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High School Dual Enrollment Programs: Are We Fast-Tracking Students Too Fast?

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Abstract

Dual enrollment (DE), an arrangement by which high school students take college courses, is becoming increasingly popular as a means of improving high school education. However, there is very little rigorous evidence on its impact on student outcomes. A particular concern in evaluating its effects is the selection bias that arises because more able students are more likely to take DE courses. In this study, I employ a quasi-experimental method to gauge the causal effects of DE on student outcomes. I conduct two regression discontinuity analyses that exploit a statutory mandate in the state of Florida requiring high school students to have a minimum academic standing in order to participate in DE. The first analysis evaluates the effects of DE using GPA as the eligibility criterion. The second analysis evaluates the effects of a particularly challenging and popular DE course, college algebra, using an eligibility criterion that is specific to that course. While the standard regression-discontinuity methods are appropriate for the first analysis, the participation criterion for college algebra is used not only for DE students but also for college students. I therefore employ an extension of the regression-discontinuity method that accounts for sequential treatments. Using data on students from two high school cohorts (2000–01 and 2001–02) in selected Florida districts who were tracked through the summer of 2007, I find no evidence that simply taking a DE course improved marginal students' rates of high school graduation, college enrollment, or college degree attainment. However, for students on the margin of participation in algebra, I find that taking such a challenging DE course had large and significant effects on college enrollment and graduation rates.

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1. Introduction

Roughly one third of high school graduates do not enroll in postsecondary institutions, and a third of those who do are required to enroll in remedial education to prepare for college-level work (National Center for Education Statistics [NCES], 2003, 2004). One type of program designed to address these educational shortcomings is dual enrollment (hereafter *DE*), an arrangement by which high school students (typically juniors and seniors) enroll in college courses and earn college credits. Proponents of DE believe that participation may promote college enrollment and completion by giving students a stronger preparation and a realistic idea of what college academics are like. However, it is also the case that DE programs could potentially discourage those students who are academically or emotionally unprepared to handle the demands of college — or they may have no effect on college enrollment and completion if they only serve college-bound students.

While there are no nationwide statistics on the growth of DE programs, the National Center of Education Statistics estimates that about 5 percent of all high school students (nearly a million students) took a college course during the 2002–03 school year (Kleiner & Lewis, 2005) and that about 71 percent of all public high schools offer DE programs (Waits, Setzer, & Lewis, 2005).¹ The current paper analyzes data from Florida, where about 14 percent of high school students take at least one college course via DE.²

Despite the prevalence of DE programs, there is little quantitative evidence on their effectiveness. Two extensive reviews of the literature (Bailey & Karp, 2003; Lerner & Brand, 2006) concluded that there is no sound evidence that DE programs contribute to students' college access and academic success. Assessing the impact of DE is difficult because of the well-known problem of selection bias. The selection problem is twofold: high school students choose to take college courses based on their academic ability, motivation, and expected gains from participation; colleges are also allowed to set their own admission requirements to ensure the integrity of their academic programs. In addition, students who have already decided to go to college would likely consider DE an attractive

¹ Part-time enrollment growth for students under the age of 18 at public two-year colleges — presumably DE students — could be taken as evidence of a rapid expansion. Between 1995 and 2005, enrollment figures at public two-year colleges for part-time students under age 18 more than doubled (NCES, 1995, 2006), while high school enrollment grew only about 19 percent over the same time period (NCES, 2008).

² DE is the second largest high school acceleration program after Advanced Placement. About 20 percent of Florida students take an Advanced Placement course.

way to obtain a head start on accumulating credits, causing a spurious correlation between participation and outcomes.

In an effort to statistically control for students' differences, a handful of studies have employed a regression framework, though the availability and quality of the data used varied considerably (e.g., Crook, 1990; Eimers & Mullen, 2003; T. G. Goodman, Lathan, Copa, & Wright, 2001; Karp, Calcagno, Hughes, Jeong, & Bailey, 2007; Kim, 2006; Nitzke, 2002; Swanson, 2008).³ DE participation has been found to be strongly positively associated with nearly every educational outcome studied. For example, using data from 2000–01 and 2001–02 high school graduating cohorts, Karp et al. (2007) found that, compared with non-DE students, DE students in Florida were 17 percentage points more likely to enroll in college and 8 percentage points more likely to initially enroll in a four-year institution, among other positive outcomes. The study controlled for characteristics that are likely correlated with both DE participation and students' outcomes, such as race, gender, academic background, free or reduced-price lunch status at school, and school demographics. While these findings are encouraging, a more rigorous analysis is needed.

This paper constitutes the first attempt to use a quasi-experimental method, namely the regression discontinuity (RD) design, to gauge the causal effect of DE on students' likelihood of high school graduation, college enrollment, and college completion.⁴ I exploit a statutory mandate in Florida that restricts enrollment, generating a source of plausible exogenous variation in DE participation. Florida's policy mandates that students have a minimum grade point average (GPA) in high school in order to take a DE course and, for enrollment in specific courses such as college algebra, have a minimum score on a college placement test (CPT). I exploit both features of the policy in two separate RD analyses.

³ Apart from the potential failure to properly account for the selection problem, the DE literature suffers from two additional shortcomings — both highlighted by Jackson (2010) in the literature on Advanced Placement. First, all studies estimate regressions that control for variables determined after DE participation, such as overall high school grade point average, placement tests taken in college, college choice, or enrollment patterns. Controlling for post-treatment variables, which may be affected by DE participation, may induce bias in the estimation of the treatment effect in a regression framework. The second limitation derives from the fact that studies, aiming to uncover the effect of DE on college outcomes, restrict the sample of analysis to college-goers. To the extent that DE affects students' likelihood of going to college, comparisons of DE and non-DE groups lack a valid causal interpretation (see Angrist and Pischke [2009] for a description of the problem). Both of these shortcomings are often inevitable due to data limitations; most data for these studies are derived from college transcript files. Even high-school-to-college longitudinal datasets, such as the National Education Longitudinal Study of 1988 (NELS:88), have this limitation because they identify DE courses from college transcripts, which were collected exclusively for students who enroll in college after high school.

⁴ The regression discontinuity approach is increasingly being used to assess causal impact of interventions in observational studies, both in education and elsewhere. Imbens and Lemieux (2008) and Lee and Lemieux (2010) provide two recent surveys of the methods.

I first examine the effect of taking at least one DE course, exploiting the GPA eligibility requirement. With the exception of a few courses (typically in math and English), an eligible GPA suffices for placement into most freshman community college courses. Because GPA by itself cannot strongly predict participation in courses with additional test requirements, this analysis captures the average effect of all other DE courses, for which eligibility is determined based on GPA. Hence, I refer to this analysis as the effect of DE-basic. While this effect is of interest in its own right, DE encompasses a wide range of course experiences, which vary in difficulty and subject area. Owing to the generality of the GPA requirement, this analysis cannot identify the effect of any particular course. A more nuanced understanding of the potential effect of DE as a policy intervention would require assessing the heterogeneity of the effect across various DE courses. To this end, I conduct a second RD analysis that examines a particularly challenging course, college-level algebra, which is the second most popular DE course after English composition.⁵

The second RD analysis exploits the course-specific CPT requirement for enrollment in algebra. An important analytical complication in measuring the effect of DE-algebra derives from the fact that the CPT math requirement is the same for both high school and college students. Thus, an eligible student who does not take the course through a DE program in high school can still take the course in college. Standard RD estimation cannot accommodate the presence of a second treatment discontinuously changing at the same cutoff and therefore cannot disentangle the effect of algebra in high school from the effect of algebra in college. Therefore, I use a sequential matching estimator that extends the traditional RD identification strategy to situations where a subsequent treatment also changes discontinuously at the eligibility cutoff (Speroni, 2011). This sequential RD approach is able to determine whether any observed effect on outcomes experienced after college can be attributed solely to the DE experience.

I estimate the effect of DE in a subset of Florida's districts where there is empirical evidence that the eligibility requirements for participation are binding. Using both high school and college transcripts for the 2000–01 and 2001–02 graduating high school cohorts, I find little indication that taking a DE-basic course significantly affected educational progress among students with a high school GPA on the margin of eligibility. The point estimates are generally negative, though I cannot consistently reject the hypothesis that

⁵ While English composition also has a CPT requirement for enrollment, the cutoff used for placement into this course also determines whether a student should enroll in remedial courses — non-credit-bearing courses designed to help prepare academically underprepared students for college-level work. CPT retesting among students who initially score just below Florida's cutoff for placement into college-level English has been documented in most colleges, raising concerns about the validity of an RD design in assessing the impacts of this course (Calcagno & Long, 2008). In contrast, the cutoff score used for placement into college algebra is higher than the one used for math remediation.

taking DE had no effect across outcome measures. Drawing from a subsample of students who took Florida's CPT, however, I find that taking college-level algebra in high school had a substantial influence on students' likelihood of going to college and obtaining a college degree, with some indication of positive effects on high school graduation. Specifically, I find that taking DE-algebra increased college enrollment by about 16 percentage points (standard RD estimate) and both associate and bachelor's degree attainment by about 23 percentage points (sequential RD estimates).

Taken together, these findings suggest that DE programs have the potential to increase college enrollment and completion but that the quality, subject area, or level of difficulty of the DE experience might influence the value of DE programs as a policy intervention. It is important to highlight, however, that the RD estimates only speak to the local effect of DE among students on the margin of eligibility and may not be representative of the gains from participation for students with different academic preparation. In addition, the two RD analyses inevitably draw inferences from different sets of students, and I cannot rule out the possibility of DE impact heterogeneity with respect to students' characteristics, in addition to the subject area or quality of the DE experience.

The remainder of the paper is organized as follows. Section 2 provides background information on DE programs, the potential mechanisms by which they affect students' outcomes, and Florida's DE policy. Section 3 describes the data. Section 4 explains the empirical strategy. Section 5 discusses the validity of the RD assumptions. Section 6 presents the main results, and Section 7 provides robustness checks. Section 8 concludes the paper.

2. Dual Enrollment Program

DE differs from other high school programs that allow high school students to earn college credits, such as Advanced Placement (AP), International Baccalaureate (IB), or the more recently established Advanced International Certificate of Education (AICE).⁶ The main difference is that DE consists of a regular college course that grants credit to students who pass. Other programs follow a standardized college-level curriculum, and college credits are only obtained with a satisfactory score on an external (often optional) end-of-course examination. Because some DE courses are taught at high school campuses, not all involve a true college experience, with high school students and college students in the same classroom. Nevertheless, DE instructors must meet the faculty qualifications for an adjunct community college instructor in most states.

Dual Enrollment Conceptual Framework

There are several potential channels through which a DE experience could foster college access and success. First, DE might help students build human capital by providing a broader and more rigorous curriculum than traditional high school courses, facilitating the “academic transition” to the demands of college (Bailey, Hughes, & Karp, 2002). Second, DE could have a signaling value for students preparing for admission at selective colleges, as it conveys information about students’ abilities and motivation, mitigating the information asymmetry in the college application process. Third, DE could potentially give students more accurate information about the institutions and their own college readiness, providing an early call to strengthen their skills in particular areas if necessary. Consistent with the theory of “schooling as experimentation” (Manski, 1989), DE would allow students to “test the waters” in college before making their postsecondary school choice. This may translate into better matches between students and institutions and may improve college persistence and degree completion (Light & Strayer, 2000). Last, by reducing the cost of college, DE might also foster college access. By enrolling in DE, students can reduce the time (and forgone earnings) required to get a college degree and, in states with subsidized DE programs, the direct cost of a degree. These financial benefits might be a key factor influencing the decision to pursue a college degree among low-income students (Greenberg, 1988). In addition, DE might help students make the “psychological transition” to college demands (Bailey et al., 2002).

⁶ While the College Level Examination Program (CLEP) is also considered an acceleration mechanism, unlike the others, it does not involve enrolling in a course, and the college credit students earn in the program does not count toward high school graduation.

While DE can be viewed as a potential tool to increase college access and success, it is not without controversy. A common concern about DE programs is that they might lower students' self-esteem and educational aspirations. It is not clear that students who are marginally successful in high school can do college-level work (Bailey & Karp, 2003). Course failure might discourage these students from pursuing postsecondary education altogether or set them on a nonacademic path. Given that DE programs are mostly offered through two-year colleges, the DE experience might also induce students who would have otherwise attended a four-year college after high school graduation to enroll in community colleges. This may reduce their educational attainment due to high transfer costs and a possible lack of emphasis on bachelor's degree attainment at community colleges, among other barriers. In addition, the ability of DE programs to provide college-level curricula has been questioned (e.g., Johnstone & Del Genio, 2001). Allowing high school students into college classes could also dilute the quality of education at the college campus, and many observers are skeptical about the quality of DE courses taught by high school teachers at the high school campus.

Florida's Dual Enrollment Policy

Florida has been at the forefront of many innovations in educational policy, including DE. Florida is one of six states that pay for DE courses, while most other states require the school district or the students themselves to pay for them (Western Interstate Commission for Higher Education [WICHE], 2006). This funding provision not only promotes program participation but also enables access for students from low-income households. In addition, Florida funds both high schools and colleges for DE courses (Office of Program Policy Analysis and Government Accountability [OPPAGA], 2006), encouraging schools to support students' participation.

Florida is also one of only 15 states that allow students to earn high school and postsecondary credit simultaneously and guarantee that the credit counts toward high school graduation requirements (WICHE, 2006). Successfully completed DE courses may apply toward the requirements necessary to earn a certificate or degree, thereby shortening the time it takes to earn a postsecondary award. Florida has also developed a statewide course numbering system that eases the transfer of credits among the state's public institutions. Since 2006, the legislature has required the state's public universities to weight DE courses the same as AP, IB, or AICE courses when calculating students' GPAs for admissions decisions. This policy, coupled with the fact that students in Florida are exempt from the payment of registration, tuition, books, and laboratory fees related to DE courses, makes the DE program an attractive acceleration mechanism.

Florida's strong support for DE has been crystallized in a widespread program, with each of the 28 community colleges in the Florida College System having an articulation agreement in place with its serving district and all school districts taking advantage of such possibility. DE is the second most popular acceleration mechanism (AP is the first). Overall, about 14 percent of the high school students in this study took at least one academic DE course, while 20 percent took AP and 3 percent IB.

Florida's Dual Enrollment Eligibility and Enrollment Process

To be eligible for DE in Florida, students are required to have a minimum unweighted GPA of 3.0 and to demonstrate college readiness on the College Placement Test (CPT) (Florida Statute 1007.271).⁷ The statute does not specify which portions of the CPT are appropriate for admission into specific courses. However, common practice has been to set the same requirements for both DE and regular college students.⁸ That is, while all candidates need to take a placement test, only a few college courses (typically in math and English) have a minimum score requirement for enrollment. In particular, students must pass the math or English portion of the CPT before enrolling in math or English courses, and they must fulfill any course prerequisites when required.⁹ Colleges must use statewide cutoff scores for placement into certain introductory courses, such as intermediate algebra or freshman English composition, but are free to define the cutoffs for placement into more advanced courses. For example, students can bypass intermediate algebra and place directly into college algebra, a particular course analyzed in this paper, provided they meet the college-specific cutoff score.

Students can only take DE courses through their local community college.¹⁰ Each community college has formal agreements with the school districts in its attendance area about the requirements for participation. I compiled DE agreements and college catalogs for

⁷ The statute also stipulates a minimum unweighted GPA of 2.0 for DE vocational courses. Vocational DE students are a small and distinct group; very few students enroll in both vocational and academic courses. Unfortunately, a separate RD analysis on vocational DE courses is not feasible due to small sample sizes.

⁸ The only difference between DE and regular college students is that DE students whose CPT placement score is below the state minimum required score for "college-readiness" (defined in Rule 6A-10.0315) are not allowed to enroll in remediation. Remedial courses and physical education courses are excluded from the DE program.

⁹ The Florida College Entry-Level Placement Test (CPT) is a computer adaptive test developed by the College Board at the request of the Florida Department of Education to establish common standards across community colleges. Students are allowed to substitute appropriate SAT or ACT scores for CPT scores.

¹⁰ Some four-year colleges offer DE, but their courses are not subsidized by the state, and participation rates are very low.

the years relevant to this study. Most colleges set their GPA requirements at the minimum of 3.0 required by statute. The statute also allows districts and colleges to make exceptions to the GPA requirement or set additional admission criteria for participation if they are included in their inter-institutional articulation agreement. Exceptions are generally granted on a case-by-case basis by the college DE coordinator and the high school counselor or principal. The most common additional admission requirement is a letter of recommendation from a teacher or counselor (required by about 65 percent of the districts, according to Florida Board of Education [2003]). Only a few colleges place restrictions based on students' age or grade level.

The application process for DE courses involves close interaction between students and their high school counselors. Based on students' scores and career goals, counselors help students choose appropriate courses and complete the application form. The form, typically a one-page document, includes a student's personal information and course selection choices and a statement (often signed by the high school counselor) of the student's current GPA and CPT scores. Students and parents sign the application and allow the release of students' scores and high school transcript to the college. After the application form is submitted, the student is considered a college student and is subject to the same standards as a regularly matriculated student.

3. Data

This study uses data from the Florida Department of Education, which includes all public school students in the 2000–01 and 2001–02 high school senior cohorts and tracks their postsecondary outcomes in the state’s public system through summer 2007. The state’s administrative records provide transcript information on courses taken and grades received in high school and college. The data contain basic demographic characteristics, such as gender, race, English language proficiency, and free lunch eligibility, as well as students’ 10th grade state standardized test scores (from the Florida Comprehensive Assessment Test, or FCAT) and college placement test scores. State records on postsecondary enrollment (though not degree attainment) are complemented with data from the National Student Clearinghouse (NSC), which tracks postsecondary enrollment of students as they enroll in out-of-state colleges or private institutions.¹¹ District characteristics, such as median income and urbanicity, are obtained from the 2000 Common Core Data and decennial census.¹²

There are two key features of the data that are particularly relevant for a study of DE. First, the data track individual students as they transition from high school to college. Most previous studies on DE use college transcript records and therefore restrict the analysis to students who go to college. Given that DE might change the composition of students who go to college, limiting the sample to college-goers induces sample selection bias, though the direction of this bias is unclear. Not only might DE students who go to college differ from those who do not, but they also might differ from non-DE students who go to college without the program’s help. Second, the data contain a unique identifier for DE courses, indicating their location (high school or college campus) and type (academic or

¹¹ Because the NSC data are limited to enrollment, degree attainment is only identified for students who enrolled in Florida’s public higher education system. In order to address this missing data problem, I follow the standard approach of imputing zero when students have no postsecondary records. This approach would induce bias in an RD estimation if students who score just above the DE eligibility cutoff were disproportionately more likely to attend college out-of-state or in one of Florida’s private institutions than students who score just below the cutoff. However, Table 3 shows that the probability of enrolling outside Florida’s public postsecondary sector does not significantly change at the DE eligibility cutoff. In addition, the number of students who would be incorrectly classified as not having a college degree is likely to be small because only 7.7 percent of the students in the full sample went to college outside Florida and 4.5 percent attended a Florida private institution.

¹² This study uses the same cohort of students used by Karp et al. (2007) but employs an augmented dataset that includes 10th grade standardized test scores (FCAT), college placement test scores, and NSC data. In addition, the data in this paper track students for two additional years, allowing sufficient time to evaluate the effect of DE on college degree attainment. I follow, however, different sample restrictions: I (1) define DE students as those who took at least one academic DE course (i.e., not vocational courses) and (2) identify DE using college transcripts instead of high school education in Florida (grades 10 through 12) in order to accurately calculate the cumulative GPA.

Identifying DE students, course location, and course type is often challenging when only college transcripts are available. The difficulty arises because not all students transfer their DE credits, and when they do, credits are recorded as transfer credits, requiring high school graduation dates (which are not always available in college transcripts) to accurately identify DE credits.

I collected GPA eligibility requirements for DE from the inter-institutional articulation agreements between the districts and colleges, personal communication with DE coordinators, and directly from the college catalogs for the years relevant for the study. The CPT cutoff scores for placement into the college algebra course are obtained from the college catalogs or, when unavailable, from state documentation on placement scores (Florida Department of Education, Articulation Coordinating Committee, 2006).¹³ Appendix tables A.1 and A.2 provide a list of requirements by college.

I examine the effect of DE with two separate RD analyses. The first analysis measures the effect of taking at least one academic DE course (regardless of subject area), exploiting the general GPA eligibility requirement for participation in the program.¹⁴ A course is considered academic if it counts toward the requirements of an associate degree, as opposed to vocational courses that are only applicable toward certificates. I focus on participation in 12th grade because that is when most DE experiences take place: 82 percent of DE students took a DE course in their senior year.^{15, 16} It is important to highlight that, despite “treatment” being defined as “any course,” this analysis only captures variation in DE participation that is generated by the GPA cutoff requirements, largely excluding

¹³ To the extent that cutoff scores changed between 2000 and 2006, the college would be excluded from the analysis due to no discontinuity in participation at the mis-specified cutoff. Visual inspection of the data, however, rules out strong evidence consistent with the use of alternative cutoffs in all but one college, whose cutoff was changed accordingly.

¹⁴ I exclude academic DE courses taken within the state university system, at special education centers, or on a full-time basis (i.e., early enrollments). Only about 0.7 percent of all DE courses are not directly offered by a community college, and about 2.5 percent are early enrollments. These courses are typically subject to different eligibility requirements. DE courses with missing enrollment data are also excluded.

¹⁵ The data do not provide enough variation in course-taking patterns to identify the effect of taking DE at different times in high school (e.g., junior or senior year). Most DE students took DE exclusively in 12th grade (about 55 percent); the majority of the remaining students took DE in both 11th and 12th grade.

¹⁶ One potential drawback of the data is that they only include the cohort of students who persisted to 12th grade and do not include data on students who dropped out earlier. To the extent that previous DE experience contributed to early dropout behavior, the estimation of the effect of DE will be biased. However, the incidence of DE participation before 12th grade is low (less than 6 percent of all students), and it is unlikely that at-risk students would be on the margin of eligibility for DE because academic requirements for participation are relatively high.

valuable information about other courses that have additional test requirements. While most college courses do not have a test requirement, a few (typically math, English, and some science courses) do. I refer to this analysis as the effect of DE-basic.

The second RD analysis examines the effect of taking one particular challenging DE course, college algebra, which covers topics such as graphing functions and solving systems of equations and is the first course in the math sequence that counts toward Florida's statewide requirements for an associate degree. This analysis exploits the course-specific CPT math score requirement for participation.¹⁷ One limitation of the placement test data is that for the cohorts used in this study, the state only kept a record of a student's highest score (if the test was taken multiple times) and did not collect information about the test date, making it impossible to infer whether the test was used for eligibility in high school or college. However, students whose scores were not used for eligibility do not generate variation in DE participation and thus are not a concern to the identification. In addition, by examining the density of the score, I show in Section 5 no evidence of "re-testing bias" being present in the sample analyzed. Because of the absence of test-date information, I define treatment as taking DE-algebra in either 11th or 12th grade, covering 93 percent of all course-takers.

Outcome Measures

This paper measures the effect of DE on several academic outcomes. The first outcome of interest is high school graduation, which includes all types of diplomas offered in Florida (regular or special education diplomas, certificates of completion, and GEDs). Second, I examine the effect of DE on college enrollment using two measures: whether students enrolled in either a two-year or four-year college after high school graduation and whether they first enrolled in a four-year college. The availability of National Student Clearinghouse data allows me to observe college enrollment for students at public and private institutions both in-state and out-of-state. Last, I examine the effect of DE on college completion as measured by the likelihood of obtaining an associate degree, a bachelor's degree, or either an associate or bachelor's degree within five years from when the high school cohort was expected to start college. The empirical approach in this paper is to include in the sample all high school students and assign a value of zero on college degree outcomes for students who do not go to college. The effect on college degree

¹⁷ While students are allowed to substitute appropriate SAT or ACT scores for CPT scores, I only exploit the variation in participation that comes from the CPT because that is the test where most of the variation in participation at the cutoff is observed.

outcomes represents the overall effect of DE, combining its effects on college enrollment and academic performance.

Table 1 shows descriptive statistics broken down by DE participation status. DE is a voluntary program, and participants are very different from non-participants. DE students are more likely to be female, White, native English speakers, and from economically advantaged households (as proxied by free or reduced-price lunch status) than non-DE students. DE students also appear to be more academically prepared than non-DE students, based on their 10th grade standardized test scores in reading and math and their high school GPAs. Given these differences, it is not surprising that DE students were more likely to experience positive postsecondary education outcomes than non-DE students. They were about 30 percentage points more likely to enroll in college after high school and 25 percentage points more likely to first enroll in a four-year institution. DE students were less likely to enroll in remedial courses in college and significantly more likely to earn college degrees than non-DE students.

Table 1
Descriptive Statistics of Dual Enrollment Participants and Non-Participants

Variable	All Students	DE	Non-DE
Student characteristics			
Female	51.3%	62.4%	49.4%
White	55.7%	77.7%	52.0%
Black	24.1%	10.6%	26.3%
Hispanic	17.2%	7.4%	18.9%
2001 cohort	50.1%	51.2%	49.9%
English language learner	3.0%	0.5%	3.5%
Free or reduced-price lunch	42.8%	22.8%	46.3%
FCAT Reading score, 10th grade	304.3	334.5	298.4
FCAT Math score, 10th grade	313.9	344.2	308.0
High school GPA	2.67	3.26	2.58
Outcomes			
High school diploma	90.6%	98.1%	89.3%
Enrollment at any college	61.8%	88.3%	57.4%
Enrollment at any four-year institution	39.5%	59.0%	34.4%
Enrollment at in-state four-year institution	33.0%	53.3%	27.8%
Persistence to second term	76.1%	83.0%	74.3%
Persistence to second year	72.6%	84.4%	69.6%
Remedial reading enrollment	23.3%	5.8%	27.8%
Remedial English enrollment	18.1%	3.8%	21.8%
Remedial math enrollment	33.0%	11.4%	38.5%
Freshman college GPA (including DE courses)	2.43	2.95	2.30
Associate degree	21.5%	32.0%	18.8%
Bachelor's degree	23.2%	42.6%	18.2%
Observations	229,904	32,980	196,924
%	100%	14%	86%

SOURCES: Florida K-20 Education Data Warehouse & National Clearinghouse data (extracted April 2008).

NOTES: DE denotes students who took at least one academic dual enrollment course.

4. Econometric Framework: Standard RD and Sequential RD Design

This section provides a simple example to convey the intuition behind the standard RD approach and the sequential RD approach, which extends the standard framework to the case where participation in a subsequent treatment changes discontinuously at the cutoff but participants only take one of the treatments. While the standard RD design is appropriate for estimating the effect of DE-basic or the effect of DE-algebra on pre-college outcomes, the sequential RD design is appropriate for estimating the effect of DE-algebra on college outcomes.¹⁸

The Intuition Behind the RD Approach: A Numerical Example

Suppose that students' participation is determined by the value of their score. Students can be divided into three groups. *Always-takers* participate in DE whether their score is below or above some cutoff z_0 . *Never-takers* do not participate, irrespective of their score. *Compliers* participate if their score is above the cutoff and do not participate if their score is below the cutoff. Suppose that there are 40 percent never-takers, 40 percent always-takers, and 20 percent compliers in the population. In addition, suppose that the average effect of DE on compliers is to increase their college graduation rates from x to y . Figure 1 plots the data that would be used by a researcher to infer the effect of DE.

The left panel in Figure 1 shows participation rates in DE as a function of a score. The jump of 20 percentage points is explained by the proportion of compliers in the population. The middle panel shows college graduation rates as a function of the score. By RD assumption, in the absence of a treatment, the outcome changes continuously in the score. Thus, the jump in outcome at the cutoff ΔY must be driven by compliers. In particular,

$$\Delta Y = P(C) \times \tau_C = .2 \times .5 = .1$$

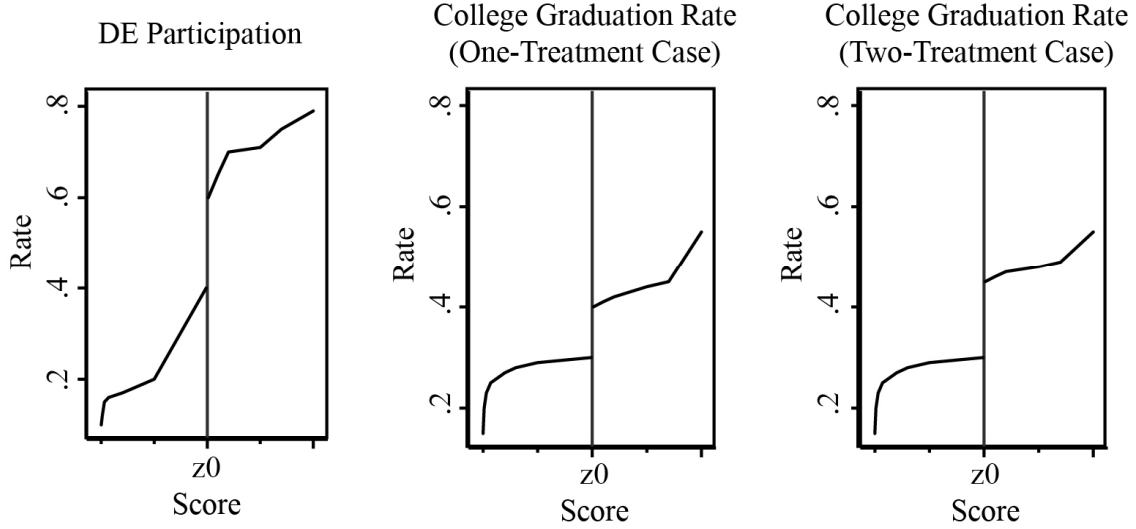
where the proportion of compliers (or jump in participation) is $P(C) = .2$ and where the effect of DE on compliers is $\tau_C = y - x = .5$. In practice, the researcher can obtain the effect of DE on compliers by dividing the jump in outcome by the jump in participation,

$$\tau_C = \frac{\Delta Y}{P(C)}.$$

¹⁸ See Speroni (2011) for the formal derivation of the sequential RD estimator.

Figure 1

DE Participation and Outcomes as a Function of a Score
(Hypothetical Example)



The above is the standard RD estimate, and it is sufficient in this paper to estimate the effect of DE on pre- and post-college outcomes as well as the effect of DE-algebra on pre-college outcomes.

Next, consider the objective of estimating the effect of DE-algebra on post-college outcomes. The issue is that the cutoff z_0 is also used in determining eligibility to take algebra while in college. Therefore, there is a greater jump in outcomes at the cutoff driven by never-takers of DE who benefit from taking algebra as college students. For example, suppose that 25 percent of never-takers are compliers in the second treatment, meaning that they would take algebra in college if they scored above the cutoff but not if they scored below the cutoff. Suppose, in addition, that these never-takers who are compliers in the second treatment would obtain a benefit of $\tau_{NT/C} = .5$ from participating in the second treatment. Then the jump in outcomes would be given by

$$\begin{aligned} \Delta Y &= P(C) \times \tau_C + P(NT) \times P(C | NT) \times \tau_{NT/C} \\ &= .2 \times .5 + .4 \times .25 \times .5 = .15, \end{aligned} \tag{1}$$

as illustrated by the right panel in Figure 1.

A naive application of the standard RD estimate would attribute the entire change in outcome to the effect of DE, thus obtaining an estimate

$$\Delta Y / P(C) = .15 / .20 = .75$$

that overestimates the true effect of DE, .5. But from equation 1, it follows that

$$\tau_C = \frac{\Delta Y}{P(C)} - \frac{P(NT) \times P(C | NT)}{P(C)} \times \tau_{NT/C} \quad (2)$$

so that the standard RD estimate must be corrected to account for the effect of the second treatment, $\tau_{NT/C}$.

The final step is to obtain an estimate for second treatment. If the population of never-takers could be identified, then the natural approach would be to apply the standard RD design for the second treatment exclusively to never-takers, obtaining $\tau_{NT/C} = \Delta Y_{NT} / P(C | NT)$, i.e., the jump in outcomes divided by the jump in participation for never-takers. The problem is that the jump in outcomes is not exactly observed for never-takers. Rather, the jump of outcomes is observed for the population of students that do not take DE. While these are exactly the never-takers when their score is above the cutoff, they can be either never-takers or compliers when their score is below the cutoff. The sequential RD estimator matches students below the cutoff to students above the cutoff to determine which students are the never-takers below the cutoff. The validity of the matching procedure follows from the same assumptions that make the RD design valid (Speroni, 2011).

Standard RD Estimation

The standard implementation of the RD design identifies the impact of the program by comparing outcomes of students who barely pass with those of students who barely miss the required GPA (or CPT) cutoff score. Because not every student above the cutoff takes a DE course and not every student below the cutoff is disallowed enrollment (because exceptions are granted), this difference in outcomes is scaled up by the difference in the probability of enrolling in DE — a design known as “fuzzy” RD (Campbell, 1969).

Following Imbens and Lemieux (2008), I use a local linear regression to estimate the program effect in a two-stage least-squares instrumental variable specification (RD-IV) with the following *first-stage* and *reduced-form* equations

$$D_i = \beta_0 + \beta_1 Above_i + \beta_2 f(Score_Gap_i \times Above_i) + \beta_3 f(Score_Gap_i \times Below_i) + X_i \delta + \varepsilon_i \quad (3)$$

$$Y_i = \alpha_0 + \alpha_1 Above_i + \alpha_2 f(Score_Gap_i \times Above_i) + \alpha_3 f(Score_Gap_i \times Below_i) + X_i \theta + \varepsilon_i \quad (4)$$

that characterize the standard RD-IV model,

$$Y_i = \gamma_0 + \tau_c \hat{D}_i + \gamma_2 f(Score_Gap_i \times Above_i) + \gamma_3 f(Score_Gap_i \times Below_i) + X_i \lambda + \varepsilon_i \quad (5)$$

where i is the student, D_i is an indicator that takes the value one if the student i took a DE-basic course in 12th grade (or DE-algebra in 11th or 12th grade) and zero otherwise, $Above_i$ ($Below_i$) is an indicator that the student is above (below) their designated college cutoff, $Score_Gap_i$ is the 11th grade GPA or CPT score centered around the cutoff (thus measuring distance to the minimum requirement), \hat{D}_i is the predicted probability from the first stage in equation 3, and X_i is a vector of covariates including students' gender, race, free or reduced-price lunch status, 10th grade standardized scores, cohort fixed effect, and high school-level demographics.¹⁹ The model is estimated locally using data incrementally close to the cutoff following different bandwidths around the cutoff. The function $f(\cdot)$ is specified as quadratic and linear when using a narrow bandwidth of the data around the cutoff. In all specifications, standard errors are heteroskedasticity-robust and clustered at the score level.²⁰ The parameter of interest is τ_c , which captures what Angrist, Imbens, and Rubin (1996) call the “local average treatment effect” (LATE) — the effect of DE participation for those students who were induced to participate because of their eligibility status (i.e., compliers). This average effect of DE participation is local in that it is only identified close to the eligibility cutoff. Thus, it is not necessarily indicative of the effect for other students with different levels of academic preparation.

Sequential RD Estimation

To simplify notation, the sequential RD equation 2 can be equivalently written as

$$\tau_C = \frac{\Delta Y}{P(C)} - \frac{P(NT)}{P(C)} \times (\Delta Y_{NT/C}) \quad (6)$$

¹⁹ Controlling for additional covariates is not necessary for identification because, by RD assumption, close-to-the-cutoff participation is “as good as” randomized (i.e., conditional on the score being close to the cutoff, other covariates are independent of participation). However, as in a pure randomized study, adding controls helps improve the precision of the estimate by reducing residual variation.

²⁰ Lee and Card (2008) recommend clustering standard errors when the variable that determines eligibility is discrete, such as CPT score. I also report clustered standard errors when using GPA, though the difference is immaterial.

I estimate the first term in equation 6 following the standard RD estimation. For the second term, I proceed as follows. I use a version of the first-stage equation 321 to obtain an estimate for the proportion of never-takers around the cutoff by

$$P(NT) = 1 - (\hat{\beta}_0 + \hat{\beta}_1)$$

To measure the $\Delta Y_{NT/C} = Y^+_{NT/C} - Y^-_{NT/C}$, I first estimate the RD-IV reduced-form equation 4 *on those students who do not take DE* and get an estimate of the outcome for students slightly above the cutoff. This is given by

$$Y^+_{NT/C} = \hat{\alpha}_0 + \hat{\alpha}_1$$

In both of the previous cases, I am careful to interact the observables X in equations 3 and 4 with the score and an indicator for whether the score is above or below the cutoff.

Finally, I estimate the outcome below the cutoff $Y^-_{NT/C}$ in several steps. I begin by identifying for each non-DE student below the cutoff the closest match to a non-DE student above based on the full set of covariates used in the regressions. I employ the nearest-neighbor matching algorithm (with replacement) developed by Abadie, Herr, Imbens, and Drukker (2004). I then estimate the proportion of never-takers below the cutoff, which is given by

$$P\left(\frac{NT}{CorNT}\right) = \frac{P(NT)}{P(CorNT)}, \text{ where } P(CorNT) = 1 - \beta_0.$$

Using the cumulative distribution function of students' distance to their nearest match,²² I then identify the closest $P\left(\frac{NT}{CorNT}\right)$ percent of the matches. Based on this subsample of students, I then run a regression using data below the cutoff of the form

$$Y_i^- = \tau Y^-_{NT/C} + f(\text{Score_Gap}_i) + X_i \varphi + \varepsilon_i \text{ for } z_i < z_{0i} \quad (7)$$

to get an estimate of $\tau Y^-_{NT/C}$.

The validity of the matching hinges on the RD assumption that around the cutoff students are similar and the proportions of student types are constant. Because the assumption holds in a narrow margin around the cutoff, I estimate equation 7 using only

²¹ To facilitate the estimation of terms defined at the boundary of eligibility, all covariates are fully interacted with an indicator variable for the CPT above and below the cutoff and the score (centered at the cutoff).

²² The distance is a metric that synthesizes the difference between the matched-pair vector of covariates (see Abadie et al., 2004).

observations close to the cutoff (within a bandwidth of 15 points), though I also experiment with alternative bandwidths as sensitivity checks. The drawback of the selection criterion is that, in practice, the differences in observable characteristics of student types are likely to be smooth over the support of the matching distance, and the selection criterion would misclassify some students. I deal with this issue by using a more stringent selection criterion for robustness.

Finally, I insert all the previous estimates in equation 6 and obtain an estimate $\hat{\tau}_C$ of the effect of DE-algebra. While in the standard RD case standard errors can be obtained with two-stage least-squares estimation, there is no explicit analytic solution for standard errors in the sequential approach. Standard errors for the sequential RD matching estimator are therefore obtained by bootstrap.

5. Validity of the RD Design

The fundamental assumption of an RD design is that, with the exception of the treatment of interest, any other determinant of the outcome varies smoothly around the cutoff.²³ In this section, I assess the validity of applying the RD design described in Section 4 to the data. First, I select the sample of colleges that exhibit a significant discontinuity in participation in DE at the GPA cutoff (GPA sample) and in DE-algebra at the CPT cutoff (CPT sample). I then examine the validity of using GPA and CPT eligibility as an instrument in the selected samples. Finally, I consider evidence of other treatments that may be affected by the same eligibility criteria as DE. The conclusion is that the standard RD-IV strategy is appropriate for all outcomes under the GPA analysis and for pre-college outcomes under the CPT analysis but that the sequential RD strategy is needed for post-college outcomes under the CPT analysis.

Discontinuity: The GPA and CPT Samples

While an RD approach requires participation in DE to “jump” at the cutoff, I find that, for most colleges, there is no evidence of discontinuity.²⁴ To avoid confounding an

²³ There are two additional assumptions that are specific to the current application. First, the sequential RD estimator assumes that students take the CPT once and that the researcher observes the test score. In practice, students may take the exam during high school and then again during college, and I only observe the highest score in the data. This does not provide a threat to the validity of the RD design as long as the probability that the observed score is the high school score is the same right above and right below the cutoff. Second, not all colleges in the selected samples have the same cutoff scores. The use of different cutoffs, however, does not present a challenge for an RD identification strategy. Since students in Florida are required to take DE courses sponsored by their local community college, there is no need for concern about self-selection bias associated with students choosing DE courses at colleges based on their cutoff policies. However, the pooling of colleges with different cutoffs to provide an estimate of the effect of DE implicitly assumes homogeneity of the treatment effect for students on the margin of those different cutoffs.

²⁴ The lack of discontinuity in participation at the official cutoffs in most colleges may be due to a number of factors. First, because colleges typically base admission decisions on GPA as reported by high school counselors on students’ DE application forms, any discrepancy between this self-reported GPA and the actual GPA in the transcripts (e.g., due to rounding of GPA decimal points) would tend to attenuate the discontinuity estimates. Another plausible explanation relates to Florida’s severe overcrowding problem during this time period, exacerbated by pressure to reduce class sizes. DE was regarded as an effective strategy to alleviate overpopulated classrooms (Florida Board of Education, 2003), and districts and colleges may have waived GPA requirements to rapidly increase DE participation. The lack of discontinuity in DE-algebra participation in some colleges may also be attributed to the fact that some colleges have additional testing requirements above and beyond the CPT for direct placement into algebra (e.g., the College Level Math test, a score not available in the data). While students may be using SAT or ACT scores instead of CPT scores in their DE applications, using

increase in participation at the cutoff with random noise, I restrict the analyses to those colleges that show a discontinuity that is significant and robust across bandwidth choice and model specification. Specifically, for each treatment (DE-basic and DE-algebra), I estimate a linear probability model of participation in 12th grade as a function of the score (11th grade cumulative high school GPA²⁵ and CPT math score, respectively) on an eligibility indicator (to capture the jump) using data incrementally close to the cutoff (within 0.5/0.4/0.3 GPA points and 40/20/10 CPT points around respective cutoffs), controlling for a quadratic function of the score (linear in smaller bandwidths), which is allowed to vary on either side of the cutoff.

The discontinuity in DE participation as a function of GPA is estimated conditional on having a college placement test score, reflecting the fact that students need to present placement scores as part of their DE application regardless of the course in which they intend to enroll. In theory, students willing to take DE-algebra are required to have an eligible GPA in addition to a passing CPT math score. In practice, however, the CPT math score cutoffs are sufficiently high that most students with scores around those cutoffs have an eligible GPA. Thus, I estimate the discontinuity in DE-algebra participation using only the CPT math score, and I show in the robustness section that results are not sensitive to considering both criteria. The findings are reported in Tables A.1 and A.2 in the appendix.²⁶

I select colleges with a significant (at the 10 percent confidence level) discontinuity in any two of the three models for the main analyses, though I relax the criterion for robustness checks. Six colleges were selected for the DE-basic analysis (hereafter *GPA sample*), and seven colleges were selected for the DE-algebra analysis (hereafter *CPT sample*²⁷). Overall, the analyses use data from 11 different colleges (two colleges are included in both GPA and CPT samples) serving 42 percent of Florida's 67 school districts and educating about a third of all DE students in the state.

the CPT score for students who took multiple tests should not prevent the detection of sizeable discontinuities as long as the CPT is used for eligibility for a fraction of students and CPT barely-failers are not SAT/ACT barely-passers or vice-versa.

²⁵ Because the largest DE enrollment occurs in the fall semester of students' senior year, I use the cumulative 11th grade GPA up through the summer before the senior year to determine eligibility for 12th grade courses. High schools with non-standard academic calendars were normalized to a semester calendar (fall, spring, summer) in the calculation of GPA. An unweighted GPA was used for all but three colleges that indicated the use of weights to determine DE eligibility. Weighted GPA was calculated using one additional grade point for AP, IB, DE, and Honors courses, following the most frequently used weighting scheme reported by districts (Florida Board of Education, 2003).

²⁶ Because students in a given district can only take DE from the local community college, the discontinuity is estimated by college attendance area.

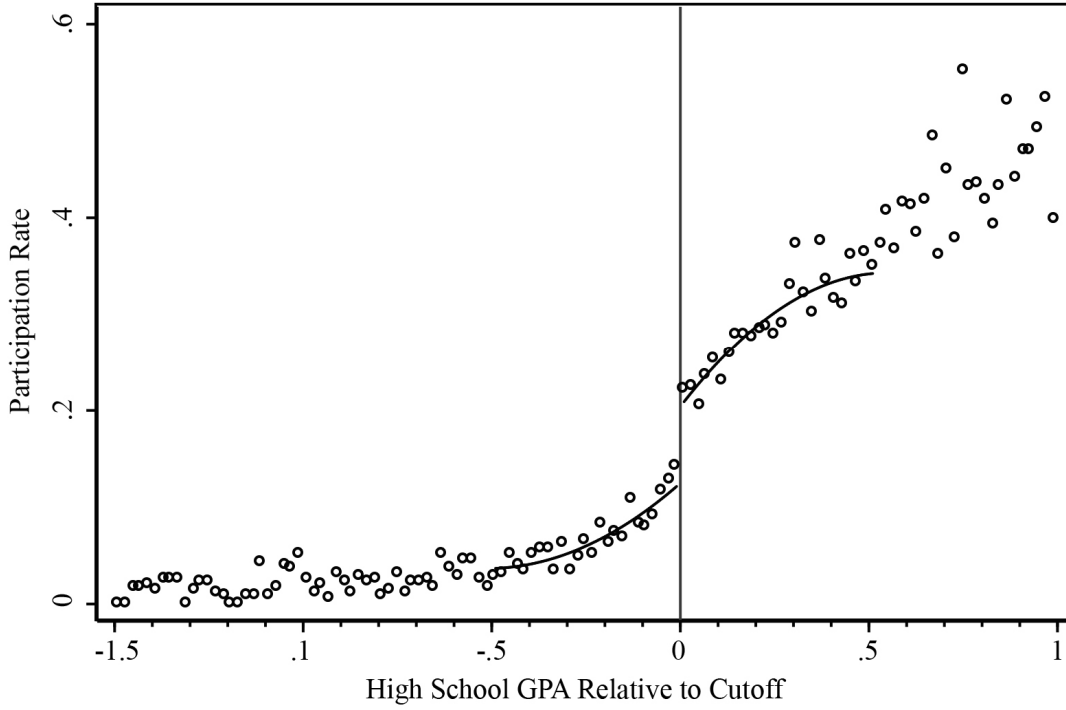
²⁷ Two colleges that use the same cutoff to determine both algebra participation and math remediation were excluded from the main analysis. Including these colleges, however, does not materially affect the results (see Section 7).

Figures 2 and 3 plot mean participation as a function of the score, along with the first-stage fitted values from equation 3 (without controlling for additional covariates). Participation in DE-basic increases from 13 percent for a GPA score slightly below the eligibility cutoff to 21 percent for a GPA score slightly above the cutoff. Participation in DE-algebra increases from 12 percent for a CPT math score slightly below the eligibility cutoff to 31 percent for a CPT score slightly above the cutoff.²⁸ As expected, participation eventually decreases for students with high CPT scores because those students place directly into higher-level math courses.²⁹ While exceptions to the GPA requirement are allowed with joint agreement of the high school counselor and the DE college coordinator, participation in DE-algebra among students scoring below the cutoff is likely due to students substituting SAT or ACT scores for CPT scores.

²⁸ The discontinuity of *ever* taking DE is remarkably similar (0.068 with a standard deviation of 0.019). This is because most students take DE for the first time in fall of their senior year, and the few who take DE exclusively during the spring would typically still use the 11th grade GPA for enrollment because spring applications begin before the end of the fall semester.

²⁹ These discontinuities, while arguably small in magnitude, are strong instruments for DE participation. The *F*-test on the excluded instrument renders statistics of about 25.6 and 25.8 for DE-basic and DE-algebra, respectively — both above the rule-of-thumb of 10 (Stock, Wright, & Yogo, 2002).

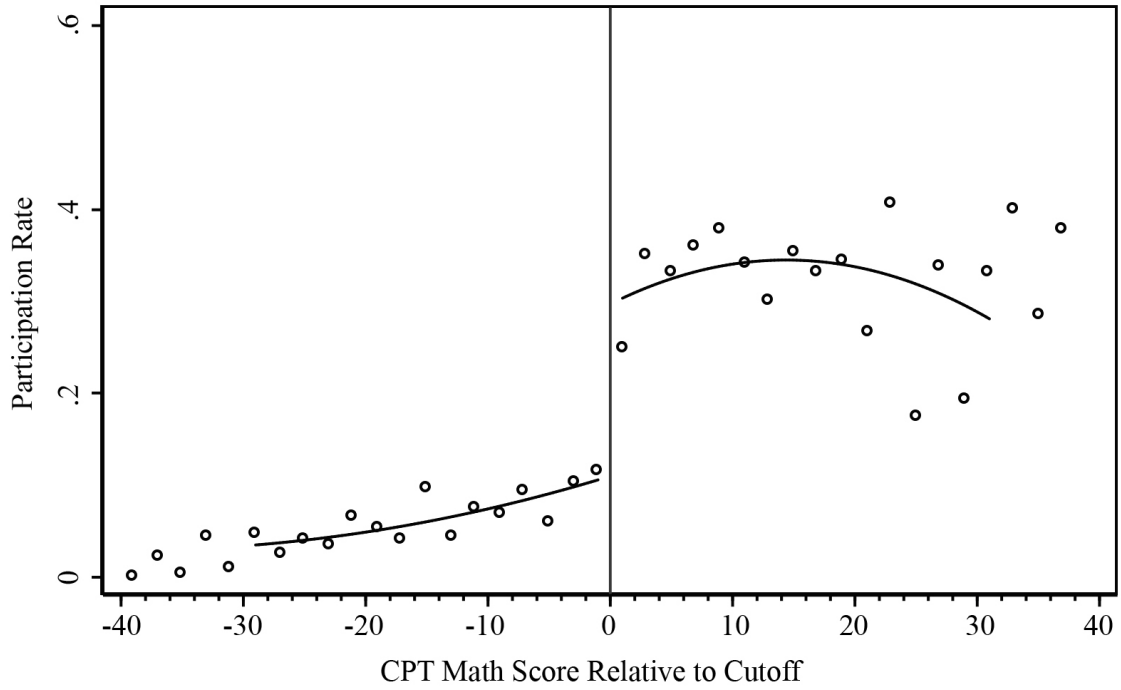
Figure 2
DE Participation in 12th Grade



NOTES: GPA is displayed in bins 0.02 points wide on either side of the cutoff. Circles are cell means at each GPA value. The solid lines are fitted values of a quadratic regression estimated within a bandwidth of 0.5 GPA points either side of the cutoff.

Estimated discontinuity = .082 (.016)

Figure 3
DE-Algebra Participation



NOTES: CPT scores are displayed in bins 2 scale score wide on either side of the cutoff. Circles are cell means at each CPT math scale score. The solid lines are fitted values of a quadratic regression estimated within a bandwidth of 30 points on either side of the cutoff.

Estimated discontinuity = .188 (.037)

Descriptive Statistics for Selected Samples

Table 2 compares the average DE student in the GPA and CPT samples with the average student in the state. The racial composition of DE participants in the GPA sample is quite similar to the state average, but participants in the CPT sample were slightly more likely to be White. Despite the higher representation of minorities, DE students in the GPA sample were less likely to come from an economically disadvantaged household: 17 percent received free or reduced-price lunch, compared with roughly 23 percent in the entire state and the CPT sample. Setting aside differences in race and economic standing, DE students in the selected samples are quite similar to the state average in terms of academic ability or preparation.

DE participation rates in the selected samples are significantly higher than the state average: the percentage of high school students who took at least one DE course is 25 percent in the GPA sample and 34 percent in the CPT sample, compared with a state average of 14 percent. A typical DE student took about four DE courses and earned about 10 college credits in high school (with a success rate of about 77 percent), with averages being similar across samples. DE-algebra was the most popular course among DE students after freshman English composition, with a take-up rate of about 18 percent. Finally, most DE students took DE courses either at the college and high school campuses (58 percent) or at the college campus (37 percent). Interestingly, only 5 percent of DE students took DE courses exclusively at the high school campus.

Instrument Validity

The RD approach requires that all determinants of the outcome except the treatment vary smoothly around the cutoff. While this assumption is at some level fundamentally non-testable, I assess whether students above and below the cutoff are observationally similar. A direct assessment of the similarity of students just above and below the cutoff is shown in Table 3. The table displays discontinuity estimates in predetermined student characteristics at the GPA and CPT cutoffs using data incrementally close to the cutoff. Overall, there is no evidence that students above the cutoff are statistically significantly different from those below the cutoff in characteristics known to affect outcomes (gender, race, English fluency, poverty, and pre-DE English and math test scores). Following Lee and Lemieux (2010), the last row in the table provides a comprehensive chi-square test that assesses whether the data are consistent with no discontinuity for any of the observed covariates. Test results support the hypothesis that students within a narrow bandwidth around the GPA and CPT cutoffs are indeed similar.

Table 2
Descriptive Statistics for Dual Enrollment Participants

Variable	All Colleges	GPA Sample	CPT Sample
DE student characteristics			
Female	62.4%	61.8%	62.3%
White	77.7%	79.6%	83.8%
Minority (Black or Hispanic)	18.0%	16.1%	12.4%
English language learner	0.5%	0.3%	0.2%
Free or reduced-price lunch	22.8%	18.6%	23.9%
FCAT Reading score, 10th grade	334.5 (32.2)	337.7 (29.4)	334.1 (31)
FCAT Math score, 10th grade	344.2 (31.6)	347.4 (30)	343.4 (30.8)
HS GPA	3.26 (0.47)	3.33 (0.41)	3.29 (0.46)
DE experience			
Participation rate	14.3%	24.9%	33.8%
Participation rate of high-ability students (11th grade GPA \geq 3)	33.0%	41.1%	59.6%
Average total DE courses attempted	3.7 (3.2)	4.0 (3.5)	3.9 (3.1)
Average DE courses attempted in 12th grade	2.9 (2.1)	3.2 (2.3)	3.0 (2.1)
Average DE credits attempted	11.0 (9.7)	12.2 (10.6)	11.7 (9.4)
Average DE credits earned	9.9 (8.9)	10.6 (9.6)	10.6 (8.8)
DE success rate (passing course with grade of C or higher)	77.0%	73.9%	74.0%
DE algebra success rate in 12th grade	77.7%	66.6%	71.6%
DE English composition success rate in 12th grade	83.1%	76.6%	79.8%
Enrolled in DE algebra in 12th grade	18.3%	19.6%	20.0%
Enrolled in DE English composition in 12th grade	29.5%	40.7%	37.9%
Enrolled in college course outside DE program	1.4%	1.1%	0.6%
Taking DE on a full-time basis (i.e., early admission)	2.3%	0.8%	3.7%
DE location			
Community college campus only	37.3%	38.4%	27.4%
High school campus only	4.9%	1.7%	1.6%
Both community college and high school campuses	57.8%	59.9%	71.0%
Schools in college attendance area			
Total senior enrollment (both cohorts)	229,904	35,526	15,667
Percent of total senior enrollment (both cohorts)	100%	15.5%	6.8%
Number of dual enrollment students	32,980	8,841	5,303
Number of districts	67	18	18
Number of colleges	28	6	7

NOTES: Standard deviations for continuous variables are shown in parentheses. Statistics are based on students who took at least one academic (i.e., non-vocational) dual enrollment course sponsored by a community college. GPA sample consists of all high school senior students who took a placement test (CPT, SAT, or ACT) in districts assigned to the community colleges selected for the DE-basic analysis. CPT sample consists of all high school senior students who took the CPT in districts assigned to the community colleges selected for the DE-algebra analysis.

Table 3
Discontinuity in Baseline Characteristics and
Participation in Other High School Programs by Sample

	GPA Sample		CPT Sample	
	± 0.4	± 0.3	± 30	± 10
Female	0.008 (0.023)	-0.003 (0.018)	0.028 (0.031)	0.038 (0.035)
Minority (Black or Hispanic)	-0.001 (0.027)	-0.001 (0.021)	0.028 (0.022)	0.006 (0.025)
English language learner	-0.002 (0.011)	-0.007 (0.008)	0.001 (0.007)	-0.004 (0.007)
Free or reduced-price lunch	-0.014 (0.020)	0.005 (0.017)	0.013 (0.026)	0.020 (0.028)
FCAT Reading, 10th grade	2.064 (2.066)	1.405 (1.570)	0.973 (2.233)	-1.436 (1.851)
FCAT Math, 10th grade	1.374 (2.044)	0.811 (1.631)	0.688 (1.463)	-0.554 (1.263)
AP 12th grade	-0.022 (0.029)	-0.028 (0.023)	0.006 (0.035)	-0.001 (0.042)
IB 12th grade	-0.009 (0.008)	-0.004 (0.006)	0.007 (0.007)	0.009 (0.006)
Honors 12th grade	-0.013 (0.032)	-0.015 (0.025)	0.030 (0.032)	0.041 (0.035)
Prob. outside Florida public higher education system	-0.000 (0.011)	-0.003 (0.008)	0.016 (0.012)	0.022 (0.017)
Chi-square test that all discontinuities in covariates are jointly zero, [<i>p</i> -value]	3.68 [0.815]	9.16 [0.241]	2.84 [0.899]	2.49 [0.928]
Number of students	15,649	12,141	7,921	2,959
Number of colleges	6	6	7	7

NOTES: Each cell represents the estimated discontinuity in the covariate from a linear probability model on a dummy indicating GPA or CPT score above the respective cutoff for each institution and a linear (odd columns) or quadratic (even columns) term on the score allowed to vary on either side of the cutoff. Fitted probabilities of participation were estimated using the full set of control variables used in the regressions as described in the text. Chi-square test was performed using seemingly unrelated regression estimation, where each equation represents a different baseline covariate listed in the table. Standard errors (in parentheses) are clustered at the GPA or CPT math score.

* $p < .10$. ** $p < .05$.

Another common concern is that highly motivated students who barely missed the cutoff may exert additional effort (or choose their courses strategically) to boost their GPA or retake the CPT in order to become eligible. This type of gaming behavior would render students on either side of the cutoff different in unobservable ways, violating the RD assumption. A diagnostic test for this endogenous sorting of students involves an analysis of the density of the score around the cutoff (McCrary, 2008).

Figure 4 presents the histogram for the GPA distribution, which follows a bell-shaped density curve with a spike at the number of students with a GPA of exactly 3.0. This spike, however, does not appear to be caused by endogenous sorting. By the nature of the GPA, spikes occur not only at the 3.0 cutoff used for DE eligibility but also at rounded GPA values that correspond to specific letter grades, such as 1, 2, 2.5, 4. In fact, Figure 5 (which describes the placebo sample) shows a similar GPA distribution in schools served by colleges where the cutoff for DE participation is not binding.³⁰

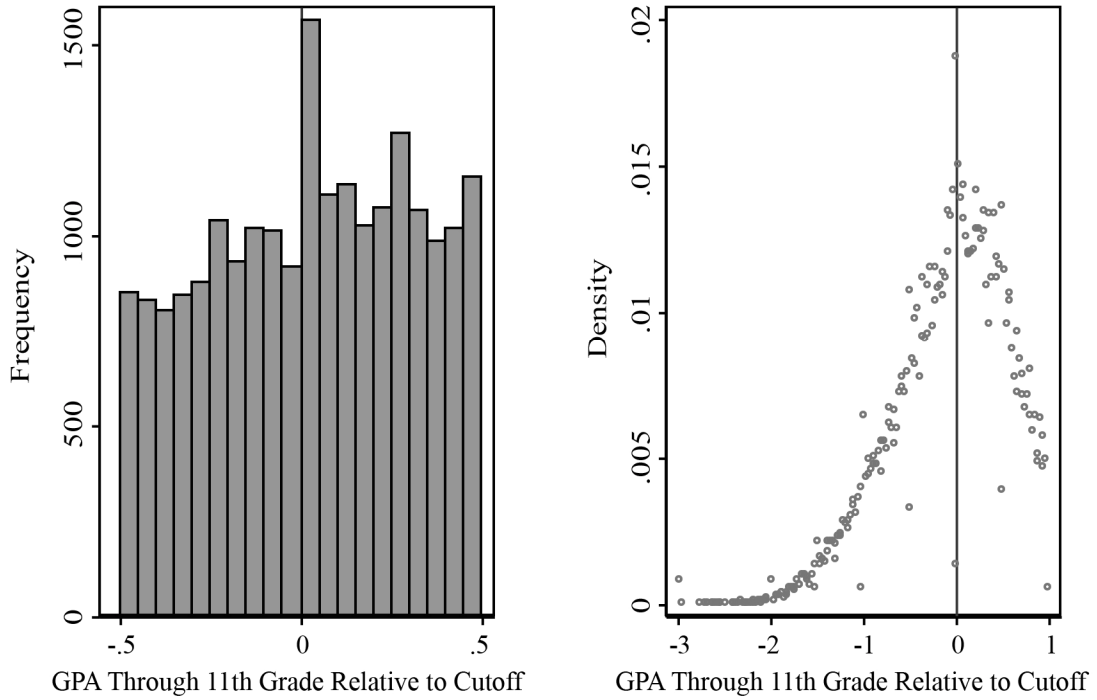
Figure 6 shows the density of the CPT score, which does not suggest the presence of students who retake the exam to become eligible for the algebra course. The estimated discontinuity in the empirical density is small and statistically insignificant (coefficient of 0.002 with a standard error of 0.001).³¹ This finding is not surprising, given the low stakes involved; students who score slightly below the CPT cutoff can enroll in a lower-level course (intermediate algebra) that also grants college-level credits.

³⁰ McCrary's estimates of the discontinuity (log difference in height) around the cutoff for the GPA and placebo samples are 0.28 ($SD = 0.03$) and 0.23 ($SD = 0.02$), respectively. This similarity alleviates concerns about teachers grading more generously to make students on the margin eligible for DE.

³¹ McCrary (2008) suggests an alternative formal test to estimate the discontinuity in the density in RD designs. Using McCrary's Stata code (see <http://www.econ.berkeley.edu/~jmccrary/DCdensity/DCdensity.ado>), I obtained a significant discontinuity at the CPT cutoff (log difference in height = 0.42, $SD = 0.05$). However, as pointed out by Martorell and McFarlin (2010), McCrary's test assumes a continuous and well-behaved distribution along the support of the variable; here, the number of students "piled up" at certain discrete values. If, instead of using McCrary's framework, I test the discontinuity in the empirical density at the cutoff locally, controlling for a linear term in the CPT on either side of the cutoff, the estimate is small and insignificant.

Figure 4

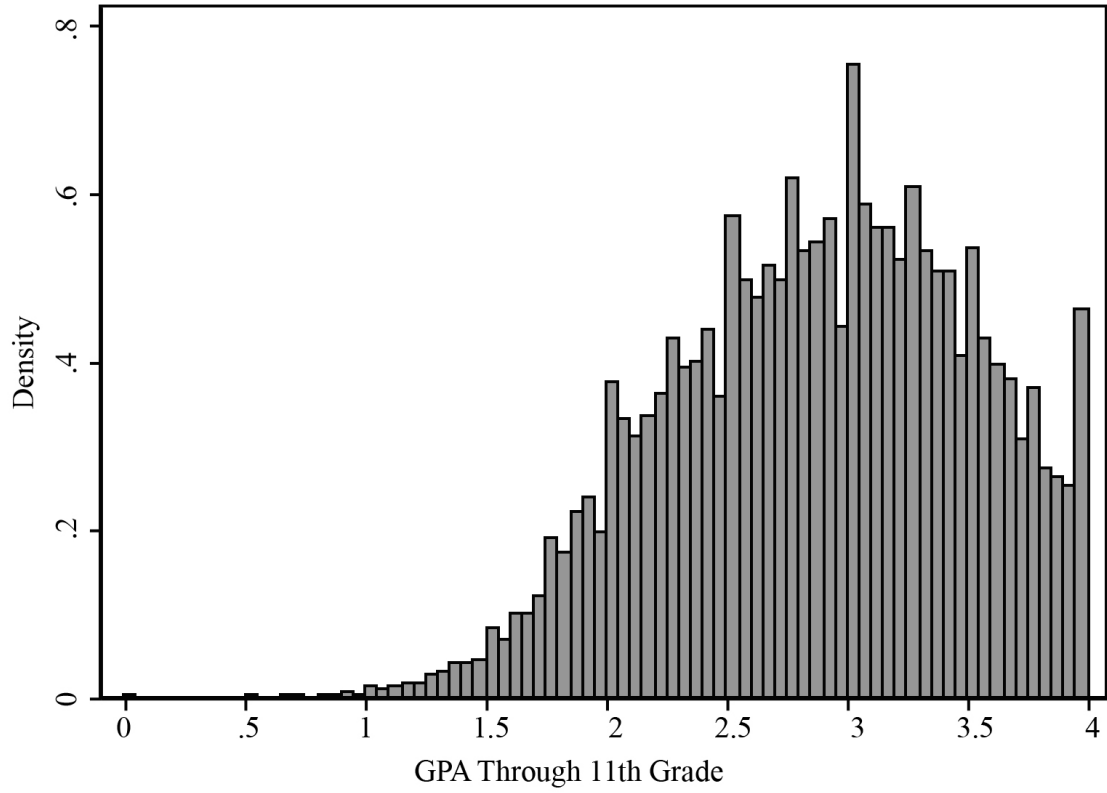
Distribution of 11th Grade High School GPA in GPA Sample



NOTES: Left figure shows the frequency distribution for GPA values close to the eligibility cutoff (zero on the x-axis). GPA is displayed in bins 0.05 points wide. Right figure shows the density on the entire GPA spectrum. Circles are cell means at each GPA value, displayed in bins 0.02 points wide on either side of the cutoff.

Figure 5

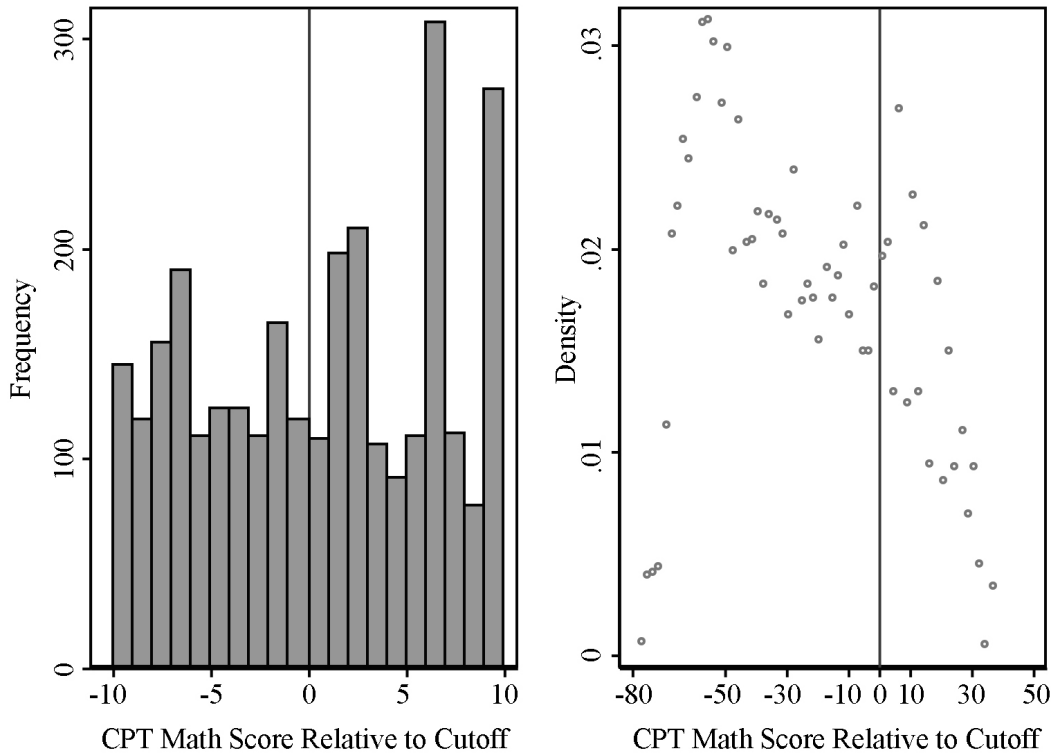
Distribution of 11th Grade High School GPA in Placebo Sample



NOTES: Full (uncentered) GPA histogram is displayed in bins 0.05 points wide. Placebo colleges are those where the GPA cutoff for DE participation was not binding.

Figure 6

Distribution of CPT Math Scores in CPT Sample



NOTES: Left figure shows the frequency distribution for CPT score close to the eligibility cutoff (at zero on the x -axis). Right figure shows the density on the entire distribution of CPT scores. Circles are cell means at the CPT math score (in 2 scale score bins) centered at the eligibility cutoff.

Discontinuity in Other Treatments

In addition to the treatment of interest, there may exist other treatments where participation is determined by the same eligibility cutoff. These other treatments may be simultaneous or sequential. A simultaneous treatment is one that may be taken by those who took the original treatment and that affects the outcome of interest, either because it occurs at the same time as the treatment of interest or because it occurs after the treatment of interest but before the outcome of interest. On the other hand, a sequential treatment is one that occurs after the treatment of interest and is only taken by those who did not participate in the original treatment. While the standard RD design is invalid in the presence of both simultaneous and sequential additional treatments, the sequential RD design is only

invalid in the presence of simultaneous treatments. In this section, I establish the lack of simultaneous or sequential treatments for the GPA sample (standard RD) and the lack of simultaneous treatments and presence of a sequential treatment for the CPT sample (sequential RD).

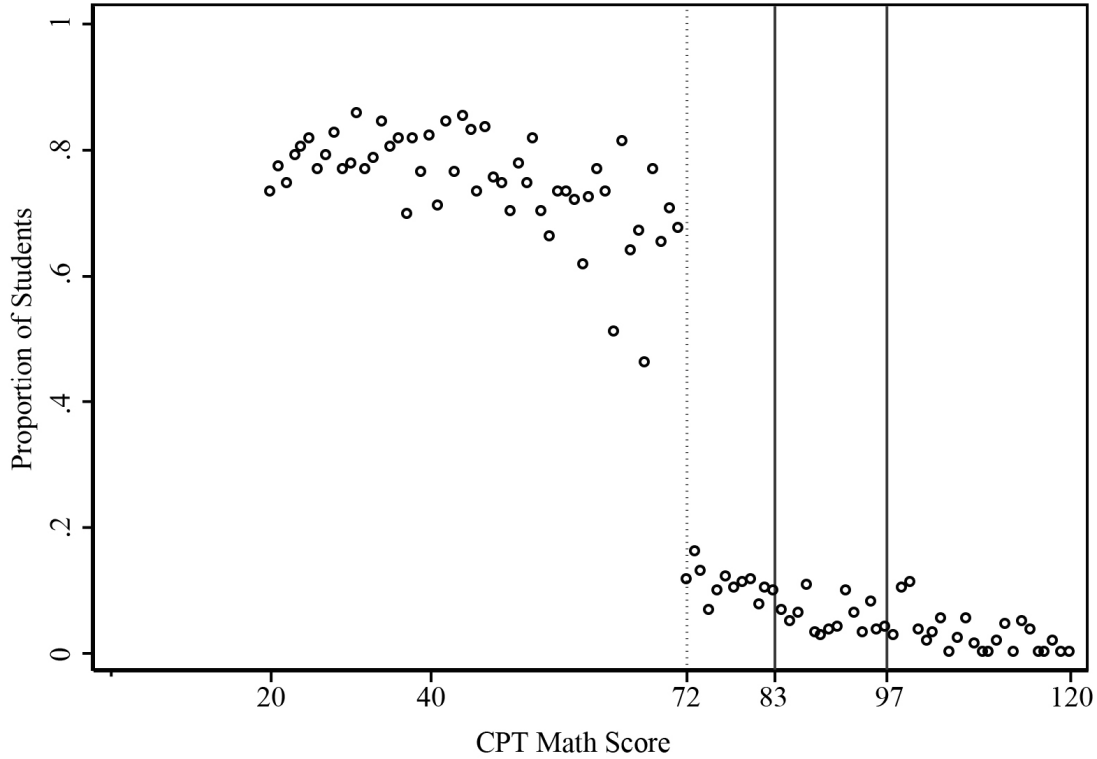
Table 3 shows that there is no evidence that schools in the GPA sample were using the GPA eligibility cutoff for DE for placement into other advanced courses, such as AP, IB, or Honors courses in high school. If anything, there is a small indication that students with an eligible GPA substituted away from other advanced courses in their senior year, though the effect is not significant. Similarly, participation in other advanced courses does not change discontinuously at the CPT math cutoff score.

While a state merit scholarship (the Florida Medallion Scholars Award, which pays 75 percent of college tuition) was awarded to students with a GPA of 3.0 or higher, DE eligibility was determined with a different GPA formula that used selected high school courses and a particular weighting scheme. To the extent that a 3.0 high school GPA was considered a milestone for college enrollment, I cannot rule out the possibility that the 11th grade GPA employed in the analysis might by itself position students just above and just below a 3.0 GPA differently in terms of college admissions. However, this is unlikely because it is common practice among selective colleges to recalculate students' high school GPAs using weights for admissions decisions.

The CPT math cutoff score for DE-algebra did not determine eligibility for other likely treatments, such as college scholarships, college admissions, or college remediation. First, the only state scholarship that required a CPT score was a vocational scholarship (Florida Gold Seal) that had a cutoff score of 72, which is considerably lower than the 83–97 range of scores used for placement into algebra. Second, the CPT score was only used in Florida's community college system for placement purposes — not for admission — because these are open-admission institutions. Four-year colleges, on the other hand, require SAT or ACT scores for admission. Third, unlike the use of cutoff scores in determining eligibility for freshman English composition, the cutoff scores for placement into remedial math and college algebra were different. This is shown in Figure 7, which displays enrollment patterns in remedial math. The state-mandated cutoff score for remediation (indicated by a dotted line) is lower than the ones used for college algebra, which were defined by each college (the range of cutoff scores used is indicated by solid lines).

Figure 7

CPT Math Scores Relative to Placement Cutoffs

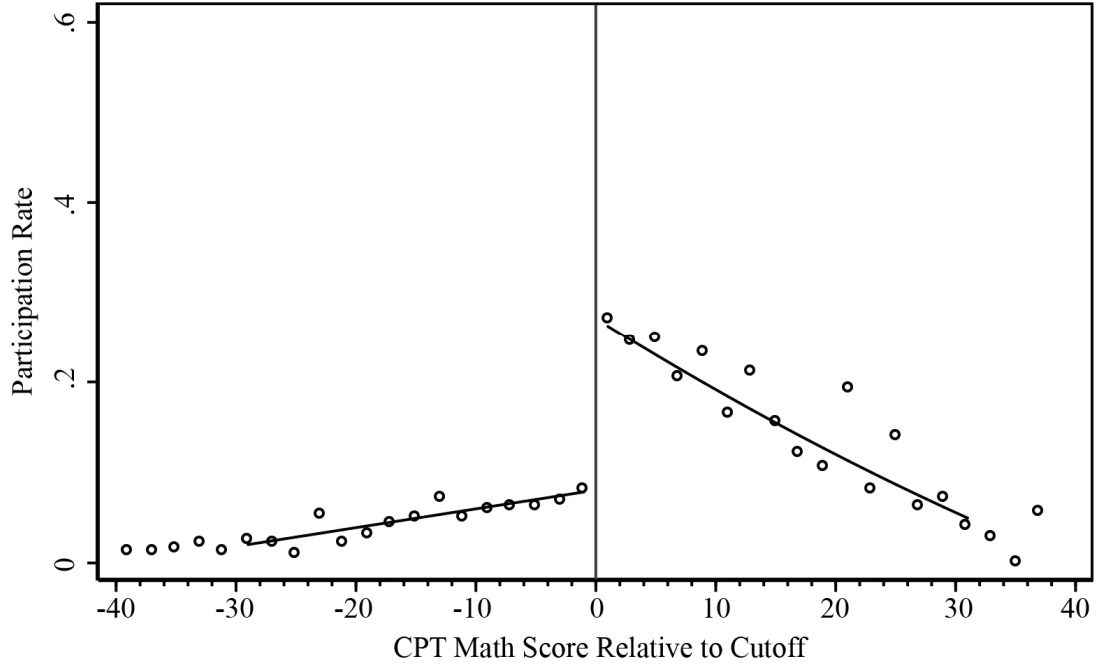


NOTES: Circles are cell means at each CPT score for students selected in the CPT sample. The dotted line indicates the statewide cutoff used for assignment into college remediation. Solid lines demark the range of cutoffs used by different colleges for placement into college algebra.

Finally, the eligibility cutoff in the CPT sample was used not only for DE-algebra but also for college algebra, and there is a jump in participation in college algebra for college students in their first postsecondary enrollment term (Figure 8). This jump in participation in the second treatment motivates the need for the sequential RD design developed in this paper. Finally, the sequential RD framework assumes that participants in the first treatment do not participate in the second treatment. This assumption is approximately valid: only 5 percent of the students who participated in DE-algebra retook algebra during the first term in college. Excluding these students from the analysis does not materially affect the results (see Table 7 for robustness checks).

Figure 8

Participation in Algebra During First Term in College



NOTES: CPT scores are displayed in bins 2 scale score wide on either side of the cutoff. Circles are cell means at each CPT math scale score. The solid lines are fitted values of a quadratic regression estimated within a 30-point bandwidth on either side of the cutoff.

Estimated discontinuity = .19 (.01)

6. Results

Effect of Dual Enrollment on Academic Outcomes

Table 4 presents the RD-IV estimates of the effect of taking a DE-basic course in 12th grade using the GPA cutoff. As mentioned before, this analysis captures mostly the effect of those DE courses for which only an eligible GPA suffices for enrollment. These courses are typically introductory community college courses in subject areas other than math or English.³² Column 1 shows the mean value of the outcome for students just below the cutoff,³³ and column 2 shows the ordinary least squares (OLS) estimates, which ignore students' self-selection into DE. The next columns report the RD-IV effect estimated using alternative bandwidths of the data and control variables (additional sensitivity analyses are discussed in Section 7). The last two columns display the reduced-form effect in the GPA sample and in the rest of the colleges (which comprise the placebo sample) where DE participation does not increase discontinuously at the cutoff. Before introducing the formal estimates, Figure 9 provides a graphical preview of DE impact. To the extent that DE participation benefits students, there should be a discontinuous increase in mean outcomes at the GPA cutoff. Visual inspection of the graphs, however, does not suggest positive effects. Because not every eligible student takes DE and some ineligible students take DE when granted an exception, the RD-IV in Table 4 scales up these differences in outcomes around the cutoff by the probability of participation (first stage shown in row 1).

First, I consider whether DE increased students' likelihood of obtaining a high school diploma. There is no evidence that DE-basic had significant leverage on high school graduation, but there is little variability in this outcome measure, given that the data consists of students who already made it to 12th grade. The next set of outcomes describes students' postsecondary enrollment, including college enrollment (of any type) and college enrollment at four-year institutions (private and public; in-state and out-of-state). The point estimate on the effect of DE-basic on postsecondary enrollment is slightly negative (-0.068)

³² The discontinuity in 12th grade DE math and English courses with respect to GPA is very small: 0.016 ($SD = 0.006$) for DE-algebra and 0.029 ($SD = 0.009$) for DE-English composition, compared with the overall discontinuity in 12th grade participation of 0.081 ($SD = 0.016$) reported in Table 4, column 3.

³³ Mean outcomes below the cutoff represent a weighted average of the outcomes for three types of students: those who would never take DE (never-takers), those who would manage to get an exception to take DE if not eligible (always-takers), and those who would only take DE if eligible (compliers). The RD-IV draws inferences from the last. In addition, the appropriate benchmark reference for the estimates should be based on the sample of students who took the CPT during high school. Because students may take the exam while in college and I do not observe the test date, mean outcomes below the cutoff are likely higher than what the benchmark outcome is. Students who take the exam in college affect the level of the outcome (e.g., higher postsecondary enrollment) but do not affect the discontinuity estimate.

but statistically insignificant, with standard errors of 0.11 (see column 7). Interestingly, the reduced-form effect on college enrollment is remarkably similar to that experienced by students in other placebo colleges where DE participation does not jump discontinuously at the cutoff, further assuring that the program was unlikely to be affecting students' college enrollment decisions (see columns 9 and 10).

Given the relative advantage that local community colleges may have in attracting DE students after high school graduation (for example, DE students are considered returning students and need not reapply to the college or transfer the accrued college credits), the next outcome evaluates whether DE diverted students who would have otherwise gone to four-year institutions into two-year colleges. While there is some indication of a negative effect on enrollment at four-year institutions, the estimate is only statistically significant in the larger bandwidth and becomes insignificant (and smaller in size) using a narrower sample of the data around the cutoff (see columns 5–8), making it difficult to draw conclusions.

There is substantial policy interest in finding effective interventions that not only get students into college but also help them succeed in college. To this end, the final set of outcomes examines measures of students' college completion.³⁴ Relative to students below the cutoff, DE-basic students just above the cutoff were not significantly more likely to obtain an associate degree. The point estimates on baccalaureate attainment roughly mirror the evidence on four-year college enrollment rates. However, the effects are fairly imprecisely estimated across bandwidths and only statistically distinguishable from zero with the inclusion of additional controls in the regression, despite the arguably large sample size. The effect on college degree attainment (either associate or bachelor's) corresponds closely to the basic pattern found in bachelor's degree attainment: large point estimates that are unstable across discontinuity samples and not consistently significantly different than zero make it difficult to draw a definitive conclusion. Overall, point estimates and standard errors rule out that DE-basic had a sizeable beneficial impact on college degree attainment (95 percent confidence interval upper bound is slightly positive or slightly negative) and, if anything, point to a relative decline in the probability of obtaining a degree.

³⁴ College outcomes are defined over the entire population of high school students (i.e., not just college-goers).

Table 4
Regression Discontinuity Estimates of the Effect of
Dual Enrollment (DE-Basic) on Student Outcomes, GPA Sample

	Mean Below Cutoff (<i>SD</i>)	OLS	Discontinuity Samples						GPA Sample	Placebo Sample
	-0.2	± 0.5	± 0.5		± 0.4		± 0.3		± 0.3	± 0.3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable										
Dual enrollment basic [first stage]			0.076 (0.014)**	0.081 (0.016)**	0.075 (0.015)**	0.081 (0.017)**	0.085 (0.012)**	0.090 (0.013)**	0.090 (0.013)**	0.010 (0.013)
			Standard RD-IV						Reduced Form	
High school outcomes										
High school diploma	0.98 (0.14)	0.002 (0.002)	-0.054 (0.069)	-0.048 (0.066)	0.051 (0.085)	0.048 (0.083)	0.012 (0.054)	0.011 (0.053)	0.001 (0.005)	0.001 (0.002)
Postsecondary enrollment	0.91 (0.29)	0.019 (0.006)**	-0.198 (0.147)	-0.189 (0.136)	-0.086 (0.172)	-0.077 (0.160)	-0.060 (0.116)	-0.068 (0.110)	-0.006 (0.010)	-0.005 (0.006)
First enrollment at four- year institution	0.36 (0.48)	0.053 (0.009)**	-0.524 (0.267)**	-0.412 (0.344)	-0.380 (0.277)	-0.311 (0.364)	-0.286 (0.192)	-0.296 (0.267)	-0.027 (0.024)	-0.002 (0.015)
College outcomes										
Associate degree (within 5 years)	0.18 (0.38)	0.077 (0.008)**	-0.202 (0.217)	-0.200 (0.211)	-0.187 (0.239)	-0.159 (0.226)	-0.109 (0.159)	-0.111 (0.155)	-0.010 (0.014)	0.009 (0.009)
Bachelor's degree (within 5 years)	0.15 (0.36)	0.080 (0.008)**	-0.559 (0.228)**	-0.492 (0.271)*	-0.531 (0.241)**	-0.438 (0.285)	-0.338 (0.158)**	-0.334 (0.203)	-0.030 (0.018)*	-0.012 (0.012)
Associate or bachelor's degree (within 5 years)	0.29 (0.45)	0.113 (0.009)**	-0.615 (0.279)**	-0.543 (0.307)*	-0.510 (0.287)*	-0.398 (0.314)	-0.344 (0.179)*	-0.334 (0.210)	-0.030 (0.018)*	-0.003 (0.013)

Table 4 (continued)

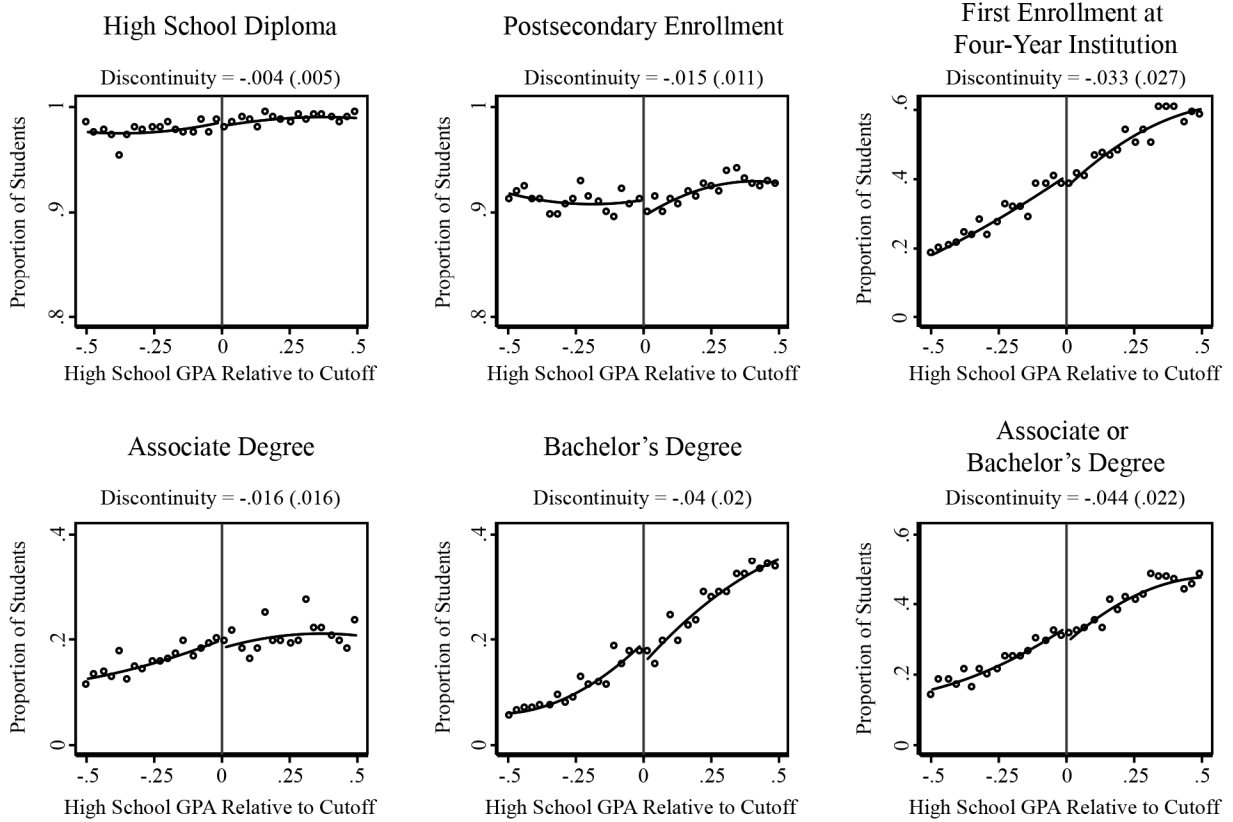
	Mean Below Cutoff (<i>SD</i>)	OLS	Discontinuity Samples						GPA Sample	Placebo Sample
	-0.2	± 0.5	± 0.5		± 0.4		± 0.3		± 0.3	± 0.3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of students	3,619	19,350	19,350	19,350	15,649	15,649	12,141	12,141	12,141	33,587
Additional controls		√	√		√		√			
Polynomial order of GPA (centered at cutoff)		Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Linear	Linear	Linear	Linear
Number of colleges in GPA sample	6	6	6	6	6	6	6	6	6	22

NOTES: Standard errors (in parentheses) are clustered at the 11th grade high school GPA. Sample includes students who took a college placement test in schools assigned to the DE program at the community college selected for the analysis. Mean outcome values are calculated with students whose 11th grade high school GPA is below the cutoff but not more than 0.2 points away from the cutoff. Each cell in the remaining columns represents a separate regression. Dependent variable is defined as taking at least one academic dual enrollment course in any content area. OLS coefficients in column 2 are regression estimates that do not account for selection into participation. The remaining columns are two-stage least-squares estimates controlling for a quadratic or linear specification of the GPA, allowed to vary on either side of the cutoff. Additional controls include gender, race dummies, English language learner status, free or reduced-price lunch status, FCAT 10th grade standardized scores in reading and math, high school-level demographics (race, English language learner status, low socioeconomic status, FCAT 10th grade scores, and total enrollment), districts' median income and urbanicity, and cohort fixed effect.

* $p < .10$. ** $p < .05$.

Figure 9

Student Outcomes by 11th Grade High School GPA



NOTES: GPA is displayed in bins 0.03 points wide on either side of the cutoff. Vertical line indicates the cutoff. Circles are cell means at each GPA value. The solid lines are fitted values of a quadratic regression estimated within a bandwidth of 0.5 GPA points on either side of the cutoff.

Effect of Dual Enrollment Algebra on Academic Outcomes

Dual enrollment, as defined in the previous section, can encompass a wide range of course areas, from life-learning skills to accounting. This section examines the impact of taking one challenging course, college algebra, which requires a particular score on the math CPT for enrollment. The effect of DE-algebra on college degree attainment is measured using the sequential RD estimator, which relies partially on matching techniques. Table 5 assesses the quality of the match by comparing mean characteristics of never-takers above the cutoff and matched-identified never-takers below the cutoff. In nearly all dimensions, the average characteristics of the matched never-takers resemble the distribution of characteristics that never-takers are known to have.³⁵

Table 5
Assessing the Matching Quality, CPT Sample

	Mean Characteristics	
	Matched-Identified Never-Takers $D^H = (0,0)$	Never-Takers $D^H = (0,0)$
Female	0.55	0.55
Black	0.09	0.13
Hispanic	0.02	0.05
English language learner	0.01	0.03
Free or reduced-price lunch	0.20	0.25
FCAT Reading <i>z</i> -score, 10th grade	0.49	0.44
FCAT Math <i>z</i> -score, 10th grade	0.52	0.66
Cohort	0.51	0.51
Rural or town district	0.06	0.05
Number of students	1,451	1,551

NOTES: Means are estimates at the cutoff using a local linear regression specification on the score using (matched) data within 15 CPT math score points around the cutoff. Matched data are nearest-neighbor (with replacement) matches of non-DE students below the cutoff for college algebra and non-DE students above it as described in the text. Never-takers' characteristics in the data are based on all non-DE students above the cutoff.

³⁵ Mean characteristics of never-takers at the cutoff are estimated with a local linear regression specification using data on non-DE (matched) students within 15 CPT math score points of the cutoff.

The only salient difference between never-takers and the matched-identified never-takers is in 10th grade math standardized scores, where the matched-identified sample has an average score about 0.15 standard deviations lower than expected. However, to the extent that this sample is thought to be negatively selected (i.e., to have characteristics associated with poor outcomes), the estimates of the effect of DE-algebra would be biased downwards. While it is only possible to assess whether the matched-based criteria identify a similar group of students in terms of observable characteristics, these statistics suggests that the sample is able to characterize the student type fairly accurately.

Figure 10 displays the mean outcome variables by CPT score (centered at each college cutoff), traced by the fitted values of a regression of the outcome on a quadratic term in the CPT estimated separately on each side of the cutoff. The reduced-form estimates are only indicative of the program effect for pre-college outcomes (shown in the first row of figures). Because students can use the same score for placement into algebra as DE or as a regular course once in college, the reduced-form figures per se are no longer informative of the program impact (see equation 5).

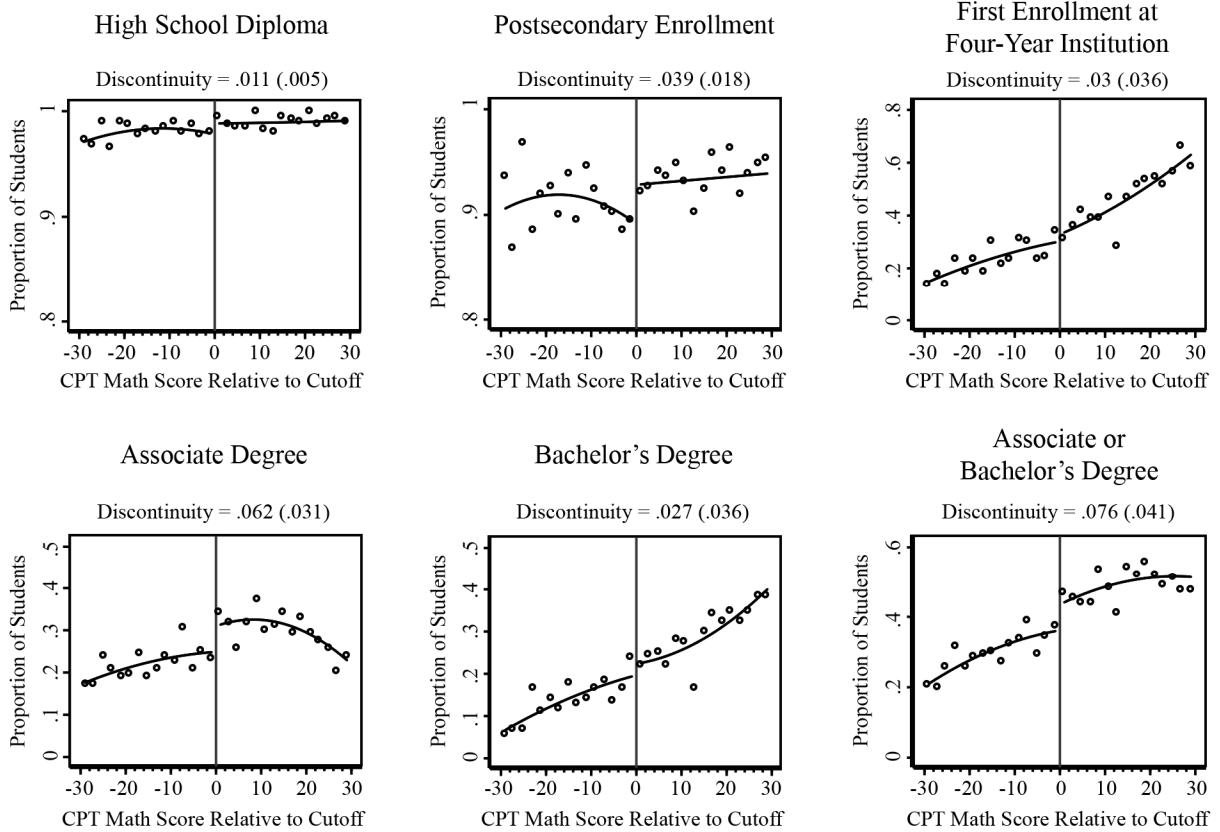
Table 6 presents the standard RD-IV estimates of taking college algebra as DE on high school outcomes (top panel) and sequential RD-matching estimates on college outcomes (bottom panel). For reference, the table also reports the standard RD-IV estimates on college outcomes that ignore the bias due to the sequential treatment.

For simplicity, the table only shows estimates with and without additional controls using one bandwidth of the data, though results across bandwidth samples are not sensitive to the use of additional controls. There is some indication that, for students with a CPT score around the cutoff used for placement, taking DE-algebra increased the likelihood of obtaining a high school diploma. However, the effect is small, ranging from a 4 to 7 percentage point increase depending on the discontinuity sample, and not always statistically significant at conventional levels.

Turning to college enrollment, there is evidence of a beneficial DE-algebra impact. The coefficients are large, statistically significant, and generally robust to discontinuity sample or regression specifications. The most conservative estimate suggests a 16 percentage point increase in postsecondary enrollment (column 4). Although it appears that DE-algebra takers were more likely to go to college after high school, they were not more likely to enroll in a four-year institution. While positive, estimates are fairly unstable across most discontinuity samples, and in all cases the possibility that DE-algebra had no effect on enrollment in a four-year institution cannot be ruled out.

Figure 10

Student Outcomes by CPT Math Score



NOTES: CPT is displayed in bins 2 scale score wide on either side of the cutoff. Vertical line indicates the cutoff. Circles are cell means at each CPT scale score. The solid lines are fitted values of a quadratic regression estimated within a 30-point bandwidth on either side of the cutoff.

Table 6

**Regression Discontinuity Estimates of the Effect of
DE Algebra on Student Outcomes, CPT Sample**

	Mean Below Cutoff (<i>SD</i>)	OLS	Discontinuity Samples				
	-10	± 30	± 30	± 20	± 15	± 10	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable							
Dual enrollment algebra [first stage]			0.185 (0.032)**	0.188 (0.037)**	0.188 (0.029)**	0.175 (0.032)**	0.176 (0.038)**
			Standard RD-IV				
High school outcomes							
High school diploma	0.98 (0.13)	0.008 (0.003)**	0.061 (0.029)**	0.040 (0.022)*	0.045 (0.027)	0.072 (0.030)**	0.071 (0.038)*
Postsecondary enrollment	0.90 (0.3)	0.014 (0.007)*	0.212 (0.090)**	0.158 (0.072)**	0.239 (0.090)**	0.253 (0.085)**	0.230 (0.090)**
First enrollment at four- year institution	0.29 (0.45)	0.112 (0.018)**	0.132 (0.138)	0.103 (0.103)	0.122 (0.129)	0.327 (0.193)	0.296 (0.219)
			Standard RD-IV		Sequential RD-Matching		
College outcomes							
Associate degree (within 5 years)	0.25 (0.43)	0.067 (0.013)**	0.357 (0.166)**	0.194 [0.072]**	0.237 [0.077]**	0.236 [0.098]**	0.223 [0.097]**
Bachelor's degree (within 5 years)	0.18 (0.38)	0.081 (0.015)**	0.144 (0.158)	0.284 [0.064]**	0.238 [0.074]**	0.267 [0.104]**	0.236 [0.101]**
Associate or bachelor's degree (within 5 years)	0.35 (0.48)	0.104 (0.015)**	0.420 (0.212)*	0.337 [0.077]**	0.343 [0.087]**	0.382 [0.104]**	0.348 [0.103]**

Table 6 (continued)

	Mean Below Cutoff (<i>SD</i>)	OLS	Discontinuity Samples				
	-10	± 30	± 30	± 20	± 15	± 10	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of students	1,360	7,921	7,921	5,626	4,374	2,959	2,959
Additional controls		√	√	√	√	√	
Polynomial order of CPT (centered at cutoff)		Quadratic	Quadratic	Linear	Linear	Linear	Linear
Number of colleges in CPT sample	7	7	7	7	7	7	7

NOTES: Standard errors in parentheses are heteroskedastic robust and clustered at the CPT score. Standard errors in brackets are bootstrapped (500 repetitions). Sample includes students with a CPT score in schools assigned to the DE program at the community college selected for the analysis. Mean outcome values are calculated with students whose CPT score is below the cutoff but not more than 10 points away from the cutoff. Each cell in the remaining columns represents a separate regression. Dependent variable is defined as taking DE-algebra. OLS coefficients in column 2 are regression estimates that do not account for selection into participation. Top panel shows two-stage least-squares estimates controlling for a quadratic or linear specification on the CPT score, allowed to vary on either side of the cutoff. Bottom panel shows RD-matching estimates using nearest-neighbor matching with replacement as described in the text. Additional controls include gender, race dummies, English language learner status, free or reduced-price lunch status, FCAT 10th grade standardized scores in reading and math, high school-level demographics (race, English language learner status, low socioeconomic status, FCAT 10th grade scores, and total enrollment), districts' median income and urbanicity, and cohort fixed effect.

* $p < .10$. ** $p < .05$.

The largest effects of DE-algebra are found in college degree attainment, both in the likelihood of obtaining an associate or a bachelor's degree. The effects are substantial; estimates close to the cutoff are 0.23 and 0.24 for associate and bachelor's degrees, respectively (see column 7), and are highly significant despite the arguably small sample size in a narrow bandwidth around the cutoff. To put the results in context, taking college algebra in high school increased associate degree attainment by 23 percentage points from a base of 17 percent just below the cutoff and bachelor's degree attainment by 24 percentage points from a base of 13 percent.³⁶ As expected, DE is also found to have had a positive impact when looking at degree attainment as a general indicator for either an associate or a bachelor's degree. Interestingly, the effect on this measure is generally larger than the individual degree effects, suggesting that some of the effect comes from students who earned one degree but not the other.³⁷

College algebra is a gatekeeper course, and having it completed at the onset of college seems to have helped students make progress toward a degree. One potential explanation for this finding is that students who experience a more rigorous curriculum in high school might be better academically prepared for college and therefore more likely to persist toward a degree. DE students might have a better experience with the course (e.g., have a higher passing rate) in high school than in college as a result of the lower work load in high school or the availability of support (high school counselors are supposed to monitor students' progress in DE courses). A different hypothesis is that students who have already taken college-level algebra start college with higher self-esteem and confidence in their ability to obtain a degree. In addition, the effect of DE-algebra on degree attainment might be due not only to improvements in academic performance but also to an increase in the rate of college attendance.³⁸

³⁶ Mean outcome just below the cutoff is estimated for the DE-algebra compliers subgroup identified using matching. These averages are lower than the corresponding 27 percent (associate degree) and 21 percent (bachelor's degree) for DE-algebra never-takers, consistent with the idea that students induced to take DE have more to gain from the experience.

³⁷ A positive effect for DE-algebra with a close-to-zero reduced-form effect (Figure 10) is explained by a significant negative effect of college algebra for the population of students who are DE never-takers but algebra-in-college compliers (see equation 5). This suggests that, for students on the margin of eligibility who are induced to take the course only in college, taking intermediate algebra instead of enrolling in college algebra provides a more solid foundation for successful completion of the math sequence necessary for a bachelor's degree.

³⁸ Because degrees are measured within a five-year window from expected college start year, it is possible that the relative advantage observed in DE students (particularly in rates of bachelor's degree attainment) would diminish if students were observed for a longer period of time.

7. Robustness

Table 7 examines the sensitivity of the DE-basic effect (Panel A) and the DE-algebra effect (Panel B) to sample selection and model specification. Specifically, I use a narrow discontinuity sample (within a bandwidth of 0.3 GPA points and 10 CPT points around the cutoff) and examine whether these baseline estimates are robust to different samples of students, to the inclusion of more colleges in the sample, and to an alternative estimation of the unobserved term in the sequential RD analysis (for DE-algebra on college degree outcomes). For ease of comparison, the baseline estimates in the main results are restated at the top of each panel.

Robustness for GPA Analysis

While all the colleges in the GPA sample present a statistically significant discontinuity at the official GPA cutoffs, the estimates could be biased if the cutoff is not the one that maximizes the likelihood of participation. This could happen if, for example, some high schools systematically round GPAs between 2.95 to 2.99 up to an eligible 3.0 GPA on students' DE application forms. Following Kane (2003) and Chay, McEwan, and Urquiola (2005), I estimate colleges' empirical cutoffs as those that maximize the log-likelihood function of participation on a dummy indicating eligibility based on alternative cutoffs in increments of 0.05 (i.e., 2.9, 2.95, 3, 3.05, etc.), controlling for a quadratic function of the GPA using a probit model on data 0.5 points around the given cutoff (empirical cutoffs reported in Table A.1 in appendix). I find supporting evidence that, in fact, for two colleges in the GPA sample the empirical cutoff is 2.95 instead of 3. In order to address the concern of cutoff mis-specification, Table 7 (second row) reports estimates discarding students whose GPA fall in the marginal area $[0.05, 0)$. While the point estimates are generally smaller in magnitude than the main results, they have comparable statistical power, and none of the effects are statistically significant.

The next five rows in Panel A show estimates using different samples of students. First, I exclude students with a GPA at the exact value of the cutoff. Because there is a disproportionate number of students with such a GPA, this analysis addresses concerns about endogenous sorting of students around the eligibility cutoff. Results are very similar to the main estimates, providing further evidence that the "stacking" of students at certain GPA values responds to the nature of GPA determination based on a letter grade system. Second, I exclude students who took DE before 12th grade.

Table 7

Sensitivity of Dual Enrollment Effect to Sample Selection and Model Specification

	High School Diploma	Postsecondary Enrollment	First Enrollment at Four-Year Institution	Associate Degree	Bachelor's Degree	College Degree
Panel A: Effect of dual enrollment (DE-basic), GPA sample						
Baseline (Table 4, column 8)	0.011 (0.053)	-0.068 (0.110)	-0.296 (0.267)	-0.111 (0.155)	-0.334 (0.203)	-0.334 (0.210)
Excluding students with GPA in marginal area [0.05, 0)	0.010 (0.044)	-0.053 (0.091)	0.042 (0.240)	-0.037 (0.130)	-0.080 (0.180)	-0.130 (0.166)
Excluding students with a GPA at the exact cutoff	0.042 (0.059)	-0.011 (0.124)	-0.324 (0.331)	-0.141 (0.190)	-0.396 (0.253)	-0.391 (0.255)
Excluding students who took DE before 12th grade	-0.001 (0.062)	-0.069 (0.132)	-0.391 (0.322)	-0.124 (0.185)	-0.372 (0.243)	-0.379 (0.257)
Excluding students who took DE-algebra or DE-English composition	0.008 (0.058)	-0.085 (0.118)	-0.309 (0.286)	-0.123 (0.163)	-0.406 (0.229)*	-0.415 (0.233)*
Excluding students who took DE-algebra	0.013 (0.066)	-0.096 (0.135)	-0.422 (0.327)	-0.143 (0.186)	-0.489 (0.257)*	-0.512 (0.263)*
Including students with no placement score (unconditional analysis)	-0.093 (0.128)	-0.337 (0.351)	-0.353 (0.352)	-0.213 (0.169)	-0.359 (0.242)	-0.451 (0.269)*
Excluding colleges where more than 10% of students below GPA cutoff take DE (4 colleges), first stage = 0.082 (0.015)	0.007 (0.068)	-0.094 (0.139)	-0.434 (0.366)	-0.137 (0.178)	-0.466 (0.293)	-0.463 (0.289)
Including colleges with significant discontinuities in DE participation in at least one model (adds 2 colleges), first stage = 0.082 (0.011)	0.002 (0.052)	-0.035 (0.108)	-0.278 (0.187)	-0.188 (0.166)	-0.253 (0.164)	-0.367 (0.191)*

Table 7 (continued)

	High School Diploma	Postsecondary Enrollment	First Enrollment at Four-Year Institution	Associate Degree	Bachelor's Degree	College Degree
Panel B: Effect of dual enrollment algebra, CPT sample						
Baseline (Table 6, column 7)	0.071 (0.038)*	0.230 (0.090)**	0.296 (0.219)	0.223 [0.097]**	0.236 [0.101]**	0.348 [0.103]
Students with GPA above cutoff	0.081 (0.041)*	0.248 (0.125)*	0.145 (0.239)	0.219 [0.117]*	0.183 [0.104]*	0.305 [0.115]
Excluding algebra repeaters (i.e., students who took both DE-algebra and college algebra)	0.074 (0.039)*	0.236 (0.095)**	0.339 (0.229)	0.206 [0.114]*	0.230 [0.089]**	0.337 [0.109]
Estimating matching term based on matched data using alternative bandwidth (20 points instead of 15)				0.256 [0.104]**	0.240 [0.089]**	0.341 [0.107]
Estimating matching term based on matched data using alternative bandwidth (10 points instead of 15)				0.257 [0.105]**	0.228 [0.092]**	0.313 [0.11]
Estimating matching term by identifying never-takers using 3/4 of their known share in the data (instead of actual share)				0.261 [0.117]**	0.254 [0.106]**	0.371 [0.136]
Estimating matching term including high school course-taking patterns in the matching process				0.208 [0.106]**	0.221 [0.086]**	0.308 [0.109]
Using mean outcomes below cutoff on all non-DE students instead of on the matched-identified sample (i.e., assuming $D^H = (0, 0)$ students are similar to $D^H = (0, 1)$ students)				0.151 [0.099]	0.186 [0.084]**	0.242 [0.103]
Including colleges with significant discontinuities in DE-algebra and those using cutoff of 72 (adds 2 colleges), first stage = 0.17 (0.03)	0.110 (0.039)**	0.261 (0.092)**	0.202 (0.221)	0.254 [0.102]**	0.248 [0.097]**	0.369 [0.111]
Including colleges with significant discontinuities in DE-algebra in at least one model (adds 5 colleges), first stage = 0.16 (0.026)	0.020 (0.030)	0.076 (0.067)	0.322 (0.210)	0.266 [0.076]**	0.200 [0.07]**	0.322 [0.085]

NOTES: Standard errors in parentheses are heteroskedastic robust and clustered at the score. Standard errors in brackets are bootstrapped (100 repetitions). Panel A uses data within 0.3 GPA points around the cutoff and Panel B uses data within 10 points around CPT math cutoff. The number of observations varies depending on the sample restriction. Estimates are based on a local linear regression discontinuity specification allowed to vary on either side of the cutoff, not including additional controls. Standard RD estimation is used in Panel A and the first three columns in Panel B. Sequential RD estimation is used for college degree outcomes in Panel B.

* $p < .10\%$. ** $p < .05$.

To the extent that there is variation in the GPA around the cutoff over time, 11th grade participation should not be correlated with 12th grade eligibility status. Indeed, results eliminating students with early exposure to DE are similar to the baseline, though reducing the sample size by 8 percent results in a loss of precision.

The previous section suggests that the returns to DE are likely heterogeneous in course type, though the GPA and CPT analyses inevitably draw inferences from different samples of students. Despite the fact that most of the variation from DE participation in the GPA sample is driven by courses for which there are no specific test requirements, as long as colleges enforce both GPA and test requirements there is still some variation driven by courses known to be gatekeepers. I therefore assess the sensitivity of the results to the exclusion of students in the GPA sample who took either DE-English composition or DE-algebra and those who took DE-algebra. Consistent with the larger returns resulting from rigorous course experiences, the estimates excluding these challenging courses are considerably lower (i.e., more negative) than baseline results, particularly for rates of four-year college enrollment and bachelor's degree attainment.

Next, I examine whether the results are robust to the inclusion of students with no college placement score. Baseline estimates are indicative of the effect of DE at the margin of the GPA cutoff among those students who took the test necessary for application. Students interested in DE might have been discouraged from taking the college placement test altogether because, for example, they did not meet other participation requirements (minimum GPA, letter of recommendation, etc). Unconditional analysis suggests lower DE returns for these types of students, though estimates are highly imprecise despite a 40 percent increase in the sample size.

Last, I explore whether the results are robust to the selection of colleges in the sample. Because students who would be induced to participate in a high-exception program might be different than those at a low-exception college, I remove from the analysis DE programs where more than 10 percent of the DE students had an ineligible GPA. While point estimates are lower than baseline, suggesting that high-exception programs make exceptions to students with positive unobservable characteristics, all coefficients are not significant at conventional levels. Finally, I include in the sample other DE programs where there is some indication of a discontinuity in participation, though the discontinuity is not robust across model specification. Estimates remain largely unchanged using this larger sample.

Robustness for CPT Analysis

Panel B shows the robustness checks for the CPT sample analysis. Given that students are required to have an eligible GPA in addition to a passing CPT score in order to take DE-algebra, the first sensitivity test estimates the effect at the CPT cutoff for those with an eligible GPA. The results are not materially affected. This outcome was expected given that the GPA requirement was not binding for participation in some of these colleges and that students at the CPT cutoff had on average an eligible GPA (the mean GPA at the CPT math cutoffs is 3.2).

An assumption of the sequential RD framework is that students can only take algebra once (either in high school or in college). In practice, however, about 5 percent of the students who took DE-algebra retook the course in college, typically because they failed the course the first time. Estimates in Table 7 show that excluding algebra repeaters from the analysis does not change the results.

The next four rows in Panel B assess the robustness of alternative estimates of the last term in the sequential RD estimator, which is not readily available in the data. I first estimate the term based on matching students within wider and narrower bandwidths of the data around the cutoff than that used for the main analysis (20 and 10 points instead of 15). I then examine whether using a stringent criteria to identify DE-algebra never-takers below the cutoff would affect the results. Specifically, I use 75 percent of the known share of DE-algebra never-takers instead of the actual share to get a sample that most closely matches the students above the cutoff. Last, I use an alternative specification of the covariates used in the matching that includes students' course-taking choices in high school (including number of math, English, science, and history courses taken and an indicator for taking math courses in the senior year). None of these exercises materially affect the results.

In addition, Table 7 also shows the estimates using all non-DE-algebra students below the cutoff instead of the matched-identified sample. While this analysis does not rely on matching, it is based on a likely strong assumption: Students who would never have taken DE-algebra are in fact similar to those who would have taken it if eligible. The effect of DE-algebra on college degree attainment is still large and significant, albeit smaller in size. Point estimates are 15 percentage points for associate degree attainment and 18 for bachelor's degree attainment, compared with 23 percentage points for the baseline estimate. In the case of associate degree attainment, the estimate is significant using larger bandwidths of the data (not shown) but trends toward significant in the reported bandwidth.

The following robustness checks include a larger sample of colleges by relaxing the criteria used for selecting colleges for the main analysis. First, I include two additional colleges in the sample that have a sizeable discontinuity in DE-algebra participation but

used the same cutoff to determine enrollment in algebra and math remediation. Despite the fact that ineligible students faced different course-taking options in these colleges than in all others in the sample, results that include these colleges in the analysis remain mostly unchanged. The last row reports the estimates using a larger sample of colleges with significant discontinuities in DE-algebra, though not all are robust across model specification. Overall, this analysis includes a total of 12 DE programs (with an addition of five colleges and almost two times the number of students), with a precise first stage of 16 percentage points. Notably, the effects on the likelihood of obtaining a high school diploma and on postsecondary enrollment rates are smaller in magnitude than the baseline and no longer statistically significant; indicating some heterogeneity in the effect on these outcomes across programs. The main conclusion on the effect of DE-algebra on degree attainment still holds in this broader set of colleges. The consistency and stability of the positive and significant estimates on college degree outcomes across samples and model specifications is reassuring.

8. Conclusion

In the presence of discouraging statistics on postsecondary enrollment and attainment, there is a growing need to find effective ways to help high school students in their transition to higher education. There is a growing body of literature providing reliable evidence that a rigorous high school curriculum, particularly one rich in math, is a key determinant of students' educational progress and earnings (e.g., J. Goodman, 2011; Rose & Betts, 2004). To this end, policymakers are increasingly viewing dual enrollment programs, which allow students to take college courses while in high school, as an appropriate intervention. This enthusiasm about dual enrollment programs has been accompanied by remarkable growth in state legislation that governs their structure and funding. While 23 states had dual enrollment legislation in 2000 (Frazier, 2000), 42 states had passed legislation related to dual enrollment by the end of 2005 (WICHE, 2006). As dual enrollment programs continue to grow in popularity, it is important to understand their impact on students' academic progress.

This paper provides empirical evidence of the effect of dual enrollment using data from Florida, a state with a well-developed, highly regulated, and fully funded dual enrollment program. Florida provides a unique opportunity to assess the effect of dual enrollment participation because participation requirements are set forth by the state. In particular, students are required to have a minimum 3.0 high school GPA in order to take an academic dual enrollment course and, for courses such as college algebra, must demonstrate a minimum proficiency on a college placement test (CPT). These features of the program allow the use of two regression-discontinuity designs to gauge the causal effect of the program by comparing the outcomes of students who barely pass with those of students who barely miss the required GPA or CPT cutoff (for algebra). The analysis of dual enrollment algebra is complicated by the fact that an eligible student who does not take the course while in high school has the opportunity to take the course later, while in college. To address this, I employ a new RD estimator, the sequential RD-matching estimator, which extends the standard RD design to accommodate a subsequent treatment with the same eligibility requirement.

Using data from the 2000–01 and 2001–02 high school graduating cohorts in selected Florida counties, I find no evidence that simply taking dual enrollment significantly increased students' likelihood of high school graduation, college enrollment, or college completion for students who were on the margin of Florida's minimum GPA requirement. While the estimates are generally negative, large standard errors imply that we cannot rule out sizeable effects in either direction. However, dual enrollment participation conceals important variation in course experience. Based on a sample of students who took

Florida's college placement test, I find that taking one popular, challenging course, college algebra, through a dual enrollment program significantly increased students' likelihood of enrolling in college by about 16 percentage points and of obtaining a college degree by about 23 percentage points, with some indication of positive effects on high school graduation.

This research presents the first attempt to use a quasi-experimental method to examine the causal effect of participation in an academic DE program. From a policy perspective, it provides credible evidence that dual enrollment programs can play a significant role in improving students' college access and success. It also highlights that factors such as the subject area, quality, or level of difficulty of the dual enrollment experience should be taken into account when expanding these programs with the objective of addressing the needs of high school students as they transition to postsecondary education.

Appendix A: Supplementary Tables

Table A.1

Estimated Discontinuity in Dual Enrollment (Any Course) in 12th Grade by Community College

	Official GPA Cutoff	GPA Type	Estimated Empirical Cutoff	Discontinuity Sample		
				± 0.5	± 0.4	± 0.3
Community College 1	3.0	Unweighted	3.05	0.003 (0.034)	0.014 (0.039)	0.014 (0.030)
Community College 2	3.0	Unweighted	3.05	-0.023 (0.040)	-0.007 (0.045)	-0.019 (0.036)
Community College 3	3.0	Unweighted	3.05	-0.048 (0.116)	-0.050 (0.131)	-0.042 (0.101)
Community College 4	3.0	Either	2.90	-0.015 (0.033)	-0.031 (0.037)	-0.010 (0.029)
Community College 5	2.5	Unweighted	2.40	-0.004 (0.033)	0.018 (0.038)	-0.006 (0.029)
Community College 6	3.0	Unweighted	2.90	-0.027 (0.044)	-0.012 (0.055)	0.007 (0.038)
Community College 7	3.0	Unweighted	2.95	-0.012 (0.081)	-0.040 (0.095)	0.024 (0.070)
Community College 8	3.0	Unweighted	2.95	0.062 (0.013)***	0.058 (0.015)***	0.072 (0.012)***
Community College 9	3.0	Unweighted	3.00	0.012 (0.016)	0.013 (0.018)	0.011 (0.014)
Community College 10	3.0	Unweighted	3.05	-0.023 (0.135)	-0.067 (0.153)	-0.002 (0.119)
Community College 11	2.75	Weighted	2.85	0.054 (0.033)	0.051 (0.038)	0.052 (0.029)*
Community College 12	3.0	Unweighted	3.00	0.031 (0.063)	0.043 (0.071)	0.054 (0.054)
Community College 13	3.0	Unweighted	3.00	0.112 (0.067)*	0.051 (0.072)	0.081 (0.058)
Community College 14	2.5	Unweighted	2.50	0.086 (0.054)	0.104 (0.062)*	0.080 (0.046)*
Community College 15	3.0 ^a	Unweighted	3.05	-0.063 (0.045)	-0.082 (0.050)	-0.048 (0.039)
Community College 17	3.0	Weighted	2.90	0.054 (0.039)	0.074 (0.040)*	0.069 (0.032)**
Community College 18	3.5	Unweighted	3.40	-0.038 (0.053)	-0.027 (0.058)	-0.023 (0.046)

Table A.1 (continued)

	Official GPA Cutoff	GPA Type	Estimated Empirical Cutoff	Discontinuity Sample		
				± 0.5	± 0.4	± 0.3
Community College 20	3.0	Unweighted	2.95	0.118 (0.060)*	0.082 (0.070)	0.130 (0.055)**
Community College 21	3.0 ^a	Unweighted	3.10	0.120 (0.077)	0.149 (0.085)*	0.098 (0.067)
Community College 22	3.0	Unweighted	2.90	-0.041 (0.095)	-0.013 (0.106)	0.014 (0.084)
Community College 23	3.0	Unweighted	2.85	-0.025 (0.027)	-0.034 (0.030)	-0.012 (0.024)
Community College 24	3.0	Unweighted	3.10	0.056 (0.059)	0.000 (0.067)	0.032 (0.051)
Community College 25	3.0	Unweighted	3.00	0.095 (0.056)*	0.116 (0.064)*	0.133 (0.053)**
Community College 26	3.0	Unweighted	2.90	-0.113 (0.060)*	-0.150 (0.067)**	-0.069 (0.052)
Community College 27	3.0 ^a	Unweighted	3.00	0.115 (0.043)***	0.103 (0.048)**	0.097 (0.038)**
Community College 28	3.0	Unweighted	3.05	-0.020 (0.044)	-0.032 (0.051)	0.009 (0.041)
GPA (11th grade) polynomial controls				Quadratic	Quadratic	Linear

NOTES: Robust standard errors in parentheses. GPA cutoffs are for academic (i.e., non-vocational) courses and were obtained through author’s compilation of inter-institutional articulation agreements, college catalogs, or personal communication with dual enrollment coordinators. Table displays estimated discontinuities in participation based on students who took a placement test (CPT, SAT, or ACT), controlling for a polynomial function on the cumulative GPA through 11th grade allowed to vary on each side of the cutoff. For colleges using weights, the GPA was calculated using one additional grade point for Advanced Placement, International Baccalaureate, DE, and Honors courses — the most frequently used weighting scheme (Florida Board of Education, 2003). Two colleges (College 16 and 19) are omitted due to small sample size. Estimated empirical cutoffs are those that maximize the log-likelihood function of participation on a dummy indicating eligibility based on alternative cutoffs in increments of 0.05 (i.e., 2.9, 2.95, 3, 3.05, etc.) controlling for a quadratic function of the GPA using a probit model on data 0.5 points around the given cutoff. Colleges were randomly assigned an identifying number.

^aState minimum requirement assumed because cutoff could not be obtained or is not explicitly mentioned in official documents.

* $p < .10$. ** $p < .05$. *** $p < .01$.

Table A.2
Estimated Discontinuity in Participation in
Dual Enrollment Algebra by College

	CPT Elementary Algebra Cutoff	Discontinuity Sample		
		± 40	± 20	± 10
Community College 1	72	-0.027 (0.030)	0.002 (0.029)	-0.045 (0.042)
Community College 2	83	0.123 (0.052)**	0.176 (0.046)***	0.076 (0.067)
Community College 3	88	-0.006 (0.040)	0.017 (0.044)	-0.093 (0.051)*
Community College 4	72	-0.006 (0.014)	-0.013 (0.013)	-0.028 (0.011)**
Community College 5	90	0.140 (0.072)*	0.178 (0.062)***	0.165 (0.081)*
Community College 6	83	-0.026 (0.053)	-0.015 (0.047)	-0.054 (0.058)
Community College 7	97	0.450 (0.149)***	0.423 (0.137)***	0.459 (0.162)**
Community College 8	83	0.012 (0.023)	0.020 (0.021)	0.012 (0.031)
Community College 9	90 ^a	-0.021 (0.016)	-0.017 (0.014)	-0.034 (0.020)
Community College 10	85	0.174 (0.123)	0.225 (0.128)*	0.089 (0.143)
Community College 11	72	0.047 (0.033)	0.048 (0.033)	0.072 (0.034)**
Community College 12	95	0.047 (0.040)	0.078 (0.038)**	0.089 (0.053)
Community College 13	83	-0.105 (0.100)	-0.041 (0.102)	-0.145 (0.088)
Community College 14	88	0.050 (0.020)**	0.041 (0.023)*	0.044 (0.019)**
Community College 15	83	0.024 (0.022)	0.004 (0.018)	0.011 (0.014)
Community College 17	90	0.075 (0.044)*	0.089 (0.041)**	0.063 (0.046)
Community College 18	72	0.061 (0.047)	0.076 (0.049)	-0.005 (0.024)
Community College 20	90	0.094 (0.079)	0.145 (0.071)**	0.010 (0.091)

Table A.2 (continued)

	CPT Elementary Algebra Cutoff	Discontinuity Sample		
		± 40	± 20	± 10
Community College 21	88	0.392 (0.078)***	0.442 (0.079)***	0.367 (0.087)***
Community College 22	72	0.186 (0.081)**	0.193 (0.080)**	0.155 (0.099)
Community College 23	95	0.001 (0.073)	-0.009 (0.067)	0.126 (0.046)**
Community College 24	72	0.101 (0.046)**	0.091 (0.039)**	0.074 (0.027)**
Community College 25	85	0.036 (0.113)	0.064 (0.095)	0.070 (0.146)
Community College 26	95	0.225 (0.114)*	0.239 (0.082)***	0.255 (0.130)*
Community College 27	72	0.028 (0.051)	0.051 (0.044)	0.018 (0.067)
Community College 28	72	0.046 (0.073)	0.053 (0.066)	0.101 (0.076)
CPT math polynomial controls		Quadratic	Linear	Linear

NOTES: Robust standard errors in parentheses. Two colleges (College 16 and 19) are omitted due to small sample sizes (less than 100 students). Table displays estimated discontinuities in participation, controlling for a polynomial function on the CPT math score allowed to vary on either side of the cutoff. Cutoffs come from author's compilation of college catalogs (years 2000 to 2003) and from state documentation (Florida Department of Education, Articulation Coordinating Committee, 2006). Colleges with cutoff of 72 either have an additional requirement (e.g., College 22 requires 2 years of high school algebra) or rely exclusively on the score on another test (College Level Math [CLM] portion of the CPT) for placement into college algebra. Since CLM scores are not available in the data, and students can only take the CLM exam if they previously scored 72 on the elementary algebra CPT, the cutoff of 72 was used for estimation. These colleges were nevertheless excluded from the analysis, since a cutoff of 72 is also used to assign students to college remediation. Colleges were randomly assigned an identifying number.

^aCutoff changed from 81 to 90 for tests taken after the summer of 2000. In the absence of test dates, students in the senior high school cohorts examined (2000–01 and 2001–02) were assumed to be subject to the higher cutoff.

* $p < .10$. ** $p < .05$. *** $p < .01$.

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