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Predictors of Student Success

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PREDICTORS OF STUDENT SUCCESS

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Kyle F. Neering

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Abstract

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by

Kyle F. Neering

The world of education is filled with policies which aim to improve student outcomes. While there are many factors which may potentially contribute to the academic success of a student, there are similarly a wide variety of measures which can be used to determine student success. This dissertation examines some specific policies and environments to which students are exposed and aims to determine their effects on those students. In doing so, the following chapters identify and explain particular predictors of student success.

The first chapter, "Course Closed: The Short- and Long-Run Impacts of Course Shutouts on University Students", examines how students are affected when they are unable to register for a university course. For a variety of reasons, demand for seats in some college courses can exceed the available supply. At the same time, many universities employ policies which allow some students to register for classes before others, creating a situation in which some students may find classes to be full when it is their time to register. In this chapter, I study how these "course shutouts" affect university students. Utilizing data from a university in which students are assigned to registration times in a quasi-random order, I use these registration times as an instrument to determine the causal effect of course shutouts on a number of student outcomes.

I show that, within a given term, students who are forced to register later experience more course shutouts and that these shutouts cause students to both attempt and earn fewer units. Students who get shutout of classes are more likely to end up in classes that start before 10 AM and are taught by instructors with historically low pass rates. All of these effects are particularly strong for students in their first two years at the university. Surprisingly, I show that students' cumulative number of shutouts across their academic career is not predictive of time to graduation, major changes, or dropout rates. However, students who accumulate more shutouts in their first few years at the university do exhibit higher rates of summer school enrollment, suggesting that they may respond to the adverse effects of shutouts by making up units in the summers. I also find this effect to be strongest for those students who show up to the university without any degree applicable units from community college courses or advanced placement exams. All together these results suggest that policies which routinely place particular students at the end of the registration period specifically first year students and those without incoming, degree applicable units may increase the cost of graduation for those students by increasing their likelihood of attending summer school.

The second chapter, "Non-Linear and Heterogeneous Effects of Peer Gender Composition on Academic Performance", explores the ways in which students' peer groups can influence academic performance. A long-standing literature has shown the gender composition of a student's peer group to be a relevant predictor of his or her performance in school. Most specifically, female students perform best when more of their peer group is female, with mixed results for males. Yet while some work has been done to establish a linear relationship between peer group gender composition and academic performance, little is known about the relevant non-linearities and heterogeneity of this relationship. Moreover, as students often non-randomly sort into schools and classrooms, plausible exogenous variation in peer gender composition is rare. To address these two points I utilize data from a randomized control trial in Duflo, et al. (2011) to explore the potential non-linearities and heterogeneities of the effects of peer gender composition alluded to in previous work. Results from this chapter show that students perform best when they are placed in a classroom with a fairly balanced gender composition, as opposed to one where students are predominantly of one gender. As part of the experiment, classrooms were randomly assigned one of two types of teachers: an existing, civil-service teacher or a new teacher on a one year contract who are shown to more consistently be in the classroom and teaching than their civil-service counterparts. I find that the effect of being in a more gender balanced classroom is strongest when students are taught by a contract teacher. Overall, these findings suggest that the effect of peer gender composition may not only be non-linear, but dependent on classroom environment.

The third chapter, "The Effects of Changing the Registration Policy at a Large Public University", examines the effect of a change in the policy that determines students' assignment to registration times at a large public university. Prior to the policy change, the university would divide the student body into 12 registration groups based on the first three letters of their last name. Each term these 12 name groups would each be assigned to one of 12 registration periods, creating an ordering of students that was unrelated to their progress towards graduation. Following the policy change, the university began determining students' academic progress, a number from 0 to 100 based on the proportion of degree applicable units completed. Under this policy, students are placed in registration times in descending order of academic progress, with the students with the most progress registering first. My estimates suggest that this reordering of students led to fewer waitlists and shutouts instances when students were unable to get into a class from a waitlist for third and fourth year students. However, the subsequent increase in waitlists and shutouts for underclassmen outweighed the decrease for upperclassmen, leading to an aggregate increase in these measures. Ultimately, seniors saw no increase in their cumulative number of units attempted and earned while that of freshmen, sophomores, and behind-schedule juniors decreased relative to the previous policy.

To my parents, my advisors,

and all of my friends.

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Chapter 1

Course Closed: The Short- and Long-Run Impacts of Course Shutouts on University Students

1.1 Introduction

Competition for seats in courses has become a serious and common concern among university students and administrators. Record enrollment numbers have in many cases been met with stagnant or shrinking resources and nearly 1 in 10 university students cite not being able to get into the classes they need as one of the top two reasons for not graduating on time.[1] Given the opportunity cost of an additional

[1]For a national survey of students from 52 universities, see Ohio State University's 2015 "National Student Financial Wellness Study". For discussions about changes in resources and crowding at universities, see Webber (2017) and Bound and Turner (2007), respectively. For an example of a state legislature trying to address the issue within its own state, see "California college students shut out of classes could earn credits online if new legislation passes", The Mercury News, March 13, 2013.

term in school, it is important to understand to what extent common university policies contribute to students' access to classes. At many universities, administrators employ policies through which students are assigned individual course registration times and forced to postpone registration as their peers register for courses in a predetermined order, often based on seniority. Yet despite the ubiquity of such policies, little is known about their impact on students' access to classes. As registration progresses and courses fill, students towards the end of the registration order may experience a more limited choice set, potentially affecting their course taking behavior and progress towards graduation. If these impacts have lasting effects or disproportionately affect certain students, their time to graduation may be impacted.

Yet such effects can be difficult to assess for several reasons. First, students who take longer to register may differ from those who register early. For example, they may be less committed to their academic progression or less familiar with the course registration process. Second, many universities assign registration times based on student traits such as academic standing, athletic participation, or units accumulated so the effects of changes in class availability due to registration timing are often inseparable from student type. That is, if students of lower ability are more likely to both fail a course and be allocated later registration times, any adverse effects of delay may be exaggerated.[2] Additionally, to address these questions extensive administrative data on registration choices, course characteristics, and student traits must be made available to

[2] See Smith et al. (2002), Hale & Bray (2011), and Gurantz (2014) for the relationship between student traits and registration timing.

the researcher in order to fully examine short and long-run outcomes and heterogeneity.

This paper addresses these challenges by utilizing the universe of student and course data from a large California university where students are assigned to registration times each term in a quasi-random fashion. In this setting, students are assigned to 1 of 12 registration times (bins) every term based only on the first three letters of their last name. This results in variation in assigned registration time that is orthogonal to a student's class standing, GPA, SAT score, and major. Using an instrumental variables approach, I exploit the variation in class shutouts namely, anytime a student waitlists for a full section and ultimately does not get in that results from a student's assigned registration time. Shutouts are then linked to a variety of within-term outcomes, such as unit accumulation, GPA, and course taking behavior.

The analysis in this paper provides the first causal estimates of the impact of shutouts that arise from delayed registration in a given term. These estimates show that students assigned to the last registration group (i.e. the last 8% of students to register) experience two more shutouts than students at the beginning of the registration period. This increase in shutouts leads to a reduction in units earned in that term. Specifically, 1 in 9 students who experience two additional shutouts will complete one less course. About 1 in 36 will complete one less course in their major. Students who are shutout are shown to attempt fewer units, attempt fewer courses, and to have a greater likelihood of dropping a course after the beginning of the term. Students who experience more shutouts are both more likely to have classes that meet before 10 AM and more likely to have an instructor who historically passes fewer students (relative to other instructors teaching the same course). However, counterintuitively, these students are less likely to fail a course, perhaps because they attempt fewer courses. The difference in earned units is thus primarily due to a reduction in units attempted. I find that these effects are strongest for students who are in the bottom quartile of their cohort's ability distribution. I also show that while registration delay can lead to shutouts at any point in a student's career, shutouts primarily reduce unit accumulation during their first two years. This difference likely arises because upperclassmen exhibit a greater likelihood of successfully getting into a full course after the term begins. Such behavior would allow them to overcome any potential adverse effects of course shutouts on units attempted and, in turn, unit accumulation.

Should the adverse effects of registration timing and shutouts set a student back sufficiently, their outcomes relating to graduation may potentially suffer. Thus, I explore the long-run impacts of accumulated course shutouts. Due to the timing of when students have their lowest registration priorities, there is significant variation across students in their expected number of cumulative shutouts. Despite the relationship between shutouts and unit accumulation in a given term, I find that shutout aggregation is unrelated to four-year graduation rate, time to graduation, or the likelihood that a student changes their major. Similarly, when focusing on shutouts accumulated in the first two years of a student's career, I find no relationship with drop out, time to graduation, or major changes.

A key contribution of this paper is the finding that students who accumulate more shutouts over their first two years because of poor registration times respond by increasing summer school participation. Specifically, students who get shutout more often attend more summer terms and attempt more total summer units throughout college.[3] Moreover, given the extent of the data available for this analysis, I can rule out other channels such as taking units outside of the university or attempting a high number of units in another term. Based on these estimates, roughly 1 in 7 students who experience a one standard deviation increase in shutouts accumulated over their first two years will take an additional four-unit course over the summer. The cost of such a course is about $1,100. Combined with forgone income and work experience associated with summer school attendance, the total cost to a student is non-negligible.

Throughout the paper, I define shutouts as any instance where a student is unable to get into a specific section of a course. As such, a student who gets shutout of two sections of a course may ultimately get into a third section (if one exists). Narrowing the definition to include only shutouts from entire courses that fulfill major requirements and serve as prerequisites to other required major courses dramatically increases the estimated effects. This result suggests that if a student experiences an additional

[3]Previous work has posited that the null effect on graduation may come from students increasing effort in other periods, but was unable to identify such channels due to data limitations (Kurleander et al (2014).

shutout from one of these courses in their first two years at the university, the number of summer units attempted increases by roughly one full course. Over the period for which I have data, roughly 10% of students experience a shutout of this nature.

Overall, the findings in this paper have important implications for course registration policies at universities. An informal survey of registration policies at a handful of flagship universities finds that the overwhelming majority of schools employ a policy that determines registration order by students' unit accumulation.[4] In such a regime, the costs of failing a course could be long lasting. For instance, take a case where a student at one of these universities experiences an unexpected shock that causes them to fail all of their classes in the first term of their freshman year. All else equal, the forgone unit accumulation in that term would put them at the end of their cohort's registration order the following term. The results of this paper suggest that this one-time shock would further hinder the student's unit accumulation in the subsequent term, reinforcing their place at the end of the registration order and ultimately leading them to incur the costs of summer attendance. In such a case, awarding the student with priority registration in their second term could potentially give them an opportunity to catch up with their cohort and get back on track to a timely graduation.

[4]Universities surveyed: University of Arizona*; University of California, Los Angeles; University of Colorado Boulder; University of Florida, Gainesville; Indiana University; Louisiana State University; University of Michigan; University of Minnesota; University of Missouri; University of North Carolina at Chapel Hill*; Ohio State University; University of Oregon; Pennsylvania State; University of Texas, Austin. Those denoted with a * assign registration only from number of terms attended, not units accumulated. The rest rely on units or credit hours accumulated.

The paper is organized as follows: section II reviews previous work on registration timing and course shutouts; sections III and IV discuss the setting in which this study takes place and the corresponding data, respectively; section V explores the effects of delay and shutout on students within a term and breaks the analysis down by term; section VI explores how short-term effects map into long-run effects; section VII discusses these findings in the greater context of the literature; and section VIII concludes.

1.2 Related Literature

Previous work on the immediate impacts of delayed course registration has largely examined community colleges, where students are often exposed to various forms of "open enrollment" policies and are given access to course offerings simultaneously. In such a setting, Smith et al. (2002) find that late registrants withdraw from twice as many course hours as their more punctual counterparts and are half as likely to return the following term. Similarly, Gurantz (2014) shows later enrollment to be associated with fewer units attempted, but suggests that the intensity with which a student searches for classes may potentially overcome the consequences of delayed enrollment. Moreover, registering later is found to be associated with a GPA reduction of as much as 0.7 points in the term (Summers, 2000; Smith et al., 2002; Hale & Bray, 2011).

Unfortunately, issues of selection may bias estimates of the impacts of registration timing. In a setting where student characteristics are strongly correlated with time of registration, any relationship between registration time and student performance is likely conflated by unobserved student characteristics. In much of the previous work, the students who register earliest are more likely to be non-minority and female (Summers, 2000; Smith et al., 2002; Hale & Bray, 2011; Gurantz 2014). Moreover, these studies suggest that students less familiar with the registration process first-time students and those of non-traditional age are more likely to wait longer before registering for their first classes. Financial aid eligibility and time to register, however, do not seem to be correlated. In general, these studies point to educational background and commitment to scholastic success as key predictors of time to registration. It is therefore unclear from previous work whether there exists a significant, causal relationship between time to registration and a student's unit accumulation and GPA in a given term.

To more directly measure the impact of course access on student outcomes, Kurlaender et al. (2014) generate a measure of cumulative failed enrollment attempts for each student over their first four years at a university. Used as a proxy for the number of courses a student desired but was unable to take, these "shutouts" are used to explain time to graduation. However, as is the case in other work, Kurlaender et al. (2014) show failed attempts to be endogenous to student type. To correct for this, the authors utilize random assignment to registration times that arises from a policy at the University of California at Davis and are able to identify students who repeatedly receive unfavorable, within-group draws over the course of their career at the university.[5] They then instrument for shutouts with a binary measure of cumulative registration assignment luck. In their analysis, poor registration luck is associated with an increase of less than a third of one course shutout per term. This is the only study, to my knowledge, that explores impacts of registration policies at a four-year university or addresses the issues of selection in registration timing. Their analysis finds that increases in cumulative shutouts are unrelated to time to degree. Thus, they conclude that course scarcity as a result of registration competition likely has little to no impact on students' graduation trajectories, possibly due to students' adjustments in course taking behavior.

The current paper builds upon previous work and contributes to the literature in several important ways. First, I provide the first evidence of a causal impact of a student not getting into a class within a given term. The nature of the registration policy in this setting generates substantial exogenous variation in course access across students within the same cohort that has not been available in prior work. Here, a student with the worst registration assignment would register after over 90% of both their cohort and the entire student body. Thus, I observe a first-stage difference between the first and last registrants of up to two shutouts in a given term. This variation in treatment, mixed with a large sample of academic years, may provide sufficient power to identify

[5]At the university in Kurleander et al. (2014), students are first organized into groups of a few hundred based on total units accumulated. These groups are then ordered by the average unit accumulation of each group. After the unit-based groups are ordered, students are randomly ordered within their group of students with similar unit totals. In this way, students will only be exogenously ordered behind a few hundred students. Conversely, in my setting a bad registration draw would mean registering after roughly 90% of the student body.

effects not found in previous work. Second, the data used in this study include the universe of course offerings, waitlists, registration behavior, instructor characteristics, and a wide range of student characteristics. The richness of the data allows for analysis of an exhaustive variety of outcomes both within a given term and over the course of a student's academic career. Within-term information makes analysis possible on not only term GPA and unit accumulation, but on course composition and the characteristics of a student's schedule, both of which may have important policy implications. Over a student's career, these data also provide the opportunity to examine how class closures impact relevant intermediate outcomes such as major changes, summer school participation, and units taken outside of the university, outcomes which are not available in previous work. Finally, the expansive set of student characteristics and academic backgrounds allow for analysis of any impacts that may be heterogeneous in student type.

III. Setting

The registration policy at the university in this study provides a useful setting for examining the effects of registration timing. The office of the registrar partitions the entire student body into 12 groups based only on last name. Incoming students (freshman or otherwise) are placed in the appropriate name groups regardless of major, class standing, standardized test scores, or place of origin. Last name cutoffs for each group remain constant across years and cohorts and do not adjust to account for group size (though group size consistently falls between 1900 and 2100). Every quarter, each of these 12 name groups is assigned to one of 12 registration bins. Each registration bin has a fixed time before which the name group assigned to that bin may not register. Once their registration period has begun, each student may register for up to 18 units and waitlist for any number of courses.[6] Once a name group's registration bin has begun, students in that name group may continue to add or drop courses until the second week of instruction. In other words, when one bin opens the others do not close.

Every quarter, each of the 12 name groups is assigned to one of 12 registration bins. For example, the name group containing students whose last names begin AAA to BEC may be assigned to registration bin number three of twelve. This assignment process is based on a predetermined sequence of registration bin assignments:

$$1 - 12 - 5 - 7 - 3 - 11 - 4 - 9 - 2 - 10 - 6 - 8$$

Specifically, if name group AAA-BEC was assigned registration bin 3 in the winter quarter of 2011, that same name group would be assigned to bin 11 in the spring quarter of 2011, bin 4 in the fall quarter of 2011, and so on. This does not, however, mean that every student begins their academic career in registration bin 1. Should last name group AAA-BEC be assigned to bin 4 in the fall of 2011, all incoming freshman with last names in that range would be assigned to bin 4. Incoming Freshman with any other last name

[6] After all 12 groups have been given the opportunity to register for 18 units, a final open-registration period opens in which students may register for more units (up to a limit of 22 units).

would be assigned to a different registration bin. All continuing students (sophomores, juniors, seniors) with a last name between AAA and BEC would be in registration bin 4 for the fall quarter of 2011. The next fall, last name group AAA-BEC would be assigned to bin 10 and all incoming freshman with last name AAA to BEC would begin their sequence in bin 10. Thus, while an individual student's assignment to a name group is determined only by the first three letters of their last name, each name group's assignment to a registration bin is determined by that name group's previous bin assignments.

With a predetermined sequence and 12 different potential starting points, students can be thought of as potentially ending up on one of 12 registration "paths". However, since registration for the fall quarter begins in May, before many students scheduled to begin in the fall have committed to the university, this first quarter of registration assignment is unrealized; students incoming for the fall register in late August, well after the rest of the (continuing) student body has been given the opportunity to register. Thus, for the remainder of this paper, we omit the fall quarter of freshman year from the analyses of the effect of registration timing on student outcomes. The potential four-year registration paths of a student can therefore, in practice, be assumed to resemble those depicted in Figure 1.

A look at course availability across the registration process better illustrates how bin assignment relates to course availability. Figure 2 shows the average share of all sections that have at least one seat available at the beginning of each registration bin.

Sections fill up continuously as the registration process progresses, and by the beginning of the final registration bin over 40% are full. Thus sections in courses required for graduation may not be available for students registering at the end of the registration process. This issue of availability is one that is common to nearly all students. About 91% of students in my sample end up on a waitlist and subsequently cannot enroll in the course at least once in their time at the university. This limited ability to enroll in sections may be compounded as students search for combinations of required courses to fill a schedule without class times conflicting.

Finally, to give the students an opportunity to avoid the assignment, each is endowed with three "priority passes" upon arrival at the university. Each pass can be used for a distinct term and allows a student to jump to a registration period before the first name-based registration bin. This new, earlier registration time operates identically to the typical registration bins; the student may register for up to 18 units and continue registering until the beginning of the term. A single priority pass can be used at any point before or during the registration process for the upcoming quarter and cannot be rescinded once used. While these priority passes allow students to manipulate their registration assignment, they can be thought of as causing a weaker first stage effect of registration assignment on class availability. Because the use of priority passes is endogenous, all estimates will be based on students' assigned bin (i.e. 1 through 12) and not their earliest realized registration time. As such, the first stage of an IV strategy which relies on bin assignment as an instrument for time to registration can be thought of as an intent to treat estimate.[7]

1.3 Data

Data cover the universe of students, courses, and registration attempts from Fall 2006 through Spring 2015 at a public, four-year university in California. Student characteristics come from information entered by the student on their initial application to the school and include gender, race, parental income (in the year preceding application), SAT and ACT scores, high school GPA, AP test scores, California and United States residency statuses, time spent on extracurricular activities in high school, whether or not they held a leadership role in high school, application fee waiver status, and high school attended. All reported test scores and high school GPAs are verified by the university before the student begins their first term and updated should any discrepancy exist.

Data from the Office of the Registrar include the universe of registration attempts in the period. This covers course name and course number for every registration attempt (i.e. waitlist or successful registration) for every student in the period, even if the student immediately dropped the course after enrolling or if the course was cancelled. These data also include an observation for every instance in which a student

[7] In practice, usage of the priority pass is almost non-existent in students' first year. Usage of the pass is, however, more common in later years. See the appendix for a table detailing priority pass usage by bin and term order.

waitlisted for a course, with information on when they joined the waitlist and whether or not they ultimately end up getting in from the waitlist. Each of these observations has a time stamp for the initial attempt as well as a time stamp of the drop when relevant. These data are merged with course-level information about course capacity, time and day of course meetings, instructor ID, instructor gender, and instructor title. Student-level data from the Office of the Registrar indicates each student's initial major(s), final major(s), date of major change, date of minor declaration, and date of graduation (when any of the above are relevant).

Finally, all of the above data are merged with student-term information about registration bin assignments and student-course data on grade points and units earned. As students with any special exceptions (e.g., athletes, military, and students with disabilities) are allowed to register before the school-wide registration process begins, a student may have more than one registration assignment time in a term. Since these students are repeatedly allowed to register before all other students at the university, I omit students who at any point received a special exception assignment.[8] If a student uses a priority pass in a term, they are also awarded a registration time that occurs before the first name-based registration bin. In all cases I see the student's registration bin based on their last name. As students have the ability to use a "priority pass" and jump to an earlier registration time at three points in their career at the university, all estimates based off of their name group's assignment to one of the twelve registration

[8]This excludes roughly 9% of the total sample.

bins should be considered an intent to treat estimate.

In all cases I restrict attention to students who begin in the fall quarter as a first time freshman. Doing so allows me to remove transfer students who not only arrive at the university with a wide range of units, ages, and college experience, but who also do not experience the registration assignment policy for their freshman and sophomore years. In all estimation relating to effects over a student's entire course career, I restrict my sample to students for whom the data period covers their first term at the university. Since I do not observe registration assignments prior to the period for which I have data, this allows me to hedge against any compositional changes that may occur due to the path a student may experience. This also allows me to verify the balance across registration paths at entry into the university.

1.3.1 Outcomes and Key Predictors

To understand how time to registration relates to course scarcity, I need a measure of students' ability to get into desired courses. Given the available data, it is possible to construct a measure of the proportion of courses or sections closed at the time a student registers. However, this measure is only a rough proxy of what courses the student is unable to take. Even limiting to courses available in a student's major and class level (i.e. upper or lower division) misses a lot of information on both what the student might have already taken and what they want to take. Moreover, this does not capture closed classes that satisfy requirements for undeclared minors or a major that the student has decided to switch into. Ultimately, the student's set of required and desired courses is most clearly revealed through their registration behavior. Specifically, any class in which a student is forced to waitlist because it is full can be considered one they desired but was full when they registered. Of course, in some cases a student will waitlist for a class and get in when a seat becomes available. Thus the key predictor, shutouts, is defined as any instance in which a student waitlists for a section, but does not get in. While this may miss an instance in which the waitlist was long enough to deter the student from ever joining the waitlist, it stands as the best proxy for course scarcity available in the data.

It should be noted that this measure is constructed using instances in which a student is unable to get into a particular section of a course. Specifically, a particular course, say Math 142: Calculus II, may offer multiple meeting times, multiple instructors, or both. In every term, I denote each unique instructor-meeting time combination within a particular course as a distinct section. Not all term-course observations offer multiple sections. However in a case where a course offers multiple sections, a student may be faced with multiple options for enrollment in a particular course. I rely on shutouts from particular sections as my primary measure for several reasons. First, in order to maintain progress towards a timely graduation, every term students must enroll in and complete several graduation requirements. To do so not only requires these requirements to have seats available, but have an available combination of sections that do not have conflicting meeting times. In this way, closures of sections within a course may be relevant, even in the presence of other unfilled sections, if the remaining sections conflict with the only available sections of other graduation requirements. Second, even if the student gets shutout of two sections of a course and gets into a third, the meeting time of the third may preclude them from filling a schedule with graduation requirements. Third, in as much as students may have an ordering of preferences across sections of a given course, closures of these more preferred sections may present costs to the student unobserved by the researcher. Finally, in cases where a course offers only one section, a shutout fully captures a student's inability to get into the entire course. In this way, my measure of shutouts may be thought of as a lower bound on the impact of the closure of an entire course.

Finally, a variety of outcomes were constructed from the data. First, a course is considered to be dropped by a student only if they withdrew from the course after the first day of the term. A student who is placed on the waitlist and successfully gets into the course after the first day of class is considered to have "crashed the course". Here, course crashes are identified in any case in which a student waitlists for a course and subsequently enrolls after the first day of class and takes the course (i.e. does not drop out of the course).

Should teachers with a reputation for passing a higher proportion of students be more desirable, their classes may be the first to fill up. Thus, for each term-course-instructor observation, I construct a measure of that instructor's most recent, "relevant" pass rate. The relevant pass rate for an instructor-term-course is the average pass rate across all courses of similar level (upper or lower division) taught by that instructor in that department in the most recent term in which they taught a course of that level. When a returning instructor teaches their first upper (or lower) division course, they are assigned the pass rate of their most recent term. As a result, first-time instructors have a missing value for "relevant" pass rate. I use this measure as a proxy for the information students have on an instructor's propensity to award a passing grade. If such a measure is pertinent to students' registration decisions, it may be relevant when a student is choosing between multiple sections of the same course.[9] An instructor-term-course-section is then considered relatively difficult if the relevant pass rate for that section's instructor is below the median "relevant" pass rate of all instructors teaching sections of that course in that term. Conversely, an instructor-term-course-section is considered relatively easy if the relevant pass rate is below the median relevant pass rate of all instructors teaching sections of that course in that term.

1.3.2 Balance

As assignment to registration path (and thus registration bin in any given quarter) is not determined by a student's class, major, or ability and cannot be manip-

[9]Here a course is designated by the catalog name (e.g., Math 142). A section is any distinct lecture taught under that catalog name (e.g., there may be four unique lectures taught by three unique instructors). In the process of designating relatively difficult or easy sections, I limit the sample of term-course observations to those in which at least two sections are taught by at least two distinct instructors.

ulated (short of a student legally changing their last name), it is possibly orthogonal to student-level characteristics upon entrance into the university. Table 1 shows means, standard deviations, and the results of a test of balance across registration bins and registration paths for a variety of student level characteristics. The p-values in column (3) come from F-tests of joint equivalence of the 12 assigned registration bins, controlling for term and cohort. Tests include all student-term observations and, thus, more than one observation per student. Across all covariates only a few exhibit a p-value below .05, suggesting that in a given term students from a given cohort assigned to registration bin 1 are roughly similar to students from that same cohort assigned to bin 12. While last name may potentially be correlated with ethnicity and other student demographics, the rotating nature of bin assignment allows me to see every last name group in every bin multiple times. Though not shown here, this balance also holds when looking across all cohorts in a term (i.e. omitting cohort fixed effects). Overall these show that bin assignment in a given term is orthogonal to observable student characteristics and, in turn, is likely unrelated to unobservables. Regardless, in all cases, regressions are shown with and without student controls to exhibit coefficients' lack of sensitivity to their inclusion.

Column (5) shows that this balance across bins relates to a balance across students' assigned registration path. The P-values in this column indicate balance across the twelve potential paths for nearly every trait. Significant differences exist across high school extracurricular activity and California residency, though related measures such as high school activity hours and foreign residency show no significant difference across paths. Moreover, key academic predictors such as SAT scores and high school GPA appear to be balanced. Regardless, estimates throughout the paper are shown with and without the inclusion of all of these variables as controls and indicate that estimates are not sensitive to their inclusion.

1.4 Within-Term Effects

1.4.1 Endogeneity of Registration Behavior

Previous within-term work suggests that time to registration is not only strongly correlated with student outcomes, but also student characteristics. Specifically, delayed registration is associated with fewer units earned, lower GPA, and a reduction in the likelihood of returning the following term. However, previous work has taken place almost exclusively at the community college level. Thus, differences in the student population and institutional setting of prior literature may make it inappropriate to ascribe previous findings to the university level.

Column 1 of Table 2 presents cross-sectional estimates of the relationship between registration timing and a variety of student-specific characteristics. Point estimates suggest that, consistent with prior work, male students with a lower high school GPA are more likely to delay registration. Moreover, prolonged time to registration is more pronounced for non-white students from lower income families, particularly those from outside of California. Despite registration assignment being unrelated to class level, freshman on average wait between a day and three days longer to register than their more senior peers. Overall, this column suggests that OLS estimates may be biased by the influence of omitted variables and overestimate the relationship between timing and within-term outcomes.

However, the time to registration may only be relevant in as much as it translates into a student's inability to get into the courses they need. Indeed the implication in previous work is that students who wait longer have fewer courses to choose from and in turn earn fewer units and are less likely to return the following term. Thus, as with registration timing, certain types of students are most likely to experience a shutout in the cross section. Column (3) of Table 2 indicates that white upper-classmen with college educated parents are less likely to experience shutouts. While these students also exhibit more punctual registration behavior, male students typically register later, but experience fewer shutouts. These contradictory results may reflect the competing forces that can lead to observed shutouts. Never the less, the strong relationship between student traits and shutouts in this column show that nave estimates of the relationship between shutout and student outcomes likely suffer from issues of endogeneity.

The remaining columns of Table 2 show that other registration behavior, namely units attempted and return the following term, is also strongly influenced by student-level traits. Thus, in the absence of a more causal estimation strategy, an estimated relationship between course availability and term-specific outcomes is likely subject to biased from omitted variables. In the following sections, the potential endogeneity of registration behavior is addressed more explicitly.

1.4.2 Instrumental Variables Empirical Strategy

To estimate the causal impact of course shutouts on student outcomes in a term, I use a student's assignment to one of twelve registration bins as an instrument for the number of times they are unable to get into a course for which they have waitlisted. In doing so, I exploit a more plausibly exogenous variation in course shutouts that is less likely to be influenced by unobservables. The first stage of this estimation strategy is as follows:

$$shutouts_{icqt} = \alpha_1 + \sum_{j=2}^{12} \alpha_j Binj_{it} + \theta_{it} + \rho_c + \delta_q + \eta_t + X_i\beta + u_{cqit}$$

Where $shutouts_{icqt}$ is the number of course shutouts student i experiences in term t. Variables $Bin2_{it}$ through $Bin12_{it}$ are a series of indicators for the student's assigned registration bin in term t. As a student's behavior may change over the course of their time at the university, a collection of fixed effects for student level (i.e. freshman through senior) in the term, θ_{it}, are included. A series of cohort fixed effects, ρ_c, are also included to account for potential differences across cohorts. Differences in course offerings due to the quarter of the year (ex: Winter or Spring) are accounted for with quarter fixed effects, δ_q. Moreover, η_t, a series of term-specific fixed effects (ex: Fall 2009) are included to account for any imbalances in student body size, resources, and other differences that may exist across terms. A vector of student-specific characteristics, $X_i\beta$, are sometimes included to show the stability of estimates to their inclusion.[10] Finally, students in the same bin and cohort will consistently be exposed to the same course openings at their grade level and will likely have correlated outcomes. Thus, standard errors are clustered at the bin-cohort level, leading to one cluster per class (i.e. freshman, sophomore) per registration bin.

The second stage equation for two-stage least squares estimation of the within-term effect of time to registration is as follows:

$$Y_{it} = \beta_1 + \beta_2 \widehat{shutouts}_{it} + \theta_{it} + \rho_c + \delta_q + \eta_t + X_i\beta + \epsilon_{it}$$

where Y_{icqt} is an outcome in term t for student i, such as units earned or GPA. The term $\widehat{shutouts}_{it}$ is a fitted term from the first-stage. The various fixed effects $\theta_{it}, \rho_c, \delta_q, \eta_t, X_i\beta$, are the same as in the first stage equation. Again, standard errors are clustered at the bin-cohort level.

It should be noted that, in order for the IV assumptions to hold, assigned registration bin can only affect the outcomes through an impact on course shutouts. Assigned

[10] In regressions that include all observations regardless of term order, multiple observations of the same student allow for the inclusion of student fixed effects in place of a vector of student-specific characteristics. Since all other analyses in the paper include just one observation per student (and thus cannot accommodate student FE), estimates using student FE are not shown here for the sake of consistency across tables. However, within-term estimates which rely on student FE are consistent with those shown in the paper.

registration bin certainly impacts days to registration, but it may be reasonable to conclude that an increase in days to registration does not impact unit accumulation in and of itself, but only through course shutouts. Shutouts may ultimately affect unit accumulation through a variety of channels such as units attempted or course composition but not in a way that would violate the exclusion restriction. In other words, a student who registers later may be more likely to take a course with a difficult instructor, but only because they were shutout of other courses with easier instructors.

1.4.3 All Class Levels

1.4.3.1 First-Stage and Reduced Form

In order for the estimation strategy to be valid, the assigned registration bin must both influence a student's number of course shutouts as well as be unrelated to their unobservable characteristics. Table 3 thus shows the results of the first stage estimation, with and without student-level characteristics. Column 1 indicates assigned registration bin to be a strong predictor of course shutouts. Students assigned to later bins experience more shutouts than those in Bin 1. Those assigned to Bin 12 on average are shutout of more than one class more than those assigned to Bin 1 in a given term. This difference is particularly large relative to the term average of 0.925 shutouts and equates to about half of a standard deviation. Column 2 shows that the inclusion of student-level characteristics has little effect on the estimates. This again suggests that assignment to registration bin is exogenous to student characteristics and is likely a valid instrument for course shutouts.

Columns (3) and (4) of Table 3 show the reduced form relationship between bin assignment and unit accumulation in a term. Similar to the first-stage estimates, bin assignment has a significant effect on units earned. While the effect is not as large in magnitude relative to the first stage, students in the last registration bins experience a statistically significant reduction in units accumulated relative to their peers in the first registration bin.

1.4.3.2 Units Earned and Return Next Term

Results from the IV estimates of the effect of shutouts on units earned in a term are shown in Table 4. Columns (1) and (2) show the estimated effect of shutouts on total units earned, with and without a rich set of student-specific controls. Based on the standard deviation in average shutouts shown in Table 3, students who experience a one standard deviation increase in shutouts earn just under a half of a unit less on average than those experiencing the mean number of shutouts. As the typical course at the university is 4 units, this amounts to one in nine students completing one less course. Similar findings can be seen in columns (3) and (4) for major units earned, though the IV estimate suggests that one in about 36 students would pass one less major course. Columns (2) and (4) show that these results are robust to the inclusion of a rich set of student characteristics.

It is worth noting that the IV estimates showing the effect on term-to-term dropout in columns (5), and (6) of Table 4 are insignificant. While the direction and significance of the coefficients from the IV estimates on units earned are in line with those from OLS estimates in previous literature, the potential omitted variable bias is less innocuous in the case of estimates on the likelihood of returning the following term. Despite the significant relationship between registration behavior and persistence identified in these studies, columns (5) and (6) of Table 4 find no such relationship. This further suggests that findings from previous work may not be, in fact, causal.

1.4.3.3 Mechanisms

If students at the end of the registration period are both shutout of desired courses and exposed to a different set of potential courses, these effects on unit accumulation may be the result of two potential mechanisms: students taking fewer courses or students taking harder courses. Table 5 explores the first these potential causes for the observed reduction in earned units. Estimates from columns (1) through (4) show the effect of shutouts on units attempted and are very similar in magnitude to those from columns (1) through (4) from Table 4, suggesting that reduced unit accumulation may be primarily driven by a reduction in units attempted. Taken with the estimates from columns (1) through (4) of Table 3, students who experience two fewer shutouts attempt half a unit more and earn just under half a unit more on average. This reduction in units attempted is likely driven by a reduction in the number of courses attempted, as evidenced in columns (5) and (6). As the typical course at this university is between 4 and 6 units, the coefficient of .05 maps almost directly into the .25 reduction in units attempted. The observed reduction in units attempted may not entirely arise from an inability to get into classes. Columns (7) and (8) show that shutouts also increase the likelihood of a student dropping a course after the beginning of the term. It is hard to say, however, whether this increase is the result of a lack of desirability of courses available or the result of students "shopping" for courses (i.e. enrolling in an excessive number of courses and trying each out before picking one).

Previous work on course choice suggests that, conditional on satisfaction of graduation requirements, students weigh a variety of factors when making section selection decisions.[11] While expected grade may weigh most heavily, influential factors can include instructor rank, time of meetings, and number of meetings per week. As such, the composition of available courses may vary across the registration period and students who experience course shutouts may take courses with traditionally "less desirable" characteristics. Table 6 thus explores the impact of shutouts on the characteristics of courses students ultimately take in a given term.

Given the potential lack of substitutability of courses (i.e. Calculus 1 for Cal-

[11]See Brown & Kosovich (2015) Ting & Lee (2015), and Wilhelm (2004) for discussions about the relative importance of various course characteristics in the course enrollment process for university students.

culus 2), it may be most relevant to focus on decisions across sections within a given course. Further, in cases where all sections of a course are taught by the same instructor, no trade-off is made with respect to instructor type when choosing between sections. Thus, for this analysis, I limit my sample to student-term observations where the student takes at least one course which offers multiple sections taught by at least two different instructors. Since Table 5 indicates that shutouts lead to fewer courses attempted, an analysis of the raw number of courses a student takes of a given type may not properly characterize the impact of shutouts on course composition. Thus, I rely on the share of multi-section courses that fall into a given category as the primary outcome of choice in Table 6.

Panel A of Table 6 indicates the effect of shutouts on instructor difficulty. Here, the instructor for each term-course-section observation is assigned a historical pass rate as specified in section IV.A. Based on these historical pass rates, section instructors are flagged as either above, below or at the median pass rate of all sections in that course in that term. Columns (1) through (8) indicate that as students get shutout of more sections, they experience a shift away from the proportion of sections taught by instructors with high pass rates towards those with low or unknown pass rates. These findings are in support of previous work based on student surveys which suggest that, when deciding between two otherwise equivalent classes, students prefer one which is perceived to be of lower difficulty or taught by an instructor who grades more leniently.

Panel B of Table 6 indicates the effect of shutouts on other section characteristics, such as instructor rank and course meeting times. Columns (1) through (4) suggest that students have a preference for instructors as opposed to tenured professors. These estimates may be difficult to draw strong conclusions from, however, as instructor rank may be strongly related to historical pass rate. Students also show a relative aversion to early morning sections (i.e. those that start before 10 AM). Columns (5) and (6) find that increases in the number of shutouts increase the proportion of these sections. Finally, students who experience shutouts subsequently enroll in sections that meet on a greater number of distinct days of the week.

If students who experience shutouts ultimately take a different, less desirable composition of courses, part of the impact of shutouts on unit accumulation may operate through these students' ability to pass classes. Table 7 explores this potential channel. Columns (1) through (4) indicate that shutouts seem to increase a student's GPA and decrease the likelihood of failing a course. While this may be surprising in light of the estimates from Table 6 that suggest later registrants are more likely to have a difficult instructor, it reinforces the conclusion that shutouts impact unit accumulation through units attempted, not course failure. Still it may be counterintuitive that students with harder instructors receive higher grades. However, as these students also attempt fewer units, they might be able to dedicate more time to each course and not suffer lower grades. Columns (5) and (6) thus explore the impact of shutouts on total grade points, the product of GPA and units earned. These columns show that despite the higher

GPA, student who experience more shutouts still do not earn enough grade points to break even with those students experiencing fewer shutouts.

Overall, these tables suggest that being assigned to a later registration bin leads students to be shutout of desired courses, take fewer units, and ultimately earn fewer units in a given term. These are the first estimates to establish a causal relationship between delay and unit accumulation. Moreover, they are the first to identify a mechanism, namely units and courses attempted, through which course availability impacts students.

1.4.4 Heterogeneity by Traits and Major

As different types of students are able to navigate institutional obstacles to varying degrees (citation), forced delayed registration may impact unit accumulation more for certain students. To check this, Table 9 replicates the estimates of Table 4 for various types of students. Here, student types are defined across two dimensions: ability and family type. In this table, ability is measured based on the index the university employs to determine a student's eligibility for being accepted into the university. Specifically, a student's eligibility index is calculated as:

$$EligibilityIndex = SAT_{Math} + SAT_{Read} + HSGPA * 800$$

From here, I determine high and low eligibility students as those who come from the top and bottom 25% of their cohort's eligibility distribution, respectively. [12] Panel A of Table 8 shows the estimates of shutouts on units earned for low and high ability students separately. The first two columns of Panel A suggest that the effect of registration timing is stronger for students with a low eligibility index. Conversely, columns (3) and (4) of Panel A suggest that high ability students are not as strongly affected by delayed registration as the average student.

Panel B of Table 8 isolates the effects of registration timing for students of various family backgrounds, namely low and high income students. As information on family income only indicates which of five different brackets the family falls into, I define a student as low income if their family falls into one of the two lowest income brackets (below $36,000). Surprisingly, these students seem to be less severely impacted by course shutouts. While I do not have information on student loan status, this resilience to course shutouts may be in part explained by stipulations in student loans that require students to take a minimum number of units every term. Conversely, high income students exhibit a relationship between course shutouts and units earned that is on par with that seen across the whole population.[13]

The impact of getting shutout of a course may also vary by major. Some

[12] I employ eligibility index in favor of either HS GPA or SAT score as it includes information from both measures and is a metric on which the university relies upon to rank incoming students. Results using HS GPA or SAT score are similar to those shown here and are available upon request.

[13] This may be unsurprising as students in the highest income bracket make up about 75% of the total population.

majors within the university require more hierarchical sequences of courses for the completion of the major. In such cases, getting shutout of a course may leave students in that major fewer alternative options. Table 9 thus explores the impact of shutouts on students in four departments on campus. Surprisingly, estimates indicate that students from a department with very hierarchical majors, Engineering, earn the same amount of units in the face of more shutouts. Conversely, the impact on students in less hierarchical majors is very large. Point estimates suggest that roughly one in nine students in these majors that experiences two additional shutouts will complete one fewer class in their major.[14]

1.4.5 Heterogeneity By Term Order

While shutouts impact unit accumulation within a term, it may be that they matter more at certain points in a student's career. If upperclassmen have fewer remaining requirements to choose from and upper division courses have fewer seats, it may be that juniors and seniors are most likely to experience a shutout from delayed registration. Panel A of Table 10 shows the results of IV estimates of the impact of delay on the number of shutouts a student experiences, broken down by the student's term at the university. With three quarters per year over four years, estimates are given

[14] It should be noted that this exercise does not entirely capture the impact on completion of graduation requirements. Here "major units" are measured as units taken within the department of the student's current major. However some majors, such as Mechanical Engineering, may require sequences of courses that are hosted in other departments, such as Math or Physics. In this way these estimates may not capture the whole impact of shutouts on major units. This is addressed more directly in the discussion section of the paper.

for all 12 quarters (though the first is omitted). Estimates show that delay can lead to course shutouts at any point in a student's career. However, this impact appears to be strongest in the first three or four terms of a student's career. During this period, students with the lowest registration priority experience 2 more shutouts than early registrants.

However, if more senior students are able to crash courses, they may be able to overcome the potentially adverse effects of course shutouts. Conversely, newer students may have less institutional knowledge and thus may be less well equipped to overcome shutouts. In either case, shutouts may unequally affect unit accumulation for students at different points in their college career. Panel B of Table 10 thus links shutouts to units earned by term order. From these results, shutout has a clear impact on unit accumulation in the first half of a student's career. Across these terms, roughly one in twelve students who experiences a shutout will accumulate one less class worth of units. However this effect disappears entirely in the final years of a student's tenure. Thus, while registration delay may lead to shutouts in most terms, it does not prevent students from accumulating units in the later part of their college career.

If delay and shutouts primarily impact unit accumulation through the early part of students' careers, there exist a few potential mechanisms through which this may occur. First, academic self-efficacy and urgency to graduate may both be greater for older students who in turn may be more likely than underclassmen to instead take a course with a difficult teacher if it helps them fulfill graduation requirements. Panel A of Table 11 attempts to explore this mechanism by estimating the effect of delay, by term, on the likelihood of a student taking a course with a difficult instructor. These estimates suggest that this mechanism cannot, in fact, explain why shutouts primarily affect underclassmen. In general, older students are not more likely to take a course with a difficult instructor if they are shutout of a desired course. Only first year students are estimated to have a different composition of teacher difficulty when shutout of courses.

This does not mean that when an upperclassman gets shutout of a course they simply attempt fewer units. Panel B of Table 11 shows that these students largely do not adjust units attempted in response to course shutout. In fact students are most likely to respond to course shutout with a reduction in units attempted in their first few years at the university. It does however appear that upperclassmen are more likely to crash a course when they are unable to get into another course. Panel C of Table 11 shows the effect of shutouts on the likelihood that a student waitlists for a course and ultimately enrolls after the term begins. For seniors, an additional shutout is associated with a twenty percentage point increase in the likelihood of crashing a course. For freshman the effect is typically less than half of that observed for fourth year students. This stark difference may potentially explain seniors' resilience to course shutouts relative to younger students. Should older students be more likely to try and succeed in crashing a course, they may be less threatened by course shutout due to delayed registration.

1.5 Long Run Effects

1.5.1 Instrumental Variables Empirical Strategy

If delayed registration leads to course shutouts and in turn fewer units earned at points in a student's career, it may potentially impact long-run outcomes such as time to graduation. In a case where a student is shutout of a key prerequisite, their entire path towards completing graduation requirements may be pushed back a whole term. Moreover, being shutout of key requirements may channel students into other courses and ultimately another major. However, a student's likelihood of delaying registration and getting shutout of a desired course may be related to student traits. Accumulated shutouts may thus be endogenous to student type and any relationship estimated between shutouts and student outcomes may be subject to bias from omitted variables. To explore the causal long-run ramifications of course shutouts, I employ an estimation strategy akin to that in Section V, but rely on students' assigned registration paths as an instrument for cumulative shutouts. The first-stage of this estimation is as follows:

$$TotalShutouts_{ic} = \alpha_1 + \sum_{j=2}^{12} \alpha_j Pathj_i + \rho_c + u_{ic}$$

where $TotalShutouts_{ic}$ is a discrete measure of course shutouts accumulated by student i over some portion of their career at the university. Variables $Path2_i$ through $Path12_i$ are a series of indicators for the student's assigned registration path.[15] A series of cohort

[15]See figure 3 for the 12 potential paths. Paths are numbered based on the bin they are assigned in the Winter of their first year.

fixed effects, ρ_c, are also included to account for potential differences across cohorts. As before, students in the same path and cohort (i.e. same cohort and name group) will consistently be exposed to the same course openings at their grade level, likely end up in the same courses, and will potentially have correlated outcomes. Therefore, standard errors are clustered at the path-cohort level, leading to one cluster per cohort per path.

The second stage of the IV estimation of the long term effect of cumulative course shutouts is:

$$Y_{ic} = \beta_1 + \beta_2 Total\widehat{Shutouts}_i + \rho_i + \epsilon_{ic}$$

where Y_{ic} is a long-run outcome, such as four year graduation rate or change of major. Fixed effects are the same as in the first stage estimation and standard errors are again clustered at the path-cohort level.

1.5.2 Long Run Effect of Shutouts

1.5.2.1 First-Stage Effect

Table 12 shows the first-stage effect of registration path on cumulative shutouts over two and four years. Columns (1) and (2) indicate a potential average difference of over two shutouts between the two paths with the most and fewest shutouts at the end of two years. Similar point estimates can be seen in columns (3) and (4) for shutouts accumulated over four years, suggesting that the differences across paths may appear early in the college career and remain steady throughout the later part of college. In both cases, estimates are stable to the inclusion of covariates, indicating that registration paths are orthogonal to student characteristics.

The existence of a first stage may be surprising given that registration assignments follow a rotation meant to expose every student to every bin. However the average number of shutouts in a given term is not constant across students' first four years at the university. Specifically, the average number of shutouts experienced is highest in the first few terms and falls almost monotonically with each successive term.[16] Thus, the timing of poor registration bins (i.e. 12, 11, or 10) may be an important determinant in the average cumulative number of shutouts experienced by each registration path.

Figure 3 exhibits this more explicitly. Here the cumulative number of shutouts at the end of each term is shown for the two paths with the highest and lowest average number of shutouts at the end of four years, paths 12 and 5 respectively. It can be clearly seen that the registration path that begins in bin 12 experiences a large number of shutouts in term 2 relative to the path that begins in bin 5. While the difference shrinks in the next term, it grows again in the first term of the second year when paths 12 and 5 are assigned to bins 7 and 3, respectively.[17] At the end of this term the difference in average shutouts across the two paths is already roughly four shutouts. Though these two paths repeatedly trade off registering before the other, the impact

[16]See Panel A of Table 10 for the mean number of shutouts in each term.
[17]Bin assignments in each term for each path are shown in Figure 2.

on cumulative shutouts is not enough to overcome the difference seen by the end of term five. Thus, despite the apparent “fairness” of the rotating bin assignment policy, differences in the timing of these bin assignments creates variation across paths in the number of shutouts accumulated over four years.

1.5.2.2 Time to Graduation and Major Changes

Panel A of Table 13 shows the estimated effect of shutouts accumulated over four years on a variety of long-run outcomes. The IV estimates from columns (1) though (6) of Table 16 indicate no discernable causal effect of cumulative shutouts on time to graduation, four year graduation rates, or the likelihood of a student changing their major. It may thus be the case that despite the single term adverse effects of course shutouts, they are not so large that students cannot recover over the course of their tenure at the university.

However, results from Section V suggest that shutouts may only be of relevance in the first half of a student’s career. Thus, panel B of Table 13 replicates the regressions of panel A, but focuses on shutouts accumulated over the first two years of a student’s career. Again, effects on long-run outcomes are found to be statistically insignificant, suggesting shutouts have no discernable bearing on dropout rates, graduation rates, or major changes.

1.5.3 Heterogeneity

As section V showed that different types of students are potentially impacted differently by forced registration delay, it may also be the case that some types of students are affected by cumulative shutouts while others are not. Table 14 breaks down the analysis from Panel A of Table 13 by subsets of student type. Panels A of Table 14 shows the distinct impact of shutouts on low and high ability students, respectively, based on their calculated eligibility indices. In all regressions shutouts accumulated over four years are found to be unrelated to time to graduation, four year graduation rates, and the likelihood of changing major. As with the findings from the full sample in Table 13, it may be that these students are able to compensate for single term shocks by increasing effort in other terms.

Panel B of Table 14 also breaks down the analysis by students majoring in two distinct majors, namely Engineering or Agriculture. Again, IV estimates indicate no significant relationship between accumulated shutouts and long-run outcomes. Taken with the findings from Panels A and B, these results suggest that the observed null effects from Table 13 hold not only for the average student, but for students across different abilities and majors.

1.5.4 Mechanisms

If shutouts impact unit accumulation at points in a student's career, it may be surprising that accumulated shutouts do not impact time to graduation. It may however be the case that students respond to any potential setbacks in unit accumulation by increasing unit accumulation at other points in time. More specifically, students who experience greater numbers of shutouts may counteract this by taking a heavier course load another term, enrolling in summer school, or taking courses outside of the university. In such a case, these students would be able to get back on track to graduate in a timely manner.

Table 15 explores these mechanisms separately by looking at the impact of shutouts accumulated over the student's first two years on additional course taking behavior. In each regression registration path is used as an instrument for shutouts accumulated over two years. The first two columns examine the impact of shutouts on the number of terms in which a student attempts a number of units beyond what would be considered a "full course load". Specifically, a term with a "heavy load" is defined as any term in which the student attempts more than 16 units.[18] Columns (1) and (2) of Table 15 indicate that students do not respond to exogenous changes in course shutouts by taking more than a full load in a different term. Columns (5) and (6) also show that students do not respond to shutouts by taking units at a different school or university.

[18]As the typical course is 4 units, this would amount to 4 distinct courses. Roughly a third of all students never exceed this unit total in their time at the university. About a third of students do so once. The remaining third take a heavy load two or more times.

However, columns (3) and (4) show that students who are shutout of more courses enroll in summer courses more often. Estimates from these columns suggest that about one in 13 students who experience a one standard deviation increase in cumulative shutouts (about 7.5 shutouts) will respond by attending summer school. This is also reflected in the increase in summer units attempted in columns (7) and (8). Estimates from these columns suggest that about one in 7 students who experience a one standard deviation increase in cumulative shutouts at the end of two years will respond by taking an additional four-unit class during a summer term. While the student may prefer summer attendance to delayed graduation, this choice is not without a cost to the student. In addition to the fees associated with summer courses, students may be forced to forfeit work hours or even internship opportunities. In such a case, students would possibly be forced to take on more debt or suffer from decreased competitiveness on the post-graduation job market.

1.6 Discussion

1.6.1 What We Know About Shutouts

Assigned course registration times can have significant impacts on university students. The findings in this paper show that students who are forced to register later in the registration period are more likely to be shutout of desired courses and in turn attempt fewer units with more difficult instructors. Though these same students are likely to earn a higher term GPA and are less likely to fail a course, they ultimately earn fewer units in that same term. These results in part affirm those from previous work on the effects of registration timing, but differ in a few key ways. Contrary to previous work, I find that students who register later do not earn lower GPAs nor are they any less likely to return the following term. This difference in results likely arises from the issues of selection that potentially plague previous work. Indeed, a key contribution of this paper is that it provides the first causal estimation of the impact of assigned registration time (and the resulting course shutouts) on all the above outcomes.

The extension of this analysis to the term order highlights the heterogeneous effects on students at different points in their career. Where Gurantz (2014) posits that search intensity may potentially overcome the consequences of delayed registration, I show that older students in fact overcome the consequences by successfully crashing courses. If this is the result of accumulated institutional knowledge, it may be possible to assuage effects on younger students by encouraging knowledge transfer from older to younger students.

The null effect of shutouts on graduation rates is in line with the only other paper to explore this relationship. In their work, Kurleander et al. (2014) find career-long totals of shutouts to be unrelated to four year graduation rates. However, their instrument of cumulative registration misfortune, leads to a first stage difference in cumulative shutouts of only one third of a shutout. In comparison, the first stage effect in column (1) of Table 12 in this paper shows a potential difference of over two shutouts between the least and most fortunate registration paths. Thus while our findings are comparable, mine is less likely to suffer from a weak instrument. Moreover, the null effect on major changes provides the first estimates for an intermediate outcome, as this measure was unavailable to Kurleander (2014).

While neither Kurleander et al (2014) nor the current paper establish a relationship between access to courses and graduation timing, I provide the first evidence of an effect on an intermediate outcome: summer school attendance. Moreover, by showing that students are neither more likely to take courses outside of the university nor increase the number of terms in which they take more than a full load of units, this paper is the first to identify the mechanism through which students may respond to course shutouts that arise from poor registration assignments. This behavioral response may, from a financial perspective, seem suboptimal. At this university, in any given term the marginal cost of a unit beyond 12 units is zero. Conversely, the cost of a four-unit summer course is over $1,000 plus the opportunity cost of the forgone wages and work experience from summer employment. Thus, for many students the price elasticity of a unit is likely very inelastic. Yet the cost of a one standard deviation increase in cumulative course shutouts is not negligible. Based on the estimates from Table 15, one in 13 students who experience a one standard deviation increase in cumulative shutouts over their first two years (7.5 shutouts) will increase their summer school attendance by one summer term.

1.6.2 What Courses are Driving These Results?

Up to this point, our measure of shutouts has treated all shutouts as equal. Specifically, an engineering student's inability to get into a two unit section of dance would count as much towards their measure of shutouts as would the inability to get into a section of Calculus 3. Yet the latter likely has much larger implications than the other on long-term outcomes. Not being able to get into a course within a sequence of required major courses can almost mechanically delay progression towards graduation.

However, identifying key major prerequisite courses can be very difficult. Many hierarchical majors require a complicated web of interdependent sequences which may need to be taken in parallel. Limiting to courses hosted within the same department as the student's major may also miss key requirements as some key sequences such as the calculus sequence not be hosted within the student's department. This is further complicated by the fact that course titles and specific major requirements may vary every other year with the introduction of new course catalogs. Because completing these courses in a timely manner requires filling an entire term schedule, a student's inability to get into a subset of a course's offered sections may also preclude them from enrolling in the entire course due to scheduling conflicts with other required courses. Moreover, due to the structure of the data, identifying shutouts of entire courses (not just sections within a course) requires going through classes one-by-one and is thus a very time and labor intensive process.

None the less, this exercise may be very informative to the policy maker. Since these specific prerequisite major courses present bottlenecks in students' progression towards graduation, their availability may be more important to student outcomes than the average course. I thus narrow my focus on shutouts of entire courses that are key prerequisites in a few majors: Economics, Mechanical Engineering, and Chemistry. Specifically, if a mechanical engineering student gets shutout of at least one Calculus II section and ultimately does not enroll in that course in that term, I consider them shutout of a prerequisite course. I repeat this process for all courses in the aforementioned majors which are both suggested to be completed in the first two years and are a prerequisite for another major requirement.

Table 16 shows the estimated effect of these shutouts on students within a term and over four years.[19] Columns (1) and (2) indicate that a complete shutout of one of these prerequisite courses results in a reduction of 6 units earned in a given term. This effect equates to an entire four-unit course and an associated two-unit lab. Columns (3) through (6) indicate that even this most strict measure of shutouts does not have an impact on four-year graduation rates or time to graduation. However, the within-term effect seems to be compensated for entirely by units attempted in summer. These esti-

[19] A table of the first-stage effect of bin and path on within-term and accumulated prerequisite shutouts is omitted for space reasons, but is available upon request.

mates suggest that students who are unable to get into required major courses respond by taking these same courses (or potentially one from the same sequence) in the summer.

It should be noted that these results may not apply to every major. These majors hopefully offer a representative sample of majors at the university; the three chosen are the largest, the median, and one of the smallest at the university. However, they may differ from other majors in their degree of course crowding or their hierarchy of graduation requirements. Still, they offer insight into the potential consequences of course crowding in particularly important major requirements. This question is not an unimportant one, as evidenced by the full review of bottleneck courses within every campus in the California State University system in 2013.

1.7 Conclusion

The issue of students having access to courses is of importance to university administrations and students alike. This concern has been heightened in recent years as many universities have been faced with increasing enrollment and mostly stagnant resources. Understanding the role of course registration policies in students' ability to access courses can help to identify ways in which administrators can potentially reduce barriers to unit accumulation among students. However, registration timing is typically determined endogenously, either by student behavior or university policy, making it difficult to empirically distinguish the effect of registration timing from the relationship between student characteristics and student outcomes.

This paper provides the first causal evidence of a relationship between university students' assigned course registration time, course availability, and unit accumulation in a given term. Using quasi-random assignment to registration periods based only on a student's last name, I also show that course shutouts resulting from assigned registration time leads students to earn fewer units. Specifically, one in nine students who are unable to get into desired courses due to being assigned to the end of the registration period will complete one less class than those at the beginning of the period. While I show that these students are more likely to enroll in a course with a historically difficult instructor, the reduction in units completed is driven by a reduction in courses attempted. I find younger students and students with the lowest combined SAT and high school GPA to be the most vulnerable to undesirable registration times as well as potentially the least well equipped to respond to course shutout.

I also show that while delayed registration leads to more course shutouts at any point in a student's career, the effects on units earned are strongest in the early stages of their tenure at the university. However, I find that cumulative course shutouts at the two-year and four-year marks have no discernable influence on dropout rates, four year graduation rates, or likelihood of changing major. Instead, it appears that students respond to course shutouts by increasing summer school attendance on both the extensive and intensive margins. Thus while students cite the inability to get into required courses as primary barrier to timely graduation, it is more likely that they are simply unable to get into the required courses they want, but find their way to graduation through other avenues. Still, these avenues can impose large costs on students. A single summer course at this university costs over $1,000 and the opportunity cost from missed income and work experience can be particularly important for college students.

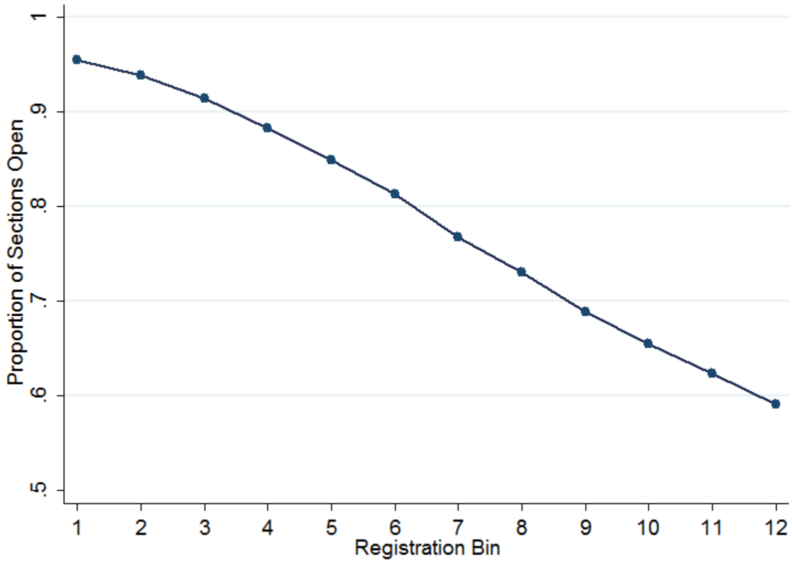
The finding that course availability most strongly and consistently impacts underclassmen presents a few important implications for university administrators. First, many universities operate under a policy in which registration preference is given to the most senior students. While the policy in this setting presents a more balanced approach, one based on seniority may further exacerbate the findings presented in this paper. In such a case, particular attention should be paid to students who struggle to enroll in graduation requirements throughout their first two years. Secondly, the summer term presents an important avenue through which these students may be able to get back on track. Should universities be faced with constraints that limit their ability to offer sections throughout the school year, a relative increase in course offerings in the summer term may help to assuage any adverse impacts on the student body.

Figure 1.1: Realized Registration Bin Assignment by Path

Year 1			Year 2			Year 3			Year 4		
Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring
-	1	12	5	7	3	11	4	9	2	10	6
-	2	10	6	8	1	12	5	7	3	11	4
-	3	11	4	9	2	10	6	8	1	12	5
-	4	9	2	10	6	8	1	12	5	7	3
-	5	7	3	11	4	9	2	10	6	8	1
-	6	8	1	12	5	7	3	11	4	9	2
-	7	3	11	4	9	2	10	6	8	1	12
-	8	1	12	5	7	3	11	4	9	2	10
-	9	2	10	6	8	1	12	5	7	3	11
-	10	6	8	1	12	5	7	3	11	4	9
-	11	4	9	2	10	6	8	1	12	5	7
-	12	5	7	3	11	4	9	2	10	6	8

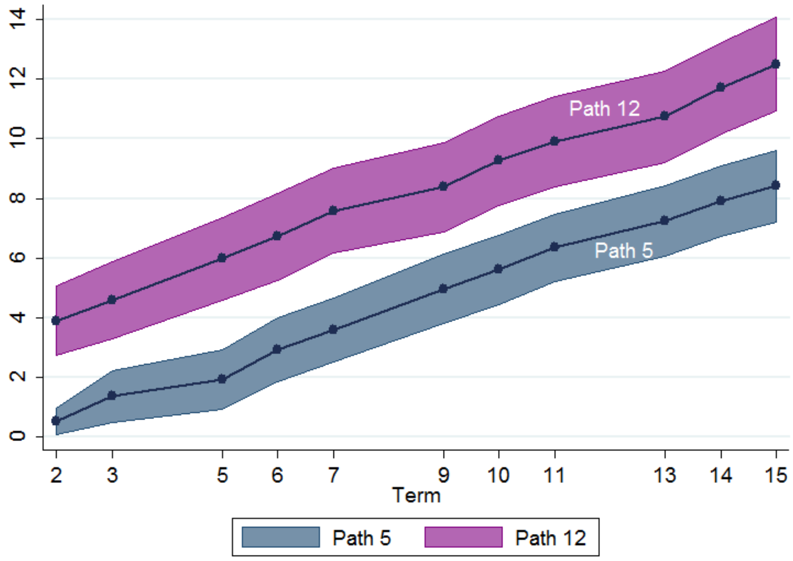
Notes: Each cell denotes the assigned registration bin (out of 12) for each term, broken up into rows based on the first bin assignment. In all cases where a specific registration path is referenced, paths are labeled based on the first bin assignment they realize. More explicitly, the first row relates to Path 1, the second row to Path 2, and so on. Bin has been omitted in the first term as all first-time students who begin in the fall term register after all continuing students.

Figure 1.2: Section Availability by Registration Bin



Notes: Each point measures the average proportion of sections with at least one seat open at the beginning of each registration bin. Averages are taken over all terms in the sample, excluding summers. The proportion available at the beginning of Bin 1 is not 1 because students with priority registration are allowed to register before the beginning of the first bin. While figure varies slightly by term, the values shown here are fairly representative of the typical term.

Figure 1.3: Accumulated Shutouts by Path by Term



Notes: Figure includes point estimates of cumulative shutouts in each term for paths 5 and 12. Terms 4, 8, and 12 are excluded because they are summer terms where registration timing follows a different process. Intervals represent the 95% confidence interval around the point estimate. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist.

Table 1.1: Balance Across Registration Bins and Registration Paths

	One Observation per Student-Term				One Obs per Student	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Std Dev	P-value on Bins	Obs	P-value on Paths	Obs
—Tests & Grades—						
SAT Total	1206.47	133.80	0.971	255,279	0.256	20,293
SAT Read	584.08	78.98	0.969	246,084	0.099	19,810
SAT Math	622.45	78.25	0.569	245,875	0.329	19,799
Miss SAT	0.0801	0.271	0.312	277,509	0.235	22,629
AP - Attempted	2.9330	2.343	0.991	270,768	0.939	21,885
AP - Pass	2.0977	2.031	0.996	270,768	0.624	21,885
HS GPA	3.7929	0.347	0.455	276,732	0.248	22,564
—Activities—						
NCAA Interest	0.2397	0.427	0.273	235,250	0.679	22,595
VA Benefits	0.00061	0.025	0.592	277,509	0.260	22,629
HS Leadership	0.7776	0.416	0.520	235,129	0.014*	22,580
HS Work Hrs	2.7007	1.546	0.941	235,233	0.029*	22,592
HS Activity Hrs	4.2463	1.388	0.911	235,233	0.446	22,592
—Family—						
CA Resident	0.9069	0.291	0.033*	277,478	0.046*	22,626
Foreign	0.0023	0.048	0.040*	277,478	0.117	22,626
Parents Coll	0.9247	0.264	0.738	226,557	0.338	21,732
Under $24k	0.0370	0.189	0.964	198,311	0.651	18,905
Over $72k	0.7531	0.431	0.889	198,311	0.659	18,905
Missing Income	0.2854	0.452	0.110	277,509	0.025*	22,629
Appl Fee Waiver	0.0408	0.198	0.615	277,509	0.843	22,629
—Demographics—						
Male	0.5387	0.498	0.319	277,427	0.032*	22,621
White	0.6312	0.482	0.329	277,509	0.916	22,629
Asian	0.1198	0.325	0.144	277,509	0.854	22,629
Hispanic	0.0858	0.280	0.006**	277,509	0.000**	22,629
Black	0.0060	0.077	0.357	277,509	0.506	22,629

Notes: Columns (1) through (4) include all student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university as they do not experience a traditional registration assignment time. P-values in column (3) come from an F-test of joint equivalence of the 12 registration bins, controlling for term & cohort. Columns (5) and (6) include one observation per student. The selection of students is limited to students who entered the university in a Fall term as a first-time freshman during the sample period. P-values in column (5) come from an F-test of joint equivalence of 12 paths, controlling for cohort. * significant at 5% level, ** significant at 1% level.

Table 1.2: Relationship Between Registration Outcomes and Student Traits

	Days to Registration (1)	Shutouts (2)	Units Attempted (3)	Return Next Term (4)
SAT Total	0.00178***	0.000218***	-0.000373***	-2.10e-05***
	(0.000173)	(4.94e-05)	(7.05e-05)	(2.48e-06)
HS GPA	-0.804***	-0.0134	0.297***	0.00760***
	(0.0662)	(0.0195)	(0.0296)	(0.000930)
HS Leadership Pos.	-0.00694	0.0264	0.0823***	0.00179***
	(0.0446)	(0.0215)	(0.0236)	(0.000630)
Resident CA	-0.425***	0.0149	0.0488*	0.00367***
	(0.0638)	(0.0214)	(0.0255)	(0.000962)
Parents AttColl	-0.0385	-0.0555*	0.0220	0.00511***
	(0.0929)	(0.0285)	(0.0401)	(0.00149)
FamInc >$72K	-0.244***	-0.0163	-0.0272	0.00257***
	(0.0514)	(0.0171)	(0.0242)	(0.000768)
FamInc <$24K	0.154	-0.0926	-0.0177	-0.00405*
	(0.156)	(0.0587)	(0.0575)	(0.00242)
App Fee Waiver	0.327**	0.144**	-0.149**	-0.00312
	(0.145)	(0.0612)	(0.0574)	(0.00200)
Male	0.503***	-0.207***	-0.243***	-0.00391***
	(0.0327)	(0.0154)	(0.0142)	(0.000481)
Asian	-0.0404	0.299***	0.0926***	-0.000487
	(0.0637)	(0.0292)	(0.0264)	(0.000736)
Hispanic	0.171**	0.00202	-0.134***	-0.00502***
	(0.0658)	(0.0236)	(0.0311)	(0.00110)
Black	0.337	0.188**	-0.0953	-0.0126***
	(0.223)	(0.0770)	(0.118)	(0.00416)
Sophomore	-1.676***	-0.0681*	0.674***	0.0168***
	(0.159)	(0.0345)	(0.0259)	(0.00107)
Junior	-2.779***	-0.181***	1.351***	0.0338***
	(0.167)	(0.0415)	(0.0424)	(0.00152)
Senior	-3.207***	-0.215***	1.974***	0.0469***
	(0.170)	(0.0418)	(0.0613)	(0.00207)
mean(Y)	3.98	0.925	14.42	0.985
stdev(Y)	(8.26)	(2.30)	(3.02)	(.117)
Observations	277,509	277,509	277,509	255,904

Notes: Columns (1) through (3) include all student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Column (4) also excludes students in their graduating term. Days to Registration is measured as the number of days between a student's first attempt and the beginning of the first name-based registration bin. The measure is non-discrete and, in a term where a student uses a priority pass, can be negative. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.3: Registration Assignment, Shutouts, and Units Earned

	Shutouts		Units Earned	
	(1)	(2)	(3)	(4)
Bin 2	0.067	0.066	-0.053	-0.051
	(0.059)	(0.059)	(0.044)	(0.044)
Bin 3	0.167***	0.166***	-0.135***	-0.138***
	(0.061)	(0.062)	(0.043)	(0.042)
Bin 4	0.232***	0.231***	-0.129***	-0.128***
	(0.059)	(0.059)	(0.043)	(0.041)
Bin 5	0.338***	0.337***	-0.153***	-0.156***
	(0.050)	(0.050)	(0.043)	(0.041)
Bin 6	0.441***	0.441***	-0.205***	-0.205***
	(0.046)	(0.046)	(0.047)	(0.044)
Bin 7	0.488***	0.488***	-0.235***	-0.240***
	(0.050)	(0.051)	(0.044)	(0.043)
Bin 8	0.555***	0.554***	-0.232***	-0.235***
	(0.049)	(0.048)	(0.041)	(0.037)
Bin 9	0.676***	0.675***	-0.282***	-0.284***
	(0.057)	(0.056)	(0.039)	(0.036)
Bin 10	0.690***	0.690***	-0.271***	-0.268***
	(0.065)	(0.065)	(0.047)	(0.040)
Bin 11	0.884***	0.883***	-0.234***	-0.234***
	(0.095)	(0.094)	(0.043)	(0.039)
Bin 12	1.156***	1.157***	-0.238***	-0.246***
	(0.164)	(0.164)	(0.048)	(0.046)
Traits		Yes		Yes
mean(Y)	0.925	0.925	13.8	13.8
stdev(Y)	(2.30)	(2.30)	(3.48)	(3.48)
Observations	277,509	277,509	277,509	277,509

Notes: All columns include all student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.4: Within-Term Effect of Shutouts on Units Earned and Dropout

	Total Units Earned		Major Units Earned		Return Next Term	
	(1)	(2)	(3)	(4)	(5)	(6)
Shutouts_{it}	-0.217***	-0.217***	-0.0574**	-0.0566**	0.000558	0.000544
	(0.0248)	(0.0249)	(0.0243)	(0.0244)	(0.000726)	(0.000727)
Traits		Yes		Yes		Yes
mean(Y)	13.891	13.891	4.461	4.461	0.985	0.985
stdev(Y)	(3.486)	(3.486)	(4.336)	(4.336)	(0.117)	(0.117)
Observations	277,509	277,509	277,509	277,509	255,904	255,904

Notes: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts in that term, t. Columns (1) through (4) include all student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Columns (5) and (6) also excludes students in their graduating term. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. Major Units Earned measured as total units earned in term t in courses hosted within the student's majors' department. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.5: Within-Term Effect of Shutouts on Units Attempted

	Total Units Attempted		Major Units Attempted		# Courses Taken		Dropped a Course	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shutouts_{it}	-0.251***	-0.252***	-0.0657***	-0.0650***	-0.0534***	-0.0538***	0.0335***	0.0335***
	(0.0231)	(0.0232)	(0.0247)	(0.0249)	(0.00734)	(0.00727)	(0.00297)	(0.00297)
Traits		Yes		Yes		Yes		Yes
mean(Y)	14.426	14.426	4.583	4.583	4.06	4.06	0.254	0.254
stdev(Y)	(3.023)	(3.023)	(4.356)	(4.356)	(1.06)	(1.06)	(0.435)	(0.435)
Observations	277,509	277,509	277,509	277,509	277,509	277,509	277,509	277,509

Notes: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts in that term, t. All columns include all student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. Major Units Attempted measured as total units attempted in term t in courses hosted within the student's majors' department. Dropped a Course is a binary outcome for whether a student enrolled in a class and then dropped it after the start of the term. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.6: Within-Term Effect of Shutouts on Course Characteristics

	PANEL A: Share of Sections with Instructor Type (by historical pass rate)							
	High Pass Rate		Median Pass Rate		Low Pass Rate		Missing Pass Rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shutouts_{it}	-0.0179***	-0.0179***	0.000250	0.000216	0.00736**	0.00743**	0.0103***	0.0103***
	(0.00346)	(0.00346)	(0.00170)	(0.00171)	(0.00305)	(0.00306)	(0.00170)	(0.00170)
Traits		Yes		Yes		Yes		Yes
mean(Y)	0.407	0.407	0.084	0.084	0.443	0.443	0.064	0.064
stdev(Y)	(0.371)	(0.371)	(0.206)	(0.206)	(0.376)	(0.376)	(0.187)	(0.187)
Observations	215,350	215,350	215,350	215,350	215,350	215,350	215,350	215,350
	PANEL B: Share of Sections with Instructor Rank or Meeting Type							
	Lecturer		Tenured Prof		Early Meetings		Avg # of Mtgs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shutouts_{it}	-0.0232***	-0.0231***	0.0110***	0.0108***	0.0169***	0.0170***	0.0162***	0.0156***
	(0.00368)	(0.00368)	(0.00317)	(0.00317)	(0.00339)	(0.00339)	(0.00551)	(0.00545)
Traits		Yes		Yes		Yes		Yes
mean(Y)	0.441	0.441	0.299	0.299	0.252	0.252	2.53	2.53
stdev(Y)	(0.378)	(0.378)	(0.351)	(0.351)	(0.352)	(0.352)	(0.728)	(0.728)
Observations	215,350	215,350	215,350	215,350	215,350	215,350	215,350	215,350

Notes: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts in that term, t. All columns include student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Observations are further limited to student-term observations where a student took at least one course which offered more than one section and more than one instructor. In 20% of cases, students do not take one of these courses. Historical pass rate is measured as the average pass rate of that instructor in a course of similar level (upper- or lower-division) in the same department from the most recent term where they taught a similar course. As historical pass rates can not be determined for any instructors in the first period of my data set, this term is omitted. "High", "Median" and "Low" pass rates are determined relative to the median historical pass rate for all instructors teaching that course in that term. See Section IV for a more detailed description. Lecturer and Tenured is measured as the number of multi-section courses taken with this type of instructor by student i in term t, divided by the total number of multi-section courses that student takes in that term. Early Meetings is Measured as the number of multi-section courses taken that meet before 10 AM by student i in term t, divided by the total number of multi-section courses that student takes in that term. Avg # of Mtgs is measured as the average of the number of meeting days across all multi-section courses taken that by that student in that term. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.7: Within-Term Effect of Shutouts on Grades and Course Completion

	GPA		Failed a Course		Grade Points	
	(1)	(2)	(3)	(4)	(5)	(6)
Shutouts_{it}	0.0167***	0.0180***	-0.00491**	-0.00515***	-0.409***	-0.390***
	(0.00332)	(0.00317)	(0.00194)	(0.00192)	(0.0713)	(0.0707)
Traits		Yes		Yes		Yes
mean(Y)	2.949	2.949	0.115	0.115	42.77	42.77
stdev(Y)	(0.706)	(0.706)	(0.319)	(0.319)	(42.77)	(42.77)
Observations	271,335	271,335	277,509	277,509	271,335	271,335

Notes: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts in that term, t. Columns (3) and (4) include all student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Columns (1), (2), (5), and (6) include fewer observations as sometimes a student will not take any courses for a grade. Grade points are measured as the product of GPA and units earned. Failed a Course is a binary outcome for whether the student failed a course in that term. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.8: Heterogeneity of Within-Term Effect of Shutouts by Student Type

	PANEL A: Calculated Eligibility Index			
	——Low Eligibility——		——High Eligibility——	
Y = Tot Units Earned	(1)	(2)	(3)	(4)
Shutouts_{it}	-0.260***	-0.262***	-0.182***	-0.183***
	(0.0435)	(0.0438)	(0.0327)	(0.0328)
Traits		Yes		Yes
mean(Y)	13.40	13.40	14.32	14.32
stdev(Y)	(3.71)	(3.71)	(3.14)	(3.14)
Observations	67,675	67,675	69,504	69,504
	PANEL B: Family Income			
	——Low Income——		——High Income——	
Y = Tot Units Earned	(1)	(2)	(3)	(4)
Shutouts_{it}	-0.150***	-0.155***	-0.221***	-0.220***
	(0.0581)	(0.0578)	(0.0266)	(0.0267)
Traits		Yes		Yes
mean(Y)	13.54	13.54	13.95	13.95
stdev(Y)	(3.72)	(3.72)	(3.35)	(3.35)
Observations	15,692	15,692	149,356	149,356

Notes: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts in that term, t. All columns include student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Eligibility index is a function of SAT and HS GPA. "High" and "Low" determined by top and bottom quartile of cohort. Low income (<$36k) and high income (>$72k) come from "estimated parental income" as reported by the student on their college application. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.9: Heterogeneity of Within-Term Effect of Shutouts by Major

	Engineering		Business		Liberal Arts		Agriculture	
Y = Maj Units Earned	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shutouts_{it}	-0.0632	-0.0631	0.0391	0.0357	-0.365***	-0.366***	-0.490***	-0.491***
	(0.0387)	(0.0388)	(0.0681)	(0.0679)	(0.0713)	(0.0714)	(0.0953)	(0.0953)
Traits		Yes		Yes		Yes		Yes
mean(Y)	4.19	4.19	6.27	6.27	5.83	5.83	5.20	5.20
stdev(Y)	(3.96)	(3.96)	(4.09)	(4.09)	(4.46)	(4.46)	(3.560)	(3.560)
Observations	50,403	50,403	38,799	38,799	42,431	42,431	20,515	20,515

Notes: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts in that term, t. All columns include student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. Major Units Earned measured as total units earned in term t in courses hosted within the student's majors' department. All columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.10: Effects of Delay and Shutouts by Term Order

	Year 1		Year 2			Year 3			Year 4		
PANEL A: Shutouts	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring
Days to Registr.$^{\dagger}_{it}$	0.197***	0.240***	0.238***	0.0759***	0.0879***	0.123***	0.0591***	0.0920***	0.102***	0.0559***	0.126***
	(0.0183)	(0.0296)	(0.0243)	(0.0150)	(0.0193)	(0.0132)	(0.0114)	(0.0195)	(0.0174)	(0.0124)	(0.0176)
mean(Y)	1.30	1.31	1.27	0.82	0.84	0.85	0.69	0.73	0.73	0.59	0.68
stdev(Y)	(2.96)	(2.99)	(3.04)	(1.99)	(2.08)	(2.07)	(1.69)	(1.74)	(1.85)	(1.50)	(1.78)
Observations	30,749	30,298	27,631	27,039	26,649	23,954	23,891	23,676	21,990	21,623	17,896
PANEL B: Total Units Earned	Year 1		Year 2			Year 3			Year 4		
	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring
Shutouts$_{it}$	-0.175***	-0.223***	-0.255***	-0.378***	-0.256***	-0.453***	-0.185	-0.0501	-0.345	-0.429*	0.0246
	(0.0284)	(0.0312)	(0.0374)	(0.146)	(0.0878)	(0.146)	(0.154)	(0.181)	(0.226)	(0.258)	(0.206)
mean(Y)	14.01	13.86	13.82	14.11	13.94	13.47	14.24	14.06	13.80	14.06	13.27
stdev(Y)	(3.34)	(3.25)	(3.56)	(3.30)	(3.15)	(4.30)	(3.27)	(3.27)	(3.93)	(3.26)	(3.55)
Observations	30,749	30,298	27,631	27,039	26,649	23,954	23,891	23,676	21,990	21,623	17,896

Note: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts or days to registration in that term, t. All columns include student-term observations for students who entered the university in a Fall term as a first-time freshman. In each column, observations are limited to the year and quarter indicated by the column header. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. Days to Registration is measured as the number of days between a student's first attempt and the beginning of the first name-based registration bin. The measure is non-discrete and, in a term where a student uses a priority pass, can be negative. All columns include FE for term and cohort and standard errors are clustered at the bin-cohort level (144 clusters). All columns include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. All regressions are stable to inclusion/exclusion of student controls and fixed effects. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.11: Effect of Delay on Instructor Difficulty and Course Crashing by Term Order

	Year 1		Year 2			Year 3			Year 4		
	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring
PANEL A: Difficult Instructor - Grades											
$Shutouts_{it}$	0.0224***	0.0160***	0.00214	0.0233	0.00899	-0.00320	0.0170	0.0385	-0.0263	-0.00566	-0.0182
	(0.00436)	(0.00343)	(0.00564)	(0.0181)	(0.0157)	(0.0151)	(0.0223)	(0.0240)	(0.0238)	(0.0310)	(0.0247)
mean(Y)	0.782	0.802	0.768	0.805	0.800	0.776	0.802	0.792	0.751	0.751	0.742
stdev(Y)	(0.41)	(0.40)	(0.42)	(0.40)	(0.40)	(0.42)	(0.40)	(0.41)	(0.43)	(0.43)	(0.44)
Observations	30,375	29,961	24,201	26,517	26,292	20,376	23,437	23,253	19,046	21,180	17,570
PANEL B: Units Attempted											
	Year 1		Year 2			Year 3			Year 4		
	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring
$Shutouts_{it}$	-0.212***	-0.236***	-0.263***	-0.354***	-0.230***	-0.495***	-0.121	-0.241	-0.414**	-0.446*	0.0255
	(0.0299)	(0.0287)	(0.0347)	(0.121)	(0.0762)	(0.127)	(0.139)	(0.183)	(0.211)	(0.242)	(0.189)
mean(Y)	14.80	14.52	14.44	14.62	14.42	14.09	14.68	14.46	14.26	14.39	13.64
stdev(Y)	(2.58)	(2.58)	(3.08)	(2.89)	(2.72)	(3.78)	(2.83)	(2.91)	(3.50)	(3.00)	(3.33)
Observations	30,749	30,298	27,631	27,039	26,649	23,954	23,891	23,676	21,990	21,623	17,896
PANEL C: Likelihood of Crashing a Course											
	Year 1		Year 2			Year 3			Year 4		
	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring
$Shutouts_{it}$	0.0621***	0.0576***	0.0318***	0.108***	0.109***	0.0728***	0.202***	0.143***	0.0654***	0.225***	0.179***
	(0.00412)	(0.00569)	(0.00414)	(0.0142)	(0.0132)	(0.0111)	(0.0246)	(0.0219)	(0.0139)	(0.0235)	(0.0209)
mean(Y)	0.117	0.126	0.077	0.108	0.113	0.068	0.115	0.122	0.088	0.135	0.139
stdev(Y)	(0.32)	(0.33)	(0.27)	(0.31)	(0.32)	(0.25)	(0.32)	(0.33)	(0.28)	(0.34)	(0.35)
Observations	30,749	30,298	27,631	27,039	26,649	23,954	23,891	23,676	21,990	21,623	17,896

Note: Coefficients come from instrumental variables regressions where within-term registration time (bin) assignment is used as an instrument for shutouts in that term, t. All columns include student-term observations for students who entered the university in a Fall term as a first-time freshman. In each column, observations are limited to the year and quarter indicated by the column header. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. Panel A uses a binary outcome for whether or not student has an instructor of that type. See section IV for measurement construction. In Panel C, a course is considered crashed if the student waitlisted and subsequently enrolled in that course after the beginning of the term. All columns include FE for term and cohort and standard errors are clustered at the bin-cohort level (144 clusters). All columns include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. All regressions are stable to inclusion/exclusion of student controls and fixed effects. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.12: Long-Run Effect of Registration Path on Cumulative Shutouts

	$\sum$ Shutouts$_i^{2yrs}$		$\sum$ Shutouts$_i^{4yrs}$	
	(1)	(2)	(3)	(4)
Path 2	-2.224***	-2.167***	-2.041***	-1.972***
	(0.446)	(0.423)	(0.570)	(0.523)
Path 3	-1.579***	-1.616***	-0.555	-0.655
	(0.409)	(0.391)	(0.483)	(0.442)
Path 4	-2.270***	-2.286***	-1.995***	-1.993***
	(0.540)	(0.538)	(0.578)	(0.581)
Path 5	-2.578***	-2.541***	-2.277***	-2.268***
	(0.557)	(0.545)	(0.701)	(0.671)
Path 6	-2.462***	-2.481***	-2.537***	-2.578***
	(0.521)	(0.520)	(0.619)	(0.623)
Path 7	-1.581***	-1.644***	-1.447***	-1.599***
	(0.453)	(0.442)	(0.496)	(0.481)
Path 8	-1.810***	-1.736***	-1.502***	-1.425***
	(0.423)	(0.418)	(0.496)	(0.486)
Path 9	-1.287***	-1.279***	-1.005**	-1.082***
	(0.405)	(0.392)	(0.418)	(0.395)
Path 10	-1.235**	-1.201**	-0.912	-0.920
	(0.483)	(0.470)	(0.676)	(0.652)
Path 11	0.0967	0.119	0.812	0.780
	(0.722)	(0.683)	(1.019)	(0.943)
Path 12	0.877	0.917	0.943	0.994
	(0.679)	(0.672)	(0.853)	(0.834)
Traits		Yes		Yes
mean(Y)	5.68	5.68	10.46	10.46
stdev(Y)	(7.61)	(7.61)	(11.64)	(11.64)
Observations	20,612	20,612	16,423	16,423

Note: All columns include one observation per student. Observations are limited to students who enter the university during the sample period as first-time freshman in a fall term and enter at least four years before the end of the sample period. Columns (1) and (2) limit to students who survive until the end of their second year. Columns (3) and (4) limit to students who survive until the first term of their fourth year. Paths are labeled 1 through 12 based on the student's registration bin assignment in the winter term of their freshman year. All columns include FE for cohort and cluster standard errors at the path-cohort level. Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.13: Long-Run Effects of Cumulative Shutouts on Graduation and Major Changes

	Panel A: Four Year Shutouts					
	Terms to Grad		4yr Grad Rate		Change Major	
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum$ Shutouts$_i^{4yrs}$	0.0131	0.00624	-0.00517	-0.00333	0.00311	0.00281
	(0.0127)	(0.0120)	(0.00331)	(0.00306)	(0.00256)	(0.00257)
Traits		Yes		Yes		Yes
mean(Y)	15.78	15.78	0.514	0.514	0.212	0.212
stdev(Y)	(2.51)	(2.51)	(0.497)	(0.497)	(0.409)	(0.409)
Observations	14,847	14,847	16,423	16,423	16,423	16,423
	Panel B: Two Year Shutouts					
	Drop Out After 2ndYr		4yr Grad Rate		Change Major	
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum$ Shutouts$_i^{2yrs}$	-0.00148	-0.00205	-0.00244	-0.00106	0.000317	0.000250
	(0.00257)	(0.00245)	(0.00331)	(0.00310)	(0.00247)	(0.00247)
Traits		Yes		Yes		Yes
mean(Y)	.123	.123	0.446	0.446	0.212	0.212
stdev(Y)	(0.328)	(0.328)	(0.497)	(0.497)	(0.409)	(0.409)
Observations	20,612	20,612	20,612	20,612	20,612	20,612

Note: Coefficients come from instrumental variables regressions where registration path is used as an instrument for cumulative shutouts at either the two-year or four-year mark. All columns include one observation per student. Observations are limited to students who enter the university during the sample period as first-time freshman in a fall term and enter at least four years before the end of the sample period. Panel A limits to students who survive until the first term of their fourth year. Columns (1) and (2) of Panel A are conditional on a student graduating. Panel B limits to students who survive until the end of their second year. Drop Out After 2ndYr is a binary outcome for whether student drops out in any term after Spring of their second year. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for cohort and cluster standard errors at the path-cohort level (72 or 84 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.14: Heterogeneity of the Long-Run Effect of Shutouts

	PANEL A: Eligibility Index - Low & High					
	Terms to Grad		4yr Grad Rate		Change Major	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum$ Shutouts$_i^{4yrs}$	0.0182	0.0108	0.000893	-0.00835	0.00199	-0.00229
	(0.0274)	(0.0225)	(0.00582)	(0.00661)	(0.00655)	(0.00341)
Traits	Yes	Yes	Yes	Yes	Yes	Yes
mean(Y)	16.1	15.7	.339	.496	.212	.214
stdev(Y)	(2.58)	(2.50)	(.473)	(.500)	(.409)	(.410)
Observations	3,588	4,471	5,637	5,638	5,637	5,638
	PANEL B: Major - Engineering & Agriculture					
	Terms to Grad		4yr Grad Rate		Change Major	
	ENG	AG	ENG	AG	ENG	AG
	(1)	(2)	(3)	(4)	(5)	(6)
$\sum$ Shutouts$_i^{4yrs}$	0.00288	0.00573	0.000653	0.000719	0.00163	0.00747
	(0.0153)	(0.0236)	(0.00501)	(0.00529)	(0.00382)	(0.00549)
Traits	Yes	Yes	Yes	Yes	Yes	Yes
mean(Y)	16.6	15.8	.282	.435	.242	.231
stdev(Y)	(2.65)	(2.41)	(.450)	(.495)	(.428)	(.421)
Observations	4,611	3,494	7,281	4,650	7,281	4,650

Note: Coefficients come from instrumental variables regressions where registration path is used as an instrument for cumulative shutouts at the four-year mark. All columns include one observation per student. Observations are limited to students who enter the university during the sample period as first-time freshman in a fall term and enter at least four years before the end of the sample period. All columns limit observations to students who survive until the first term of their fourth year. Columns (1) and (2) of both panels are conditional on a student graduating. Eligibility index is a function of SAT and HS GPA. "High" and "Low" are determined by the top and bottom quartile of the student's cohort. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for cohort and cluster standard errors at the path-cohort level (84 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.15: Long-Run Course Taking Responses to Cumulative Shutouts

	"Heavy" Terms		Summer Terms		Terms w/ Outside Units		Summer Units	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\sum \text{Shutouts}_i^{2yrs}$	0.00683	0.00926	0.00963***	0.00991***	0.00310	0.00557	0.0716**	0.0716**
	(0.0122)	(0.0116)	(0.00356)	(0.00356)	(0.00555)	(0.00521)	(0.0327)	(0.0321)
Traits		Yes		Yes		Yes		Yes
mean(Y)	1.54	1.54	0.468	0.468	0.601	0.601	3.39	3.39
stdev(Y)	(1.71)	(1.71)	(0.723)	(0.723)	(0.837)	(0.837)	5.79	5.79
Observations	20,612	20,612	20,612	20,612	20,612	20,612	20,612	20,612

Note: Coefficients come from instrumental variables regressions where registration path is used as an instrument for cumulative shutouts at the two-year mark. All columns include one observation per student. Observations are limited to students who enter the university during the sample period as first-time freshman in a fall term and enter at least four years before the end of the sample period. All columns limit observations to students who survive until the spring term of their second year. A "heavy" term is defined as a term in which a student attempts more than 16 units. Summer Terms is the number of summer terms the student enrolls in at least one course at the university. Terms w/ Outside Units measured as the number of terms the student attempts units an an outside institution. Shutouts are measured as any instance where a student waitlists for section of a course because it is full and is unable to enroll from the waitlist. All columns include FE for cohort and cluster standard errors at the path-cohort level (72 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 1.16: Effects of Being Shutout of Major Prerequisite Courses

	—W/in Term—		——Across College Career——					
	Units Earned		Terms to Grad		Four Year Grad		Summer Units	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prereq Shuts_{it}	-5.747**	-5.951**						
	(2.579)	(2.568)						
$\sum$ Prereq Shuts_i^{2yrs}			1.460	1.148	-0.0896	-0.0926	4.958*	4.442*
			(0.958)	(0.969)	(0.262)	(0.254)	(2.769)	(2.590)
Traits		Yes		Yes		Yes		Yes
mean(Y)	13.75	13.75	16.48	16.48	0.358	0.358	3.33	3.33
stdev(Y)	(3.46)	(3.46)	(2.45)	(2.45)	(0.479)	(0.479)	(5.63)	(5.63)
Observations	20,572	20,572	1,111	1,111	1,500	1,500	1,500	1,500

Note: Coefficients in columns (1) and (2) come from instrumental variables regressions where within-term registration time (bin) is used as an instrument for shutouts from major prerequisites in that term, t. These columns include FE for quarter (Fall, etc.), term, and cohort and cluster standard errors at the bin-cohort level (144 clusters). These columns include student-term observations for students who entered the university in a Fall term as a first-time freshman. Observations exclude summer terms and students' first term at the university. Columns (3) through (8) include one observation per student. Observations are limited to students who enter the university during the sample period as first-time freshman in a fall term and enter at least four years before the end of the sample period. All columns limit observations to students who survive until the spring term of their second year. Summer Units is the total number of units the student attempts at the university in summer terms over their first four years. Prerequisite shutouts are measured as any instance where a student can not get into any section of a course that is a major requirement (and prerequisite to other major requirements) because it is full and they are unable to enroll from the waitlist. These columns include FE for cohort and cluster standard errors at the path-cohort level (72 clusters). Columns labeled "Yes" include a vector of student-specific controls for class level (by units as denoted by the university) at beginning of the term, SAT score, HS GPA, high school leadership participation, the number of AP tests passed, state & foreign residency, parents' college attainment, family income, gender, and race. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Chapter 2

Non-Linear and Heterogeneous Effects of Peer Gender Composition on Academic Performance

2.1 Introduction

The influence of a student's peer group has long been a point of discussion for economists and education policy makers alike. While research has focused on a wide variety of potentially relevant peer traits, one branch of this work has focused on peer group gender composition. Staring with Hoxby (2000), researchers have explored how the proportion of females in a student's peer group affects academic outcomes such as performance and their choice of subject and career path. Operating under models which suggest that students with similar traits are more effective at sharing information and exchanging human capital, these papers largely find that girls perform better in school when their peer group has a greater share of students who are female. While true exogenous variation in peer group gender composition is rare in a realm where students often non-randomly sort into schools and classrooms, these papers have generally relied on idiosyncratic changes in gender composition across school cohorts to establish a relationship between the proportion of a cohort that is female and students' academic performance.[1] In nearly all cases, estimates hold up to the inclusion of measures of cohort ability, suggesting that gender composition is not simply a proxy for average peer ability.

Yet while some work has been done to establish a linear relationship between peer group gender composition and academic performance, little is known about the relevant non-linearities and heterogeneity of this relationship. Indeed, Sacerdote (2014) notes that "peer effects with nonlinearities may be much more interesting because nonlinearities open up the possibility that some people (or students) could be helped by a change in peers without making other people worse off", suggesting particular attention should be paid to instances of non-linears effects. Moreover, nearly all the standing literature explores these effects in resource rich areas where students are likely at a very different point on the education production function than students in the world's developing regions. As such, estimates from previous work may not be applicable to students in more severely resource constrained areas[2] It is these two gaps in the literature that

[1]See Hoxby (2000), Lavy and Schlosser (2011), Eisenkopf et al. (2015), Schneeweis and Zweimuller (2012), and Anderson and Lu (2015) for a sequence of papers on this topic.

[2]See Sacerdote (2014) for a more in-depth discussion about the sensitivity of estimates of peer effects to context.

motivate the current paper.

To address these questions, I use data from an experiment conducted with 121 primary schools in rural Kenya by Duflo, et al. (2011). Prior to the experiment, all grade 1 students in each of these schools were traditionally taught in one classroom by one teacher. For each of the the 61 schools in the treatment group of the experiment, Duflo, et al.(2011) split students into two classes based on their performance on a baseline exam. In the remaining 60 control schools, grade 1 students were randomly assigned to one of two separate grade 1 classes. It is the 3,300 students from these 60 control schools that make up the sample used in the current paper.

This paper therefore relies on variation in classroom-level gender composition that arises from the random assignment of students to classrooms, a feature that is missing from most other work on this topic. Because of this random assignment, the estimated effects of gender composition in this paper can be thought of as reliably causal estimates, not influenced by students' selection into classrooms or teachers being matched with certain students. Further, the existence of two classrooms in each school allows for an estimation based on school fixed effects that relies on variation in gender composition within schools instead of across schools. In this way the empirical strategy in this paper is not influenced by bias due to selection of students into schools, a primary point of concern in much of the prior literature.

The findings in this paper provide two key contributions. First, I present the first documented causal relationship between a student's academic performance and their peer-group's gender composition in a developing country, Kenya. While such a relationship between performance and peer group has been established in prior work, this work has taken place exclusively in developed, resource rich regions such as the United States and Switzerland. [3] In this way, the estimates in this paper serve as a check of external validity of those from prior work. Overall my findings suggest that peer group gender composition has a significant impact on students' academic performance. Specifically, while I find no evidence of a linear relationship between classroom gender composition and student performance, I show that students perform best when they are in classrooms that have a more balanced gender composition. On average, students in classrooms that are between 45% and 55% female earn endline test scores that are about a seventh of a standard deviation higher than students in classrooms that are more predominantly comprised of one gender. In other words, I show that student performance is not monotonically increasing in the proportion of students that are female. This finding is in contradiction with prior work that suggests that gender composition peer effects follow a linear-in-means model.

Second, I document a potentially important dimension of heterogeneity in the impact of gender composition on student performance, namely teacher type. While prior work has alluded to potential inequities in the effect of gender composition, much of the

[3] Indeed, Anderson and Lu (2015) conduct a study in China. However, their study takes place in a region with an average income that more closely mirrors a developed country than rural Africa.

work has been observational in nature. In contrast, the setting in this paper provides exogenous variation in the type of teacher that teaches in each classroom. Because the experiment in Duflo, et al. (2011) created an additional grade 1 class in every school, these schools had to hire an additional grade 1 teacher. These new teachers were given a one-year contract with the potential to secure full employment and, like the existing civil service teachers, were randomly assigned to a classroom in their school. I show that the effect is stronger when students are taught by a "contract" teacher, as opposed to a tenured "civil-service" teacher. Due to the random assignment of teachers across classrooms in each school, this estimated heterogeneity is not the result of these teachers teaching better students or smaller classes.

While contract teachers differ from civil service teachers on a few dimensions, I show that the heterogeneity exhibited across teacher type is best explained by differences in effort exerted across teacher type. Specifically, the increase in test scores associated with being near the center of the gender composition distribution is strongest when students are taught by teachers who are more likely to be found in class teaching. This effect is even stronger when focusing attention to female students. Female students taught by contract teachers (or teachers who exert more effort) in classrooms with more balanced gender compositions earn endline test scores that are roughly 0.8 standard deviations higher than those in classrooms with a poor gender balance. This finding points to a potentially important relationship between teacher behavior and the strength of intra-student knowledge transfer. If teacher type or teacher effort can influence the presence of peer effects, this may be a relevant dimension of the importance of teacher quality in educational outcomes.

The rest of the paper is organized as follows: Section 2 provides an in depth discussion of the prior literature. Section 3 discusses the setting in which the study takes place as well as the data used in this paper. Section 4 lays out the empirical approach. Section 5 presents the results. Section 6 concludes.

2.2 Existing Literature

Though the peer effects literature has long been interested in how the quality of a student's peers impacts his or her academic performance, the issue of self-selection into peer groups has presented an obstacle to estimating plausibly causal relationships between performance and peer group. While some papers have overcome this issue by studying random assignment to barracks or dorm-mates, pure random assignment into classrooms is rare.[4] Classroom composition is often a reflection of school composition, something that results largely from selection by students and parents. Parents who are concerned with education have children who are more likely to succeed academically, but also more likely to sort into good schools with other good students, making cross-school comparisons misleading. Moreover, classroom organization within schools is potentially subject to non-random sorting, whether by teachers and administrators

[4]See Sacerdote (2014) for an extensive review of studies on peer effects.

or by students and parents. Such manipulation can conflate any impacts of classroom composition with student-teacher compatibility or disproportionate resource allocation.

With this in mind, Hoxby (2000) put forth a non-experimental approach to overcoming such selection issues. Using a panel of schools in Texas, she exploits the natural variation in cohort-level peer composition that arises from differences between adjacent cohorts of students within a given school. This helps to remove the bias due to student selection into schools that often affects comparisons of students across schools of differing peer groups. Specifically, if girls disproportionately sort into a school that publicly stresses the success of female students, high test scores (relative to other schools) may be unjustly attributed to high proportions of girls. Moreover, girls sorting into such a school will likely come from families that particularly care about girls' education. But if composition varies across years within a school due to natural variation in gender of local births, gender differences between adjacent cohorts may be plausibly exogenous.

Using this identification strategy, Hoxby (2000) finds cohort gender composition to be a significant determinant of standardized test performance, even when controlling for peer ability. A 20 percentage point increase in the share of a cohort that is female is associated with a 0.1 standard deviation increase in the average test score of all girls. This also raises boys' average score by the same amount. These effects are strongest for math scores and range up to a 0.4 standard deviation increase in average score, depending on the grade. Hoxby also finds that estimates vary depending on the initial gender composition of the cohort. A 20 percentage point increase in the share of a cohort that is female would be of greater benefit to a cohort that is 50% female than to one that is only 25% female.

Since Hoxby (2000), others have replicated the approach. Lavy and Schlosser (2011) extend this identification to high school, middle school, and elementary school students in Israel. Their estimates are smaller in magnitude, but closely mirror that of Hoxby; a 20% increase in a cohort's proportion of girls leads to a .05 standard deviation increase in standardized test scores for boys and girls alike. The same increase in the proportion of girls increases matriculation rates for girls and boys by 3% and 2%, respectively. In contrast to Hoxby (2000), data used by Lavy and Schlosser (2011) cover individual students instead of sub-group averages. Using individual characteristics they find that the effect of cohort female share is 50% larger when the student's parents have less than 12 years of education, suggesting that effects may be heterogeneous in student type.

Schneeweis & Zweimuller (2012) also rely on natural cross-cohort variation to estimate the effect of gender composition on girls' choice of secondary school (and thus career path) in Austria. They find that a 20 percentage-point increase in the proportion of a cohort that is female is associated with a 20% decrease in the likelihood that a girl attends a traditionally female secondary school. Here "traditionally female" is defined as a school with a history of student bodies greater than 50% female (weak definition) or

66% female (strict definition). Their estimates come from girls who attend "low-track" schools.

While these papers rely on idiosyncratic variations in gender composition, Eisenkopf et al. (2015) exploits the random assignment of female, Swiss high-school students to co-educational or single-sex classes. Though the binary nature of their treatment limits their ability to examine non-linear effects of changes in gender composition, their paper is the only one that can identify gender composition at the classroom level. Because other work has relied on cohort level measures of gender composition, this measure more precisely identifies a student's relevant peer group than prior research. Their findings suggest that females assigned to an all-female class receive grades in German equivalent to females in a co-educational. However, those in the single-sex class earn grades in math that are about .25 standard deviations higher than co-educational females. They conclude that the effect of gender composition is likely to be highly non-linear since "the mere presence of male students compromises the educational environment". Moreover, the discrepancy in effect across subjects suggests further heterogeneity in the settings in which gender composition may be a relevant factor in performance. Finally, this is reinforced by the observed positive impact of teacher gender in single-sex classrooms. Students in all-female classrooms perform significantly better in math when instructed by a male teacher. In this vein, teacher or class type may be relevant factors in the effect of classroom gender composition.

Identification from random assignment is also present in Anderson and Lu (2015). Here, the authors estimate the effects of a student being randomly assigned to sit adjacent to students of the same or opposite gender in a Chinese middle school. In this setting, students are non-randomly arranged into rows based on height (with tallest students in the back), then randomly assigned to a two-person desk within their given row. Estimates indicate that having a female desk mate increases a student's test score by 7% of a standard deviation, regardless of gender. Separating analysis by gender however reveals potential heterogeneity in the effects of peer gender. Specifically, girls perform best when surrounded by girls and boys perform best when surrounded entirely by boys. This finding calls into question the conclusion of other papers that classes with greater proportions of girls benefit from the mere absence of males. Moreover, in separating by high and low performing students, the authors find that it is the lowest performing of students that benefit from a female desk mate, while higher performing students attain no benefit. In this way, Anderson and Lu (2015) show that some types of students may disproportionately benefit from more female dominated classrooms. All estimates are robust to the inclusion of neighbors' baseline test scores.

It is these observed heterogeneous effects that in part along with the observed heterogeneity based on parental education from Lavy and Schlosser (2011) motivate the current study. While these papers suggest the existence (and potential heterogeneity) of peer-gender effects, the exact structure of these effects is unclear. Many of the above papers suggest non-linearities in the effects of gender composition without testing the structure of these effects directly. While empirically desirable, the linear-in-means model of peer effects has been shown to be unreliable (Sacerdote 2014). Moreover, the establishment of a purely linear effect of classroom female share would mean there would be no Pareto improving classroom organization policies as the movement of girls from one class to another would cause as much benefit for the receiving class as harm to the class losing girls. If the effect of classroom gender composition is either non-linear or depends on student, family, or classroom characteristics, this could have important implications for policies related to classroom organization. Thus it is relevant to explore under which conditions female share can be a useful policy tool.

2.2.1 Mechanisms

Though most of the literature measures somewhat modest effects of peer gender composition, these observed effects appear to be independent of peers' ability level. In other words, while some settings show boys and girls to have different average test scores at baseline, the impact of changes in cohort or classroom gender composition is not due to changes in cohort or classroom ability. Hoxby (2000) shows that the estimated effect of cohort gender composition is robust to the inclusion of cohort ability.

This notion is reinforced by Lavy and Schlosser (2011). They note that, "while girls perform remarkably better than boys in Hebrew and English, the effect of the proportion of girls on students' performances is only visible in math and science, subjects where girls have little to no advantage compared to boys", a caveat suggesting that observed effects do not arise from being surrounded by peers of higher ability. To try to identify the channels through which gender composition effects operate, Lavy and Schlosser (2011) administer year-end surveys to students and teachers about classroom activity and their experiences in the classroom that year. They show that students (of both genders) in classrooms with greater proportions of girls experience fewer disruptions and violence, better inter-student relationships, and better teacher-student relationships. Moreover, teachers in elementary school classrooms with greater proportions of females tend to report lower levels of fatigue. The authors suggest that these surveys indicate that these changes in classroom environment are due to changes in classroom composition, but not changes in individual student behaviors.

2.3 Data and Setting

The data in this paper come from Duflo, Dupas, & Kremer (2011). To explore the effects of ability-based tracking on academic performance, Duflo, Dupas, & Kremer (2011) conduct a field experiment in Kenya where schools with a single, grade-one classroom are assigned an additional teacher and, in turn, are able to split Grade 1 students across two classrooms. In their experiment, there are two treatments and four possible groups for any student to be in. The first randomly assigned treatment is at the school level and determines whether grade 1 students will be separated into two ability-based classrooms. In such schools referred to as "tracking" schools students are placed in either a high-ability or low-ability classroom based on their performance on a standardized test taken at the beginning of the school year. Students scoring in the top 50% of their school are assigned to the high-ability (high-track) class while students in the bottom 50% are placed in the low-ability (low-track) class. Of the 121 schools involved in the experiment, 61 are randomly assigned to the "tracking" group.

The remaining 60 schools are assigned to the control group. In the control group (i.e. non-tracking schools), Grade 1 students are randomly assigned across the two classrooms. It is these schools that I use as the basis of my analysis. As much of the gender composition literature is faced with issues of non-random sorting across classrooms, this setting provides a rare case of truly exogenous variation in classroom gender composition. Moreover, because the experiment generated two clearly identified classrooms (where students can be traced back to the exact class within their school), this setting creates identifiable variation in gender composition across classrooms within each school. As a result I am able to conduct within school estimation which, coupled with the random classroom assignment, produces estimates that should be free of bias due to selection into schools and selection into classrooms. While this is present in one other paper, Eisenkopf et al. (2015), their focus on single gender versus mixed gender classrooms limits their ability to more accurately identify any non-linearities in the effect of classroom gender composition. A key contribution of the current paper is thus an improvement in both peer group identification and variation over previous papers.

Relying on data from Duflo, et al. (2011) provides a few additional benefits. First, the use of grade 1 students precludes any accumulated effects of exposure to previous gender composition mixes in prior grades. Second, the data include scores on standardized pre-tests administered to students before entry into grade 1, allowing me to control for initial ability for each student. Combined with standardized tests at the end of grade 1, I am able to construct a measurement of a student's progress over the year as opposed to just final score. Finally, because test scores are broken down by subject, I am able to examine whether students of a particular gender see any differential benefit in subjects in which they typically struggle.

2.3.1 Variation in Gender Composition

Because random assignment to classes would lead all classes to be equally split in expectation, there may be concern that such assignment to classrooms may create insufficient variation in the proportion of the classroom that is female[5] (i.e. the female share). A histogram of the distribution of female share by classroom is shown in Figure 1. The average proportion of the class that is female is about 49% with a standard deviation of 7.5%. Classroom female shares range from 35% up to 75%, suggesting a wide dispersion of female shares.

Figure 2 indicates that variation in female share across all classrooms in the

[5]This measure is also used in Hoxby (2000) and Lavy and Schlosser (2011).

sample is not driven entirely by differences in female share across schools. This figure shows differences in female share between classrooms in the same school can exceed 10%. It should be noted that the largest within school differences are not driven entirely by schools in which the average class size is particularly small. Figures 3 and 4 show how classroom female share and the difference in female share across classrooms in the same school vary with respect to class size and average class size, respectively. Jointly, these figures suggest that there is no relationship between class size and female share.

2.3.2 Variation in Teacher Type

Since schools in the experiment historically had only one Grade 1 classroom, an additional teacher was needed to instruct the additional classroom. To fill this need, each school was assigned a newly-hired contract teacher to compliment the school's existing, lone civil service teacher. However, contract teachers were on average potentially more motivated due to the expressed possibility of being hired again for the following school year, conditional on performance. As a result, each school was staffed with two inherently different Grade 1 teachers. To avoid any issues resulting from teachers self-sorting across the two classrooms, contract and civil-service teachers were randomly assigned to one of the two classrooms. This creates a second layer of treatment and, in turn, exogenous variation in the classroom characteristics. Duflo, Dupas, & Kremer (2011) find that having a contract teacher raises a student's performance on the endline test. In this paper, I explore this variation in teacher type as a source of potential heterogeneity in the effect of female share on academic performance.

2.4 Empirical Approach

The prevailing conclusion of the gender peer effects literature is that greater percentages of girls in a classroom lead to better academic performance, particularly for girls. Yet, such studies have been conducted primarily in developed countries, where students arguably have greater access to educational resources than their developing world counterparts. Thus, as an informal check of external validity, I first run a series of regressions to check for an effect of gender composition on academic performance of Grade 1 students in Kenya using the following specification:

$$Y_{ics} = \beta_0 + \beta_1 FemShare_{ic} + \phi_s + \rho_{ic} + u_{ics} \tag{2.1}$$

where Y_{ics} is the standardized endline test score for student i in classroom c in school s. The primary explanatory variable, $FemShare_{ic}$, is the proportion of student i's classmates that are girls (excluding student i). While previous work has identified a linear relationship between peer-group female share and student performance, I run a variety of related specifications that allow the effect to be non-linear. Specifically, subsequent iterations of equation (1) include a quadratic term, $FemShare^2_{ic}$. Similarly, I consider non-parametric relationships that rely on a set of dummies for whether or not the peer-group female share is in the first, second, or third tercile of the distribution of observed female shares. I also extend this specification to quartiles and quintiles. Because different schools likely host different populations of students that differ in proportion of students that are female, this natural variation in peer-group female share across schools may be related to differences in peer-group ability or age. I thus rely on the variation in peer-group female share within schools by including a series of school fixed effects, ϕ_s. Standard errors are also clustered at school level. Finally, I include a vector of controls, ρ_{ic}, for student age, gender, and performance on a baseline test, as well teacher type and the average baseline test score for student i's classmates.

As gender composition effects may differ by student gender, student ability, or class environment (contract or civil-service teacher), I also run a series of regressions to test for heterogeneous effects of female share on end-line test performance. While conventionally such heterogeneity can be explored by using only the relevant subsets of the population, the need for school fixed effects presents a challenge of collinearity to this strategy. Specifically, running a within-school estimation and limiting observations to students with contract teachers almost perfectly identifies a student's female share measure. Such an estimation would in turn drop the female share variable, the independent variable of interest. I therefore rely on an interaction term based approach with the following regression:

$$
\begin{aligned}
Y_{ics} = {} & \beta_0 + \beta_1 fsQuartile2_{ic} + \beta_2 fsQuartile3_{ic} + \beta_3 fsQuartile4_{ic} \\
& + \beta_4 (fsQuartile2_{ic} * X_i) + \beta_5 (fsQuartile3_{ic} * X_i) + \beta_6 (fsQuartile4_{ic} * X_i) \\
& + \phi_s + \rho_{ic} + e_{ics}
\end{aligned} \tag{2.2}
$$

where Y_{ics} is again the standardized endline test score (total score, literature score, or math score)for student i in school s. The terms $fsQuartile2_{ic}$, $fsQuartile3_{ic}$, and $fsQuartile4_{ic}$ are indicators for whether the female share for student i in classroom c is in the second, third, or fourth quartile of the distribution of observed female shares in the sample, respectively. Similarly, the terms $fsQuartile2_{ic} * X_i$, $fsQuartile3_{ic} * X_i$, and $fsQuartile4_{ic} * X_i$ are a series of interactions where X_i is one of the aforementioned dimensions of potential heterogeneity, such as student ability or teacher type. In cases where the coefficient on an interaction term is significant and of opposite sign of the coefficient on X_i, I conduct an F-test on whether the cumulative effect is significantly different from zero. The terms ϕ_s and ρ_{ic} are the same as in equation (1).

2.4.1 Balance

To be able to interpret the estimates from equations (1) and (2) as causal, differences in peer-group female share must be orthogonal to other student and classroom traits. In other words, classroom female share cannot be correlated with class ability, class size, or teacher type. Table 1 estimates these relationships at the classroom level (i.e. one observation per classroom). Here the female share measure is limited to either a linear term or a collection of dummies for female share quartile, but results shown in this table hold for higher order polynomials and different centile cutoffs. Across all columns, female share and female share quartile have insignificant effects on a variety of student and teacher characteristics. Columns (1) and (2) show that classroom level female share does not explain the classroom's performance on the baseline standardized test. These remaining columns suggest that classrooms with higher proportions of girls are neither taught disproportionately by one type of teacher, nor any different in size or average age.

To ensure that the insignificant coefficients in Table 1 are not due to insufficient power because of sample size, Table 2 re-estimates Table 1 using student-level observations. All regressions again include school fixed effects and reveal no significant relationship between peer group female share and a wide variety of student and instructor traits. Together with Table1, these estimates suggest that differences in a classroom's female share are unrelated to a variety of characteristics that may influence students' year-end test scores. Importantly, any co-movement between students' year-end test scores and their peer-group's gender composition can be interpreted as a causal relationship and not due to differences in peer-group ability, age, or any other factor.

2.5 Results

Table 3 displays estimates of the effect of classroom female share on students' end-line test scores. The outcome in all columns is a normalized, end of the year test score with a mean of 0 and a standard deviation of 1. All coefficients can be interpreted as a standard deviation change in the performance on an end of the year test. Columns (1) and (2) indicate that classroom female share has neither a linear nor quadratic impact on end-line test scores. This finding is in contradiction to Hoxby (2000) and Lavy and Schlosser (2011), which both find that increases in the female share of a cohort due to natural variation in gender composition will increase the performance of all students. While those papers do reasonably well to address the issues of selection into schools, both are done in the context of a developed country and neither estimates the effect for Grade 1 students. In this way the estimates in this paper may not be in contradiction with those of previous work.

Columns (3), (4), and (5) identify female share as falling into a particular tercile, quartile, or quintile of the distribution of observed female shares, respectively. Generally, these columns indicate that students who are exposed to a female share near the center of the distribution do better at endline than students who are in classrooms with relatively low proportions of girls. Particularly, students who have peer groups in the second tercile or quartile of classroom female share achieve end of the year test scores that are roughly one seventh of a standard deviation higher than the excluded group.

Conversely, students exposed to a female share in the third tercile, fourth quartile, or the fourth and fifth quintile earn endline test scores that are statistically indifferent from students in the lowest centiles. The estimates in columns (3) through (5) are the first to document an impact of gender composition on student performance in a developing setting.

Together these estimates suggest a potential non-linear relationship between peer-group female share and student performance. Specifically, students benefit as the peer composition moves from heavily male towards gender parity. However, as the gender composition becomes more disproportionately female, the benefit from more girls begins to fade. While prior work does not test for non-linear effects in this way, subgroup analysis from Hoxby (2000) suggests that the effect of a modest increase in female share may be strongest for classes at the center of the female share distribution. In this way my findings support some of the conclusions of prior work.

2.5.1 Heterogeneity by Student Gender

While previous work has consistently found greater shares of girls to be to the benefit of girls, the results are more mixed for boys. Hoxby (2000) and Lavy and Schlosser (2011) both show that boys perform better in heavily female classes. However, estimates from Anderson and Lu (2015) suggest that while girls' perform best when sharing a desk with another girl, boys' performance is highest when surrounded by male classmates. To examine this in my setting, I check the heterogeneity of the effect of classroom gender composition in Table 4.

Columns (1) through (3) show the estimated effect of female share quartile on overall score, literature score, and math score at endline for all students. These estimates indicate that the observed effect of being in the center of the female share distribution may be driven mostly by an effect on literature test scores. Specifically, the effect of the second and third quartiles on total score is reflected in comparable significant effects on literature score. Column (3) suggests that female share may also influence math performance. Specifically, being in the third quartile of the peer-group female share is associated with a fifth of a standard deviation increase in endline math score, relative to the omitted group.

Columns (4) through (6) check for any differential effect by student gender. The interactions in all columns indicate that any benefit derived from peer-group gender composition is not felt disproportionately by one gender. This finding mirrors that of both Hoxby (2000) and Lavy and Schlosser (2011).

2.5.2 Heterogeneity by Student Ability and Teacher Traits

While I find no evidence of heterogeneity on the dimension of student gender, previous work has suggested other potential dimensions of heterogeneity. In their work, Lavy & Schlosser (2011) find the effect of classroom female share to be 50% stronger for students from families with little education. An implication of their work, in part, is that lower ability students potentially stand more to gain from greater densities of girls in their classroom. I test for this type of heterogeneity in the current setting in Table 5 by including interaction terms between female share quartile and a student's ranking within their school on the baseline test. By and large, the estimated interaction terms in this table do not support the finding of Lavy & Schlosser (2011). The only place where student ability interacts with peer-group gender composition is in the second quartile for literature scores. Looking across this row suggests that this is driven entirely by an effect on literature scores for boys. Moreover, the sign of this term indicates that the effect is stronger for boys with higher baseline ability, contradicting the direction of the effect in Lavy & Schlosser (2011). Beyond this term, I find little evidence of any heterogeneity in the effect of gender composition on the dimension of student ability.

As part of the experiment conducted by Duflo, et al. (2011), classrooms were randomly assigned either an existing civil service teacher or a newly hired contract teacher. While civil service teachers had job security as tenured government employees, the newly hired teachers were given a one year contract with the potential of being rehired the following year, conditional on performance in their first year. Consequently, Duflo, et al. (2011) found that contract teachers were more likely to be found in the classroom teaching than their civil service coworkers. The random assignment of these two types of teachers across the two classrooms thus creates an additional layer of (exogenous) heterogeneity in teacher effort.

Table 6 examines how having a contract teacher influences the effect of peer group gender composition. Columns (1) through (3) indicate that there is little differential impact of female share by teacher type. This is reaffirmed in columns (7) through (9) which limit the sample to male students. Isolating the sample to female students in columns (4) through (6) however reveals a strong differential effect. All three interaction terms are significant predictors of girls' total scores and literature scores. The coefficient on "Contract Teacher" however is negative and significant, making it possible that the cumulative effect of switching from a civil service teacher to a contract teacher in zero. The last three rows test this for all significant interaction terms. The respective F-tests show that the overall effect of a girl going from a civil service teacher to a contract teacher is positive for students with peer groups in the second and third quartiles of the female share distribution.

A key conclusion of Lavy and Schlosser (2011) is that boys are more likely to disrupt the classroom and disproportionately draw the teachers attention. In their setting, teachers assigned to classrooms with higher proportions of boys report higher levels of fatigue. If teachers are not present enough to mitigate this disruption, the ability for girls to exchange knowledge may suffer. It may thus be the case that teacher effort plays a role in limiting the degree to which boys adversely impact girls' learning environment. While I cannot test this directly, I can check to see if the heterogeneity observed across teacher type is reflected in a heterogeneous effect in teacher effort. As part of their study, Duflo, et al. (2011) do random spot checks at schools to see if teachers are in their classrooms teaching. I interact this measure with female share in Table 7 to check if increased teacher presence increases the effect of classroom female share. The first three columns indicate that increased teacher presence strengthens the effect of gender composition for students with peer groups in the top quartile of the female share distribution. Columns (4) through (6) suggest that this is driven by an effect on girls. Similar to Table 6, the majority of interactions are significant for female students and none are significant for boys. While the coefficient on teacher presence is negative, the last rows of Table 7 indicate that extra effect of female share significantly outweighs the coefficient on contract teachers for students in the top two female share quartiles, particularly for scores in literature. It should be noted that while contract teachers are randomly assigned to classrooms, differences in teacher presence across classrooms are not necessarily exogenous. Thus, the estimated effects from this table should not be interpreted as causal, but as a potential mechanism that may explain the findings in Table 6.

One challenge to the conclusion that having a contract teacher influences the effect of peer-group gender composition is whether contract teachers teach different types of students than civil service teachers. Specifically, if contract teachers are more likely to be matched with classes of higher ability, the observed effects in Table 6 and Table 7 may be due to differences in student ability and not teacher type. The first four columns of Table 8 show how the classes taught by contract and civil service teachers differ. These differences are estimated in regressions that include school fixed effects and can therefore be thought of as within school comparisons of contract and civil service teachers. This methodology mirrors that from the previous tables. Overall, these estimates indicate that the two types of teachers teach similar classes. This parity also affirms that the random assignment of teachers across classrooms was successful in ensuring that both teachers taught similar types of students.

As stated above, contract teachers were shown to be more consistently present in the classroom than civil service teachers. Admittedly, civil service and contract teachers may differ in more than just their observed effort. Previous work suggests that students perform better when they have a teacher that shares some of their characteristics[6]. Thus, if contract teachers are disproportionately female, the observed increase in the effect of female share that results from switching from a civil service teacher to a contract teacher would potentially be driven by higher proportions of girls being matched with female teachers. Columns (5) through (8) of Table 8 compare the two types of teachers across a variety of traits. While contract teachers are found to be in the class teaching 25% of the time more than civil service teachers, they are also less experienced and more predominantly male. These observed differences make the conclusions from Table 7 less clear as in class presence may just be a proxy for teacher gender. Specifically, if teacher gender influenced the effect of gender composition on

[6]See Fairlie, et al. (2014)

student performance, the results in Table 7 would be the result of a correlation between in class presence and teacher gender.

Table 9 thus checks to see if the heterogeneous effects found in Table 7 are reflected in a relationship between teacher gender and the effect of female share. The interaction terms and F-tests across Table 9 generally imply that the observed heterogeneity in Table 7 was not a proxy for heterogeneity by teacher gender. Overall these effects also suggest that teacher gender does not play an important role in fostering an effect of female share on student performance.

Finally, Table 10 examines if the see if the heterogeneous effects found in Table 7 are a proxy for a relationship between teacher experience and the effect of female share. While some of the interaction terms are significant, the F-tests in the bottom rows indicate that nearly all effects sum to zero when taking into account the coefficient on teacher experience. Again, the estimates in this table indicate that the heterogeneity by teacher effort is not due to a heterogeneity in another teacher trait. Additionally, teacher experience does not seem to magnify any effects of peer-group gender composition on student performance, either for girls or boys.

Overall the analysis outlined in this paper suggests that students perform best when the classroom gender composition is equally mixed, as opposed to heavily male or female. Students in these types of classrooms perform up to a fifth of a standard deviation better at endline than their peers in classrooms with less balanced gender compositions. Moreover, teacher effort may play an important role in fostering these types of peer effects. While the exact mechanism is unclear, students in classrooms with gender parity do not exhibit increased test performance if the teacher is not frequently in the classroom. This heterogeneity is particularly acute for female students.

2.6 Conclusion

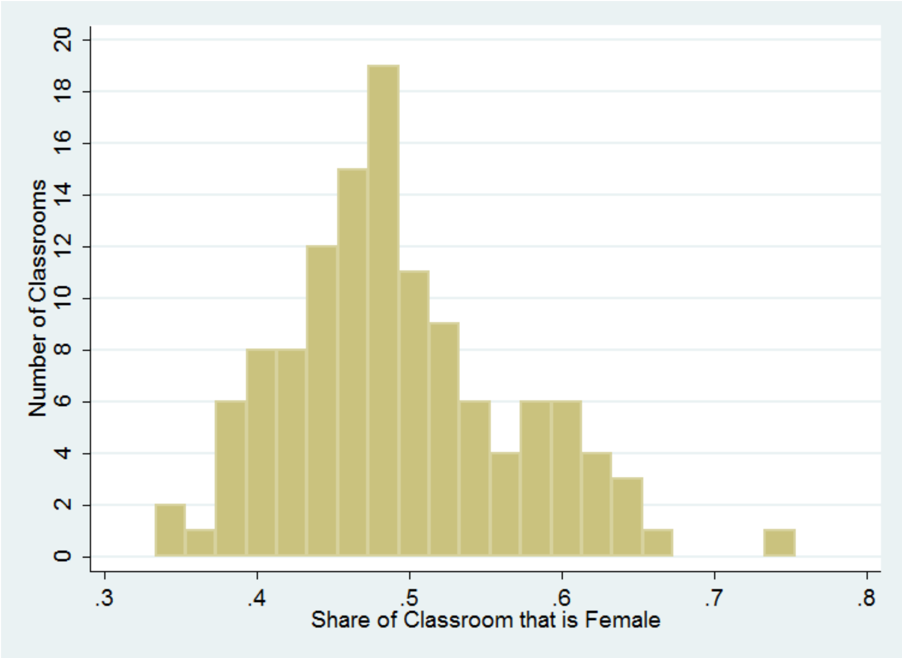
In this paper, I examine the impact of the gender composition of a student's peer group on his or her academic performance. Using data from an experiment that randomly assigned grade-one students in Kenya to one of two classrooms in 60 schools, I estimate both the linear and non-linear effects of the proportion of a student's classmates that are female on a standardized end of the year test. The exogenous variation in classroom gender composition from the experiment provides a rare setting in which concerns of selection into classrooms are mitigated. Additionally, an arm of the experiment that randomly allocated two teacher types across the two classrooms in each school allows me to examine how gender composition and teacher type interact in a plausibly causal manner. Finally, this paper stands as an informal check of external validity to previous papers about peer gender composition in more developed settings.

Contrary to prior work, I find no evidence of a linear effect of peer group gender composition on academic performance, either for male or female students. However, I show that students in classrooms with more balanced gender compositions perform better than students in other classrooms. Specifically, students in classrooms that fall in the second tercile, second and third quartiles, or third quintile of the distribution of classroom gender compositions earn endline test scores that are roughly one seventh of a standard deviation higher than students in other classrooms. This finding holds for both male and female students and are robust to the inclusion of controls for student ability and classroom ability. These are also the first estimates which document an effect of peer group gender composition on student performance in a developing country setting.

Breaking the analysis down by both student gender and teacher type show that this effect is four to five times larger for girls in classrooms taught by newly hired "contract" teachers. Because teachers were randomly assigned to classrooms, this interactive effect can be seen as causal and not coming from any selection of teachers into classes. While the new contract teachers differ from the existing civil service teachers on a few dimensions, I show that the difference in effect of classroom gender composition across these two types of teachers is best explained the difference in effort exerted by contract and civil service teachers. As part of their experiment, Duflo et. al (2011) conduct random spot checks on teachers' attendance. I show that estimates that focus on observed teacher attendance produce coefficients similar to those based on teacher type, suggesting that teacher effort may play a strong interactive role with classroom gender composition. Indeed, no significant relationships are found when interacting classroom gender composition with any other teacher traits.

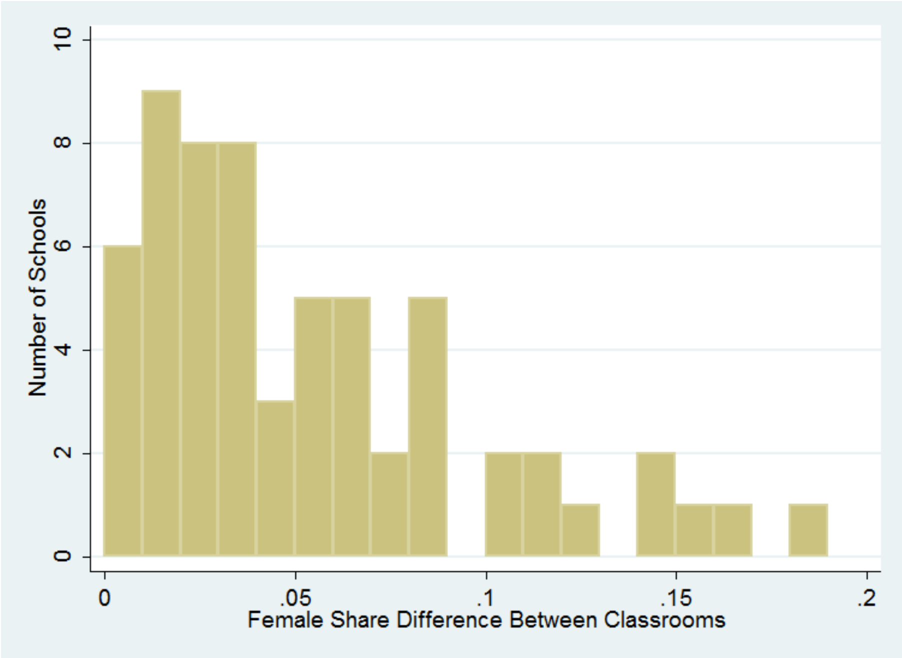
Overall these findings provide a few key insights into the relationship between classroom gender composition and student performance. First, the finding that the type of peer effect in this paper is non-linear points to the possibility of gender-base classroom organization policies that are not zero sum. While the linear estimates in previous work present challenges to actionable policies, the findings here may give renewed hope to the benefits of this type of classroom organization. Second, the estimates shown in previous work may not be limited to those particular settings. The establishment of a relationship in a context such as Kenya suggests that policies that aim to improve student learning via classroom organization may be worth pursuing in regions where more costly interventions are not possible. Finally, that teacher type interacts significantly with the effect of classroom gender composition may point to another dimension in which teachers matter. While a longstanding education literature has documented teacher quality as one of the more important determinants of student learning, future work may benefit from exploring what types of intra-student information exchange are promoted when students are exposed to better teachers.

Figure 2.1: Distribution of Classroom Female Share



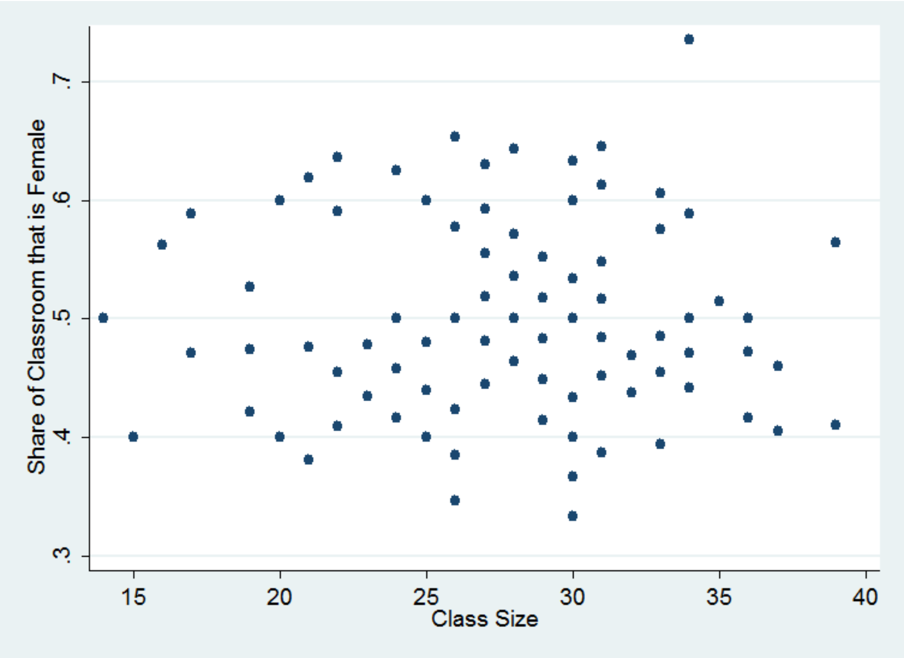
Notes: Figure includes one observation per classroom for all non-tracking schools for a total of 120 observations.

Figure 2.2: Distribution of Within-School Female Share Differences



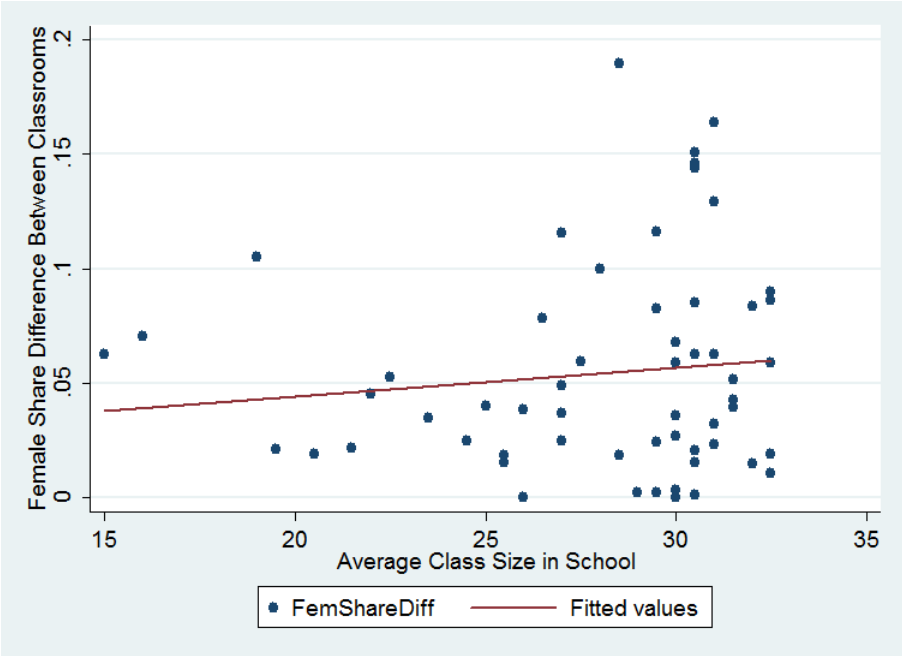
Notes: Figure includes one observation per school for all non-tracking schools for a total of 60 observations. The value for each school is calculated as the absolute value of the difference in classroom female share between the two classrooms in that school.

Figure 2.3: Classroom Female Share versus Class Size



Notes: Figure includes one observation per classroom for all non-tracking schools for a total of 120 observations.

Figure 2.4: Female Share Difference versus Average Class Size



Notes: Figure includes one observation per school for all non-tracking schools for a total of 60 observations. The value for each school is calculated as the absolute value of the difference in classroom female share between the two classrooms in that school. The fitted line comes from a regression of each school's average class size on the absolute value of the classrooms' female share difference. The line has an estimated slope of 0.0012 and an associated standard error of 0.0013, suggesting the relationship is not significantly different from 0.

Table 2.1: Balance (One Observation Per Class)

	Percentile		Class Size		Age		Contract Teacher		Yrs Experience		Female Teacher	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
FemShare	-5.276		0.581		-0.771		0.607		-16.811		-0.665	
	(19.867)		(14.579)		(1.149)		(2.619)		(43.076)		(1.883)	
fsQuartile2		1.916		-1.250		0.011		0.250		-2.013		-0.107
		(3.393)		(1.976)		(0.156)		(0.410)		(7.359)		(0.307)
fsQuartile3		-0.227		0.721		-0.096		0.228		-3.530		-0.159
		(4.290)		(2.191)		(0.130)		(0.416)		(7.052)		(0.254)
fsQuartile4		-2.504		0.952		0.114		0.407		-6.712		-0.287
		(3.811)		(3.351)		(0.224)		(0.607)		(9.419)		(0.382)
Observations	102	102	120	120	120	120	120	120	120	120	120	120
Mean(Y)	51.28	51.28	27.90	27.90	9.15	9.15	0.50	0.50	8.44	8.44	0.53	0.53
StDev(Y)	3.62	3.62	4.99	4.99	0.51	0.51	0.50	0.50	8.64	8.64	0.45	0.45

All regressions include school fixed effects and cluster standard errors at the school level.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.2: Balance (One Observation Per Student

	Percentile		Class Size		Age		Contract Teacher		Yrs Experience		Female Teacher	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
FemShare	-6.011		0.201		-0.866		1.081		-26.171		-0.478	
	(14.410)		(10.634)		(0.855)		(1.868)		(30.442)		(1.395)	
fsQuartile2		2.024		-1.422		0.003		0.362		-4.284		-0.116
		(2.399)		(1.451)		(0.113)		(0.274)		(4.891)		(0.227)
fsQuartile3		-0.057		0.505		-0.100		0.309		-5.067		-0.179
		(3.064)		(1.604)		(0.094)		(0.281)		(4.701)		(0.188)
fsQuartile4		-2.364		0.951		0.131		0.595		-9.786		-0.234
		(2.696)		(2.332)		(0.160)		(0.424)		(6.449)		(0.269)
Observations	2,819	2,819	3,348	3,348	3,182	3,182	3,348	3,348	3,348	3,348	3,348	3,348
Mean(Y)	51.25	51.25	28.79	28.79	9.16	9.16	0.50	0.50	8.45	8.45	0.53	0.53
StDev(Y)	28.55	28.55	4.69	4.69	1.47	1.47	0.50	0.50	8.60	8.60	0.45	0.45

All regressions include school fixed effects and cluster standard errors at the school level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.3: Effect of Female Share (Linear & Centiles) on Endline Score

	Outcome: Normalized Endline Test Score				
	(1)	(2)	(3)	(4)	(5)
FemShare	0.034	8.210			
	(0.751)	(5.567)			
$FemShare^2$		-8.219			
		(6.018)			
fsTercile2			0.137**		
			(0.056)		
fsTercile3			0.050		
			(0.105)		
fsQuartile2				0.145**	
				(0.071)	
fsQuartile3				0.222**	
				(0.089)	
fsQuartile4				0.105	
				(0.147)	
fsQuintile2					0.059
					(0.097)
fsQuintile3					0.155*
					(0.083)
fsQuintile4					0.040
					(0.099)
fsQuintile5					-0.025
					(0.139)
Observations	2,322	2,322	2,322	2,322	2,322

All regressions include school fixed effects and a vector of controls.
Standard errors are clustered at the school level.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Heterogeneity by Student Gender

	Total Score	Lit	Math	Total Score	Lit	Math
	(1)	(2)	(3)	(4)	(5)	(6)
fsQuartile2	0.145**	0.124*	0.137	0.096	0.091	0.079
	(0.071)	(0.064)	(0.087)	(0.079)	(0.073)	(0.100)
fsQuartile3	0.222**	0.191*	0.206**	0.188	0.182	0.153
	(0.089)	(0.099)	(0.091)	(0.114)	(0.124)	(0.103)
fsQuartile4	0.105	0.103	0.084	0.157	0.137	0.144
	(0.147)	(0.152)	(0.132)	(0.161)	(0.167)	(0.147)
Girl	0.041	0.087**	-0.023	-0.003	0.067	-0.084
	(0.039)	(0.040)	(0.041)	(0.070)	(0.072)	(0.073)
fsQuart2_Girl				0.116	0.077	0.135
				(0.081)	(0.084)	(0.087)
fsQuart3_Girl				0.078	0.023	0.121
				(0.121)	(0.132)	(0.106)
fsQuart4_Girl				-0.078	-0.055	-0.088
				(0.111)	(0.122)	(0.104)
Observations	2,322	2,323	2,322	2,322	2,323	2,322

All regressions include school fixed effects and a vector of controls.
Standard errors are clustered at the school level.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Heterogeneity by Student Ability (Percentile Rank in Their School)

	All Students			Girls			Boys		
	total score	lit	math	total score	lit	math	total score	lit	math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
fsQuartile2	-0.033	-0.172	0.139	0.103	0.029	0.167	-0.169	-0.362**	0.100
	(0.104)	(0.122)	(0.106)	(0.159)	(0.171)	(0.173)	(0.160)	(0.179)	(0.156)
fsQuartile3	0.186	0.070	0.278**	0.281	0.198	0.308	0.071	-0.067	0.217
	(0.151)	(0.168)	(0.131)	(0.186)	(0.190)	(0.205)	(0.210)	(0.220)	(0.186)
fsQuartile4	-0.003	-0.122	0.139	0.037	-0.099	0.190	0.057	0.013	0.094
	(0.193)	(0.213)	(0.160)	(0.219)	(0.247)	(0.207)	(0.270)	(0.272)	(0.246)
Percentile	0.015***	0.010***	0.018***	0.017***	0.013***	0.019***	0.013***	0.008***	0.017***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
fsQuart2_Percentile	0.003	0.006**	-0.000	0.001	0.002	-0.001	0.007**	0.009***	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
fsQuart3_Percentile	0.001	0.002	-0.001	-0.001	-0.000	-0.001	0.002	0.004	-0.000
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
fsQuart4_Percentile	0.002	0.004	-0.001	-0.001	0.002	-0.004	0.004	0.003	0.004
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)
Observations	2,322	2,323	2,322	1,108	1,109	1,108	1,214	1,214	1,214

Percentile is measured as the student's percentile rank (out of 100) on the baseline test, relative to all students in their school. All regressions include school fixed effects and a vector of controls. Standard errors are clustered at the school level.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Heterogeneity by Teacher Type

	All Students			Girls			Boys		
	Total Score	Lit	Math	Total Score	Lit	Math	Total Score	Lit	Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
fsQuartile2	0.061	0.064	0.043	-0.126	-0.165	-0.049	0.224	0.223*	0.172
	(0.092)	(0.084)	(0.115)	(0.131)	(0.163)	(0.125)	(0.140)	(0.125)	(0.158)
fsQuartile3	0.193	0.156	0.193*	0.080	-0.025	0.192	0.272	0.264	0.216
	(0.116)	(0.134)	(0.107)	(0.168)	(0.190)	(0.148)	(0.177)	(0.199)	(0.144)
fsQuartile4	-0.022	-0.024	-0.014	-0.185	-0.189	-0.135	0.156	0.082	0.206
	(0.165)	(0.168)	(0.153)	(0.197)	(0.226)	(0.192)	(0.234)	(0.209)	(0.225)
Contract Teacher	-0.067	-0.038	-0.086	-0.286*	-0.368**	-0.120	0.064	0.163	-0.068
	(0.159)	(0.122)	(0.186)	(0.158)	(0.181)	(0.127)	(0.209)	(0.135)	(0.262)
fsQuart2_Contract	0.262	0.205	0.266	0.653***	0.722***	0.417**	-0.013	-0.144	0.144
	(0.190)	(0.163)	(0.215)	(0.208)	(0.249)	(0.165)	(0.260)	(0.198)	(0.310)
fsQuart3_Contract	0.171	0.169	0.128	0.484**	0.590**	0.234	-0.027	-0.102	0.067
	(0.188)	(0.160)	(0.214)	(0.202)	(0.222)	(0.181)	(0.255)	(0.209)	(0.295)
fsQuart4_Contract	0.302	0.288**	0.246	0.432**	0.493**	0.258	0.244	0.220	0.214
	(0.195)	(0.142)	(0.235)	(0.207)	(0.200)	(0.245)	(0.242)	(0.176)	(0.293)
Observations	2,322	2,323	2,322	1,108	1,109	1,108	1,214	1,214	1,214
fsQ2_Contract>Contract	-	-	-	0.001***	0.012**	0.000***	-	-	-
fsQ3_Contract>Contract	-	-	-	0.090*	0.064*	-	-	-	-
fsQ4_Contract>Contract	-	0.005***	-	0.269	0.176	-	-	-	-

Contract is a dummy variable for whether or not the student is taught by a (newly hired) contract teacher.
All regressions include school fixed effects and a vector of controls. Standard errors are clustered at the school level.
The final three rows show the p-value on an F-test on whether the two specified coefficients sum to 0 (if interaction is significant).
*** p<0.01, ** p<0.05, * p<0.1

Table 2.7: Heterogeneity by Proportion of Time that Teacher is Found in Class

	All Students			Girls			Boys		
	Total Score	Lit	Math	Total Score	Lit	Math	Total Score	Lit	Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
fsQuartile2	-0.131	-0.058	-0.185	-0.272	-0.385	-0.071	0.027	0.168	-0.147
	(0.226)	(0.229)	(0.213)	(0.330)	(0.390)	(0.236)	(0.317)	(0.270)	(0.338)
fsQuartile3	-0.151	-0.062	-0.217	-0.411	-0.509	-0.192	0.092	0.276	-0.147
	(0.212)	(0.226)	(0.200)	(0.307)	(0.339)	(0.272)	(0.309)	(0.304)	(0.302)
fsQuartile4	-0.404	-0.265	-0.470*	-0.627**	-0.479	-0.649**	-0.116	-0.115	-0.088
	(0.257)	(0.250)	(0.243)	(0.306)	(0.351)	(0.260)	(0.397)	(0.355)	(0.384)
In Class	-0.545	-0.393	-0.593*	-0.591	-0.553	-0.492	-0.549	-0.372	-0.627
	(0.343)	(0.311)	(0.348)	(0.429)	(0.522)	(0.296)	(0.413)	(0.297)	(0.489)
fsQuart2_InClass	0.442	0.295	0.509	0.609	0.742	0.306	0.291	-0.003	0.567
	(0.328)	(0.312)	(0.321)	(0.450)	(0.546)	(0.301)	(0.418)	(0.326)	(0.478)
fsQuart3_InClass	0.680*	0.471	0.761**	1.099**	1.110**	0.816**	0.307	-0.030	0.630
	(0.351)	(0.329)	(0.367)	(0.460)	(0.541)	(0.379)	(0.433)	(0.380)	(0.481)
fsQuart4_InClass	0.992**	0.721**	1.074***	1.250***	0.980*	1.268***	0.719	0.586	0.703
	(0.377)	(0.325)	(0.399)	(0.456)	(0.543)	(0.383)	(0.515)	(0.438)	(0.553)
Observations	2,322	2,323	2,322	1,108	1,109	1,108	1,214	1,214	1,214
fsQ2_InClass>InClass	-	-	-	-	-	-	-	-	-
fsQ3_InClass>InClass	0.543	-	0.454	0.087*	0.040**	0.309	-	-	-
fsQ4_InClass>InClass	0.066*	0.078*	0.086*	0.017**	0.086*	0.031**	-	-	-

In Class is a measure (from 0 to 1) of the proportion of times the teacher was found in the classroom teaching during random spot checks. All regressions include school fixed effects and a vector of controls. Standard errors are clustered at the school level. The final three rows show the p-value on an F-test on whether the two specified coefficients sum to 0 (if interaction is significant).

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

Table 2.8: Balance Across Contract and Civil Service Teachers

	FemShare (1)	Class Size (2)	Age (3)	Percentile (4)	Yrs Taught (5)	Female (6)	In School (7)	In Class (8)
Contract Teacher	0.003	0.400	0.076	-0.779	-13.948***	-0.166**	0.051	0.250***
	(0.014)	(0.915)	(0.093)	(0.716)	(0.928)	(0.081)	(0.042)	(0.057)
Observations	120	120	120	102	120	120	120	120
Mean(Y)	0.49	27.90	9.15	51.28	8.44	0.53	0.84	0.58
StDev(Y)	0.08	4.99	0.51	3.62	8.64	0.45	0.23	0.34

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

Table 2.9: Heterogeneity by Teacher Gender

	All Students			Girls			Boys		
	Total Score	Lit	Math	Total Score	Lit	Math	Total Score	Lit	Math
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
fsQuartile2	0.363**	0.374**	0.262	0.307	0.373	0.155	0.431**	0.374**	0.396
	(0.158)	(0.154)	(0.170)	(0.221)	(0.271)	(0.169)	(0.194)	(0.159)	(0.238)
fsQuartile3	0.289*	0.232	0.290	0.299*	0.274	0.264*	0.291	0.206	0.321
	(0.159)	(0.144)	(0.173)	(0.177)	(0.220)	(0.148)	(0.220)	(0.176)	(0.257)
fsQuartile4	0.294	0.335*	0.175	0.049	0.212	-0.155	0.579**	0.476**	0.563*
	(0.211)	(0.184)	(0.219)	(0.265)	(0.285)	(0.237)	(0.252)	(0.213)	(0.282)
Female Teacher	0.297	0.232	0.303*	0.380	0.394	0.272*	0.233	0.131	0.296
	(0.179)	(0.171)	(0.177)	(0.249)	(0.310)	(0.149)	(0.214)	(0.154)	(0.261)
fsQuart2_FemaleTeach	-0.312	-0.346	-0.197	-0.282	-0.386	-0.091	-0.322	-0.317	-0.251
	(0.243)	(0.263)	(0.220)	(0.366)	(0.460)	(0.205)	(0.311)	(0.286)	(0.344)
fsQuart3_FemaleTeach	-0.058	0.020	-0.139	-0.110	-0.115	-0.089	-0.019	0.079	-0.130
	(0.210)	(0.206)	(0.210)	(0.296)	(0.363)	(0.203)	(0.247)	(0.180)	(0.305)
fsQuart4_FemaleTeach	-0.269	-0.365*	-0.091	0.006	-0.245	0.302	-0.539*	-0.513**	-0.441
	(0.232)	(0.199)	(0.249)	(0.294)	(0.316)	(0.284)	(0.273)	(0.235)	(0.294)
Observations	2,322	2,323	2,322	1,108	1,109	1,108	1,214	1,214	1,214
fsQ2_Female>Female	-	-	-	-	-	-	-	-	-
fsQ3_Female>Female	-	-	-	-	-	-	-	-	-
fsQ4_Female>Female	-	0.304		-	-	-	0.120	0.053*	-

Female is a dummy variable for whether or not the student is taught by a female teacher.
All regressions include school fixed effects and a vector of controls. Standard errors are clustered at the school level.
The final three rows show the p-value on an F-test on whether the two specified coefficients sum to 0 (if interaction is significant).
*** p<0.01, ** p<0.05, * p<0.1

Table 2.10: Heterogeneity by Teacher's Years of Experience

	All Students			Girls			Boys		
	Total Score (1)	Lit (2)	Math (3)	Total Score (4)	Lit (5)	Math (6)	Total Score (7)	Lit (8)	Math (9)
fsQuartile2	0.359**	0.300**	0.345**	0.489**	0.544**	0.311*	0.304*	0.139	0.426**
	(0.141)	(0.132)	(0.153)	(0.201)	(0.230)	(0.163)	(0.178)	(0.160)	(0.203)
fsQuartile3	0.391**	0.344**	0.351**	0.519***	0.513**	0.394**	0.321	0.222	0.361
	(0.153)	(0.143)	(0.165)	(0.173)	(0.211)	(0.158)	(0.224)	(0.191)	(0.243)
fsQuartile4	0.313	0.298	0.257	0.189	0.258	0.066	0.527**	0.425*	0.522*
	(0.197)	(0.182)	(0.201)	(0.225)	(0.236)	(0.232)	(0.250)	(0.226)	(0.263)
Yrs Experience	0.011	0.010	0.010	0.017	0.022	0.008	0.007	0.002	0.012
	(0.009)	(0.008)	(0.009)	(0.012)	(0.014)	(0.010)	(0.010)	(0.009)	(0.011)
fsQuart2_TeachExp	-0.017*	-0.014	-0.017	-0.032**	-0.038**	-0.018*	-0.006	0.004	-0.016
	(0.010)	(0.009)	(0.011)	(0.013)	(0.016)	(0.010)	(0.013)	(0.010)	(0.015)
fsQuart3_TeachExp	-0.011	-0.009	-0.009	-0.022**	-0.026**	-0.011	-0.003	0.002	-0.008
	(0.009)	(0.009)	(0.010)	(0.011)	(0.013)	(0.010)	(0.013)	(0.012)	(0.014)
fsQuart4_TeachExp	-0.021*	-0.020**	-0.018	-0.017	-0.020	-0.010	-0.030*	-0.029**	-0.023
	(0.012)	(0.009)	(0.014)	(0.015)	(0.013)	(0.020)	(0.016)	(0.014)	(0.017)
Observations	2,322	2,323	2,322	1,108	1,109	1,108	1,214	1,214	1,214
fsQ2_TeachExp>TeachExp	0.464	-	-	0.208	0.239	0.363	-	-	-
fsQ3_TeachExp>TeachExp	-	-	-	0.607	0.688	-	-	-	-
fsQ4_TeachExp>TeachExp	0.357	0.306	-	-	-	-	0.184	0.090*	-

TeachExp is a discrete measure of the number of years of teaching experienced possessed by that student's teacher.
All regressions include school fixed effects and a vector of controls. Standard errors are clustered at the school level.
The final three rows show the p-value on an F-test on whether the two specified coefficients sum to 0 (if interaction is significant).
*** p<0.01, ** p<0.05, * p<0.1

Chapter 3

The Effects of Changing the Registration Policy at a Large Public University

3.1 Introduction

Each academic term, universities around the country go through the process of getting students registered for courses for the upcoming term. This process typically involves the university's Office of the Registrar splitting the student body into smaller registration groups and assigning each of these groups to a specific registration time period. While the specifics of the size of and criteria for the groups can differ across universities, the predominant policy constructs registration groups based on unit accumulation and places students with the highest unit total in the first registration group. Though this policy is often preferred because of its perceived ability to allow more senior students to complete their final requirements and graduate, little comparison has been made between this policy and other alternatives.

In the fall quarter of the 2015-2016 academic school year, a large, public university in California changed its process of assigning registration periods to students from one based on last name to one that relied on students' academic progress. Prior to the policy change, students were organized into twelve groups based on the first three letters of their last name and each group was assigned to one of twelve registration periods. As a result, registration times were not determined by unit accumulation or academic progress and students near to the completion of their degree could be found across the range of registration times. Since the beginning of registration for the fall quarter of 2015, the Office of the Registrar implemented a new policy wherein students' progress towards graduation (on a scale of 0 to 100) determines the order in which they registered for classes. Specifically, students are broken into roughly 60 groups based on academic progress cutoffs and these groups are assigned to registration periods in descending order of academic progress. This paper is an analysis of the effects of the registration policy change.

Utilizing the universe of student, course, and registration data over a three year period at the university, I rely on a difference-in-difference approach to assess the effect of the policy change on cumulative waitlists, cumulative shutouts (instances when a student cannot get into a class from the waitlist), cumulative units attempted, and ultimately cumulative units earned. Because the policy change is likely to have different effects on students at different points in their career, I break the analysis down by year in school and academic progress level throughout the paper. The four academic progress levels used in this paper are based on official definitions from the university and roughly correspond to quartiles of the range of academic progress. Using this difference-in-difference approach on each subset of students, I estimate the impact of changing to the academic progress registration policy on the total number of waitlists, shutouts, units attempted, and units earned that students accumulate over the course of an academic year.

My estimates suggest that changing from the alphabetical rotation policy to one based on academic progress significantly increased total waitlists and shutouts for freshman and sophomores. Specifically, these students experienced as many as four more waitlists and three more shutouts over the course of the academic year. As a result, these students saw significant reductions in the number of units attempted throughout the year. Ultimately, sophomores, particularly those who had not reached academic progress level 2 by the beginning of their sophomore year, earned on average one unit less than in the previous registration policy. This estimate translates to roughly one in four sophomores completing an entire class less.

Conversely, the analysis shows that waitlists and shutouts fell for juniors and seniors. These reductions, however, were less in magnitude than the increases experienced by underclassmen, leading to an aggregate increase in total waitlists and shutouts. Despite the reduction in waitlists and shutouts for these students, they experienced no increase in units attempted relative to the previous policy. Interestingly, students who had not progressed beyond level 3 by the start of their junior or senior year actually earned fewer units. This reduction in units earned coincides with an increase in the proportion of courses that these students fail throughout the academic year.

The work in this paper is most closely related to that of Kurleander, et al. (2014) and Neering (2018) which both examine the impact of course access, as predicted by registration timing, on a variety of student outcomes. The university in Kurleander, et al. (2014) conducts registration in a fashion similar to that of the academic progress policy conducted in the university in this paper. While the focus of their work is more related to graduation outcomes, they show that registering later is predictive of experiencing more shutouts in a given term. They also show the average number of shutouts per quarter to be falling as students progress through their time at the university. Ultimately, they are unable to establish a significant relationship between exogenous changes in shutouts and the time it takes a student to graduate.

The analysis in Neering (2018) is conducted on data coming from the alphabetical rotation policy at the university in this paper. The findings of that paper suggest that students who register later in the registration process experience more shutouts, and in turn attempt and earn fewer units. Shutouts are also linked to changes in the composition of classes that a student takes. Specifically, students who experience more shutouts are more likely to end up in early classes, have more class meetings per week, and end up in a section taught by an instructor with a historically low pass rate. The effects on units attempted and earned are shown to be strongest on students who are in their first two years at the university, a phenomenon in part explained by upperclassmen's greater likelihood of getting into a class from the waitlist. This finding is of particular relevance to the analysis conducted in this paper. The aggregate increase in waitlists and shutouts that occurs after the policy change is likely an artifact of the findings in Neering (2018).

A collection of other papers about the effects of registration timing and student performance are also related to this paper in a few ways.[1] While the analysis in these papers centers primarily on students in "open registration" policies in community colleges, they consistently document adverse impacts of registering later on students academic outcomes. Results across this literature implies that students who register later withdraw from more course hours, attempt fewer units, earn lower GPAs, and are more likely to fail courses than students who choose to register earlier in the registration process. While the estimates across these papers are likely subject to some issues of selection bias, overall they suggest that forcing a student to register later may have a variety of adverse impacts on those students.

[1]See Summers (2000), Smith et al. (2002), Hale & Bray (2011), and Gurantz (2014).

The rest of the paper is organized as follows: Section II details the two registration policies at the university, Section III describes the data used in the analysis, Section IV outlines the empirical strategy for analyzing the effect of the policy change, Section V discusses the results of the analysis, and Section VI concludes.

3.2 Setting

The analysis in this paper is based on a policy change at a public university in California with a typical annual enrollment of almost 20,000 students. Prior to the fall 2015 term, students were assigned specific registration times based on an alphabetical rotation policy. Under this policy, the entire student body was divided into 12 groups based on the first three letters of their last name. Specifically, the student body was sectioned off into 12 alphabetically congruent groups with fixed cut points such as "Bol", "Coh", and "Elz". In this setting, a student with the last name Bolman would be in the "Aaa-Bol" name group, while a student with the last name Bomkin would be placed in the "Bom-Coh" group. Students were assigned to these last name groups based solely on the basis of their last name. In other words, GPA, major, term of admittance, and unit accumulation had no bearing on group assignment. As a result, these 12 registration groups each consisted of a mixture of students from various majors and cohorts as well as students of a wide variety of performance and progress levels.

Each term, the university would assign each name group to one of twelve registration periods (or bins) that related to a specific time in which students in that name group could begin registering for classes in the upcoming term. Each term, bin 1 would be allowed to begin registering at a predetermined date and time, followed by Bin 2, then Bin 3, and so on. Once students in Bin 2 were allowed to begin registering, students from Bin 1 would not be barred from continuing to register, however each student was only allowed to register for a maximum of 18 units over the course of the entire registration process.

Each name group was assigned to a specific registration bin each term based on the following predetermined sequence:

$$1 - 12 - 5 - 7 - 3 - 11 - 4 - 9 - 2 - 10 - 6 - 8$$

Specifically, if name group "Bom-Coh" was assigned to Bin 5 of 12 in the winter of 2014, they would be assigned to Bin 7 in the spring of 2014, and so on. Incoming students would be placed in their respective last name group alongside continuing students with alphabetically similar last names. Because students would start the predetermined rotation at different points depending on the combination of their last name and starting date, students would end up on one of 12 registration paths throughout their career at the university. Figure 1 depicts each of the potential paths students could experience while at the university.

There were a few additional caveats to this alphabetical rotation registration policy. First, students in their first term at the university were given “priority” (for one term) over continuing students and allowed to register before registration Bin 1. They therefore did not experience their first assigned registration bin. Additionally, each incoming student was endowed with three “priority passes”. Each pass could only be used once and in a distinct term. When used, this pass would allow the student to begin enrolling before students in bin 1. The following term, that student would be again placed in the appropriate registration bin with students in their name group (unless they once again used a priority pass). Once a student used all three priority passes (i.e. used a priority in three distinct terms), they were not allotted any more priority passes and would from then on be forced to register in their assigned registration bin.

Beginning in the fall of 2015, the university changed the registration assignment policy to one based on academic progress. Each term, prior to registration, the Office of the Registrar would calculate each student’s academic progress score. This score is a number from 0 to 100 and is essentially calculated as the total number of degree applicable units earned divided by the total number of units required for completion of their degree.[2] Once each student’s academic progress score is calculated, students are assigned to registration times based on their score. Specifically, students with academic progress scores above 95.7 are assigned to the first registration period, followed by students with academic progress scores between 95.7 and 93.6, and so on.

[2]As different degrees (such as Mechanical Engineering or Anthropology) may have different unit requirements, the denominator of this value may vary slightly across students.

Finally, the university identifies four different levels of academic progress, based on the academic progress score. Levels 4, 3, 2, and 1 are identified as being above academic progress scores of 75.1, 45.1, 20.1, or 0, respectively.

The relationship between academic progress and assigned registration period for each policy is depicted in Figures 2 and 3. In Figure 1 students in registration bin 1 clearly exhibit a wide variety of academic progress scores. Figure 3 indicates that registration periods strictly cutoff at specific academic progress levels.

3.3 Data

The data used in this analysis cover the universe of students, courses, and registration information from the fall 2013 term through the summer 2016 term. Student level information includes gender, race, parental income (in the year preceding application), SAT and ACT scores, high school GPA, AP test scores, California and United States residency statuses, time spent on extracurricular activities in high school, whether or not they held a leadership role in high school, application fee waiver status, and high school attended. Values for each of these variables come from information entered by the student on their application to the university and are all verified by the university before the student enrolls in their first term. Student level data related to their career at the university come from the Office of the Registrar and include admit term, initial major, the date (and discipline) of any major changes, the date (and discipline) of any minors, the completion status of the student, and the date of completion (when relevant).

Course level information also comes from the Office of the Registrar and covers course specific information such as the subject, course number, section code, times of class meetings, and a unique instructor identifier. This information also includes enrollment caps, total final enrollments, waitlists totals, and whether the class is a lecture, independent study, or ungraded extension of a specific lecture (i.e. a lab or discussion section). These data are merged with student-specific registration attempts for each course. Information on registration attempts detail the exact date and time of the registration attempt, the outcome (enrolled or waitlisted), the reason for waitlisting (course full or by choice), whether the student dropped the class and, if so, why. Finally, all the above data are merged with student level outcomes for each class, including final grade and pass/fail status. Together all of these variables clearly depict the registration process and outcomes for each student for each class, regardless of if they ultimately dropped the class or dropped from the waitlist without ever enrolling.

This data set also covers students' registration assignments for each term as well as their usage of priority passes. As students with any special exceptions (e.g., athletes, military, and students with disabilities) are allowed to register before the school-wide registration process begins, a student may have more than one registration assignment time in a term. Since these students are repeatedly allowed to register before all other students at the university, I omit students who at any point received a special exception assignment. This population represents roughly 9% of the student body.

In all cases I restrict attention to students who begin in the fall quarter as a first time freshman. Doing so allows me to remove transfer students who not only arrive at the university with a wide range of units, ages, and college experience, but who also do not experience the registration assignment policy for their freshman and sophomore years. In all estimation relating to effects over a student's entire course career, I restrict my sample to students for whom the data period covers their first term at the university. It should be noted that while the information on academic progress level only goes back as far as fall 2013, all other variables extend back to the fall term of 2006. As a result, my analysis will include all relevant students enrolled at the university in Fall 2013, not just first time students.

3.4 Empirical Strategy

To evaluate the impact of the policy change on students, I focus my attention on both students' access to courses (i.e. waitlists and shutouts) and students' unit accumulation (i.e. units attempted and units earned). Specifically, I compare the aggregate levels for students at the end of the academic year in which students enrolled based on AP level to the aggregate levels for students at the end of the two academic years in which students enrolled based on the alphabetical rotation. However, comparing aggregate levels of these metrics may miss important information such as any differences in the initial levels at the beginning of the academic year, as well as any baseline differences in students across cohorts. Therefore, to estimate the impact of the policy change on student level outcomes, I rely on a differences-in-differences (DiD) approach.

The DiD strategy relies on comparing the change in some outcome for a "treated" group to the change in the same outcome for an untreated group, typically over the same time period. These changes are measured as the value of the outcome at some point after an intervention or policy change less the value prior to the intervention or policy change. The change in the outcome for the untreated group is meant to serve as a counterfactual to the treated group, an estimate to how much the outcome would have changed for the treated group had the policy change or intervention never taken place. In this way, the DiD strategy requires two groups (treated and untreated) in two time periods (before and after the policy change).

Because the university changed the registration policy for all students at the beginning of the 2015-2016 academic year, there is no year in which I see some students being treated (i.e. enrolling based on academic progress) while others remain untreated (i.e. enrolling based on the alphabetical rotation). I therefore define the two groups in two time periods as follows. The treated group in the pre-policy change period is defined as students at the beginning of the 2015-2016 academic year. Specifically, the initial level for the treated group will be measured as cumulative number of units earned (or waitlists) for each student over the course of their academic career as of the start of the fall 2015 quarter. The treated group in the post-policy-change period is defined as the cumulative number of units earned (or waitlists, etc.) as of the end of the spring 2016 term. Similarly, the pre-period for the untreated group is defined as the beginning of either the 2013-2014 academic year or the 2014-2015 academic year as these years both operated under the alphabetical rotation registration policy. The post-period for the untreated group is measured at the end of the spring 2014 and spring 2015 terms, respectively.

To estimate the impact of the policy change, I estimate the following model:

$$Y_{it} = \alpha_1 + \alpha_2 Treat_{it} + \alpha_3 Post_{it} + \alpha_4 Treat * Post_{it} + X_i\phi + \theta_{it} + \pi_i + u_{it} \tag{3.1}$$

Where Y_{it} is the cumulative number, across the entire academic career, of any of the following for student i in term t: waitlists, shutouts, units attempted, or units earned. The variables $Treated_{it}$ and $Post_{it}$ are binary variables that are defined as in the previous paragraph. The coefficient on the key variable of interest, $Treated * Post_{it}$, gives the estimated effect of the policy change on the outcome of interest. To adjust for any basic differences across students in each year, I include a vector of student specific, time invariant traits, X_i. This vector includes gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school. Also included is a time varying measure of total priority passes used as well as a vector of dummies, θ_{it}, for whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business.

Because a student's aggregate level of waitlists, shutouts, or units at the beginning of the Fall 2013, Fall 2014, and Fall 2015 terms likely depends on the path of alphabetical rotation assignments they experienced prior to that term, I include a vector, π_i, of fixed effects for the 12 potential registration paths a student could experience at the university. Because continuing students who enrolled at the university at the beginning of Fall 2015 had experienced the alphabetical rotation policy prior to that term, I have information about the path they would have continued to experience in the 2015-2016 school year, had the registration policy not changed. Further, I estimate Equation 1 for a number of subsets of students based, in part, on the number of years since their admittance to the school at the start of the pre period. Specifically, in an estimation limited to Juniors, I limit the sample to students who are at the beginning of their third year at the university in Fall 2013, Fall 2014, or Fall 2015. This subsampling, mixed with the path fixed effects, allows me to generate estimates that are based on a difference-in-difference estimate that compares students on a particular part of a specific registration path in the alphabetical rotation era (i.e. the 2013-2014 or

2014-2015 school years) with students in the AP era (i.e. the 2015-2016 school year) who would have experienced the same part of that specific path if the university never changed the policy. In this way, I employ an estimation strategy that creates the most accurate counterfactual possible given the setting and data available.

3.5 Results

This change put younger, "less progressed" students at a relative disadvantage to older students further along in their degree. The previous policy did not routinely advantage students by progress or class level. While the "priority passes" allotted to students in the alpha rotation policy were more often used by upperclassmen and thus put older students, on average, slightly ahead of Freshmen these advantages were neither guaranteed or permanent. Being forced to register relatively later in the registration period led to a redistribution of waitlists across class and academic progression levels. Figure 4 depicts the average number of waitlists students accrue in a given academic year, broken down by year at the university. Under the alphabetical rotation policy older students average slightly fewer waitlists, but on the whole the differences are minimal. Under the academic progress policy however, the distribution of waitlists shifts noticeably towards underclassmen, creating a much larger disparity between under and upper classmen.

Table 1 shows the estimated effect of the policy change on students based on their year at the university and academic progress level at the beginning of the academic year. Each number comes from a separate regression of Equation 1 on the denoted subgroup. These estimates show that, relative to the alpha rotation regime, students who begin their first year at the university (i.e. Freshman) at Academic Progress Level 1 experienced almost 2 more total waitlists over the course of the academic year (Fall, Winter, and Spring) than students in the alphabetical rotation regime. The effect was even stronger for sophomores (i.e. students in their second year at the university) who begin the year at Level 2 or Level 3. These students accumulated roughly 3 and 4.7 more waitlists, respectively, during their second year than similar students in the alphabetical rotation regime. Conversely, Level 3 Juniors and Level 4 Seniors saw reductions in their overall waitlist totals for the academic year. Level 3 juniors accumulated roughly one fewer waitlist. Level 4 seniors saw the largest reduction: about 1.5 fewer waitlists.

While the numbers of waitlists may be indicative of students' access to classes, it may be informative to examine instances in which students were unable to get into a class from the waitlist. These instances are denoted as "shutouts" and the year-long totals, broken down by class level, are depicted in Figure 5. With the average year-end totals are lower in magnitude than that of waitlists, the pattern across class level and registration policy closely mirrors that of total waitlists from Figure 4. Again, freshmen experience the highest totals under the alphabetical rotation policy with the average total slowly tapering off across the class levels. Under the academic progress policy, the distribution again skews heavily towards underclassmen. In sum, the reductions in shutouts for juniors and seniors appear to be offset by the larger increase for freshmen and, particularly, sophomores.

Table 2 shows the estimated effect of the policy change on students' total shutouts based on their year at the university and academic progress level at the beginning of the academic year. As in Table 1, each estimate comes from a separate regression of Equation 1 on the denoted subgroup. Similar to the effect on waitlists, freshmen and sophomores experience significant increases in cumulative shutouts over the course of the academic year, ranging from an increase of about 1.4 for Level 1 freshmen to over 2 and 3 for Level 2 and 1 sophomores, respectively. The reduction in waitlists experienced by Level 3 juniors translates into a similar reduction in shutouts, roughly 0.6. However, the drop in waitlists does not translate into any significant reduction in shutouts for Level 4 seniors. Findings from Neering (2018) may help to partially explain this null effect. In that paper, seniors are documented as being significantly more likely to make it into a class from the waitlist than students in lower class levels. Finally, the null effect on shutouts for Level 2 juniors and Level 3 seniors is in line with the null effect on waitlists for these groups that was documented in Table 1. These findings also fall closely in line with that of Neering (2018), which suggests that registration timing has a bigger impact on shutouts for underclassmen than for upperclassmen. Despite the new policy only rearranging the registration order, overall waitlists and shutouts are higher per student across the university under the academic progress policy.

If students at different class and academic progress levels have seen changes in their access to classes, this may have a bearing on the number of units they ultimately take and earn. Figure 6 indicates the total units attempted, on average, over the course of the academic year for each class level. Unsurprisingly, the figure suggests that sophomores the group that experienced the largest increase in waitlists and shutouts see the largest reduction in total units attempted. However, the reduction in waitlists and shutouts for upperclassmen indicated in the previous figures does not seem to translate to any increase in units attempted. Total units attempted over the year actually appear to fall slightly for both juniors and seniors. Conversely, units attempted for freshmen seem relatively stagnant despite the significant increases in waitlists and shutouts documented in the previous figures.

The values in Figure 6 represent simple averages for each group and do not take into account any differences in traits across students in each regime, nor do they adjust for any initial difference in cumulative units attempted at the start of the year. Table 3, as with the previous tables, provides estimates of the effect of the policy change which take these differences into account and rely on a potential outcomes framework. The estimates in this table indicate that the policy change reduced the total number of units attempted for underclassmen without improving the total units attempted for upperclassmen. This effect is strongest for sophomores who begin the year at Level 1. These students attempt more than a full unit less over the course of the year when registration is based on academic progress. This effect roughly corresponds to 1 in 3 students attempting one full class less under the academic progress policy. The effect is smaller for sophomores who being the year at Level 2 and thus register before their Level 1 peers. The effect is even smaller, but significant, for freshmen despite being forced to register at the end of the registration period. However, because students in their first term at the university are automatically enrolled into courses in that term, they ultimately do not experience the adverse registration timing for the entire academic year. In this way, the smaller effect size relative to Level 2 sophomores is not unsurprising.

Table 4 shows how the policy change impacted total units earned over the academic year for each subgroup of students. The reduction in units attempted for sophomores documented in the previous table is reflected here in a similar reduction in units earned. Level 2 and Level 1 sophomores earn .7 and 1.2 units fewer over the course of the year when registration is assigned by academic progress. Similarly, Level 4 seniors saw neither a reduction in units attempted nor in units earned. However, junior and Level 3 seniors, who did not exhibit reductions in total units attempted, ultimately earn fewer total units over the academic year. Estimates in this table indicate that Level 3 juniors earn .7 units fewer on average under the academic progress policy while off diagonal students (i.e. Level 2 juniors and Level 3 seniors) experience reductions in units earned in excess of 1 unit.

Findings from Neering (2018) suggest that students who are forced to register later are more likely to end up in a class with an instructor who historically passes fewer students. Because Level 2 juniors and Level 3 seniors are forced to register later than other students in their cohort, it may be the case that these students earn fewer units (despite attempting the same number of units) because they end up in harder classes and are more likely to fail a course. Similarly, if Level 3 juniors are competing for spots in the same upper division electives as Level 3 and 4 seniors, they may be more likely to be placed in and fail difficult courses. Table 5 provides evidence in support of this claim. Coefficients in this table indicate the effect of the policy change on the proportion of classes a student fails over the course of the academic year. These estimates show that Level 2 juniors and Level 3 seniors fail around 1.5% more of their courses throughout the year when registration is based on academic progress. Similarly, Level 3 juniors fail about a half of a percent more of their courses. While there may be other mechanisms that contribute to the reduction in units earned for these groups of upperclassmen, the estimates in this table suggest that course difficulty may also play a role.

3.5.0.1 Graduation Outcomes

Ultimately, a key motivation for changing the registration policy was to give registration priority to students who have progressed the furthest towards graduation in hopes that this would give them full access to the courses they need and allow them to finish as quickly as possible. In this regard, the most relevant outcomes to examine may not be unit accumulation, but the likelihood of graduation for fourth and fifth year students.

Table 6 indicates how the policy change impacted the likelihood of students graduating by the end of the spring term, broken down by class level and academic progress level at the beginning of the academic year. Because not all fourth and fifth year students that enroll in the fall term persist to the end of the academic year (due to graduating mid-year), the estimates in this table do not follow the specification outlined in equation 1. All estimates come from regressions which simply compare the year end outcome for students in the alphabetical rotation regime to those in the academic progress regime. In this way, each regression includes only one observation per student. However, since the level for a binary graduation variable will be 0 for all students in the fall term, the estimation can be seen as a operationally equivalent to the difference in difference estimation laid out in equation 1. Overall, the estimates in Table 6 suggest that fourth and fifth year students are less likely to graduate by the end of the year when registration is assigned by academic progress. Level 3 seniors are roughly 3 percentage points less likely to graduate by the end of their fourth year. Given that the baseline average graduation rate for these students is about .33, this represents nearly a 10% decrease in the likelihood of graduating by the end of the year. The estimated reductions for level 4 students correspond to roughly a 6% decrease in the likelihood of graduation (that year) for level 4 students.

While previous tables suggested that Level 3 seniors earn fewer units under the academic progress regime, this result may not be terribly surprising. However, the finding that Level 4 seniors are less likely to graduate by the end of the year seems a bit in contradiction with the prior estimate that found that these students earn roughly the same total units under the new policy.

If these students are less likely to graduate by the end of the year, it is possible that they are more likely to leave the university without completing their degree. Table 7 shows the effect of the policy change on the likelihood of fourth and fifth year students dropping out. Overall the estimates suggest that students are less likely to drop out. Specifically, these students are about 2 percentage points less likely to drop out of college in fall, winter, or spring of their fourth or fifth year.

Taken together, Table 6 and 7 would suggest that if students are both less likely to graduate and less likely to drop out, they may be more likely to return (conditional on not graduating). Table 8 examines this outcome by estimating the effect of the policy change on the likelihood that the fourth or fifth year student persists beyond the academic year.[3] The coefficients here indicate that third year seniors are about 4.6 percentage points more likely to return after that academic year when they register under the academic progress policy. Similarly, fifth year seniors are 2 percentage points

[3] Note, because the data available for the academic progress policy only cover fall, winter, spring, and summer for one year, persistence is measured as whether or not the student shows up in the summer population (regardless of whether they take units in the summer). As such, I am unable to see if the student returns for the following academic year and, ultimately, if these students persist to graduation.

more likely to return the following year. Based on baseline pre-policy change persistence averages of 62% and 4.3%, these estimates represent roughly 7% and 46% increase in the likelihood of returning, respectively, when a fourth or fifth year student does not graduate by the end of the spring quarter.

3.6 Conclusion

Prior to the 2015 policy change, the university in this paper organized students in groups based on the first three letters of their last name and assigned each group to one of twelve registration periods based on a predetermined rotation. In the fall of 2015, the registrar began ordering students on the basis of progress towards graduation, giving students with the most progress the earliest registration times. This change in policy gave priority to more senior students, in turn reducing the number of times they waitlist for or get shutout of classes over the course of the year. However, this change in registration ordering shifted the burden of waitlists and shutouts on to first and second year students in such a way that the relative increase in waitlists and shutouts for underclassmen outweighed the decrease for upper classmen. This finding echoes one from Neering (2018) which suggests that underclassmen experience the largest increases in waitlists and shutouts in the face of later registration times, either due to a lack of institutional knowledge or because of policies which give preference to upperclassmen on the waitlist when courses are full.

This increase in waitlists and shutouts for underclassmen also coincided with a reduction in both units attempted and units earned for sophomores. These reductions were particularly acute for second year students who had fallen behind their expected level of progress by the beginning of their second year at the university. The policy change led to as many as one in three second year students to earn one fewer class worth of units across the course of the school year, conditional on them being behind at the beginning of the school year.

The reduction in waitlists and shutouts for upperclassmen did not lead to an increase in the number of units attempted across the school year. In fact, the total number of units earned went down for third and fourth year students who had not progressed beyond the third of four progress levels before the beginning of the school year. This finding is best explained by a relative increase in the proportion of courses failed for these students when they register according to academic progress. This finding also echoes one from Neering (2018) which finds that students who register later than their peers are more likely to end up in sections of a course that are taught by a historically difficult teacher and, in turn, more likely to fail a course. In the end, the policy change did not lead to an increased likelihood of graduating by the end of the year for fourth and fifth year students. It did however increase retention of these students, conditional on them not graduating by the end of spring quarter.

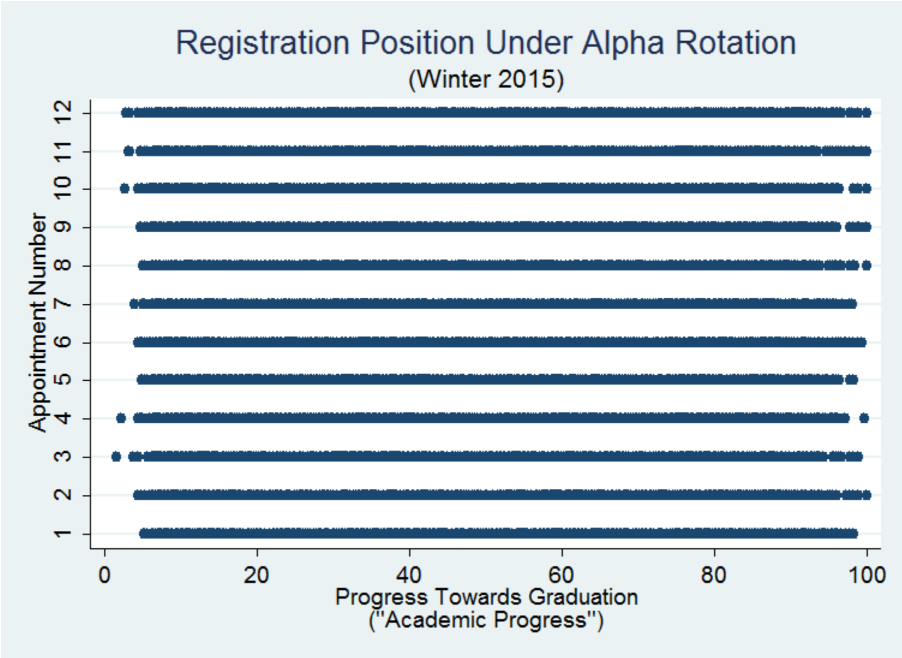
Moving forward, it may be worth examining the progress of the first wave of students who experienced the academic progress registration policy as underclassmen. If the progress of these students has been hindered, on time graduation rates may have the potential of falling. The results from this study suggest that the university should pay particular attention to students who fall behind in the new registration policy, as the work here, and in Neering (2018), suggest that these students may continue to fall further behind the peers in their cohort with each successive term. Finding a way to keep them on pace to graduate may go a long way in reducing attrition in the coming years of the new registration policy.

Figure 3.1: Registration Paths in the Alphabetical Rotation Policy

Year 1			Year 2			Year 3			Year 4		
Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring	Fall	Winter	Spring
1	12	5	7	3	11	4	9	2	10	6	8
2	10	6	8	1	12	5	7	3	11	4	9
3	11	4	9	2	10	6	8	1	12	5	7
4	9	2	10	6	8	1	12	5	7	3	11
5	7	3	11	4	9	2	10	6	8	1	12
6	8	1	12	5	7	3	11	4	9	2	10
7	3	11	4	9	2	10	6	8	1	12	5
8	1	12	5	7	3	11	4	9	2	10	6
9	2	10	6	8	1	12	5	7	3	11	4
10	6	8	1	12	5	7	3	11	4	9	2
11	4	9	2	10	6	8	1	12	5	7	3
12	5	7	3	11	4	9	2	10	6	8	1

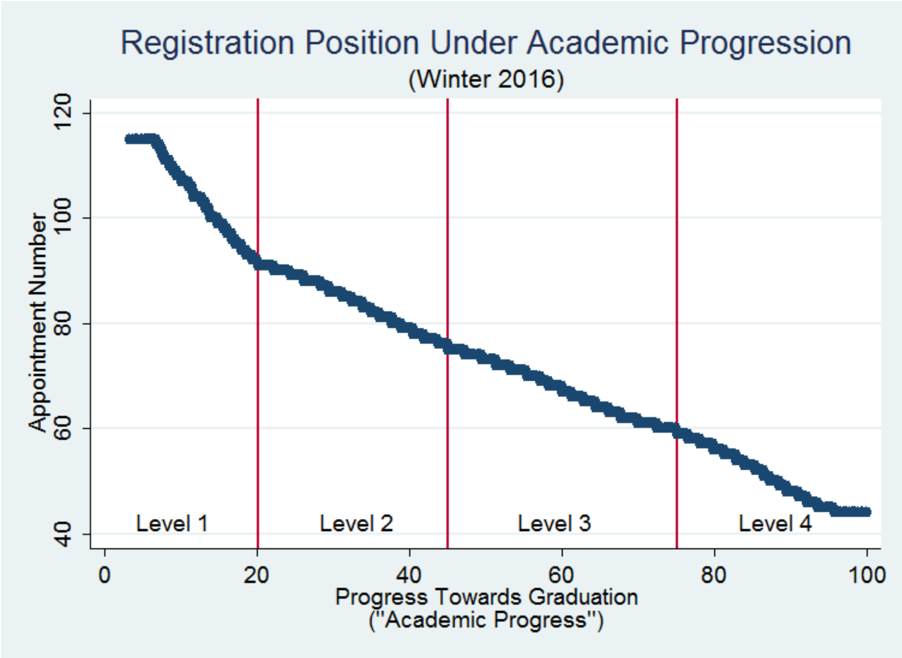
Notes: Each cell denotes the assigned registration bin (out of 12) for each term, broken up into rows based on the first bin assignment. In all cases where a specific registration path is referenced, paths are labeled based on the first bin assignment they realize. More explicitly, the first row relates to Path 1, the second row to Path 2, and so on.

Figure 3.2: Registration Time vs Academic Progress - Alpha Rotation Policy



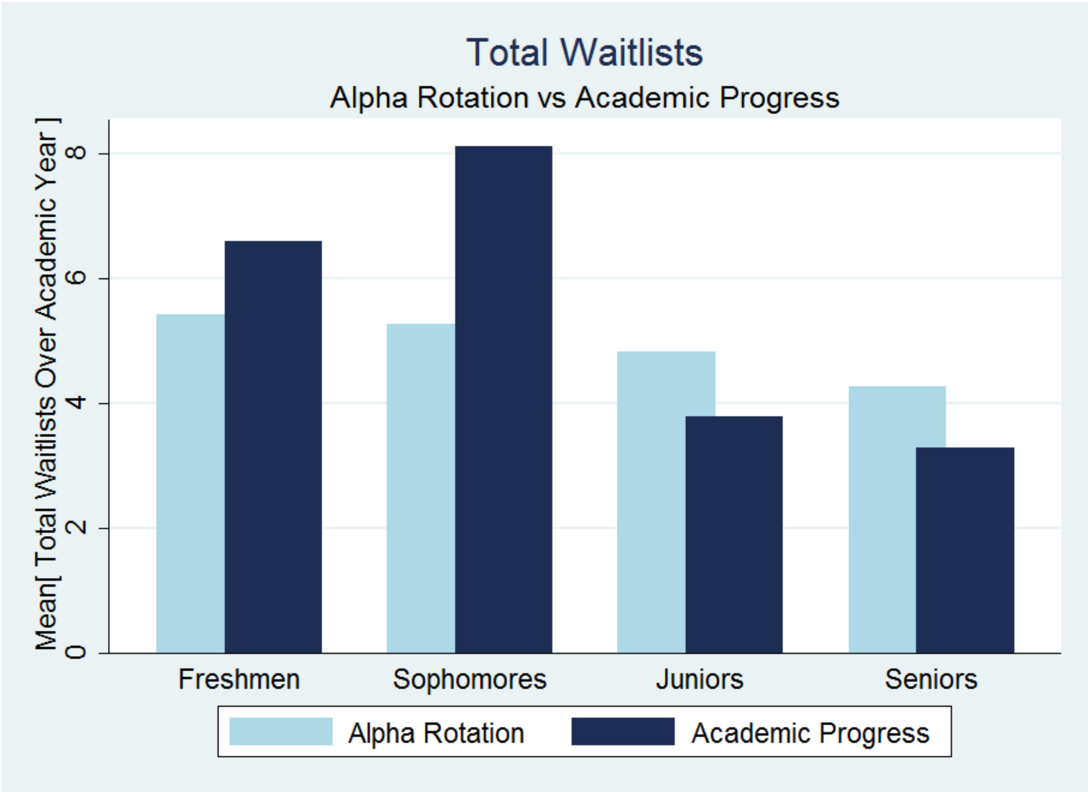
Notes: Figure 2 shows the registration bin assignment and ”Academic Progress” measure for each student at the university for Winter 2015. In this term, students were assigned to registration times based on the alphabetical rotation policy. Each point represents one student and shows that each of the 12 registration bins include students from a wide variety of academic progress levels.

Figure 3.3: Registration Time vs Academic Progress - A.P. Policy



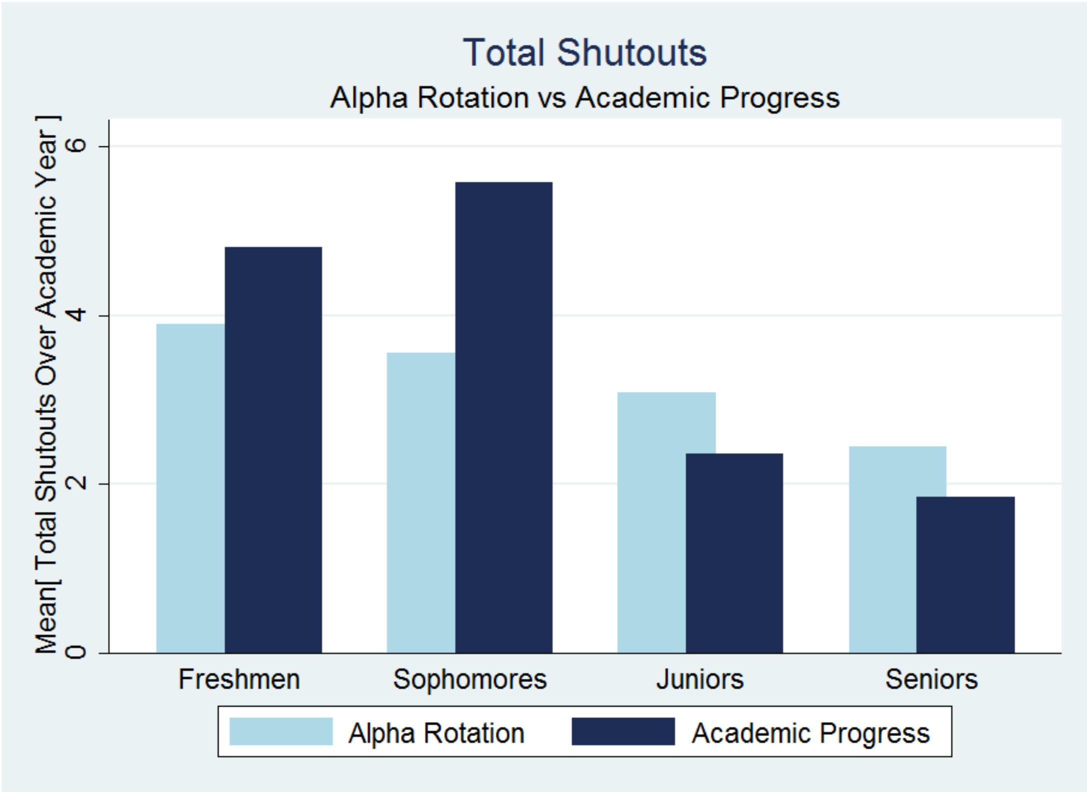
Notes: Figure 3 shows the registration assignment and "Academic Progress" measure for each student at the university for Winter 2016. In this term, students were assigned to registration times based on the academic progress policy. Lower appointment numbers represent earlier registration times and each point represents one student. This figure indicates that registration time is perfectly related to academic progress and that students with lower levels of progress register later.

Figure 3.4: Comparison of Waitlists by Policy



Notes: Figure 4 shows the number of waitlists under the alphabetical rotation and academic progress policies for first, second, third, and forth year students, separately. Each bar represents the mean of the total number of waitlists students experienced over the course of their first, second, third, or forth year in the university, depending on which registration policy they experienced in that year. Thus, the measure for sophomores under the alpha rotation policy (for example) comes from a different population of students than the measure of waitlists for sophomores under the academic progress policy.

Figure 3.5: Comparison of Shutouts by Policy



Notes: Figure 5 shows the number of shutouts under the alphabetical rotation and academic progress policies for first, second, third, and forth year students, separately. Each bar represents the mean of the total number of shutouts students experienced over the course of their first, second, third, or forth year in the university, depending on which registration policy they experienced in that year. Thus, the measure for sophomores under the alpha rotation policy (for example) comes from a different population of students than the measure of shutouts for sophomores under the academic progress policy.

Table 3.1: Change in Total Waitlists Across the Year

	Begin Academic Progress Level				
Class Level	1	2	3	4	All
Freshman	1.867***	.	.	.	1.110***
Sophomore	4.687***	3.053***	.	.	3.116***
Junior	.	-0.969	-0.974**	.	-1.000***
Senior	.	.	-0.543	-1.46*	-0.965*
All	2.071***	2.389***	-0.773**	-1.43*	0.988***

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the the policy change on how many waitlists students in each group accrued over the course of the academic year. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university. * significant at 10% level, ** significant at 5% level, *** significant at 1% level

Table 3.2: Change in Total Shutouts Across the Year

	Begin Academic Progress Level				
Class Level	1	2	3	4	All
Freshman	1.447***	.	.	.	0.846***
Sophomore	3.307***	2.148***	.	.	2.234***
Junior	.	-0.819	-0.648*	.	-0.694**
Senior	.	.	-0.283	-0.97	-0.590
All	1.580***	1.661***	-0.458	-0.94	0.754***

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the the policy change on how many shutouts students in each group accrued over the course of the academic year. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university. * significant at 10% level, ** significant at 5% level, *** significant at 1% level

Table 3.3: Change in Total Units Attempted Across the Year

	Begin Academic Progress Level				
Class Level	1	2	3	4	All
Freshman	-0.347***	.	.	.	-0.237***
Sophomore	-1.253**	-0.778***	.	.	-0.792***
Junior	.	-0.673	-0.312	.	-0.370
Senior	.	.	-0.583	-0.14	-0.489
All	-0.417	-0.694*	-0.265	-0.21	-0.446

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the the policy change on how many units students attempted in each group over the course of the academic year. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university. * significant at 10% level, ** significant at 5% level, *** significant at 1% level

Table 3.4: Change in Total Units Earned Across the Year

	Begin Academic Progress Level				
Class Level	1	2	3	4	All
Freshman	-0.165	.	.	.	-0.0426
Sophomore	-1.276*	-0.717***	.	.	-0.769***
Junior	.	-1.315*	-0.707**	.	-0.817***
Senior	.	.	-1.865***	-0.78	-1.498***
All	-0.257	-0.763*	-0.910	-0.86	-0.674

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the the policy change on how many units students earned over the course of the academic year. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university. * significant at 10% level, ** significant at 5% level, *** significant at 1% level

Table 3.5: Change in Proportion of Classes Failed Across the Year

	Begin Academic Progress Level				
Class Level	1	2	3	4	All
Freshman	-0.000421	.	.	.	-0.000824
Sophomore	-0.00207	-0.00373*	.	.	-0.00357*
Junior	.	0.0167***	0.00453***	.	0.00667***
Senior	.	.	0.0134***	0.01***	0.0110***
All	-0.000692	0.000514	0.00603***	0.01***	0.00109

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the the policy change on the proportion of classes students failed in each group accrued over the course of the academic year. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university.
* significant at 10% level, ** significant at 5% level, *** significant at 1% level

Table 3.6: Change in Likelihood of Graduating by the End of Spring

	Begin Academic Progress Level		
Class Level	3	4	All
Senior	-0.0494***	-0.0642***	-0.0595***
5th Yr Senior	.	-0.0646***	-0.101***

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the policy change on the likelihood of students graduating by the end of the academic year in each group. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university. * significant at 10% level, ** significant at 5% level, *** significant at 1% level

Table 3.7: Change in Likelihood of Dropping Out During That Year

	Begin Academic Progress Level		
Class Level	3	4	All
Senior	-0.0173***	-0.00378	-0.0107***
5th Yr Senior	.	-0.0182*	-0.0190**

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the policy change on the likelihood of students dropping out by the end of the academic year in each group. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university. * significant at 10% level, ** significant at 5% level, *** significant at 1% level

Table 3.8: Change in Likelihood of Returning Next Year (Conditional on Not Graduating)

	Begin Academic Progress Level		
Class Level	3	4	All
Senior	0.0467***	0.00150	0.0299***
5th Yr Senior	.	0.0202**	0.0586***

Notes: Each value represents the estimate of α_4 from equation 1 for the specified subset of students. In this way, each value is a difference-in-difference estimate of the effect of the policy change on the likelihood of students returning to the university the following academic year in each group. Each regression includes individual-level controls for gender, race, high school GPA, SAT score, parental education, family income, number of AP tests passed, and a variety of measures for different types of activities the student engaged in during high school as well as time-varying measures of total priorities used before that term and whether or not the student is enrolled in a major in one of a variety of different departments at the university, such as Engineering or Business. All regressions also include fixed effects for the 12 potential registration paths a student could experience at the university. * significant at 10% level, ** significant at 5% level, *** significant at 1% level

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