a reduction in college quality.

Though we find no average effect of shutouts on completion rates for associate degrees, certificates, or bachelor's degrees within five years, there are divergent impacts by student ethnicity. Shutouts caused underrepresented minority students to transfer to another two-year school while Asian students, the largest ethnicity group at the college, responded to rationing by transferring to four-year colleges sooner than they would have otherwise. This led to a corresponding increase in bachelor's degree completion rates within five years of the waitlist for Asian students. Ethnicity is most likely a proxy for other unobservable skills and advantages- or lack thereof- in navigating the higher education system and illustrates the potential for heterogeneity in how the community college system is used. That is, the two-year system may be a direct substitute for the early years at a four-year school among students who have access to both options, a stepping-stone to eventually gain access to the four-year system, or a terminal setting itself.

Taken together, the impacts from a relatively small friction demonstrates that oversubscribed courses can meaningfully alter a student's path. These results document that limited course availability, an often cited mechanism through which funding matters for college students, can interrupt and distort student's educational trajectories

1.1 Related Literature

Broadly, this paper contributes to the literature on the impacts of resources in higher education. Most of the work in this space has used aggregate data, and in some cases even use variation in the incoming class sizes- which could induce course scarcity- as instruments for variation in dollars spent per student. 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 bachelor's 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 that budget cuts reduce the number of bachelor's degrees, driven by a decrease in persistence among students who were already enrolled rather than decreases in matriculation rates. The authors write that, "our results are consistent with the much broader trend of informal capacity constraints in public institutions, including reduced course offerings [and] long waitlists," yet they do not have data to test this mechanism directly (Deming and Walters, 2017).

Notwithstanding the work using aggregate data, there is limited causal evidence on micro-level 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 (Bettinger and Baker, 2014), or full-service wrap-around programs such as the CUNY ASAP experiment (Scrivener et al., 2015).[1] These studies generally have found positive effects.

To the best of our knowledge, there are two other papers that estimate the effects of course scarcity (Kurlaender et al., 2014; Neering, 2018). Both use administrative data from public, four-year universities in California and instrument for course shutouts using variation in the timing when students are first allowed access to the course registration systems. In Kurlaender et al. (2014), students at U.C. Davis (a moderately selective school) were put into registration priority blocks where all students in a higher priority block gained access before those in lower priority blocks, but within each block the day on which any given student could first register was randomly assigned. In Neering (2018), students in an anonymous public university were assigned to a priority sequence based on the first three letters of a student's last name, such that students randomly assigned an earlier time in one term are intentionally assigned a later time the next term. Kurlaender et al. (2014) instruments for the average number of shutouts a student experiences over their first four years to estimate the impact of a shutout on bachelor's degree completion and time to degree. The paper does not detect any effects of course shutouts. Neering (2018) instruments for the number of shutouts a student experiences in a given term and finds that shutouts reduce the number of credit units students attempt in that term, which seems to be offset by a rise in the rate at which students enroll over the summer. Consistent with Kurlaender et al. (2014), the author finds no downstream effects on graduation rates or time to graduation.

This paper extends the literature on course shutouts in two ways. First, we offer a new identification strategy, which directly compares students just able to get off of waitlists and into their desired courses to those who are stuck on the waitlist. Second, the granularity of the registration attempt data allow the analysis to examine heterogeneity by course characteristics (such as the subject of the course, or how popular the course is). Third, this is the first paper to document the impacts of course shutouts in a community college, where there are at least four reasons why course scarcity may be more salient. On the

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

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. These first three 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 (IHE, 2009). Finally, section enrollment at many community colleges in California is capped at 40 students due to classroom size, while class sizes at four-year schools may be allowed to expand more readily. The potential for sectoral heterogeneity leave a gap in the current understanding of the effect of course capacity constraints.

This paper also contributes to a small 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. Unlike other studies of course scarcity, the approach in this paper can be applied in settings where registration priority is not randomly assigned.

Finally, findings from this study can speak to documented longterm trends in the U.S., including the downward trend in bachelor's degree completion rates conditional on enrolling in 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 Institutional Background

The study uses administrative data from De Anza Community College, a large two year college located in the Bay Area which is part of the California Community College system, the largest higher education system in the United States. 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 in the US ($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, yet enrollment is much lower during the summer term.[2]

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.[3] 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 accumulation of human capital and production of labor market skills.

[2] Curious readers can see Fairlie, Hoffmann, and Oreopoulos (2014) for more details about De Anza Community College.

[3] See U.S. Department of Education (2017); CCCCO (2012); and Sengupta and Jepsen (2006).

Most pertinent to this study, De Anza is a likely setting for observing course scarcity due to non-selective admissions, low tuition, small and capped 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 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.[4] Furthermore, De Anza can not simply increase the number of students permitted into a section. Class sizes are set around the 40 student mark. Changes in class sizes are limited by available classroom configurations and need to be approved by the faculty labor union.

[4] Larger classes also count as double or even triple teaching credit for instructors.

2.1 Data Sources

This study benefits from access to community college institutional records and data from the National Student Clearinghouse (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 associate 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 a student identifier, 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, which records enrollment at most postsecondary institutions in the United States, through the summer of 2016. The NSC also provides data on degrees earned from these institutions, supplementing administrative records on degree completion from De Anza. This allows us to examine effects on certificate and associate degree completion from two-year colleges as well as bachelor degree completion from four-year schools many years after a students' registration attempt at De Anza.

2.2 Section Enrollment

The online registration process takes place one to 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, we conduct all analysis 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, 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. We check for robustness to the policy by restricting the analysis to attempts between 2008-2010 in Table A11.

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 being enrolled in the waitlisted section at the end 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 being enrolled after the add/drop period a few weeks into the term.

2.3 Sample Characteristics

Students are part of the sample if they registered for a course waitlist 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 focuses on students attempting to get a two year associate degree or transfer to a four year institution, and for whom enrolling in a bachelor's program in a four-year institution could be considered a reasonable substitute. This allows for ease of interpretation and makes a cleaner comparison to previous studies on course shutouts at four-year schools. Upon enrolling, students are asked to declare their educational goal or intention. 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 a four-year school, earn an associate degree, or who are undecided. The analysis is robust to including all students though. In addition, we exclude registration attempts in the optional summer term from the analysis sample.[5]

We focus on the first waitlist a student ever signed up for in order to avoid dynamic RD issues. While students may sign up for waitlists in subsequent terms, the analysis is explicitly testing the hypothesis that missing a waitlist cutoff influences whether a student appears in a subsequent semester. In addition, students may sign up for another waitlist in the same term. To the extent that the first waitlist a student signs up for represents the course that they most desire to enroll in, the analysis can be thought of as the effect of scarcity in the courses students most care about. Ultimately, the results are robust to including all waitlists and clustering standard errors at the student level.

Table 1 reports summary statistics at the section and student levels. Column (1) of Panel A 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

[5]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.

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 other courses. For classes that were oversubscribed, the average waitlist 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. By definition, these sections all had waitlists. 34% of sections included in the analysis were in STEM fields, 28% where in arts and humanities, 12% were social science courses, and 26% fell into other subject areas. Average waitlist lengths at the end of the registration period for sections in the analysis were slightly lower, at 8.01 students.

Panel B shows descriptive statistics for students in the analysis compared to the California average. Column (1) reports demographics for all two-year colleges in California from the Integrated Postsecondary Education Data System (IPEDS). Column (2) contains information for all students who ever enrolled or attempted to enroll for a course at De Anza Community College during the sample period, as measured by the administrative registration records, and 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 California average. The ethnic breakdown reflects the demographics of the Bay Area: in Column (2), 40% of students are Asian and 26% are White, while Black and Hispanic students make up only 19% of the student body. Relative to the state average, De Anza students are much less likely to be underrepresented minorities and less likely to receive financial aid.

As show in Column (3), the analysis sample contains registration attempts from 4,258 unique students. These students are more likely to receive financial aid and is younger than the De Anza student population. Students in Column (3) take an average of 1.81 courses in their first observed term relative to the population average of 1.70. Finally, in-sample students appear on 1.01 waitlists during the registration period in their first term, on average. 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 just 0.42.

De Anza students as a whole are thus less likely to sign up for waitlists and take fewer courses. Like the differences in age, 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.

3 Empirical Strategy

The analysis employs a fuzzy regression discontinuity design using waitlist queues to form a running variable. To illustrate the intuition behind the design, suppose a course section has a waitlist 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. While the decision to sign up for a waitlist is clearly endogenous, it is difficult to anticipate how many spots will open for any given section, and therefore how deep into the queue admission offers will be extended. This makes the cutoff very difficult, if not impossible, 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. 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 waitlist. A student who enrolls in a section without ever having been on a waitlist also has a position P_i. However, X_i, the initial waitlist position, is only defined for students who enter a waitlist 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 D_i, the number of students who registered before student i and dropped out during the registration period after student i registered. 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- those with $RV_i = 0$- to those who just missed it- 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 to a waitlist that already has two people on it, 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.[6]

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.That is, students with $RV \leq 0$ might not actually be enrolled in the section at the end of the registration period.[7] 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. $NotEnroll_{ist}$ is a measure of rationing and indicates the treatment. It is one if the student does not enroll in her desired section s in term t during the registration period, and zero otherwise. Let $Y_i(NotEnroll_{ist} = 1)$ be her educational outcome if she does not enroll in her preferred section and $Y_i(NotEnroll_{ist} = 0)$ be her educational outcome if she does. The analysis estimates $\mathbb{E}[Y_i(NotEnroll_{ist} = 1) - Y_i(NotEnroll_{ist} = 0) \mid RV_{ist} = 1]$. This is interpreted as the local average treatment effect (LATE) for compliers, students who are rationed out of a section if they miss the cutoff and 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. Students discouraged by a waitlist cutoff could be less motivated, less organized, or both. Furthermore, they may be less savvy navigators of institutions for reasons that reflect social inequality.

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 $RV_{ist} \in [0, 1]$:

[6] In fact, the construction of a cutoff time fully depends on the construction of the current running variable. That is, without a cutoff waitlist position, there can be no cutoff time. Appendix C tests the robustness of the results to a time-based running variable; the findings remain similar.

[7] By definition, students with $RV > 0$ could not have enrolled during the registration period though. In this sense, we observe only one-sided noncompliance.

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

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

where $NotE\hat{n}roll_{ist}$ 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 fixed effects, special admit status, special program status, as well as indicators for missing variables. 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.

Local randomization is appropriate for settings with extremely discrete running variables, as opposed to the more commonly used RD assumptions involving continuity of the regression function, which require a continuous running variable.[8] In fact, Cattaneo, Idrobo, and Titiunik (Forthcoming) argue that in settings with a very discrete running variable, local randomization is "possibly the only valid method for estimation and inference." The full set of assumptions include

[8]The results are robust to using a larger bandwidth and treating the running variable as if it were continuous, however.

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.

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 out of her control.[10] 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 our sample which is mostly incoming students who may not know many people. Section 3.3 formally tests for manipulation around the cutoff.

[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).

[10]In some sections, no students drop. In others, as many as twenty students drop. Among sections in the analysis, the 10th percentile number of drops is zero and the 90th percentile is five.

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 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. Although Lee and Card (2008) suggest clustering standard errors by the value of the running variable when the running variable is discrete, 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.

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, introduced in Frandsen (2017). 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 the author notes that 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. If there are about twenty support points within one standard deviation of the cutoff, then the rule of thumb maximum is 0.005, whereas if there are only six support points, it is 0.047. We test for manipulation using many values of k but note that in our context there are about eight support points within one standard deviation. The density test fails to reject the null of no manipulation at the five percent level for all values of k and fails to reject it at the ten percent level at values that are much smaller than the rule of thumb maximum in our context.

Another testable implication of the FRD assumptions is that predetermined characteristics should be balanced across the waitlist cutoff. Table 3 reports the results of linear regressions testing for imbalance across the waitlist cutoff in student characteristics.[11] The regressions condition on term by year fixed effects, registration priority fixed effects, and special student categories that affect registration priority. None of the student characteristics are statistically significant at the five percent level, although age is significantly different across the threshold at the ten percent level. The difference is small in magnitude however, equal to about four months. Furthermore, the covariates are not jointly significant, with a joint F-test yielding a p-value of 0.242. In addition, although the analysis relies only on variation between students with a running variable of zero or one, Figures B1 through B3 show that these baseline characteristics are similar across the cutoff when looking at a wider bandwidth.

The analysis also examines two other student characteristics that should be very similar

[11]The balance tests do not include registration time because it will mechanically be earlier for those with a running variable of zero.

across the threshold if there is no selection into treatment. First, students across the cutoff sign up for a similar number of other waitlists during the registration period- 1.07 on average for those with running variable of zero and 1.09 for those with running variable of one. Since this behavior occurs after students sign up to the initial waitlist, the student had some information about their likely schedule, even if it wasn't full information. Therefore, while this variable can't be tested for balance formally, the similarity in waitlist enrollment behavior is consistent with "as-if" random assignment. In addition, there was less than a day between the registration attempts of students in the same section with running variable zero or one. Specifically, the average amount of time between registration for these students was 19 hours, just 13% of a standard deviation between any two registration attempts in a waitlisted section.

4 Course Scarcity and Student Outcomes

4.1 First Stage Estimates

The first stage estimates can be easily seen in discontinuities at the cutoff. Figure 3a shows a discontinuity at the waitlist cutoff for enrollment in the waitlisted section at the end of the registration period. 64% of students just to the left of the cutoff- the last to have the opportunity to enroll in the section during the registration period- ended up enrolled in the waitlist section. In accordance with the definition of the running variable, students who miss the waitlist cutoff are not able to enroll during the registration period. Figure 3b 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% of students who do not miss the cutoff ultimately enroll in the waitlisted course, relative to 64% 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 and the desired course, 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. As reported in Panel A, 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. The results are nearly identical regardless of whether the analysis defines treatment at the section or course level though.

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 one through five years of the waitlisted term. Although the estimates identify effects by comparing students immediately on either side of the cutoff, Figures B4 through B6 visually depict the reduced form effects using a larger window. They plot the residuals of the main outcome variables, conditioned on the observable, pre-determined characteristics, and binned by values of the running variable. Figure B4 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 and whether they enrolled in any course in the following non-summer 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 a 0.016 percentage point jump up in percentage point jump in same-term dropout, and smaller, less prominent jumps in the other enrollment outcomes.

Figure B5 shows the reduced form impact on whether the student transfers to another two-year school. There is no noticeable rise in the share who transfer within one year, but a large 2.3 percentage point increase in the share of students who transfer within two years to another two-year school for those who missed the waitlist cutoff. Since the data only include enrollment in other two-year schools and not transcript records from those schools, the analysis cannot disentangle whether students transfer only to take their waitlisted course or for their entire course load. The difference in transfers to other two-year schools on either side of the cutoff gets smaller in later years. While reduced form effects of two percentage points may seem small, these translate to meaningfully large effects relative to the control means. For example, only 10.7% of students transfer to another two year within two years. Finally, Figure B6 shows that there is no noticeable change in transfers to four year schools across the cutoff at any time point.

Table 5 presents formal estimates of the LATE of being shut out of a course on enrollment patterns in the concurrent semester. 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 2.6 percentage points more likely to "drop out" in the waitlisted term; that is, to take no course at all that term. The estimated increase in same-term dropout is an increase of 25% relative to the same-term dropout rate of the control compliers, which is 10.4%. There are also negative, though not statistically significant, effects on course-taking for students who do take a course. The rise in same-term drop-out is accompanied by a 1.7 percentage point decrease in the probability of enrolling in three or more classes- a full course load- relative to a control complier mean of 55.9%. We also estimate a 0.8 percentage point decrease in the likelihood of enrolling in one to two courses that term relative to a control complier mean of 33.7%. These results cannot distinguish between a cascading effect- 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.

Table 6 shows the effect of course shutouts on transfer rates and degree completion for associate degrees, certificates, and bachelor's degrees. Students were no more or less likely to transfer to another two or four-year school within one year of being stuck on a waitlist. Taken together with the increase in same-term dropout at De Anza, this suggests that course scarcity increases the likelihood of dropout from college altogether for that term.

This dropout effect does not persist, however. 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 10.5%, which means the transfer rate increases by 34%. The point estimates for transfers to other two-year schools within three, four, and five years are also meaningfully large, 2.6, 2.6, and 2.7 percentage points respectively, but not statistically 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 associate degrees or certificates from De Anza or any other school, or bachelor's degrees up to five years out.

In general, the three most frequent recipients of De Anza's transfer students are: Foothill 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. Foothill college in particular is almost seamlessly integrated, with cross-registration between De Anza and Foothill being common and easy to do because it uses the same registration system.[12] However, as shown in Table A2, when estimating the treatment effect on attending each of these alternative

[12]To be clear, although students often take some classes at De Anza and others at Foothill in the same term, the data consists only of course registration attempts at De Anza.

schools separately, there is a statistically significant increase in enrollment at Evergreen, San Jose City College, and all other two-year schools, but not at Foothill. It's likely that students consider classes at Foothill as part of the initial choice set when they are registering, and not as a back-up option after the fact. 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. In addition, both by revealed preference and by online ranking services such as NICHE and Wallethub, which consistently rank De Anza above Evergreen and San Jose City, it is likely that students are worse off from having to substitute for the courses they need at these common alternatives.

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 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. Course popularity and subject are meant to test the idea that not all courses are equally important to a student's educational and labor market goals.

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, either gender or ethnicity.

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 across the sample period 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. Indeed, course catalogs confirm that these five classes were all prerequistes for a variety of other courses at the college. Furthermore, they were part of the Intersegmental General Education Transfer Curriculum (IGETC), which allows them to be easily transferred toward a bachelor degree in the UC system. As shown in Table A5, the point estimates for same semester drop-out are more than twice as large for the top five most popular classes, although they are not statistically different from each other. 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 less popular classes cause relatively larger, though not statistically significant, decreases in full-time enrollment instead. This suggests that students enrolled in the most popular classes are relatively less attached to college. Finally, as shown in Table A6, differences in impact by subject matter such as STEM, arts and humanities, social studies, and other subjects, however, are minimal.

Interesting dynamics emerge in transfer and degree completion by ethnicity categories. 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, multi-racial students, and students who do not fit into any other category. The point estimates are plotted in Figures 4 and 5.

There is a divergence in transfer responses by ethnicity. As seen in Figure 4, 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. For these students, transfers to two-year schools continue to increase every year through five years out. Meanwhile, other students do not transfer to two-year schools at an appreciably high rate, including near zero point estimates for Asian students and negative point estimates for white students.

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 5, 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 bachelor's degree completion for Asian students. Indeed, Figure 6 shows a positive effect of rationing on bachelor's degree completion among Asian students, especially at the five year mark. There is no impact on bachelor's degree completion for URM students, although the control complier mean for this group is near zero for the first three years after the waitlist and still quite low at 5.8% in the fifth year out. Finally, there is evidence that bachelor's degree attainment among White students is hampered by course rationing. Being rationed out of a course reduces bachelor's degree completion within five years by 56% for White students, relative to a control complier mean of 13.4%. The estimates plotted in Figure 6 can be found in Table A9.

This analysis suggests that students use the community college system differently in California. Perhaps students better prepared to navigate the college landscape, as proxied by ethnicity, strategically enroll in community college after high school because it is easier to get into a UC school as a community college transfer. For example, Berkeley and UCLA acceptance rates are almost twice as high for transfer students than for freshman admits. Anecdotally, these statistics seem well known on college discussion forums. The results highlight the many potential responses to course scarcity in a community college setting- some transfer to four-year schools thus accelerating their time to a bachelor's degree, while others transfer to lower-quality two year schools.

4.4 Sensitivity Analysis

The results of this paper are robust to several design decisions. First, as is standard in a regression discontinuity analysis, the analysis checks whether there are treatment effects at placebo thresholds.[13] Figure 7 plots the reduced form coefficients the two main outcomes

[13]An FRD that relied on continuity assumptions might also check for sensitivity to bandwidth choices and controlling for different polynomials of the running variable. The local randomization assumptions are only valid within one position of the cutoff, however. In particular, conditional independence does not hold

affected by course shutouts, estimated for ten different waitlist thresholds.[14] The outcomes are: took zero courses in the waitlisted term and transferred to another two-year school within two years. Table A10 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 A11 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 Table A1 or to using student's first waitlist. Column (2) restricts the sample to students who declared an intention to transfer to a four year. Column (3) includes only terms after 2007, when documentation on enrollment rules is available (see section 2.2 for a discussion of the issue). Column (4) uses the waitlist cutoff to instrument for course enrollment, rather than course section enrollment. Finally, Column (5) uses the main analysis sample but treats the running variable as if it were continuous, performing a traditional regression discontinuity analysis using a bandwidth of ten and a linear function form.

The estimates on taking zero courses in the waitlisted term are still positive and all but one are statistically significant, though the magnitudes are somewhat smaller than those reported in Table 5. Worth noting, although column (3) is an outlier in terms of magnitude,

as the bandwidth is increased. This is not surprising because increasing the bandwidth creates a comparison between students who signed up to the waitlist at increasingly far apart in time. Given that the identification is only valid within one position around the cutoff, testing sensitivity to bandwidth and functions of the running variable are not relevant.

[14]We perform this placebo threshold exercise using the reduced form rather than the two-stage least squares estimates because the first stage is zero at placebo cutoffs by constuction. That is, all students to the right of the true cutoff, where the running variable is equal to zero, were not able to enroll in the waitlisted section at the end of the registration period.

including only years after 2007 greatly reduces the sample and roughly doubles the size of the standard errors relative to the main result. As such, it is not statistically different from the main result. In addition, the analysis is nearly identical both when instrumenting for course enrollment rather than section enrollment and when treating the running variable as if it were continuous.

4.5 Complier Densities

This section estimates outcome densities for treated and untreated compliers in order to better understand how enrollment patterns change. Up to this point, the analysis has looked at discrete changes in course load, examining whether students respond to course shutouts by dropping out, taking one or two courses, or three or more. This analysis may mask greater heterogeneity at different points in the course load distribution. Following Abdulkadiroglu, Pathak, and Walters (2018), the paper 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 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.[15]

Figure 8a shows the complier densities for the number of courses a student is enrolled in after the add/drop date. Figure 8b shows the densities for the time it takes students to earn an associate degree, certificate, or bachelor's degree. For ease of interpretation, students who

[15]For more examples and discussion of estimating complier densities, see Angrist et al. (2016); Walters (Forthcoming).

do not earn a degree within five years are coded as receiving a degree in six.

The orange dashed line represents the density for compliers who missed 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 access, 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 who just miss the waitlist cutoff to those who just make it, the study finds that students who are not able to enroll in their preferred section due to oversubscription are 2.6 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 within two years. 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. 10.4% of control compliers dropped out in the waitlisted semester and 10.5% transferred to another two year within two years. Therefore, the results represent a 25% increase in same-term dropout and a 34% increase in transfers to other two-year colleges.

The results of our study contrast with earlier work that suggests course scarcity in college does not have downstream effects on student outcomes (Kurlaender et al., 2014; Neering, 2018). One likely reason for this contrast is that there are important institutional differences between community colleges and public four-year universities, the settings studied in earlier work. For example, community colleges have open enrollment policies, binding class size constraints, lower tuition rates, and heavy reliance on state funding. Moreover, 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; 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 often face more than one waitlist during their college careers. For example, 81% of students in our sample sign up for more than one waitlist. In this sense, we present 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.

While we find no average impacts of course rationing on transfers to four-year schools or bachelor's 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. Underrepresented minority 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 bachelor's 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 bachelor's degrees at such a low rate within five years of the waitlist that they exhibit a floor-effect- they can't do any worse. 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 not 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 their initial plans to start in a community college, leading them to transfer to a four-year sooner. One hypothesis is that ethnicity serves as a rough proxy for student resources and ability to navigate the higher education system. Finding differential 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 (Cohodes and Goodman, 2014).

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, this study continues the work of documenting and quantifying the effects of higher education funding and specifically funding for community colleges, 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 money influences student 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	0.34
% Arts/Humanities with WL	0.50	0.28
% Social Sciences with WL	0.60	0.12
% Other with WL	0.30	0.26
WL Length	8.98	8.01
WL Length (SD)	9.15	7.07
Observations	29,614	3,499

Panel B: Student-level statistics

	CA 2-year Public Colleges (1)	All De Anza (2)	Analysis Sample (3)
Female	0.53	0.52	0.51
Asian	0.13	0.40	0.44
White	0.27	0.26	0.24
Hispanic	0.45	0.14	0.16
Black	0.07	0.05	0.05
Ever Receives Aid	0.59	0.17	0.32
Age Under 25	0.63	0.59	0.80
Age 25 and Over	0.37	0.41	0.20
# Courses, first term		1.70	1.81
# Waitlists, first term		0.42	1.01
Observations	1,234,509	179,596	4,258

Notes: Panel A presents section-level statistics for De Anza Community College between Fall 2002 and Summer 2010. Column (1) reports the average share of sections with waitlists, by subject and before sample restrictions. For all sections in the analysis, column (2) reports the share in each subject. By definition, all sections in the analysis have a waitlist. The STEM definition follows the National Science Foundation. Waitlist length measures how many students remain on the waitlist at the end of the registration period for oversubscribed sections. In Panel B, column (1) describes student characteristics at all two-year colleges in the California, 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). Data for all two-year public colleges in CA comes from IPEDS for Fall 2014, except for financial aid receipt which is from the 2014-2015 school year. In column (1), financial aid receipt and age represent a cross section of all undergraduates at public 2-year schools in CA. In columns (2) and (3), a student is counted as receiving aid if they received it at any time in the sample period and age represents their age in their first term in the sample period. The number of courses is the number a student was enrolled in after the drop date in the first observed term. 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.

Table 2: Frandsen Manipulation Test for Discrete Running Variables

Nonlinearity Parameter (k) (1)	P-value (2)
0.005	0.078
0.010	0.091
0.015	0.115
0.020	0.148
0.025	0.185
0.030	0.234
0.035	0.291
0.040	0.350
0.045	0.416
0.050	0.483

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 and Column (2) reports the p-value of a test of the null hypothesis that no manipulation occurred.

Table 3: Test for Balance of Pre-determined Student Characteristics Across the Waitlist Cutoff

	Coefficient (1)	Standard Error (2)	P-Value (3)
White	-0.019	0.013	0.142
Asian	0.004	0.015	0.807
Hispanic	0.018	0.011	0.103
Black	0.001	0.007	0.883
Other Race	0.007	0.007	0.290
Missing Race	-0.011	0.008	0.184
Female	-0.017	0.015	0.257
Missing Gender	0.002	0.001	0.165
Age	0.351	0.199	0.078
Missing Age	-0.001	0.001	0.315
International Student	0.004	0.014	0.792
Received Financial Aid	-0.012	0.014	0.396
Missing Financial Aid Receipt	0.001	0.002	0.775
First Time Student	0.001	0.003	0.751
Joint p-value			0.242
Observations (N_l/N_r)	1,977	2,281	

Notes: Each row reports results from a linear regression of the covariate on an indicator for missing a waitlist cutoff, term by year fixed effects, registration priority fixed effects, and indicators for special student categories. The sample includes students within one position of the waitlist 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 an F test of whether the differences in each characteristic are jointly significant, conditional on the fixed effects and special student categories previously listed.

Table 4: First Stage Effect of Missing the Waitlist 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	0.499
F-Statistic	3526	3583
Controls	N	Y
Control Mean	0.641	0.641
Panel B: Course Enrollment		
Missed WL Cutoff	-0.645***	-0.648***
	(0.011)	(0.011)
R-squared	0.485	0.494
F-Stat	3516	3555
Controls	N	Y
Control Mean	0.655	0.655
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, where enrollment is equal to one if the student was enrolled at the end of the advanced registration period. 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, first time students, special student status, special program status, registration priority fixed effects, term by year fixed effects, and indicators for missing variables. The control mean is the mean of the dependent variable for students with a running variable of zero. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table 5: Effect of Missing the Waitlist Cutoff on Course Load and Persistence

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero (1)	One or Two (2)	Three or More (3)	Next Term (4)
2SLS	0.026*	-0.008	-0.017	-0.019
	(0.014)	(0.021)	(0.022)	(0.021)
Reduced Form	0.016*	-0.005	-0.011	-0.012
	(0.009)	(0.014)	(0.014)	(0.013)
CCM	0.104	0.337	0.559	0.688
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. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table 6: Effect of Missing the Waitlist Cutoff on Transfers and Degree Completion

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	0.026	0.027
	(0.011)	(0.015)	(0.017)	(0.019)	(0.020)
CCM	[0.056]	[0.105]	[0.152]	[0.189]	[0.222]
Reduced Form	0.006	0.023**	0.017	0.017	0.017
	(0.007)	(0.010)	(0.011)	(0.012)	(0.013)
Transfer Four-Year	0.000	0.009	0.004	0.013	0.019
	(0.008)	(0.012)	(0.017)	(0.019)	(0.020)
CCM	[0.030]	[0.062]	[0.141]	[0.190]	[0.219]
Reduced Form	0.000	0.006	0.003	0.008	0.013
	(0.005)	(0.008)	(0.011)	(0.012)	(0.013)
Certificate/ Associate	0.003	-0.002	-0.010	-0.016	-0.011
	(0.005)	(0.009)	(0.013)	(0.015)	(0.015)
CCM	[0.008]	[0.033]	[0.077]	[0.106]	[0.117]
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	0.006	-0.003	-0.002
	(0.003)	(0.003)	(0.006)	(0.010)	(0.014)
CCM	[0.002]	[0.006]	[0.014]	[0.043]	[0.094]
Reduced Form	0.003	0.001	0.004	-0.002	-0.001
	(0.002)	(0.002)	(0.004)	(0.007)	(0.009)
Observations (N_l/N_r)	1,977	2,281			

Notes: This table shows results from a 2SLS regression as in equation 3. The outcomes are indicators for transferring and degree completion at different time horizones: within one through five years of the waitlisted term. Associate and certificate completion data comes from both De Anza administrative records and the National Student Clearinghouse. The standard errors are in parentheses, with the control complier means (CCM) and the reduced form displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Figure 1: A Hypothetical Registration Log

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

Notes. 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

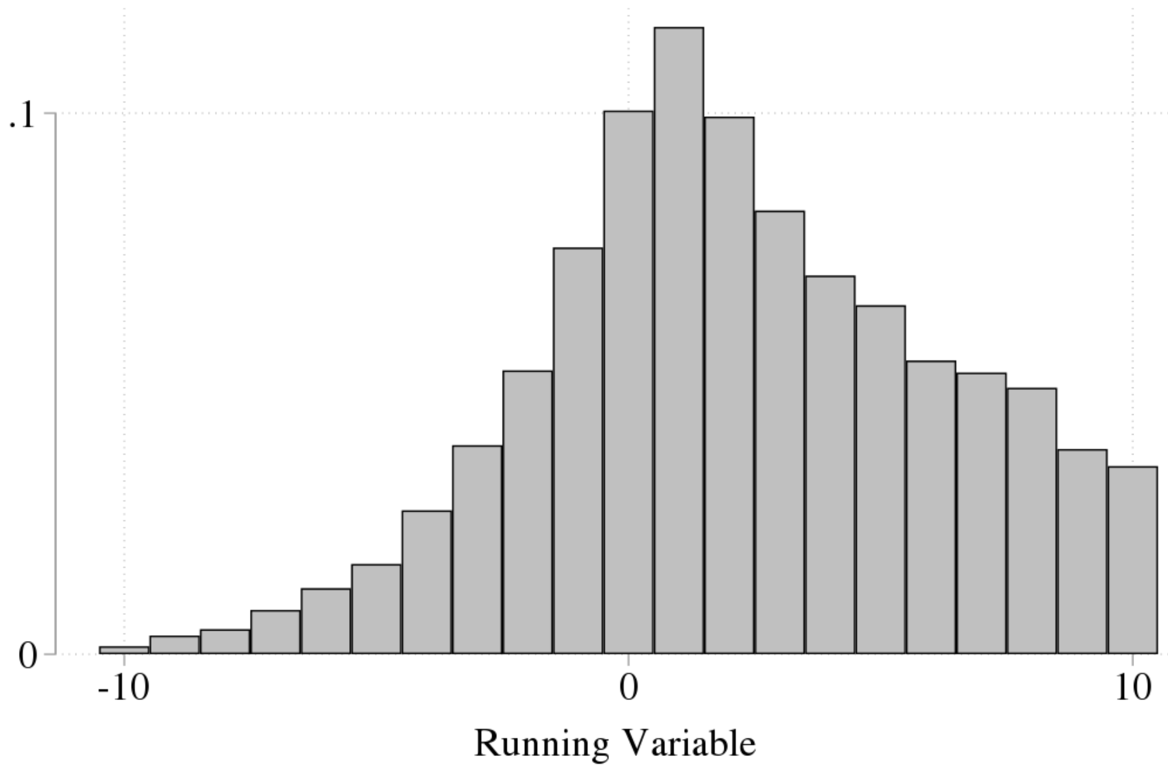
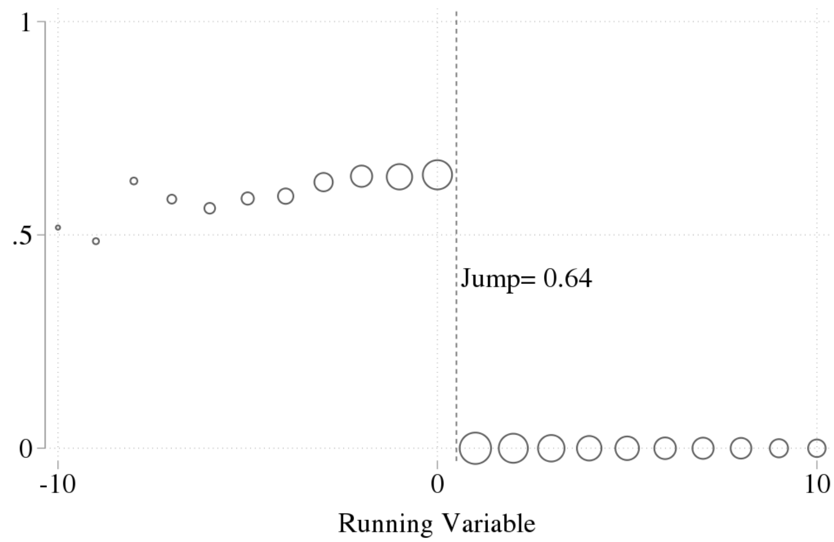
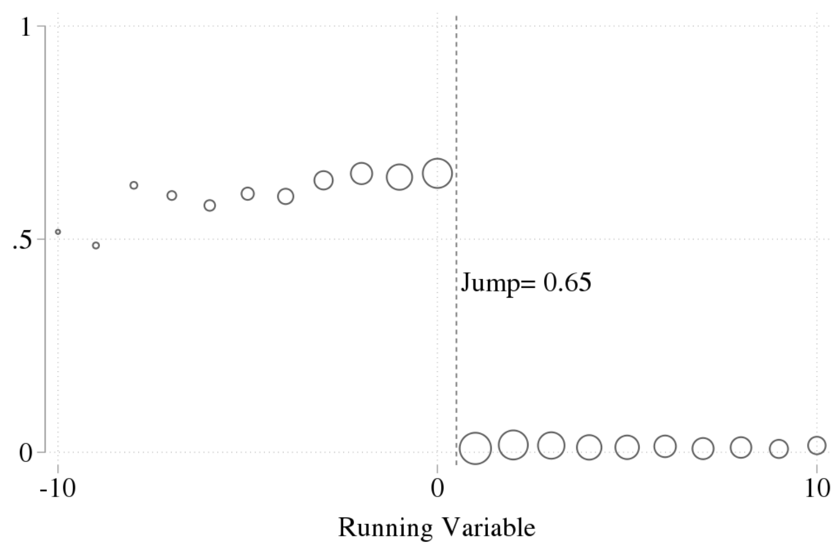


Figure 3: First Stage Effect of Missing the Waitlist Cutoff on Enrollment in Waitlisted Section and Course

(a) Enrolled in Waitlisted Section

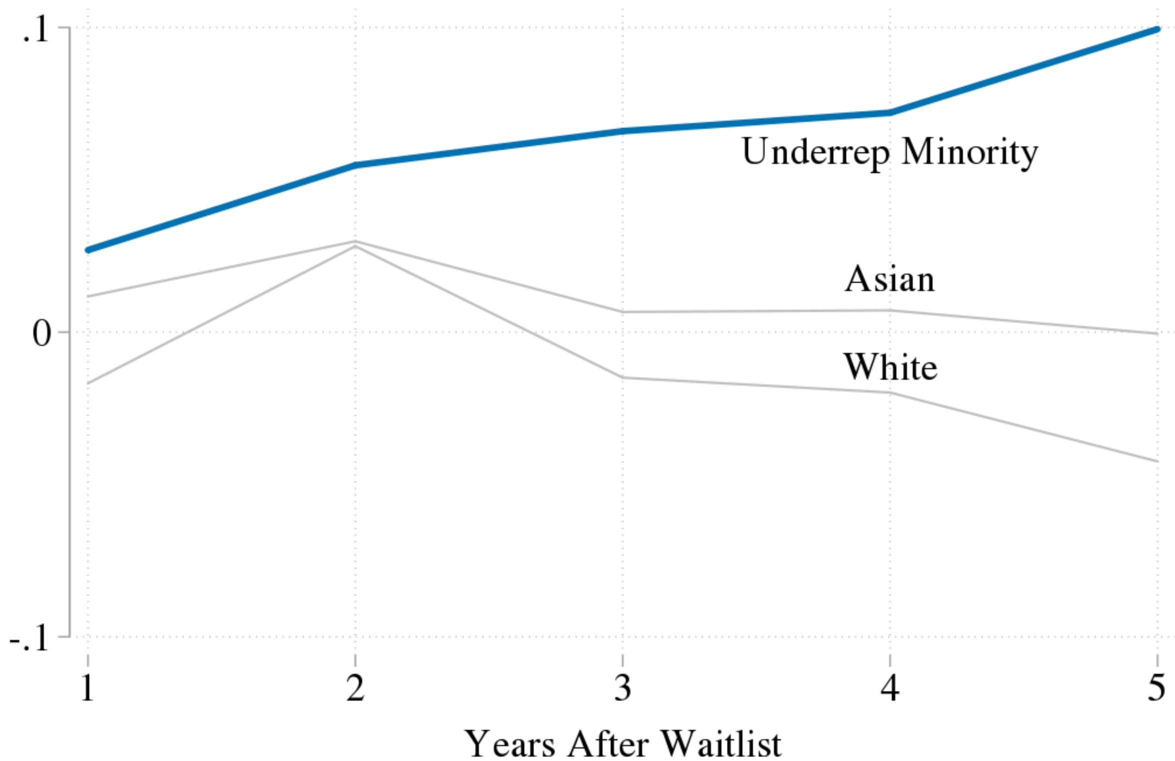


(b) Enrolled in Waitlisted Course



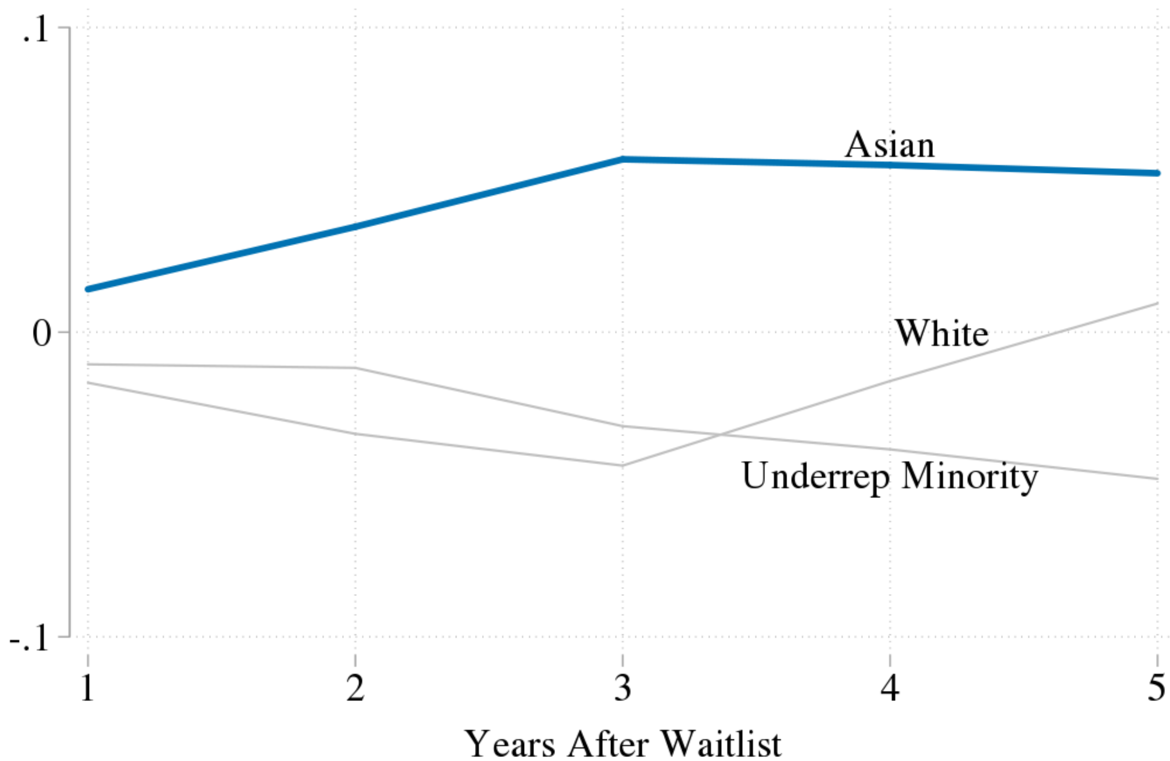
Notes. Each dot represents enrollment binned by the value of the running variable, where is enrollment is equal to one if the student was enrolled in the section or course at the end of the advanced registration period. Both section and course enrollment are equal to zero for students with a running variable greater than zero by construction. The size of the dot reflects the number of observations in each bin.

Figure 4: Effect of Missing the Waitlist Cutoff on Transfers to Other Two-Year Schools, by Ethnicity



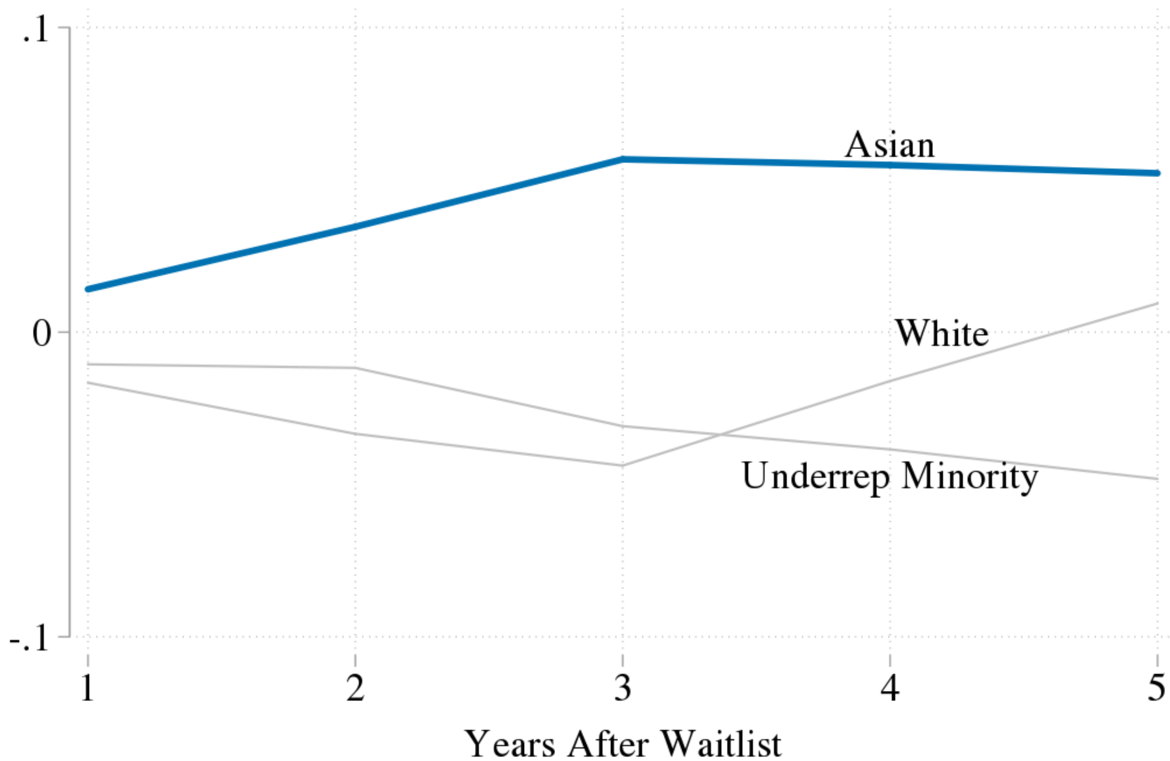
Notes. This figure shows results from a 2SLS regression as in equation 3, where effects are estimated separately by ethnicity. The outcomes are indicators for transfers to other two-year schools at different time horizons: within one through five years of the waitlisted term. All specifications include the covariates listed in Table 4. The exact point estimates and standard errors are reported in Table A7.

Figure 5: Effect of Missing the Waitlist Cutoff on Transfers to Four-Year Schools, by Ethnicity



Notes. This figure shows results from a 2SLS regression as in equation 3, where effects are estimated separately by ethnicity. The outcomes are indicators for transfers to four-year schools at different time horizons: within one through five years of the waitlisted term. All specifications include the covariates listed in Table 4. The exact point estimates and standard errors are reported in Table A8.

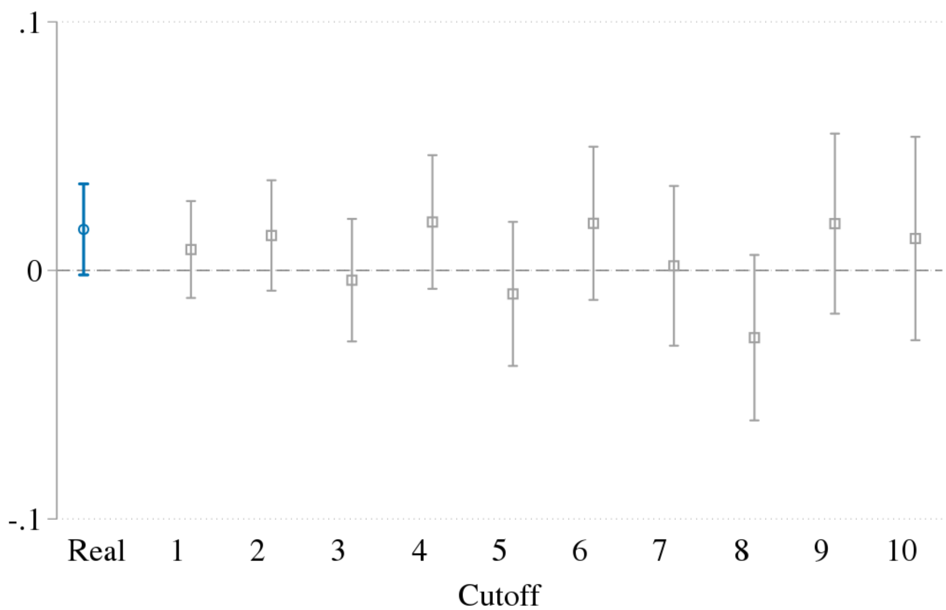
Figure 6: Effect of Missing the Waitlist Cutoff on Bachelors Degree Completion, by Ethnicity



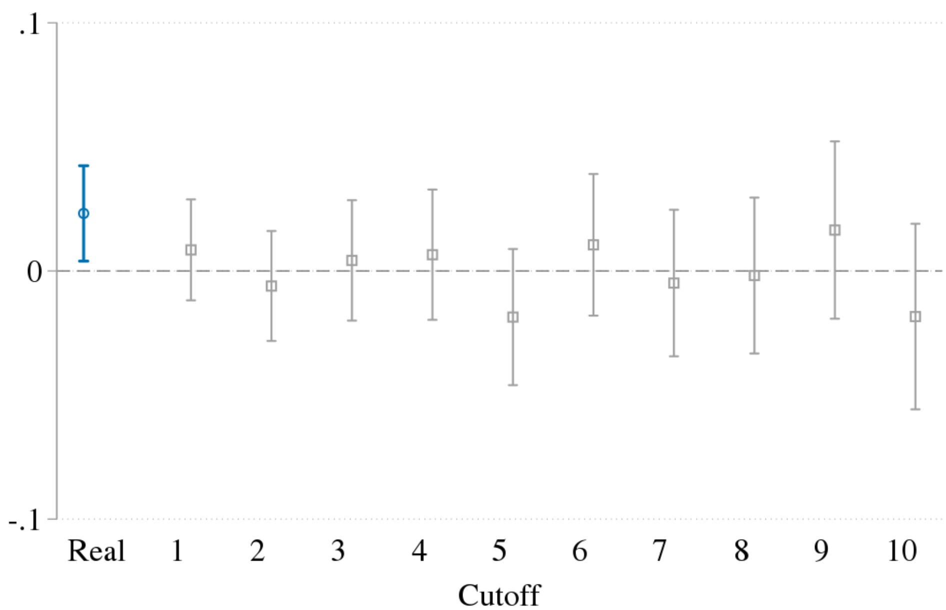
Notes. This figure shows results from a 2SLS regression as in equation 3, where effects are estimated separately by ethnicity. The outcomes are indicators for bachelors degree completion at different time horizons: within one through five years of the waitlisted term. All specifications include the covariates listed in Table 4. The exact point estimates and standard errors are reported in Table A9.

Figure 7: Reduced Form Effect of Missing a Placebo Cutoff

(a) Enrolled in Zero Courses



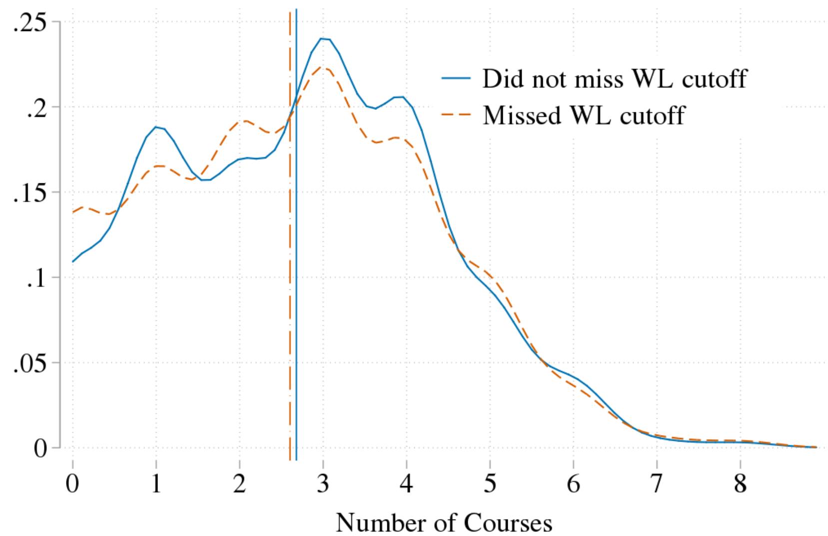
(b) Transferred to Another Two-Year School within Two Years



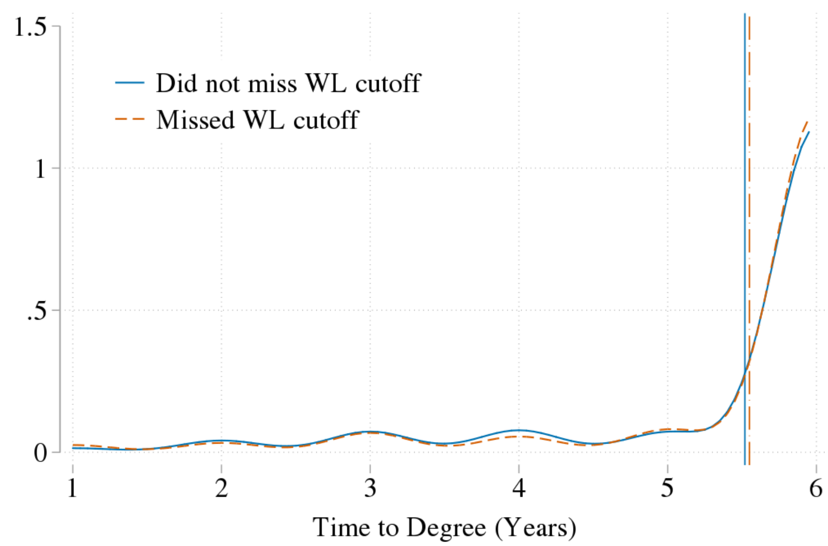
Notes. This figure plots point estimates and confidence intervals for the reduced form effect of missing a placebo cutoff using a bandwidth of one. For example, at the placebo cutoff of three, we show the effect of having a running variable of three relative to a running variable of four. The actual reduced form effect- where the cutoff is equal to zero- is shown in blue, whereas the other values on the x-axis represent the effects of the other placebo cutoffs.

Figure 8: Density of Potential Outcomes for Treated and Untreated Compliers

(a) Number of Courses



(b) Time to Degree



Notes. 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. The vertical lines represent the average outcomes for each group. Number of courses is defined as the number of courses a student was enrolled in after the add/drop date. Time to degree measures the number of years from the waitlisted term until a student earned any higher education degree, including associates, certificates, or bachelors degrees. Time to degree is equal to six for students who either take six years to complete a degree, or do not complete a degree within six years of the waitlisted term.

A Supplemental 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 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 community college students who might consider a bachelors degree at a four-year institution a reasonable substitute to their current program.

Table A2: Effect of Missing the Waitlist Cutoff on Transfers to Nearby Two-Year Schools within Two Years

Outcome	Foothill (1)	Evergreen Valley (2)	San Jose City (3)	Other Two-Year (4)
2SLS	-0.008	0.013**	0.015**	0.017**
	(0.010)	(0.006)	(0.006)	(0.008)
Reduced Form	-0.005	0.008*	0.010**	0.011**
	(0.007)	(0.004)	(0.004)	(0.005)
CCM	0.051	0.013	0.013	0.027
Observations (N_l/N_r)	1,977	2,281		

Notes: This table shows results from a 2SLS regression as in equation 3. The outcomes are indicators for whether the student transferred to Foothill College within two years of the waitlist in Column (1), Evergreen Valley College in Column (2), San Jose City College in Column (3), and any other two-year college in Column (4). The standard errors are in parentheses, with the control complier means (CCM) and the reduced form displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A3: Effect of Missing the Waitlist Cutoff on Course Load and Persistence, by Gender

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
Male	0.016	0.014	-0.030	-0.004
	(0.020)	(0.031)	(0.032)	(0.030)
CCM Male	0.108	0.310	0.581	0.689
N Male (N_l/N_r)	963/1,142			
Female	0.034*	-0.028	-0.006	-0.031
	(0.021)	(0.029)	(0.029)	(0.029)
CCM Female	0.100	0.362	0.537	0.687
N Female (N_l/N_r)	1,012/ 1,131			
P-value Male=Female	0.530	0.313	0.575	0.525

Notes: This table shows results from a 2SLS regression as in equation 3, where effects are 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 p-value from a test for the difference in point estimates between groups displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A4: Effect of Missing the Waitlist Cutoff on Course Load and Persistence, by Ethnicity

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
Asian	0.027	-0.006	-0.022	-0.023
	(0.022)	(0.032)	(0.033)	(0.031)
CCM Asian	0.094	0.287	0.619	0.758
N Asian (N_l/N_r)	860/988			
White	0.022	0.013	-0.035	-0.047
	(0.032)	(0.046)	(0.047)	(0.046)
CCM White	0.121	0.362	0.518	0.654
N White (N_l/N_r)	484/ 517			
URM	-0.000	-0.016	0.017	0.034
	(0.027)	(0.041)	(0.042)	(0.042)
CCM URM	0.101	0.390	0.509	0.623
N URM (N_l/N_r)	478/617			
P-value White=Asian	0.885	0.737	0.820	0.659
P-value URM=Asian	0.425	0.841	0.477	0.274
P-value URM=White	0.598	0.635	0.416	0.190

Notes: This table shows results from a 2SLS regression as in equation 3, where effects are estimated separately by ethnicity. 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 p-value from a test for the difference in point estimates between groups displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A5: Effect of Missing the Waitlist Cutoff on Course Load and Persistence, by Course Popularity

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero (1)	One or Two (2)	Three or More (3)	Next Term (4)
Top 5	0.057	-0.107*	0.050	-0.008
	(0.037)	(0.064)	(0.066)	(0.066)
CCM Top 5	0.071	0.304	0.625	0.705
N Top 5 (N_l/N_r)	170/209			
Other Courses	0.024	0.003	-0.027	-0.025
	(0.015)	(0.022)	(0.023)	(0.022)
CCM Other	0.107	0.340	0.552	0.687
N Other (N_l/N_r)	1,807/2,072			
P-value Top 5= Other	0.401	0.106	0.274	0.803

Notes: This table shows results from a 2SLS regression as in equation 3, where effects are estimated separately by popularity of the course. Top five courses are those that are the most frequently 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 p-value from a test for the difference in point estimates between groups displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

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Table A6: Effect of Missing the Waitlist Cutoff on Course Load and Persistence, by Waitlisted Subject

	# Courses Enrolled in Concurrent Term			Enrolled
	Zero	One or Two	Three or More	Next Term
	(1)	(2)	(3)	(4)
STEM	0.007	0.018	-0.025	-0.011
	(0.024)	(0.035)	(0.036)	(0.034)
CCM STEM	0.098	0.294	0.607	0.738
N STEM (N_l/N_r)	678/771			
Arts/Humanities	0.053**	-0.051	-0.002	0.021
	(0.027)	(0.038)	(0.039)	(0.039)
CCM Arts/Hum.	0.099	0.347	0.554	0.649
N Arts/Hum. (N_l/N_r)	542/663			
Social Studies	-0.015	-0.007	0.023	-0.092
	(0.041)	(0.062)	(0.062)	(0.062)
CCM Soc. Stud.	0.129	0.355	0.516	0.658
N Soc. Stud. (N_l/N_r)	242/261			
Other	0.050*	0.013	-0.063	-0.033
	(0.030)	(0.043)	(0.044)	(0.042)
CCM Other	0.105	0.373	0.521	0.680
N Other (N_l/N_r)	515/586			
P-value STEM=Arts/Hum	0.194	0.178	0.663	0.532
P-value STEM=Soc. Stud.	0.646	0.717	0.509	0.255
P-value STEM=Other	0.261	0.928	0.500	0.689
P-value Arts/Hum = Soc. Stud	0.164	0.545	0.742	0.124
P-value Arts/Hum= Other	0.940	0.264	0.295	0.348
P-value Soc. Stud.=Other	0.203	0.785	0.259	0.433

Notes: This table shows results from a 2SLS regression as in equation 3, where effects are estimated separately by the subject of the waitlisted course. Top five courses are those that are the most frequently 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 p-value from a test for the difference in point estimates between groups displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A7: Effect of Missing the Waitlist Cutoff on Transfers to Other 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	0.007	0.007	-0.000
	(0.016)	(0.022)	(0.027)	(0.029)	(0.031)
CCM Asian	0.046	0.090	0.148	0.185	0.217
N Asian (N_l/N_r)	860/988				
White	-0.017	0.028	-0.015	-0.020	-0.042
	(0.026)	(0.033)	(0.038)	(0.041)	(0.043)
CCM White	0.072	0.117	0.176	0.215	0.257
N White (N_l/N_r)	484/ 517				
URM	0.027	0.055*	0.066*	0.072**	0.099**
	(0.023)	(0.030)	(0.034)	(0.037)	(0.039)
CCM URM	0.049	0.110	0.114	0.175	0.206
N URM (N_l/N_r)	478/617				
P-value Asian=White	0.352	0.968	0.641	0.590	0.426
P-value Asian=URM	0.587	0.508	0.171	0.165	0.043
P-value White=URM	0.208	0.556	0.113	0.094	0.014

Notes: This table shows results from a 2SLS regression as in equation 3, where effects are estimated separately by ethnicity. The outcomes are indicators for transfers to other two-year schools at different time horizones: within one through five years of the waitlisted term. The standard errors are in parentheses, with the control complier means (CCM) and p-value from a test for the difference in point estimates between groups displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A8: Effect of Missing the 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	0.014	0.035*	0.057**	0.055*	0.052
	(0.012)	(0.020)	(0.028)	(0.032)	(0.033)
CCM Asian	0.025	0.063	0.142	0.206	0.237
N Asian (N_l/N_r)	860/988				
White	-0.017	-0.033	-0.044	-0.016	0.009
	(0.020)	(0.027)	(0.036)	(0.040)	(0.043)
CCM White	0.046	0.085	0.173	0.212	0.251
N White (N_l/N_r)	484/ 517				
URM	-0.011	-0.012	-0.031	-0.038	-0.048
	(0.014)	(0.019)	(0.027)	(0.032)	(0.034)
CCM URM	0.021	0.034	0.092	0.135	0.156
N URM (N_l/N_r)	478/617				
P-value Asian=White	0.182	0.042	0.028	0.169	0.431
P-value Asian=URM	0.170	0.091	0.024	0.040	0.036
P-value White=URM	0.800	0.512	0.772	0.664	0.295

Notes: This table shows results from a 2SLS regression as in equation 3, where effects are estimated separately by ethnicity. The outcomes are indicators for transfers to four-year schools at different time horizones: within one through five years of the waitlisted term. The standard errors are in parentheses, with the control complier means (CCM) and p-value from a test for the difference in point estimates between groups displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A9: Effect of Missing the 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	0.006	0.012	0.040	0.094
N Asian (N_l/N_r)	860/988				
White	-0.002	-0.008	-0.017	-0.039	-0.075**
	(0.007)	(0.009)	(0.015)	(0.024)	(0.031)
CCM	0.007	0.013	0.029	0.068	0.134
N White (N_l/N_r)	484/ 517				
URM	0.002	0.002	0.008	-0.014	-0.025
	(0.002)	(0.002)	(0.006)	(0.016)	(0.021)
CCM	0.000	0.000	0.003	0.025	0.058
N URM (N_l/N_r)	478/617				
P-value Asian=White	0.279	0.246	0.057	0.050	0.001
P-value Asian=URM	0.276	0.639	0.437	0.160	0.007
P-value White=URM	0.593	0.298	0.121	0.392	0.188

Notes: This table shows results from a 2SLS regression as in equation 3, where effects are estimated separately by ethnicity. The outcomes are indicators for bachelors degree completion at different time horizones: within one through five years of the waitlisted term. The standard errors are in parentheses, with the control complier means (CCM) and p-value from a test for the difference in point estimates between groups displayed below. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A10: Reduced Form 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)
0 (Real)	0.016*	0.023**	1,977/2,281
	(0.009)	(0.010)	
1	0.008	0.009	2,281/1,955
	(0.010)	(0.010)	
2	0.014	-0.006	1,955/1,613
	(0.011)	(0.011)	
3	-0.004	0.004	1,613/1,377
	(0.013)	(0.012)	
4	0.019	0.007	1,377/1,269
	(0.014)	(0.013)	
5	-0.009	-0.019	1,269/1,068
	(0.015)	(0.014)	
6	0.019	0.011	1,068/1,024
	(0.016)	(0.015)	
7	0.002	-0.005	1,024/969
	(0.016)	(0.015)	
8	-0.027	-0.002	969/746
	(0.017)	(0.016)	
9	0.019	0.016	746/684
	(0.018)	(0.018)	
10	0.013	-0.018	684/579
	(0.021)	(0.019)	

Notes: This table shows the coefficient from a regression of the outcome on an indicator for missing the placebo cutoff- equal to one if the student has the running variable of the cutoff 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. All columns include the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A11: Robustness Checks

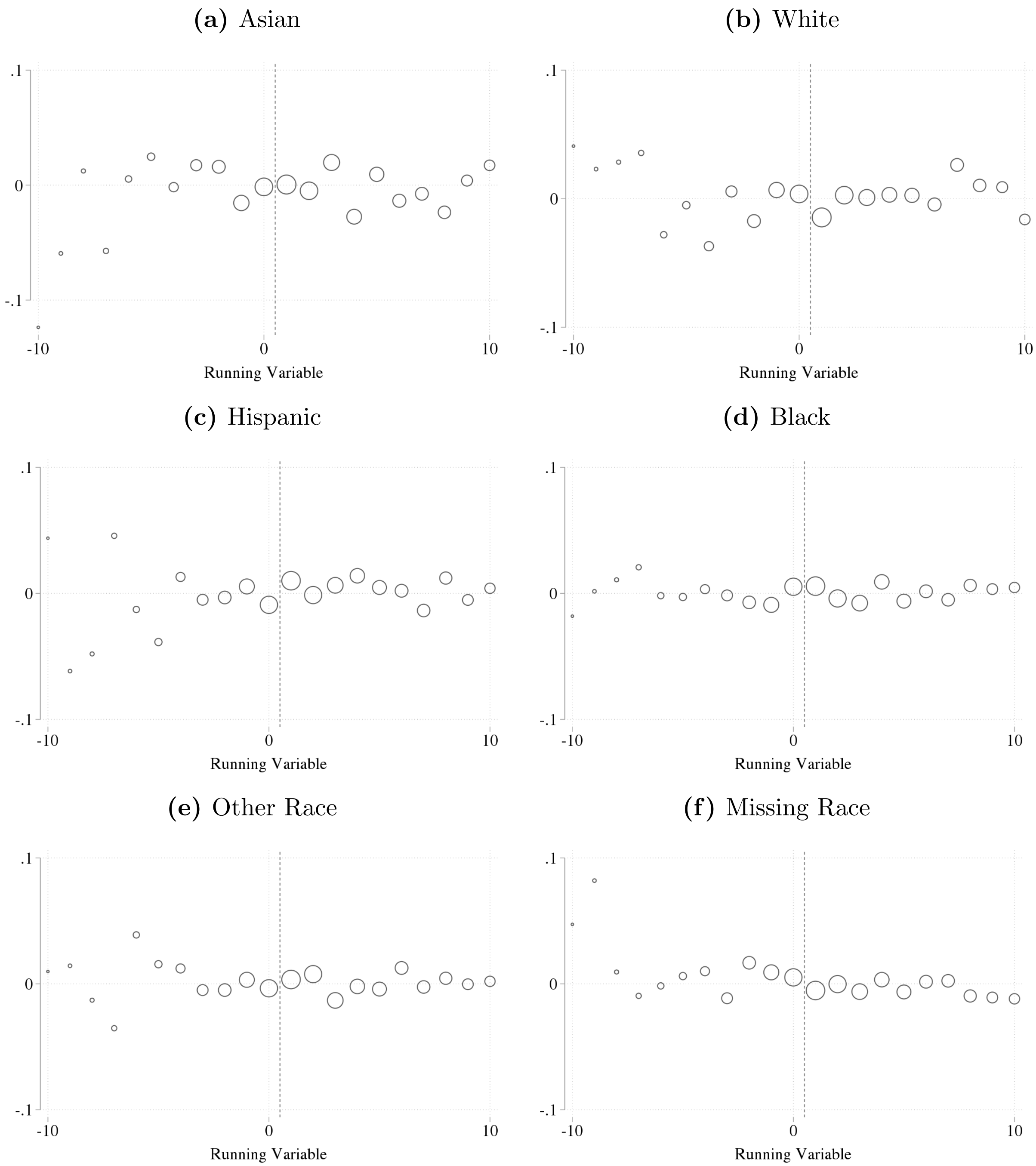
Outcome	No Sample Restrictions (1)	Intend to Transfer (2)	Post 2007 (3)	Course Enrollment (4)	Continuous RDD (5)
Enrolled in 0 Courses	0.012***	0.012***	0.007	0.025*	0.023**
	(0.003)	(0.004)	(0.026)	(0.014)	(0.012)
Enrolled in 1-2 Courses	0.003	0.005	-0.031	-0.008	-0.003
	(0.005)	(0.006)	(0.037)	(0.021)	(0.016)
Enrolled in 3+ Courses	-0.015***	-0.017***	0.024	-0.017	-0.020
	(0.005)	(0.007)	(0.038)	(0.021)	(0.017)
Observations (N_l/N_r)	30,329/37,103	17,338/20,873	637/692	1,977/2,281	6,659/12,986

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. Column (4) uses the sample in the main analysis but uses the waitlist cutoff to instrument for course enrollment, rather than course section enrollment. Column (5) uses the sample in the main analysis with a continuous regression discontinuous design with a bandwidth of ten and a linear function form. The standard errors are in parentheses. All columns include the covariates listed in Table 4. Standard errors are clustered at the student level when more than one observation per student is used, and are robust to heteroskedasticity otherwise. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

B Covariate Smoothness and Reduced Form Figures Further from the Cutoff

The main analysis relies on a local randomization approach to identify the effects of course shutouts on student outcomes. We use local randomization because the running variable is discrete. As such, the estimates are identified only from variation between students assigned a running variable of zero and one. Although we do not identify effects off of variation from values of the running variable further from the cutoff, it may still be useful to see the larger picture. This section shows a variety of figures in the spirit of a regression discontinuity design with a continuous running variable to show smoothness in both pre-determined covariates and discontinuities in the main outcome variables across the cutoff.

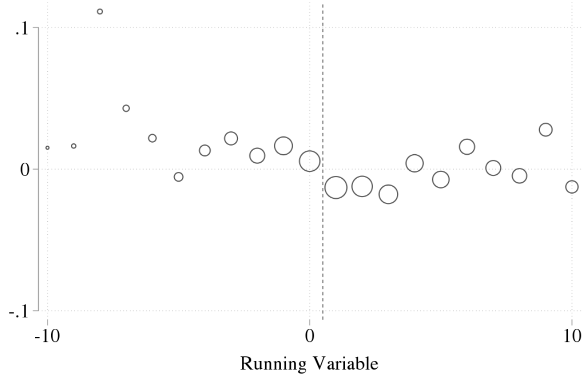
Figure B1: Covariate Smoothness Across the Waitlist Cutoff



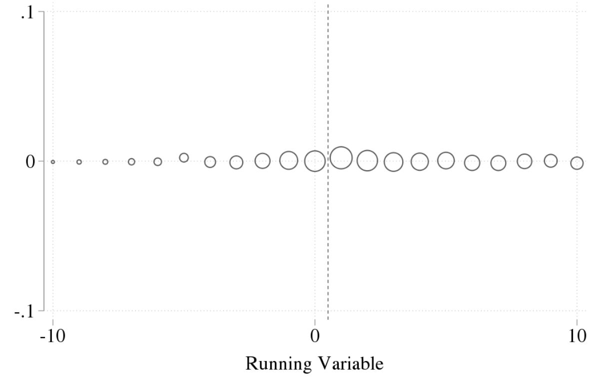
Notes. Each dot represents the mean of the residualized covariate, conditioned on the value of the running variable. The covariates are residualized by term by year fixed effects, registration priority fixed effects, and indicators for special student categories. The size of the dot reflects the number of observations in each bin.

Figure B2: Covariate Smoothness Across the Waitlist Cutoff

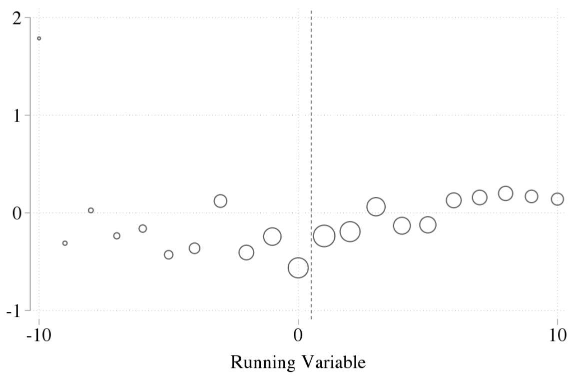
(a) Female



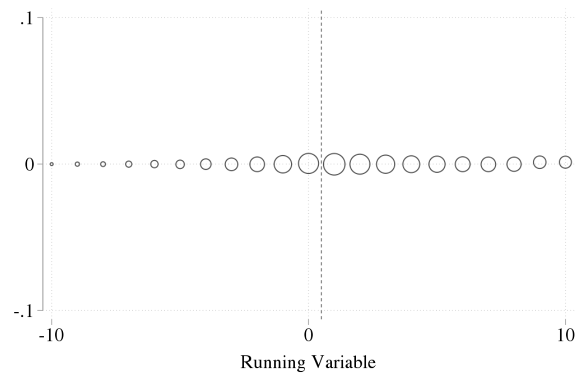
(b) Missing Gender



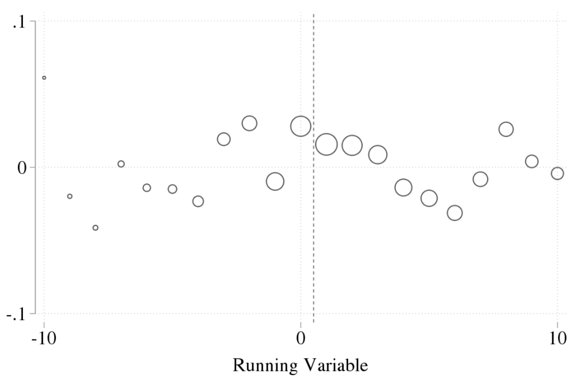
(c) Age



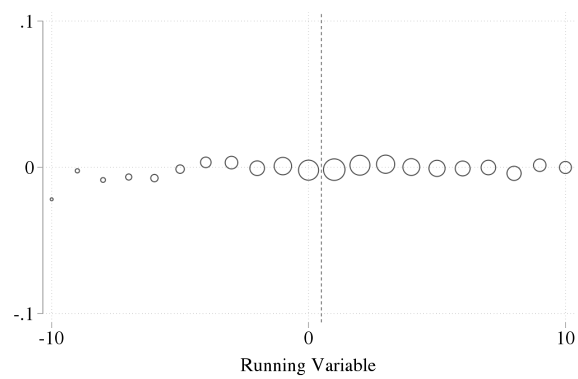
(d) Missing Age



(e) Received Financial Aid



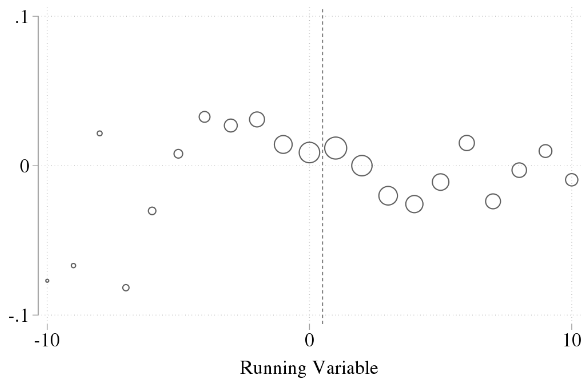
(f) Missing Financial Aid Receipt



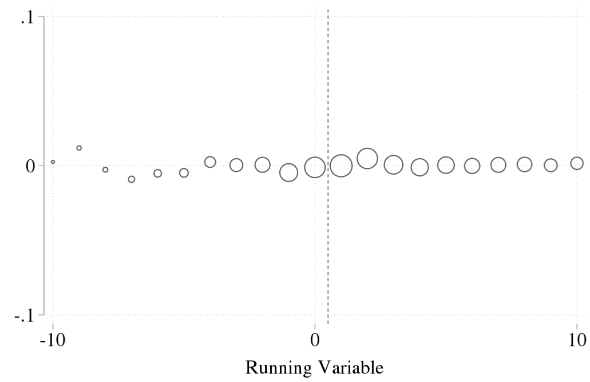
Notes. Each dot represents the mean of the residualized covariate, conditioned on the value of the running variable. The covariates are residualized by term by year fixed effects, registration priority fixed effects, and indicators for special student categories. The size of the dot reflects the number of observations in each bin.

Figure B3: Covariate Smoothness Across the Waitlist Cutoff

(a) International Student



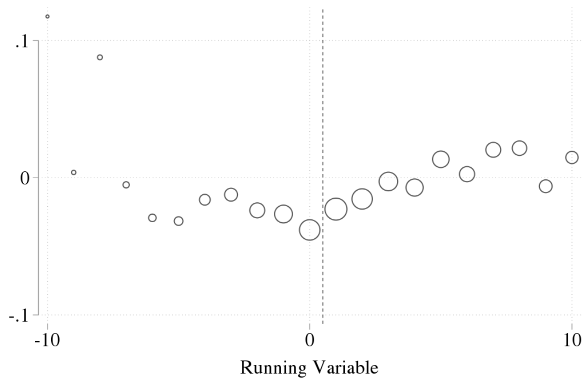
(b) First Time Student



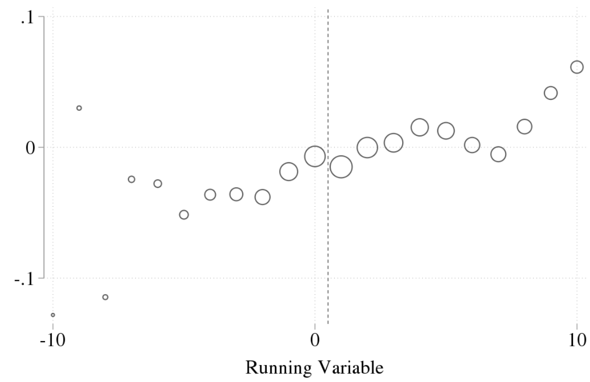
Notes. Each dot represents the mean of the residualized covariate, conditioned on the value of the running variable. The covariates are residualized by term by year fixed effects, registration priority fixed effects, and indicators for special student categories. The size of the dot reflects the number of observations in each bin.

Figure B4: Reduced Form Effect of Missing the Waitlist Cutoff on Course Load and Persistence

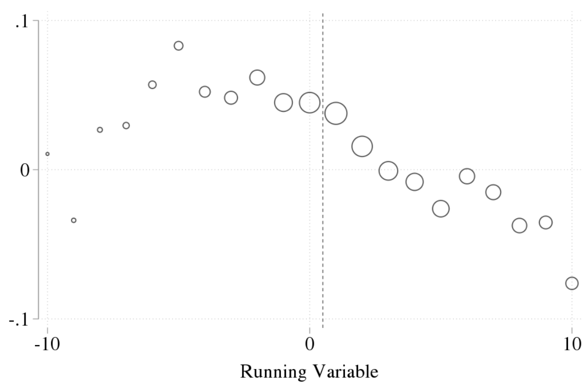
(a) Enrolled in Zero Courses



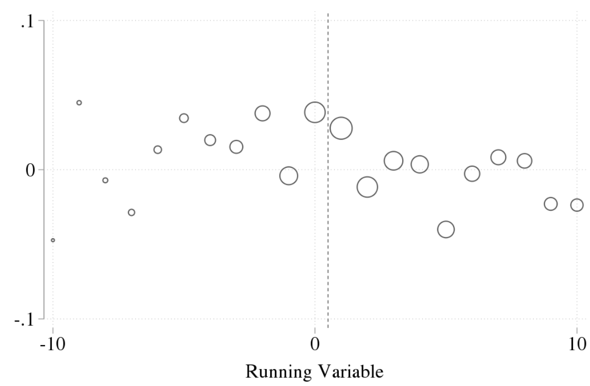
(b) Enrolled in One or Two Courses



(c) Enrolled in Three or More Courses



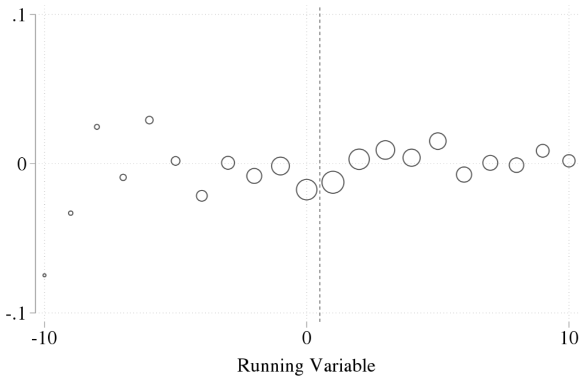
(d) Enrolled Next Term



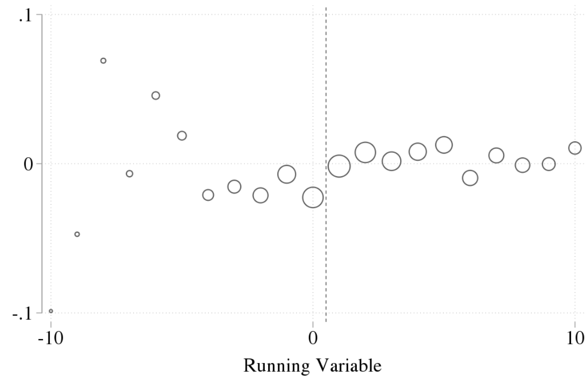
Notes. Each dot represents the mean of the residualized outcome, conditioned on the value of the running variable. The covariates are residualized by the control variables described in Table 4. Enrollment during the waitlisted term is defined as being enrolled after the add/drop period. The size of the dot reflects the number of observations in each bin.

Figure B5: Reduced Form Effect of Missing the Waitlist Cutoff on Transfers to Another Two-Year School

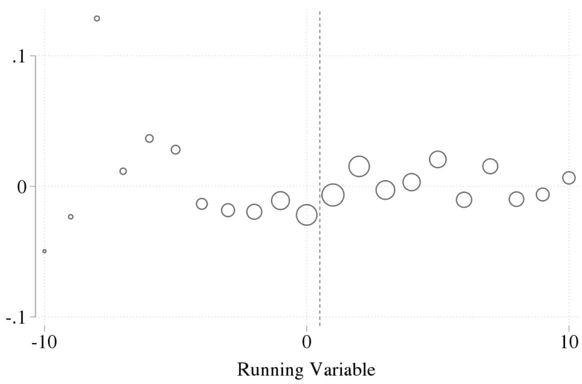
(a) Within One Year



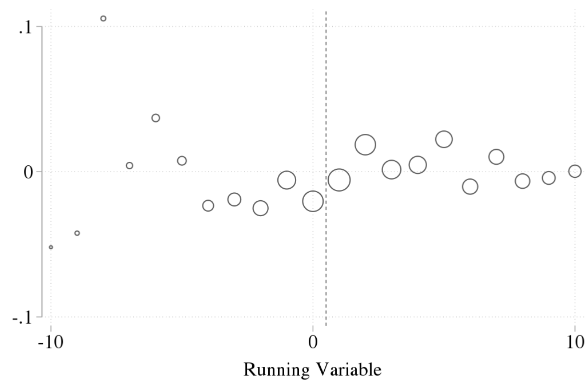
(b) Within Two Years



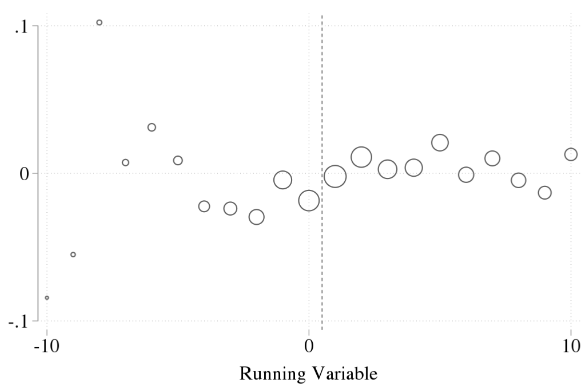
(c) Within Three Years



(d) Within Four Years



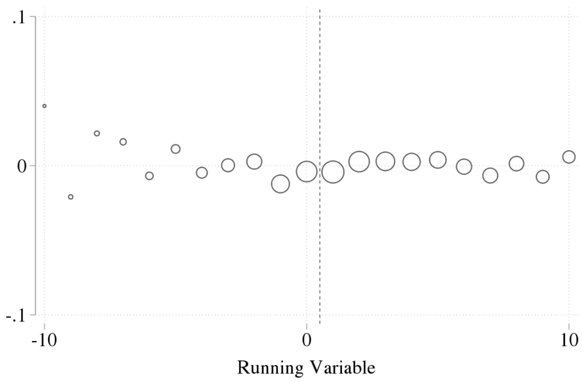
(e) Within Five Years



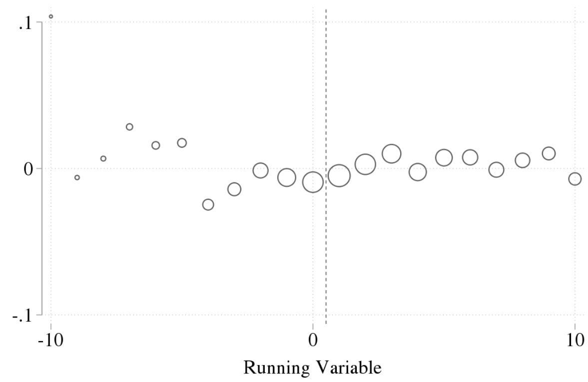
Notes. Each dot represents the mean of the residualized outcome, conditioned on the value of the running variable. The covariates are residualized by the control variables described in Table 4. The size of the dot reflects the number of observations in each bin.

Figure B6: Reduced Form Effect of Missing the Waitlist Cutoff on Transfers to a Four-Year School

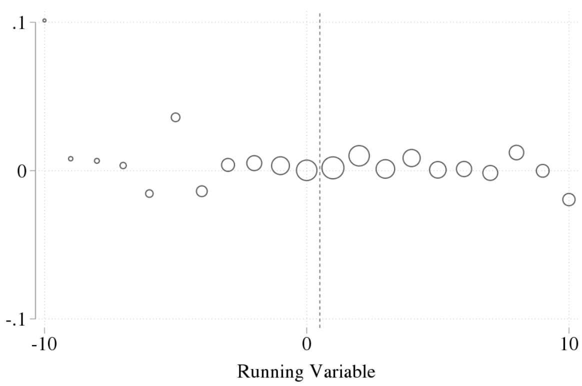
(a) Within One Year



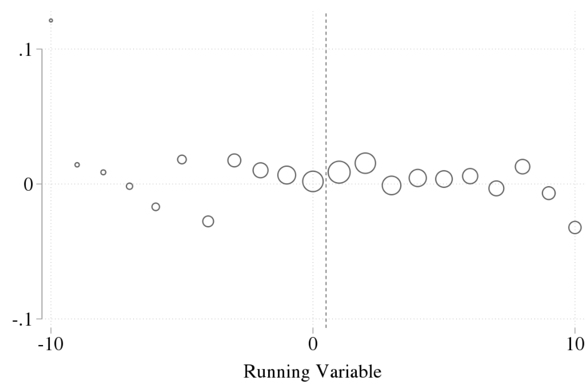
(b) Within Two Years



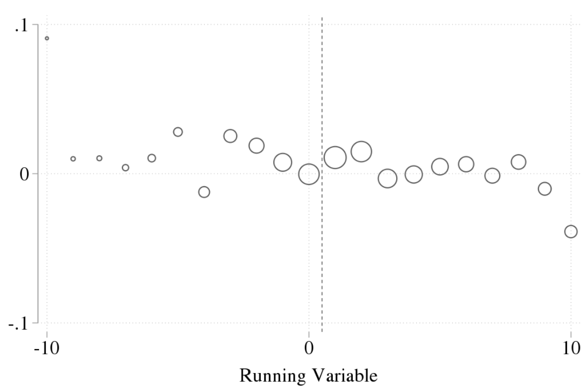
(c) Within Three Years



(d) Within Four Years



(e) Within Five Years



Notes. Each dot represents the mean of the residualized outcome, conditioned on the value of the running variable. The covariates are residualized by the control variables described in Table 4. The size of the dot reflects the number of observations in each bin.

C Using Time as the 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.[16]

Figure C1 shows the density of the time RV, using the analysis sample without the restriction of students being within one position of the cutoff. 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 control it.

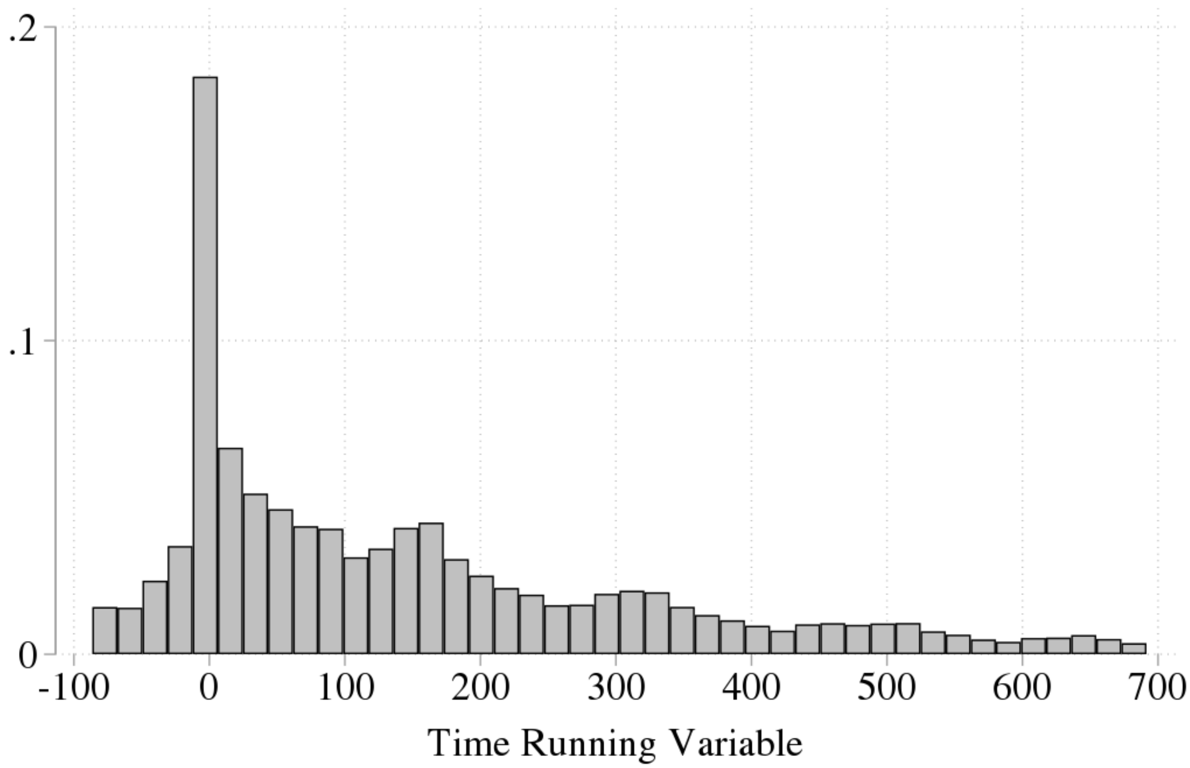
Figure C2 plots section enrollment rates at the end of the advanced registration period binned by values of the time running variable. There is a clearly visible jump in enrollment to the left of the cutoff. Table C1 shows formal estimates of the first stage and confirms that there is a discontinuity in the probability of section enrollment. Students who miss the waitlist cutoff are 82 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 C2 shows the estimates of the LATE on enrollment patterns in the concurrent term. The results are nearly identical to the main analysis. There is a 2.8 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 are smaller than the drop-out estimate. These results almost perfectly line up with

[16]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 4% of the registration attempts in the sample.

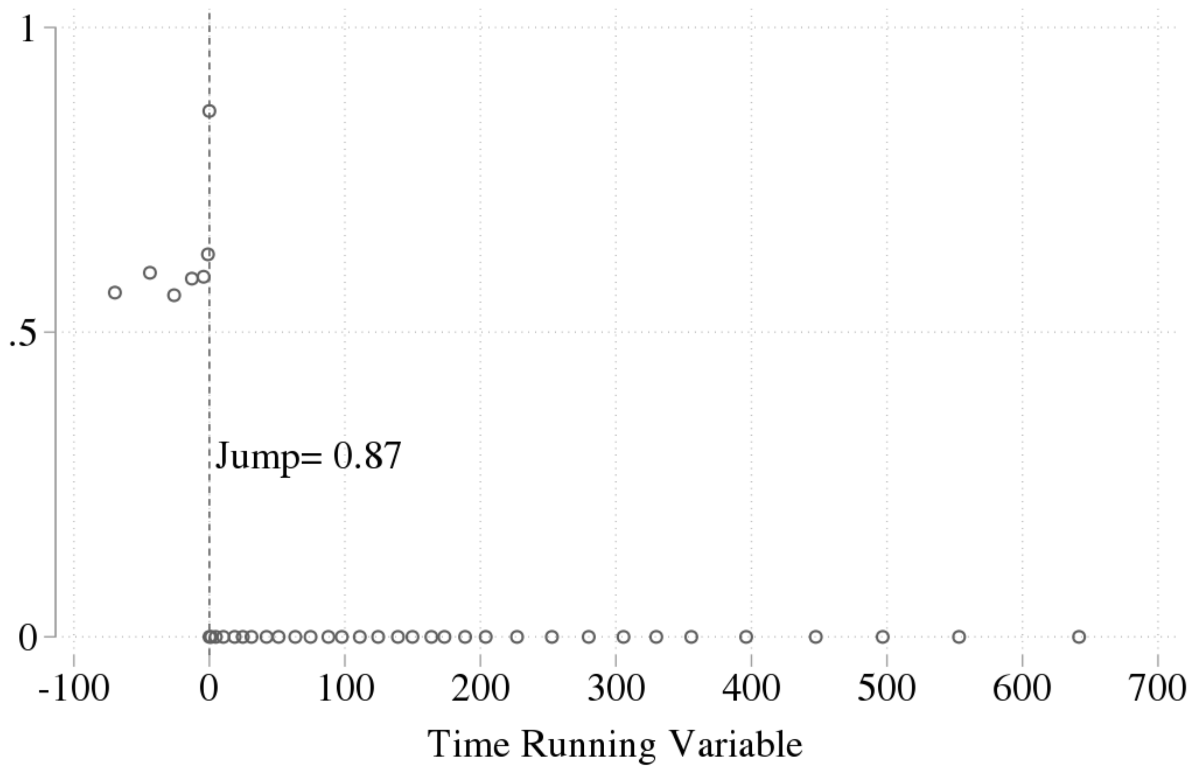
the main specification; not being able to enroll in a desired section leads to same-semester drop-out.

Figure C1: Density of the Time Running Variable



Notes. The time running variable 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. The figure censors time running variables smaller than the 10th and greater than the 90th percentile in order to make it more easily interpretable.

Figure C2: First Stage Effect of Missing the Waitlist Cutoff on Enrollment in the Waitlisted Section



Notes. Each dot represents section enrollment binned in forty quantiles by the value of the time running variable, where enrollment is equal to one if the student was enrolled in the waitlisted section at the end of the advanced registration period. Section enrollment is equal to zero for students with a time running variable greater than zero by construction. The time running variable 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. The figure censors time running variables smaller than the 10th and greater than the 90th percentile in order to make it more easily interpretable.

Table C1: First Stage Effect of Missing the Waitlist Cutoff on Enrollment in Waitlisted Section, Using Time Running Variable

	(1) Enrolled in Section	(2) Enrolled in Section
Missed WL Cutoff	-0.818***	-0.817***
	(0.009)	(0.009)
Observations (N_l/N_r)	2404/1285	2403/1282
CCT BW	10.797	10.748
Controls	N	Y

Notes: Results are from a local linear regression using time as the continuous running variable. The dependent variable is enrollment in the waitlisted section, where enrollment is equal to one if the student was enrolled at the end of the advanced registration period. The bandwidth is calculated according to the CCT optimal bandwidth selection procedure. The first column does not include controls while the second controls for the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table C2: Effect of Missing the Waitlist Cutoff on Course Load, Using Time Running Variable

	# Courses Enrolled in Concurrent Term		
	Zero (1)	One or Two (2)	Three or More (3)
2SLS	0.028*	-0.010	-0.017
	(0.016)	(0.023)	(0.023)
Reduced Form	0.022*	-0.008	-0.013
	(0.013)	(0.018)	(0.018)
Observations (N_l/N_r)	3,057/2,582	3,054/2,574	3,175/2,831
CCT BW	29.877	29.684	35.336

Notes: Results are from a local linear regression using time as the continuous running variable. 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 bandwidth is calculated according to the CCT optimal bandwidth selection procedure. The first column does not include controls while the second controls for the covariates listed in Table 4. Standard errors are robust to heteroskedasticity. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)