Can Light-Touch Interventions in High School Impact Education Outcomes?

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Abstract

In response to growing concerns over the academic preparation of college-going students, policy makers have suggested increasing the rigor of high school classes. However, there are concerns over whether differential expectations may exacerbate existing inequities in participation. By evaluating the Academic Acceleration Program (AAP), this paper examines whether switching the default of advanced coursework enrollment encourages high school students to take dual-credit courses. I estimate the impact of qualifying for AAP using a fuzzy regression discontinuity design to evaluate subsequent education outcomes, such as on-time graduation, final high school grade point average, matriculation into any public college, remedial coursework, and the number of credits attempted/earned in the first year of college. I find that students just qualifying for AAP based on their English Language Arts (ELA) test scores increase their likelihood of taking a relevant dual-credit course by 8 percentage points. The first-stage results are stronger for boys, ever FRPL and White students. However, qualification for AAP does not significantly alter education outcomes. As policymakers continue to discuss the expansion of these programs, it's important to understand whether and for which groups of students these classes are beneficial.

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I Introduction

The lack of academic preparation of college-going students is an ongoing concern. During the 2019-2020 academic year, 31.4% of first-year undergraduates reported taking a remedial course upon entering post-secondary education (National Center for Education Statistics, 2023). One solution proposed by policy makers to address this concern is to raise the rigor of high school coursework through greater participation in dual credit classes.[1] However, there are significant gaps in the participation rates of students in dual credit classes across both racial and income groups (Dalton et al., 2016). These differences are due, in part, to differential expectations about the likelihood of succeeding in these classes (Giani et al., 2023). One way to change expectations, as shown extensively in the behavioral economics literature, is to change the default option (Madrian, 2014). Since 2012, school districts across Washington state have adopted a program, Academic Acceleration (AAP), which switches participation in advanced coursework to the default based on students' test scores. The Academic Acceleration Program, therefore, provides a context to answer the question: does changing expectations about academic potential impact educational outcomes?

In this paper, I address this question empirically by evaluating the educational impacts of the Academic Acceleration Program (AAP). Students who qualify for AAP are automatically enrolled in relevant advanced coursework with the intention that the student would take a dual credit course by the time they finished high school. Since admission to the program is based on a student's test score, I estimate the effect of the program using a fuzzy regression discontinuity comparing those who score just above and just below the admissions threshold. The timing of available data makes it possible to evaluate both impacts of the program on high school outcomes including on-time graduation and final high school grade point average and college outcomes including matriculation into any public college, remedial coursework, and the number of credits attempted/earned in the first year of college.

I find that students just qualifying for AAP based off their English Language Arts (ELA) test score are 8 percentage points (p.p.) more likely to ever take a relevant dual credit course. Off a base mean of 49 percent, this result suggests that AAP increased participation in dual credit classes by 16.3 percent. Interestingly, the increase in dual credit course taking is driven by boys. Boys who score just above the ELA threshold for AAP are 11 p.p. more likely to take a relevant dual credit course compared to a statistically insignificant increase of 4 p.p. for just qualifying girls. The baseline dual credit participation rate for boys is nearly 12 p.p. lower than that of girls, which suggests that AAP may be serving as a catch-up mechanism for boys. Additional heterogeneity results suggest

[1] dual credit courses allow students to earn college credits while still in high school without requiring extra instructional time outside the classroom.

that the first-stage results of AAP are stronger for students ever qualifying for free/reduced-price lunch (FRPL) and students identifying as White when compared to students that never qualify for FRPL and students that identify as non-White, respectively. I also find that students just qualifying for AAP based off their Math scores are no more likely to take a dual credit Math course than those in the control group. One potential reason AAP was unable to induce students to take Math dual credit courses has to do with the graduation requirements of Washington state. Students are only required to take 3 years of math to graduate from high school and since many students take the math exam in 11^{th} grade, AAP would have to induce students to take an extra year of math in order to see any possible effects. I do not find any evidence that qualification for AAP induces students to take a fourth year of math.

I show that first-stage results using the ELA threshold are robust to model specification choices and the adoption of other policy interventions that could threaten the validity of my findings. First, I re-do the analysis with several placebo cutoffs to ensure that the first-stage results are only found at the actual threshold for program participation. I re-estimate the first-stage assuming the cutoffs are 0.5-0.7 standard deviations below (above) the actual threshold and find that the estimates are significantly smaller (or in the wrong direction) and statistically indistinguishable from zero, supporting the claim that it is the threshold for participating in AAP that influences whether students take relevant dual credit courses. Second, I show that the magnitude and significance of my first-stage results are robust to bandwidth and kernel choices, as well as the elimination of the observations closest to the cutoff in a "donut hole" approach (Cattaneo et al., 2019). Third, I implement a falsification test to ensure that the results are due to qualification of AAP rather than some other policy change at the cutoff. Specifically, I estimate the first-stage on students attending high school in the Seattle Public School District, which had not adopted AAP during my sample time frame. I find no evidence that, absent the policy, students see an increase in their likelihood to take a relevant dual credit class when they cross the cutoff score. This result bolsters the claim that qualification for AAP drives the increases in dual credit participation I find.

I further explore the effects of AAP by examining whether qualification impacts high school and college outcomes. Using the ELA threshold, I find that eligibility for AAP is not associated with improved educational outcomes. Students just above and just below the threshold are just as likely to have an on-time graduation, matriculate into any public college and take remedial coursework upon entering college. Students just below and above the threshold also finish high school with similar grade point averages (GPA). However, students just eligible for AAP saw a statistically significant decrease in the number of credits earned in their first year enrolled in college. The reduced-form result suggests that on average, those just qualifying for AAP, earn 4.67 fewer credits in their first year compared to the control group. This result may be a function of how dual credit courses work. When student take a dual credit class in high school they earn the same credits as if they had taken the class while in college. Together, the reduced-form results suggest that qualifying for AAP does not meaningfully alter education outcomes.

This paper contributes to two distinct literatures. First, it speaks to the growing economic literature that directly examines the impact of access to dual credit courses on educational outcomes. One set of papers examines the impact of the introduction of these courses into schools and finds either null or modest, positive effects on college matriculation and performance measures (Jackson, 2010; Hemelt et al., 2020; Conger et al., 2022). This paper complements this prior work by examining the impacts of dual credit classes on the marginal student induced to participate. Understanding the impacts for this group of students is particularly relevant since many school districts have had established dual credit programs for years. Speroni (2011) also implements a regression discontinuity design to evaluate the effects of dual enrollment courses on students who just pass the cutoff to participate in Florida. The author finds no effect of dual enrollment on high school or college outcomes, except for those students qualifying to participate in college algebra. Compared to the program studied in Speroni (2011), Academic Acceleration is much broader as it allows for the possibility to participate in several types of dual credit classes. The setup of Academic Acceleration better matches what these classes look like in high schools today, so understanding the impacts of this program may be of particular interest to policymakers.

More broadly, this project contributes to the literature on the educational impacts of light-touch college going interventions. Several papers find that reminders and well-framed encouragements through experimental interventions can have positive impacts on the probability students matriculate into college. These interventions are often targeted at high-achieving students (Hoxby and Turner, 2015; Hyman, 2020), those who have already taken steps to apply to college (Smith, 2014; Pallais, 2015; Hurwitz et al., 2017), or those who have already graduated high school (Castleman et al., 2012, 2015; Castleman and Page, 2015). My findings add to this literature by examining whether a light-touch intervention targeted at the average student during their high school career can alter their educational outcomes.

The remainder of this paper is organized as follows. Section II provides background information on the Academic Acceleration Program. Section III summarizes the data used in this paper. Section IV describes the reduced-form empirical strategy and lays out the regression specifications. Section V contains the results of the program, which include the first-stage results, heterogeneity analysis, validity checks, and the reduced-form results. Section VI offers conclusions from this research.

II The Academic Acceleration Program

The Washington Academic Acceleration Program (WAAP) was first implemented in 2012 in Federal Way School District. The goal of the program was to encourage qualified students to participate in available dual credit classes to better prepare them for college. A student's eligibility into the program was determined off their score on the Smarter Balanced Assessment, which was taken in the 10^{th} or 11^{th} grade[2] as a part of the accountability requirements for public schools. For both English Language Arts (ELA) and Mathematics, if the student scored above a certain threshold they were automatically enrolled in relevant advanced coursework with the intent that the student would take a dual credit course by the time they completed high school. If a student was above the threshold on the ELA exam, they were qualified to take advanced coursework in English, Social Science and Humanities. For Mathematics, they qualified to take advanced mathematics courses. The idea was to inform students that they are ready to take on harder classes and have the ability to go to college (Gustainis, 2018).

dual credit courses offer the ability for students to receive college credits while still in high school. Within Washington State, school districts are required to offer at least one of the five types of dual credit courses: Advanced Placement (AP), Cambridge International, College in High School, International Baccalaureate (IB), and Running Start. Additionally, schools can offer a sixth type of dual credit course, Tech Prep. While each specific dual credit course has its own nuances, they all provide the opportunity for students to complete a college course during students' high school careers. These courses are taught during the regular class time and generally substitute for another course in the same subject, for another elective course, or for a free period. While these courses still count towards high school grade point average, they are above and beyond what is required for graduation. Importantly, these courses are often crafted not only to address academic barriers, but to mitigate informational and financial barriers as well.

However, it is unclear whether the expansion of dual credit coursework would improve educational outcomes for all groups of students. Proponents often point to the association of participating in these programs with improved high school graduation rates, college grades, and degree attainment as reasons to encourage students to partake in these classes (Chajewski et al., 2011; An, 2013; Saavedra, 2011). Furthermore, dual credit programs are often praised for their wide availability and flexibility to reflect local educational strengths and labor markets (Karp, 2015). However, there are concerns over the differences in quality across dual credit classes (Karp et al., 2004; Lowe, 2010) and there remains the question of whether widening access to these classes would set up middle- and

[2]During my sample period the grade in which students take the Smarter Balanced Assessment switched from the 10^{th} to 11^{th} grade and back.

low-achieving students for failure, since the typical high school student may not be able to handle college-level work (Bailey and Karp, 2003). This paper will empirically address this question by evaluating the educational impacts of the Acaademic Acceleration program.

From 2012 to 2018, nearly 50 school districts adopted an Academic Acceleration program. In 2019, the Washington State legislature passed House Bill 1599 that required all school districts to implement an Academic Acceleration Program by the 2022-2023 academic year. Figure 1 displays the school districts that had adopted AAP prior to the house bill. The locations of the earliest adopters are spread across the state. There is not a clear urban/rural divide in what areas had the program. In this paper, I focus on 9 school districts adopted AAP before 2016 including Federal Way, Franklin Pierce, South Kitsap, Spokane, Sultan, Tacoma, Tukwila, and Yelm School Districts. I chose these schools districts based on the year they adopted an Academic Acceleration Program and their average high school enrollment to ensure I would have sufficient number of observations for my empirical strategy.

Table 1 reports summary statistics for school districts across the state of Washington, school districts that adopted AAP before the passing of HB 1599 and my sample of school districts, respectively. Each entry in the table presents statistics for students attending the 9^{th}-12^{th} grades in the 2014-2015 academic year. Adopting and sample districts differ from the average school district across the state on several dimensions. Both adopting and sample districts are larger than the average school district in Washington, have higher percentages of low-income students and have slightly higher four-year graduation rates. Sample districts are larger, have slightly lower shares of White students, and have higher shares of low-income students compared to adopting districts. Sample districts also had slightly lower participation in dual credit classes compared to both the state and adopting districts, which is important when considering the external validity of these results. It is possible that a program such as Academic Acceleration is most effective in school districts with relatively lower baseline participation rates.

Academic Acceleration is a unique program because of its target population. Unlike other interventions that often focus on top performing students (Hoxby and Turner, 2015; Hyman, 2020), or those that are already in the process of applying to college (Castleman et al., 2012, 2015; Castleman and Page, 2015), AAP targets students in the middle of the distribution, while they are in the midst of their high school career. Figure 2 presents the distribution of test scores for ELA and Mathematics with their corresponding cutoffs. The cutoff for eligibility into the program is at the 37^{th}-percentile for ELA and at the 61^{st}-percentile for Mathematics. Understanding the impacts of a program targeted at this group of students may be of particular interest to policymakers because these students are much more likely to be at the margin of deciding whether to attend college.

III Data

The data for this project comes from the Office of Financial Management (OFM) in Washington's Education Research and Data Center (ERDC). ERDC provided records of all students that were in the 9th through 12th grades between the 2014-2015 and 2020-2021 academic years.[3] Each of the students included in the sample took the Smarter Balanced Assessment either in the 10th or 11th grade for English Language Arts and Mathematics. I proxy for participation in Academic Acceleration by indicating whether a student enrolled in a relevant dual credit course following the assessment period. This project focuses on 10 school districts within the state of Washington: Federal Way, Franklin Pierce, Seattle, South Kitsap, Spokane, Sultan, Tacoma, Tukwila, and Yelm School Districts. All districts besides Seattle, had adopted an Academic Acceleration by the 2015-2016 AY. Students from Seattle Public Schools will serve as a falsification test against the results.

ERDC provided information on student enrollment, demographics[4], exam scores on the Smarter Balanced Assessment, the courses each student had taken, and subsequent educational outcomes including on-time high school graduation, final high school grade point average (GPA), enrollment in public colleges (2 and 4-year), participation in any remedial course work, and the number of credits attempted/earned in each college term. In order to follow a consistent sample of students throughout the paper, I exclude those students that are ever enrolled in a detention center or "alternative" school, have missing data, have left the school district or are outside of 1 standard deviation of the cutoff threshold. My primary estimating sample will include 16,756 students.

One consideration for this dataset is that there is a slight difference in the number of students used across the first-stage and educational outcomes samples. The differences across these samples stems from the fact that there are students who took the Smarter Balanced Assessment exam, enrolled in subsequent courses, but did not graduate high school by the end of the sample period. While the first-stage results includes this group of students, the outcome sample does not. Appendix Table A1 reports the summary statistics comparing the first-stage and outcomes samples and shows that there are only small differences between the two. Furthermore, Appendix Table A3 reports the results when the first-stage is estimated using only students in the outcomes sample and shows that my findings are robust to this restriction. Students on either side of the threshold have the same probability of appearing in the outcomes sample as shown in Appendix Figure A3.

Table 2 presents the summary statistics of students in different subsets of the sample. In the

[3]Washington State dropped exam scores as a high school graduation requirement during the 2019-2020 and 2020-2021 academic years; however, all students in my sample took the the SBA exams by the 2018-2019 academic year.

[4]Demographic characteristics include race, gender, subsidized lunch status, English language learner status and special education status.

primary estimating sample, as shown in Column 1, around 51 percent of students identify as Male, 47 percent identify as White and 64 percent ever qualify for free/reduced-price lunch FRPL. Compared to the full sample, students that enroll in a dual credit class are slightly less likely to identify as Male, less likely to identify as White and less likely to ever qualify for FRPL. About 67 percent of students in the sample ever take a dual credit class,[5] with the unconditional average of number of dual credit classes equaling 1.9. Conditional on taking one dual credit class, the average student takes around 3 by the time they finish high school. Students near the threshold of AAP qualification (Column 3) are generally quite similar to the full set of students in the sample, but slightly less likely to identify as White and more likely to ever qualify for subsidized lunch.

In terms of outcomes, as shown in Panel C and D of Table 2, students enrolled in a dual credit course outperform those in the full sample. Students that participate in a dual credit class graduate on-time from high school at higher rates (96.6% versus 93.8%) and enroll in public colleges at higher rates (38.9% versus 35.4%). Furthermore, on average, students who take a dual credit class attempt and earn more credits in their first year of college (32.7 & 31.6 versus 26.8 &25.5). While it seems that students that participate in dual credit classes do better on important outcomes, it is unknown whether these differences in outcomes are due to participation in dual credit classes or selection bias. It is possible that students who take dual credit classes would have done just as well in the absence of these classes, perhaps because they are high achieving students or because of family support. This paper determines if any of these positive outcomes associated with dual credit classes can be causally attributed to the program.

IV Empirical Strategy

All high school students in participating school districts have the opportunity to qualify for AAP when they take the Smarter Balanced Assessment in either the 10$^{\text{th}}$ or 11$^{\text{th}}$ grade[6]. The Smarter Balanced Assessment takes the correct answers a student completes and converts it to a scale score between 2000 and 3000. This underlying scale score is then converted to a scale of 1 through 5 which is then reported to teachers and students. Eligibility for AAP participation is determined by whether or not a student surpasses the level 3 cut score, which is set every year by the State Board of Education. The level 3 cut score is always set above the requirement for graduation, but below

[5]This number slightly differs from the statistics reported in Table 1. This difference is most likely due to the restrictions I make on the sample as discussed above.

[6]It is important to note that the SBA is the state-exam required for accountability purposes. All high school students attending a public school in the state of Washington take this exam, not just those students in participating school districts.

that of the most proficient level. To identify the cutoff for each cohort, I take the minimum scale score for all the students identified at the level 3 cut.

It would be misleading to report a raw comparison of students who took a dual credit class to students who did not. Any difference between the two groups may be due to underlying ability rather than a program effect. An OLS regression of student outcomes on observable characteristics would not fully address these concerns. If there are unobserved differences in characteristics between the two groups, such as motivation or family interest, the estimated effect of the program would be biased. To estimate the causal impact of dual credit course taking (and AAP participation) on students' outcomes unconfounded by omitted variable bias, I compare students just above and just below the eligibility thresholds to form regression discontinuity estimates of AAP's effect (Hahn et al., 2001; Lee and Lemieux, 2010). The only difference between students on either side of the threshold is the offer to participate in AAP. The assumption here is that performance on a standardized test is a random draw from a student's underlying ability distribution since students cannot precisely control their score on the test. Comparing students just above and below the threshold for eligibility is analogous to a randomized control trial since students are in as-good-as random order within a small window of points on an exam.

The key assumption behind regression discontinuity designs is that it is impossible to manipulate scores in order to qualify for the program (McCrary, 2008). This assumption is likely to hold in this context. The threshold changes yearly, the exam is scored centrally and students and teachers do not know the algorithm that translates correctly answered questions into exam scores, it is unlikely that students are able to manipulate their scores to qualify. In addition, students are able to take dual credit classes without qualifying for AAP and cannot be discouraged from taking such courses if they fall below the cutoff (Washington House of Representatives. 66th Legislature, Regular Session, 2019). Hence, there is no incentive for a student to manipulate their score to qualify for AAP. Empirically, this proves to be the case. Figure 3 presents the results of the McCrary (2008) density test for the ELA and Math test scores. The density of test scores moves smoothly through the threshold, with no jump at any particular score around the cutoff.

I further check the validity of the regression discontinuity design by showing that student background characteristics are smooth functions across the threshold in Figure 4. Additionally, I use these covariates to generate predicted outcomes based on students beneath the threshold. Applying those predicted probabilities to all students is an approximation of what we would expect in the absence of the program. Figure 5 plots these predicted outcomes and show no discontinuities at the threshold, further bolstering the claim that student characteristics are not what is driving differences across the threshold. The AAP eligibility threshold is determined by a cut score for the

ELA and Mathematics exams as previously described. A measure of distance to the threshold, *Gap*, is the difference between the threshold and the required score. Adherence to the threshold hold is not perfect. A relatively large share of student below the cutoff take dual credit classes since the program cannot discourage participation and a good portion of students who qualify do not take a dual credit class, likely because they opt out (shown in the first-stage pictures). Thus, to estimate the causal effect of AAP/dual credit participation, I use a fuzzy regression discontinuity framework that accounts for the imperfect compliance in a two-stage least squares (2SLS) setup. The estimates from this strategy will yield local average treatment effects (LATEs) in the sense that the results will be a weighted average treatment effect with weights proportional to the likelihood a student will be in the "neighborhood" near the threshold (Lee and Lemieux, 2010) and the results will be local to compliers[7] (Angrist and Imbens, 1995; Angrist et al., 1996).

I model outcomes as a function of AAP/dual credit participation. For student i, in the 10^{th} or 11^{th} grade in school s in school year t, I estimate the following system of local linear regressions:

$$AAP_{ist+K} = \alpha_0 + \alpha_1 Above_{ist} + \alpha_2 Gap_{ist} + \alpha_3 Gap_{ist} \cdot Above_{ist} + \epsilon_{ist} \tag{1}$$

$$Y_{ist+K} = \beta_0 + \beta_1 A\hat{A}P_{ist} + \beta_2 Gap_{ist} + \beta_3 Gap_{ist} \cdot Above_{ist} + \eta_{ist} \tag{2}$$

where Gap_{ist} measures distance to the eligibility threshold, $Above_{ist}$ is an indicator variable for being above the threshold in a given year, AAP_{ist+K}, is an indicator for enrollment in a dual credit class in any year after taking the exam, and Y_{ist+K} is an outcome variable of interest in some year, $t+k$ subsequent to the exam. The causal impact of AAP participation is represented as β_1 from the second stage regression, with program participation instrumented by program eligibility, $Above_{ist}$. I separately estimate the results using the ELA and Math cutoffs.

My preferred model estimates local linear regression with a triangular kernel of bandwidth around 0.254 on either side of the program cutoff. The triangular kernel weights points near the threshold more heavily that those farther from the threshold. I settled on the bandwidth by estimating the optimal bandwidths for the six key outcome variables using the mean square error optimal bandwidths generated by the Calonico et al. (2017) and Calonico et al. (2018) procedure and averaging the bandwidth across the six outcomes to have a consistent sample. In later sections, I test the robustness of my first-stage findings to several additional bandwidths.

[7] The set of students who take a dual credit course if their score passes the threshold and do not if their score falls below the threshold.

V Results

V.A Effects of Qualification on dual credit Participation

First-stage estimates of AAP for ELA and Math are presented in Figure 6.[8] The first panel shows the impact of AAP eligibility on the likelihood a student ever takes a dual credit class in English, Social Studies or Humanities given their score on the ELA Smarter Balanced Assessment. Students scoring just above the ELA threshold are 8 percentage points more likely to enroll in a relevant course. Off a base mean of 49 percent, this suggests that the Academic Acceleration program increased participation in dual credit classes by 16.3 percent. This is not the case for students scoring just above the Math threshold. The second panel shows that eligibility for AAP in Math does not induce students to participate in Math dual credit courses. A potential reason the policy fails to push students into these courses has to do with the graduation requirements in Washington state. It is only required that students take 3 years of math courses to graduate, meaning that since students take the SBA in 11^{th} grade, the policy would have to also induce students to take a fourth year of math to see any possible effects of the program. All students are required to take four years of English. Appendix Figure A2 shows that the program is not pushing students to take a fourth year of math, thus it is unlikely they would then take a dual credit math class. I continue the rest of this paper relying solely on the eligibility cutoff for English Language Arts, since this is where we see the program is effective.

I further explore the effects of qualification on dual credit participation by gender. There are several reasons we might expect differential impacts of the program by gender. Females take dual credit classes at higher rates (Burns and Leu, 2019) and have been shown to be more responsive to interventions than males (Angrist et al., 2009; Angrist and Lavy, 2002). However, when I split the sample by gender, Figure 7 shows that the first-stage results are driven almost solely off of the response of males. The first panel shows that males just above the ELA threshold for AAP are 11 p.p. more likely to participate in a relevant dual credit course and off a base mean of 44 percent, the result suggest an increase of 25%. The second panel shows statistically insignificant increases for females just above the threshold. The results by gender suggest that AAP is serving as a catch-up mechanism for males, since their baseline participation rate is nearly 12 p.p. below that of females.

It has been established that dual credit participation of minorities and low-income students tends to be lower than that of middle-class white students at the same high schools (Xu et al., 2021). If these differences are due to informational constraints, one might expect larger increases in dual credit participation among these groups. I explore possible heterogeneous effects of the qualifying

[8] The number of observations included in the first-stage sample are reported in Appendix Table A1.

for AAP by race and free/reduced-price lunch status in Figure 8. I find that qualifying for AAP has a stronger impact on the likelihood a student takes a relevant dual credit course for FRPL students (22% increase) compared to never-FRPL students (statistically insignificant 10% increase). These results suggest that the adoption of Academic Acceleration achieved its original goal of increasing access to dual credit classes for groups of students with historically lower participation rates. On the race dimension, qualification for AAP had a larger impact for students that identify as White (38% increased) compared to students that identify as non-White (statistically insignificant 6% increase). This result may be due to differences in baseline levels of dual credit participation, which could be driven by Asian students as they have the highest levels of participation (U.S. Department of Education, 2012). Due to data limitations I cannot confirm this statement and leave this question for future work.[9]

Given that students qualifying for AAP are more likely to take relevant dual credit courses, I next examine whether the increase in participation is driven by any particular class type. In particular, I re-run the first-stage on each of the six types dual credit courses offered in Washington state: Tech Prep, International Baccalaureate (IB), College in High School (CHS), Running Start (RS), Advanced Placement (AP), and Cambridge International (Cambridge). Table 3 reports the results of Equation 1 for each dual credit class type. I do not find strong evidence that qualification for AAP induced significant increases in any particular class type. There is some suggestive evidence that compared to students who just missed qualifying for AAP, just qualified students were more likely to take ELA relevant AP and Running Start courses. This result may be due to the fact that AP courses are the most common dual credit class type available in high schools.[10] Furthermore, AP English Language Arts and Composition has the highest participation rate of any AP class and is especially popular for students in the 11$^{\text{th}}$ and 12$^{\text{th}}$ grade (College Board, 2020).

These estimates are relatively large in magnitude and show that qualification for AAP significantly increases the likelihood that a student takes a relevant dual credit course. My estimate of a 8 p.p. increase falls right in the middle of the estimates in the current literature. Jackson (2010) shows that introduction of AP courses through the Advanced Placement Incentive Program leads to 2.3 p.p.increase in the share of 11$^{\text{th}}$ and 12$^{\text{th}}$ graders taking AP/IB exams. Other programs, such as those evaluated in Conger et al. (2022) and Hemelt et al. (2020), have been shown to increase participation in specific dual credit classes to a much larger extent (21 and 12 p.p., respectively). Using GPA and test score cutoffs, Speroni (2011) shows that students who qualified were 9-10 p.p. more likely to take a dual enrollment course than those in the control group. Overall, my findings

[9]In Appendix Figure A1, I show that the null effect using the math cutoff holds does not differ across subgroups.

[10]In 2008, nearly 80% of public high schools offered at least one AP class (Mathews, 2016) compared to only 2% of public high schools offering an IB class (U.S. Department of Education, 2009).

suggest that qualifying for AAP has similar effects on dual credit course taking as other programs across the country.

V.A.1 Validity Checks

The previous section shows that qualification for the Academic Acceleration program is associated with an increase likelihood of taking a relevant dual credit class. However, there remain several potential threats to the validity of my results that should be addressed. Specifically, (1) the results at the threshold may be spurious in that other placebo cutoffs may show similar increases in the likelihood to take a relevant dual credit course, (2) the results may be sensitive to the exclusion of observations right at the cutoff or to the specific choice of bandwidth and kernel type, and (3) there may be other policy innovations that occur at the cutoff that may be driving the results.

To ensure that the findings are not spurious, Table 4 reports the first-stage results at the actual program cutoff (Column (1)) and six placebo cutoffs (Columns (2)-(7)). This falsification test replaces the true cutoff by another value (0.5 to 0.7 standard deviations below and above the actual cutoff) at which treatment status does not really change, and performs estimation and inference using this artificial cutoff point (Cattaneo et al., 2019). For placebo cutoffs above the real threshold I only use treated observations and for placebo cutoffs below the real threshold I only use control observations in order to avoid contamination due to real treatment effects. This restriction ensures that the analysis of each placebo cutoff uses only observations with the same treatment status. The bandwidth and kernel type remain the same as the previous section. Across Columns (2)-(7) the robust p-value is greater than the conventional significance level of 0.05, which is consistent with the conclusion that the likelihood of taking a relevant dual credit course does not jump at the placebo cutoffs. Furthermore, I find that in all of the placebo cutoffs, the RD point estimators are smaller in magnitude or in the opposite direction when compared to the true estimate. Therefore, I conclude that likelihood of taking a relevant dual credit class does not jump discontinuously at the placebo cutoffs considered.

The second concern is that the first-stage estimates are sensitive to certain modeling choices. Table 5, therefore, presents the results of a series of falsification tests to examine the sensitivity of my findings. Panel A investigates how sensitive the results are to the response of units who are located very close to the cutoff. This strategy is useful to assess the sensitivity of the results to the extrapolation involved in the local polynomial estimation. Panel A of Table 5 reports the first-stage estimates where observations with a score within 0.01-0.04 of the cutoff are excluded from the analysis. Across each of the specification, the results show that the first-stage findings are robust. The exclusion of these observations changes the point estimate only slightly and all but one of the estimates are statistically significant at the 5% level. Panel B examines the sensitivity of the results to the bandwidth choice. This test investigates the sensitivity of the first-stage results as observations are added or removed to the end points of the bandwidth. Panel B of Table 5 reports the estimates when under four separate bandwidth calculations: the mean squared error (MSE)-optimal bandwidth, the separate MSE-optimal bandwidth, the coverage error probability (CER)-optimal, and the separate CER-optimal bandwidth. The separate bandwidth selectors simply compute distinct bandwidths for below and above the cutoff. Across each of these specifications, the estimates remain relatively stable and are consistently significant at the 5% level. Panel C examines the sensitivity of the results to how the observations are weighted as a function of distance to the threshold. Again, the first-stage estimates are not sensitive to the particular choice of kernel used.

Another concern is that other policy interventions beyond the Academic Acceleration Program are driving the first-stage results. Specifically, one may be concerned that there are other changes occurring at the cutoff score. To address this issue, I implement a falsification test using students that attended the Seattle Public School District. The Seattle Public School District did not adopt an Academic Acceleration program during my sample time period, but students were still required to take the Smarter Balanced Assessment and had the opportunity to take dual credit classes. Therefore, I can test whether there is jump in the likelihood students take any relevant dual credit classes absent of the program. Figure 9 presents the results of this exercise. I find no evidence that absent of the program, students see a jump in their likelihood to participate in dual credit class at the threshold, bolstering the claim that it is qualification for the Academic Acceleration Program that drives my results.

V.B The Effects of Qualification on Education Outcomes

In this section, I present the reduced-form results of AAP eligibility on subsequent educational outcomes. Column (1) of Table 6 presents the results on on-time high school graduation, final high school grade point average, any public college matriculation (2 and 4-year), any English remedial coursework, and total credits attempted and earned in the first year of college.

Across the board, eligibility for AAP is not associated with improved educational outcomes. Students just above and just below the cutoff exam score are just as likely to graduate high school on-time, they have similar final GPAs, and are just as likely to matriculate into public college and take remedial coursework, despite having being induced into dual credit classes. However, students just qualifying for AAP did see statistically significant decreases in the number of earned credits in their first year enrolled. The results suggest that on average, those that just qualified for AAP decreased the number of credits they earn in the first year of college by 4.67. This result may be an artifact of how dual credit classes work. When students take a dual credit class in high school they earn the same credit had they taken the class in college. How the number of credits a student takes impacts later college outcomes is a relatively understudied area. Theoretically, time-to-degree can be reduced if students take more credits each term, but there is the concern that high course loads can have a negative effect on student performance. Huntington-Klein and Gill (2021) show descriptive evidence that high course loads do not have a negative impact on student grades – however, Agasisti et al. (2022) examine a program in Italy that increased the number of credits students had to earn to receive financial aid and found that the program discouraged lower-ability students from continuing their studies. Whether the reduction in credits found in this context would be beneficial for students is left for future work.

The previous results suggest that qualifying for AAP does not have an effect on subsequent education outcomes. However, these average estimates across all students in the sample could differ across various subgroups. Therefore, I disaggregated the results by the following characteristics: gender, race and free/reduced price lunch status. I calculated these estimates by splitting the sample by the particular subgroup. Columns (2)-(7) of Table 6 displays the results of the heterogeneity analysis by student subgroup for on-time graduation, final high school GPA, matriculation into any public college, any remedial ELA coursework, total number of attempted and earned credits in the first year of college, respectively. Columns (2) and (3) display the reduced-form results for student that identify as White and non-White, Columns (4) and (5) display the results for students ever qualifying for FRPL and never qualifying for FRPL, and Columns (6) and (7) display the results for Men and Women.

Similar to the results shown in Column (1) of Table 6, there does not seem to be systematic evidence that qualifying for AAP improves educational outcomes for any particular subgroup. In Columns (2)-(7) of Table 6 only 5 of the 36 coefficients are statistically significant at the 5 percent level. Furthermore, the results that are statistically significant are concentrated on the subgroups that had the weakest first-stage results, suggesting that this effect may be due to some other policy intervention rather than the Academic Acceleration Program. Future work will dive deeper into the effects of this particular outcome variable. As an additional check, I run a joint hypothesis for the impact of qualification for AAP across all outcomes of interest within each subgroup. To conduct this exercise I use a slightly altered regression specification, which changes the magnitude of some of the coefficients. Specifically, this specification uses uniform weighting compared to the triangular weighting used in the preferred specification. The results of this exercise, reported in Appendix Table A2, show that I fail to reject the null hypothesis that there is no effect of qualifying for AAP on subsequent educational outcomes at the 5 percent level. These results further support the claim that qualifying for AAP has no impact on subsequent education outcomes.

My findings are very in-line with the estimates found from the previous literature. Speroni (2011) shows that just qualifying for dual enrollment courses (based off a GPA cutoff) does not impact the likelihood of receiving a high school diploma, matriculating into any college, or attending a 4-year college. Hemelt et al. (2020) show that the introduction of college algebra in Tennessee high schools only slightly increased the likelihood of students enrolling in 4-year colleges, with no effect on all other outcomes. Jackson (2010) shows that the introduction of the Advanced Placement Incentive Program increased the likelihood of having a high SAT/ACT score and had a marginally significant effect on college matriculation. Together, these results suggest that while AAP is targeted at different groups of students and introduces a wider variety a dual credit classes than the aforementioned programs, we may not expect to see different results.

VI Conclusion

Policymakers consider increasing access to dual credit classes a potential solution to address issues of reduced academic preparation of college-going students. However, there are concerns that differential expectations may exacerbate existing inequities in participation. By evaluating the educational outcomes of the Academic Acceleration Program, this paper examines whether switching the default of advanced coursework enrollment changes expectations about academic potential.

I show that automatically enrolling students in relevant advanced coursework has a meaningful impact on their likelihood to enroll in dual credit coursework. Specifically, qualifying for the Academic Acceleration Program via the standardized English Language Arts exam increased the likelihood that a student would later enroll in a dual credit English, History or Humanities by 16.3 percent. However, the preliminary reduced-form results suggest that the increase in the likelihood to take these classes did not translate into meaningful changes in subsequent educational outcomes.

These results suggest that while automatically enrolling students in advanced coursework may be an effective tool to increase access to dual credit courses, it may not be sufficient to improve educational outcomes for all students. It is important to consider the potential drawbacks of these programs, such as the quality of the courses offered and the potential for these courses to place extra stress on students. As such, policymakers and educators must carefully consider the design and implementation of dual credit programs to ensure that they are effective and equitable.

Future research could expand on this study by examining the longer-term impacts of dual credit programs, including the impact on post-college outcomes such as employment and earnings. Furthermore, Academic Acceleration was implemented in elementary and middle schools across the state, so it is possible to answer questions about whether the timing of these programs matter. Ultimately, as policymakers continue to discuss the expansion of these programs, its important to understand for which groups of students these classes are beneficial.

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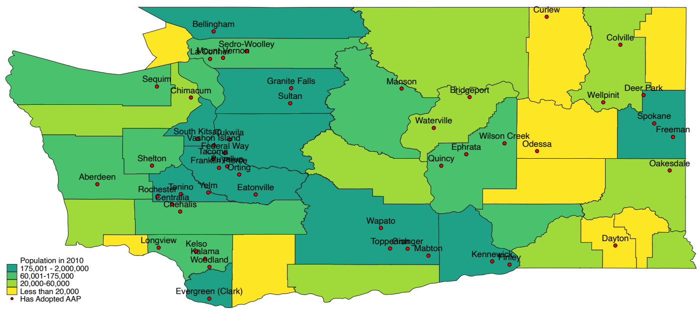
Figures

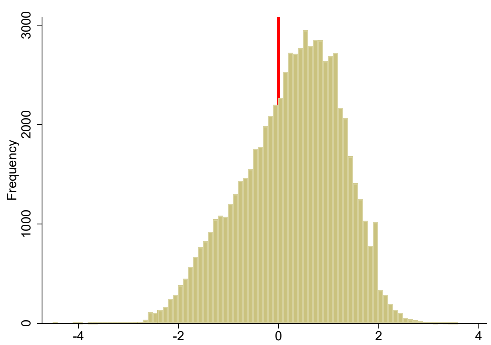
Figure 1: Adopting School Districts between 2012-2018



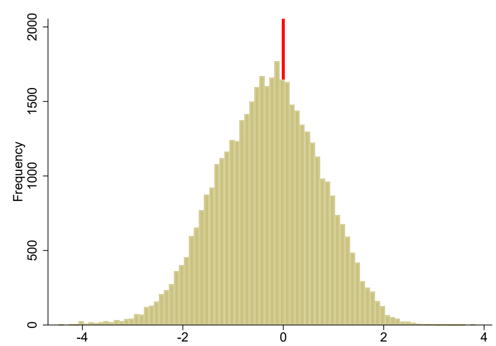
Notes: This map presents the locations of each school district that had adopted an Academic Acceleration Program prior to the enactment of HB 1599 against the 2010 population of each county in the state. Implementation dates for AAP was provided by the non-profit Stand for Children. 2010 Population count come from the U.S. Census Bureau.

Figure 2: Scaled Test Score Histograms

(a) English Language Arts



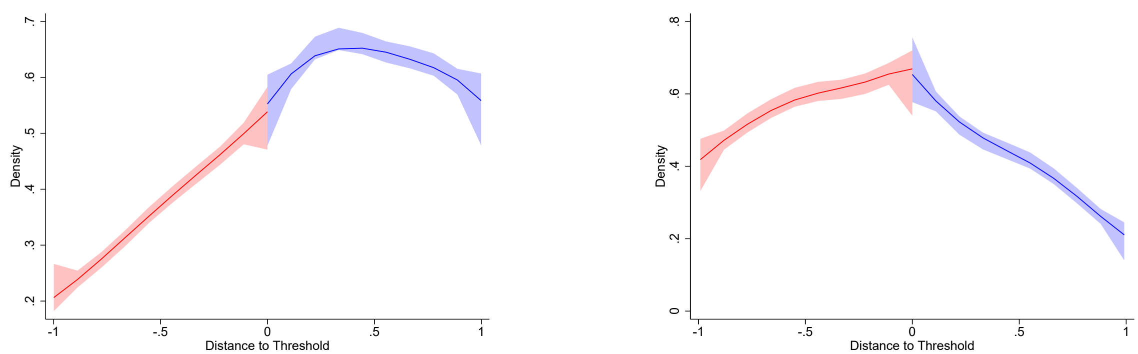
(b) Mathematics



Notes: This figure presents the distributions of the English Language Arts and Mathematics SBA scores over the sample period. The solid, red line highlights the cutoff for eligibility into the Academic Acceleration Program. Data on students' test scores and cutoff for eligibility come from the ERDC.

Figure 3: McCrary (2008) Density Test Results

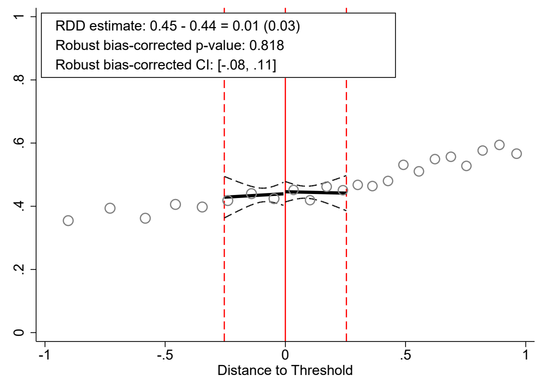
(a) English Language Arts (b) Mathematics



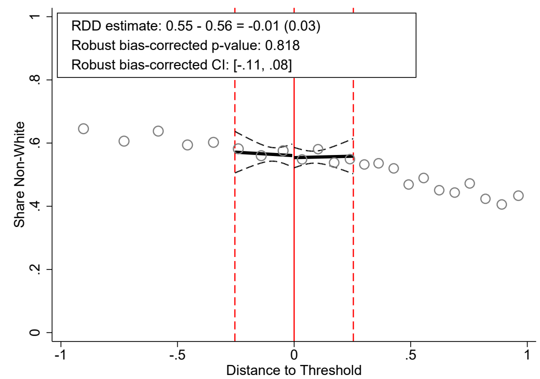
Notes: This figure presents the results of the McCrary (2008) density test for ELA (Panel A) and Math (Panel B) test scores across the eligibility threshold. The red lines and confidence intervals indicates the observations below the threshold, while the blue lines and confidence intervals indicate the observations above the threshold. Data on students' test scores come from the ERDC.

Figure 4: Covariate Balance Checks

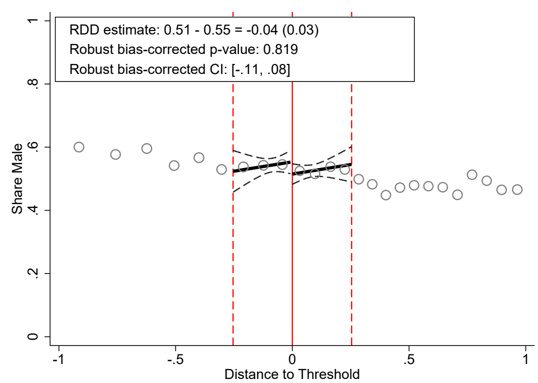
(a) Share White



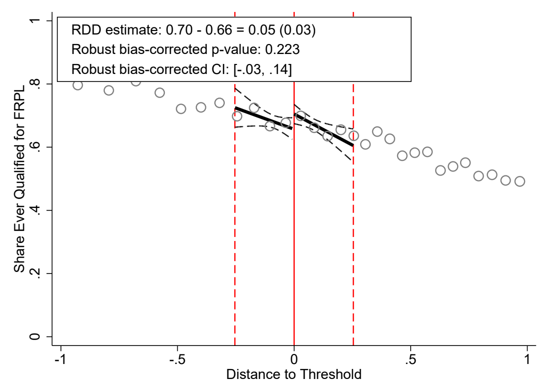
(b) Share Non-White



(c) Share Male



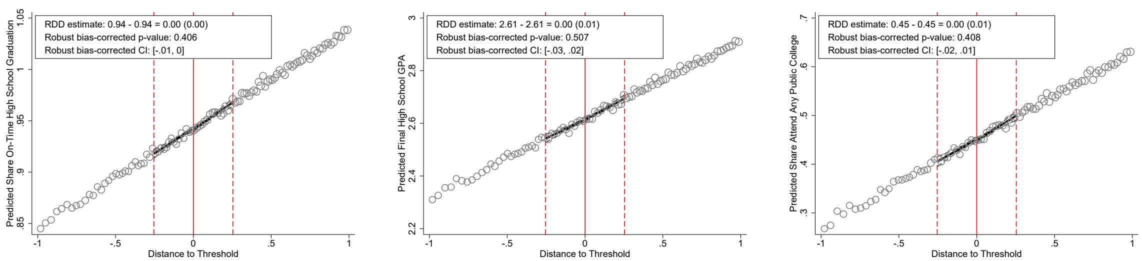
(d) Share Ever FRPL



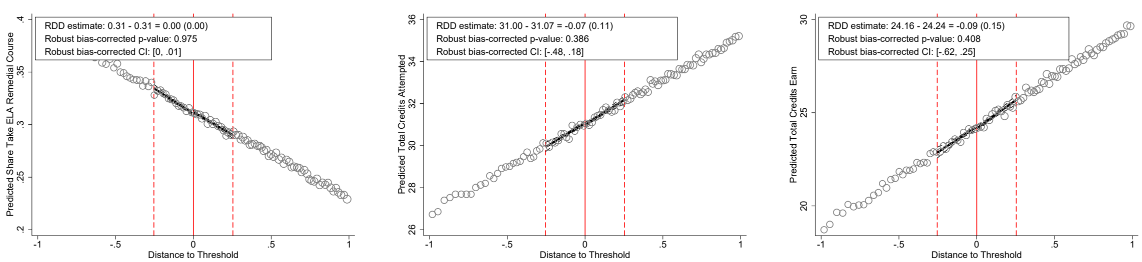
Notes: This figure shows descriptive characteristics of students by the running variable for students from 2014-2015 through 2002-2021. I impose a linear fit on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on student characteristics comes from the ERDC.

Figure 5: Predicted Outcomes

(a) On-Time Graduation | (b) Final HS GPA | (c) Attend Any Public College



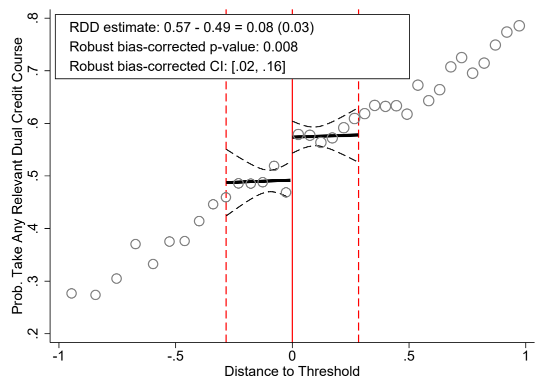
(d) Any Remedial ELA Courses | (e) Total Credits Attempted | (f) Total Credits Earned



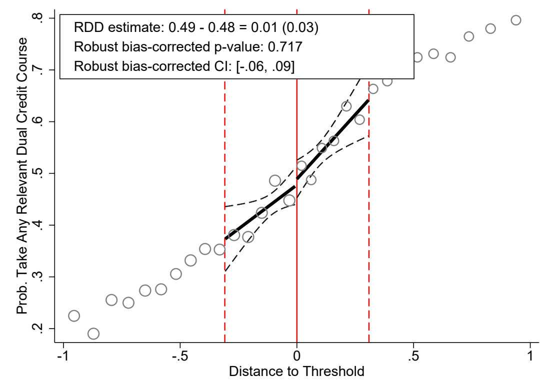
Notes: This figure shows average predicted educational outcomes for quantile-spaced bins on either side of the threshold. I impose a linear fit on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Predicted outcomes are generated by predicting the relationship between baseline characteristics and outcomes for student below the threshold of AAP eligibility and assigning those fitted values to students' outcomes.

Figure 6: First-Stage Results of AAP Eligibility

(a) English Language Arts



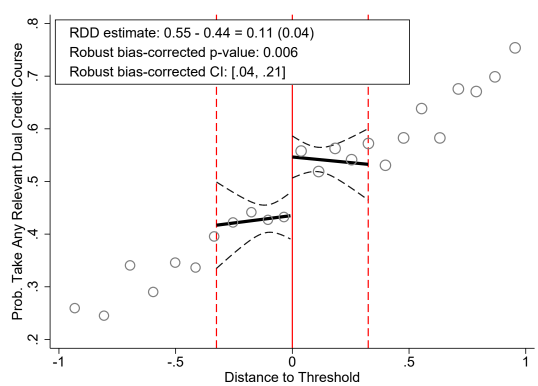
(b) Mathematics



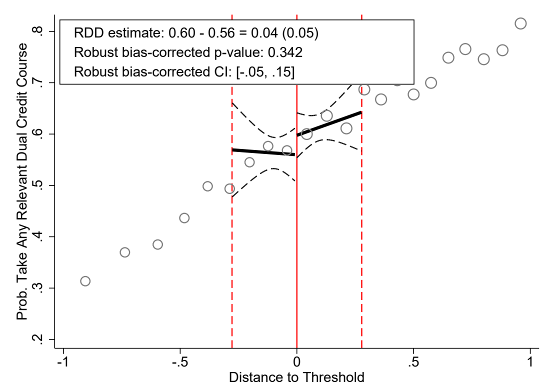
Notes: This figure shows dual-credit/AAP participation by the running variable for students between the 2014-2015 and 2020-2021 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Panel A shows participation in English, Social Studies and Humanities dual-credit classes. Panel B shows participation in math dual-credit classes. Data on courses comes from the ERDC. $N = 2,060$ to the left of threshold and $N = 2,090$ to the right of the cutoff. Total observations in the the sample, 16,756.

Figure 7: ELA First-Stage Results by Gender

(a) Male Students



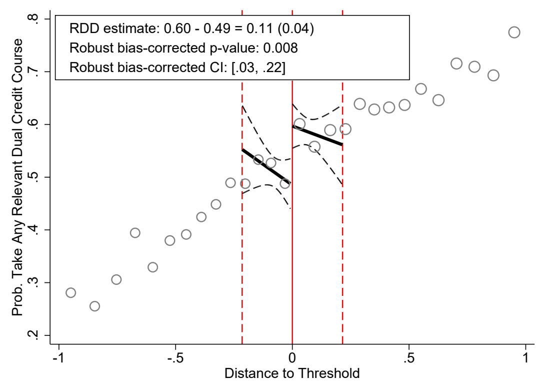
(b) Female Students



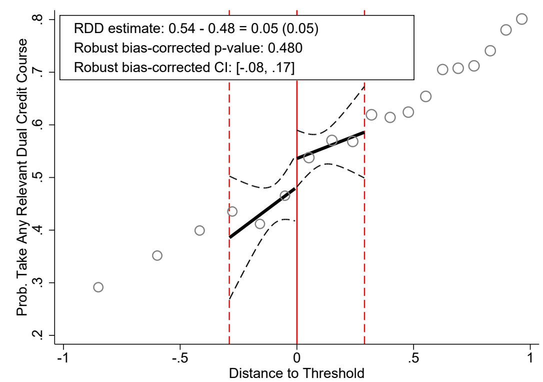
Notes: This figure shows the first-stage results of AAP eligibility on dual-credit participation by gender for students between the 2014-2015 and 2020-2021 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

Figure 8: ELA First-Stage by Subgroup

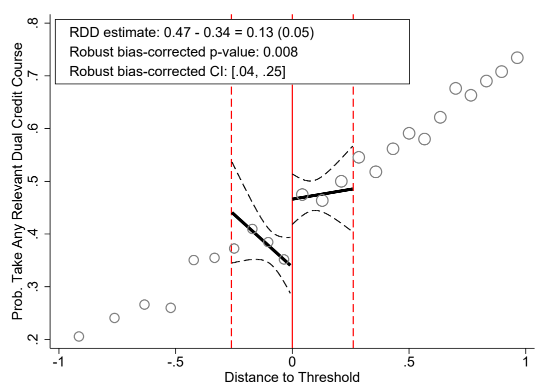
(a) Ever FRPL Students



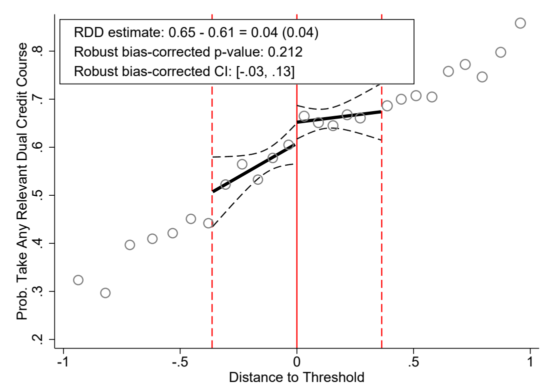
(b) Never FRPL Students



(c) White Students

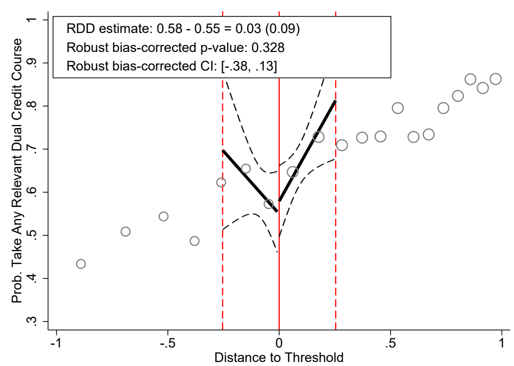


(d) Non-White Students



Notes: This figure shows the first-stage results of AAP eligibility on dual-credit participation by race and FRPL status for students between the 2014-2015 and 2020-2021 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

Figure 9: Falsification Test - Seattle



Notes: This figure shows the first-stage results of AAP eligibility on dual-credit participation for the Seattle School District the 2014-2015 and 2020-2021 academic years. Seattle did not adopt an Academic Acceleration program in the time frame of this study. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

Tables

Table 1: State vs. Adopting vs. Sample Districts 2014-2015 AY

	State	Adopting Districts	Sample Districts
	(1)	(2)	(3)
Average Enrollment in District	1,310	1,723	3,701
% Female	48.49	48.60	48.68
% White	59.82	59.69	53.36
% Low-Income	45.04	52.83	57.02
4-Year Graduation Rate^a	78.9	80.27	80.27
% Met Standard on Math SBAC - 11$^{\text{th}}$ Grade^b	13.19	12.15	11.46
% Met Standard on ELA SBAC- 11$^{\text{th}}$ Grade^c	25.28	26.46	26.44
% Took Any Dual Credit Class in 11$^{\text{th}}$/12$^{\text{th}}$ Grade^d	64.36	63.52	60.58

Notes: a, b, c, d - Only includes information from districts that do not require the suppression of data. This table presents summary statistics for students attending grades 9$^{\text{th}}$-12$^{\text{th}}$ in the 2014-2015 academic year across three samples: the entire state of Washington, school districts that adopted an Academic Acceleration Program before the passage of HB 1599, and the nine school districts included in the sample. State and district level information come from published Report Card data from the Washington Open Data Portal. SBAC stands for Smarter Balanced Assessment Consortium.

Table 2: Summary Statistics

	All Students	Enrolled in Dual-Credit Class	RD Sample
	(1)	(2)	(3)
Panel A: Demographics			
Male	0.515	0.486	0.532
	(0.500)	(0.500)	(0.499)
White	0.467	0.435	0.440
	(0.499)	(0.496)	(0.496)
Ever Subsidized Lunch	0.642	0.621	0.672
	(0.480)	(0.485)	(0.469)
Panel B: AAP Participation			
Take Any Adv. Class	0.673	1.000	0.648
	(0.469)	(0.000)	(0.478)
Number of Adv. Class	1.908	2.837	1.651
	(1.972)	(1.775)	(1.769)
Panel C: High School Milestones			
On-Time Graduation	0.938	0.966	0.937
	(0.242)	(0.181)	(0.243)
Final GPA	2.777	2.968	2.719
	(0.621)	(0.570)	(0.596)
Panel D: College Outcomes			
Any Public College	0.354	0.389	0.354
	(0.478)	(0.487)	(0.478)
Total Credits Attempted Year 1	31.607	32.716	30.680
	(14.724)	(14.401)	(15.060)
Total Credits Earned Year 1	25.532	26.815	24.072
	(16.400)	(16.223)	(16.459)
Number of Observations	16,756	11,273	4,697

Notes: Mean values of each variable are shown by sample. Column (1) is the full sample of students included in the first-stage analysis. Column 2 restricts that sample to the set of students who had ever enrolled in a dual-credit class. Column 3 restricts the full sample to those within a bandwidth of 0.254 around the eligibility threshold. Student-level data comes from the ERDC database.

Table 3: ELA First-Stage Results by Course Type

	Any ELA Tech Prep	Any ELA IB	Any ELA CHS	Any ELA RS	Any ELA AP	Any ELA Cambridge
	(1)	(2)	(3)	(4)	(5)	(6)
RDD Estimate	0.004	-0.001	-0.011	0.028	0.016	0.000
Robust BC 95% CI	[-0.002 ; 0.018]	[-.016 ; .017]	[-.071 ; .042]	[-.031 ; .061]	[-.026 ; .035]	[-.015 ; .012]
Robust BC p-value	0.097	0.923	0.616	0.524	0.779	0.850
Bandwidth	[.254; .254]	[.254; .254]	[.254; .254]	[.254; .254]	[.254; .254]	[.254; .254]
Observations Left	2,060	2,060	2,060	2,060	2,060	2,060
Observations Right	2,090	2,090	2,090	2,090	2,090	2,090

Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each coefficient is the result of a separate estimation. Sample includes students who took the SBAC ELA exam in high school in the sample districts. The outcome variable is defined as taking at least one relevant dual-credit class of the specific type. Student-level data comes from the ERDC database.

Table 4: First-Stage Results Using Placebo Cutoffs

	Actual Cutoff	Cutoff of -0.7	Cutoff of -0.6	Cutoff of -0.5	Cutoff of 0.5	Cutoff of 0.6	Cutoff of 0.7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RDD Estimate	0.080	0.029	-0.061	0.007	0.031	0.030	0.008
Robust BC 95% CI	[.020 ; 0.160]	[-.112 ; .127]	[-.162 ; .061]	[-.106 ; .11]	[-.018 ; .145]	[-.038 ; .123]	[-.062 ; .095]
Robust BC p-value	0.008	0.906	0.373	0.970	0.128	0.301	0.677
Observations Left	2,060	1,104	1,237	1,392	2,854	2,784	2,724
Observations Right	2,090	1,482	1,649	1,817	2,719	2,654	2,577

Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each coefficient is the result of a separate estimation. Sample includes students who took the SBAC ELA exam in high school in the sample districts. The outcome variable is defined as taking at least one relevant dual-credit class. Student-level data comes from the ERDC database.

Table 5: First-Stage Results Robustness Checks

Panel A: Donut Estimation				
	Donut Size 0.01	Donut Size 0.02	Donut Size 0.03	Donut Size 0.04
RDD Estimate	0.071	0.087	0.089	0.090
Robust BC 95% CI	[-.035 ; .212]	[-.006 ; .289]	[-.003 ; .351]	[.007 ; .427]
Robust BC p-value	0.158	0.061	0.054	0.043
Observations Left	1,985	1,906	1,832	1,768
Observations Right	2,472	2,398	2,316	2,227
Panel B: Bandwidth Selection				
	MSE-Optimal	Separate MSE-Optimal	CER-Optimal	Separate CER-Optimal
RDD Estimate	0.079	0.083	0.098	0.097
Robust BC 95% CI	[.022 ; .157]	[.020 ; .171]	[.022 ; .184]	[.014 ; .189]
Bandwidth	[.300; .300]	[.236; .293]	[.174; .174]	[.143; .183]
Robust BC p-value	0.009	0.014	0.013	0.023
Observations Left	2,397	1,925	1,521	1,206
Observations Right	3,120	3,051	1,839	1,804
Panel D: Kernel Type				
		Triangular	Uniform	Epanechnikov
RDD Estimate		0.085	0.080	0.081
Robust BC 95% CI		[.013 ; .204]	[.004 ; .181]	[.016 ; .202]
Robust BC p-value		0.026	0.041	0.022
Observations Left		2,060	2,060	2,060
Observations Right		2,637	2,637	2,637

Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each coefficient is the result of a separate estimation. Sample includes students who took the SBAC ELA exam in high school in the sample districts. The outcome variable is defined as taking at least one relevant dual-credit class. Student-level data comes from the ERDC database.

Table 6: Results of Academic Acceleration on Education Outcomes

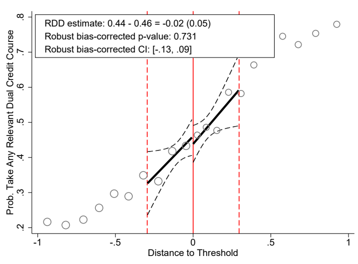
	Entire Sample	White	Non-White	Ever FRPL	Never FRPL	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
On-Time Graduation	0.002	-0.002	-0.003	0.009	-0.022	-0.017	0.014
Robust BC 95% CI	[-.035 ; .067]	[-.037 ; .12]	[-.073 ; .062]	[-.037 ; .095]	[-.089 ; .067]	[-.074 ; .074]	[-.035 ; .105]
Robust BC p-value	0.543	0.295	0.869	0.383	0.782	0.997	0.327
Observations	3,849	1,692	2,157	2,581	1,268	2,069	1,780
Final HS GPA	-0.018	-0.019	0.047	0.049	-0.035	-0.048	0.079
Robust BC 95% CI	[-.075 ; .182]	[-.106 ; .286]	[-.142 ; .195]	[-.074 ; .233]	[-.22 ; .248]	[-.167 ; .183]	[-.072 ; .285]
Robust BC p-value	0.415	0.370	0.761	0.313	0.907	0.926	0.243
Observations	3,849	1,692	2,157	2,581	1,268	2,069	1,780
Any Public College	-0.046	-0.050	-0.042	-0.031	-0.069	-0.052	-0.046
Robust BC 95% CI	[-.154 ; .054]	[-.19 ; .115]	[-.198 ; .084]	[-.187 ; .065]	[-.208 ; .164]	[-.205 ; .069]	[-.186 ; .129]
Robust BC p-value	0.347	0.628	0.430	0.340	0.820	0.333	0.722
Observations	3,849	1,692	2,157	2,581	1,268	2,069	1,780
Any Remedial ELA	-0.066	-0.002	-0.107	-0.038	-0.120	-0.128	-0.010
Robust BC 95% CI	[-.202 ; .107]	[-.36 ; .163]	[-.205 ; .175]	[-.124 ; .246]	[-.516 ; .016]	[-.332 ; .109]	[-.215 ; .22]
Robust BC p-value	0.545	0.460	0.877	0.519	0.066	0.322	0.983
Observations	3,849	1,692	2,157	2,581	1,268	2,069	1,780
Attempted Credits	-4.162	-1.804	-5.638	-2.633	-6.864	-2.917	-5.313
Robust BC 95% CI	[-8.786 ; .958]	[-4.588 ; 12.002]	[-14.254 ; -2.507]	[-8.817 ; 3.408]	[-14.561 ; 1.847]	[-7.701 ; 8.030]	[-13.337 ; -1.251]
Robust BC p-value	0.115	0.381	0.005	0.386	0.129	0.967	0.018
Observations	3,849	1,692	2,157	2,581	1,268	2,069	1,780
Earned Credits	-4.673	-2.782	-5.862	-2.429	-8.466	-4.467	-4.930
Robust BC 95% CI	[-11.054 ; -.080]	[-7.134 ; 10.858]	[-16.868 ; -3.265]	[-10.833 ; 3.261]	[-17.881 ; -.458]	[-12.384 ; 4.655]	[-14.294 ; .059]
Robust BC p-value	0.047	0.685	0.004	0.292	0.039	0.374	0.052
Observations	3,849	1,692	2,157	2,581	1,268	2,069	1,780

Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each coefficient is the result of a separate estimation. Sample includes students who took the SBAC ELA exam in high school in the sample districts. Student-level data comes from the ERDC database. The table presents reduced-form effects of the policy and has not been scaled by the first-stage.

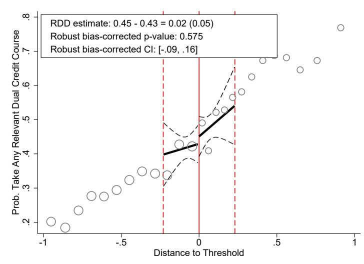
A1 Appendix Figures and Tables

Figure A1: Math First-Stage Results by Subgroup

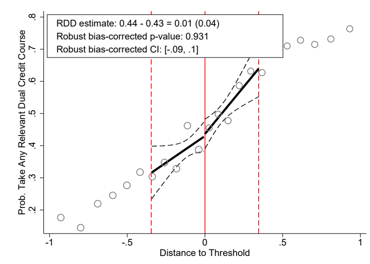
(a) Male Students



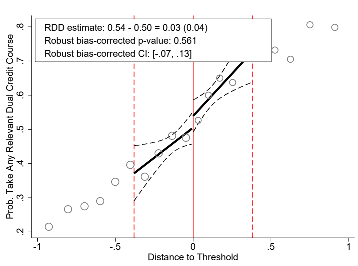
(b) Ever FRPL Students



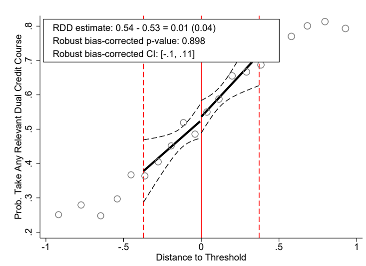
(c) White Students



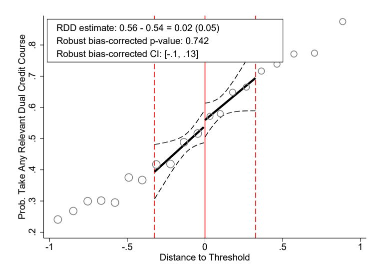
(d) Female Students



(e) Never FRPL Students

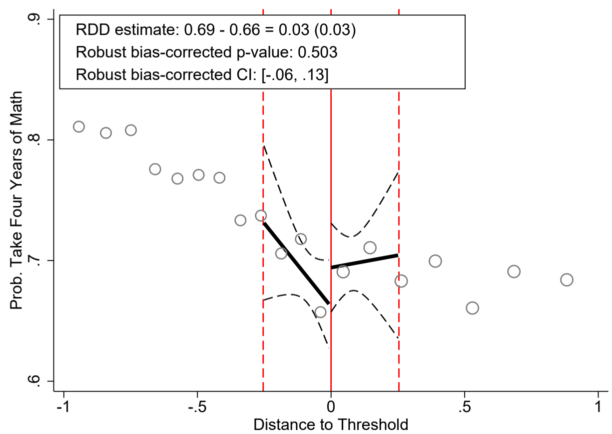


(f) Non-White Students



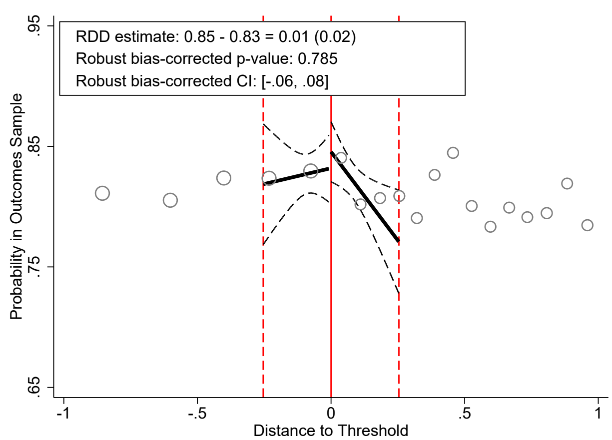
Notes: This figure shows the first-stage results of AAP eligibility on dual-credit participation using the Math score cutoff by gender, race, and FRPL status for students between the 2014-2015 and 2020-2021 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

Figure A2: Effects of AAP Eligibility on Taking a 4^{th} Year of Math



Notes: This figure shows the average likelihood of taking four years of math classes between the 2014-2015 and 2020-2021 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

Figure A3: Attrition



Notes: This figure shows average likelihood of remaining in the outcomes sample across the 2014-2015 and 2020-2021 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

Table A1: Summary Statistics Across Samples

	First-Stage Sample	Outcomes Sample
	(1)	(2)
Any ELA Dual Credit	0.562	0.552
	(0.496)	(0.497)
Distance to Cutoff	0.151	0.147
	(0.528)	(0.527)
Final HS GPA	2.761	2.750
	(0.633)	(0.628)
Share White	0.467	0.471
	(0.533)	(0.529)
Share Non-White	0.533	0.529
	(0.500)	(0.500)
Share Male	0.515	0.518
	(0.500)	(0.500)
Share Ever FRPL	0.642	0.639
	(0.480)	(0.480)
Observations	16,756	13,590

Notes: Mean values of each variable are shown by sample. Column (1) restricts that sample to the set of students who are included in the first-stage analysis. Column(2) restricts the full sample to those I observe following their high school careers. Student-level data comes from the ERDC database.

Table A2: Education Results of AAP by Subgroup - Alternate Specification

	Entire Sample	White	Non-White	Men	Women	Ever FRPL	Never FRPL
On-Time Graduation	-0.002	-0.024	0.013	-0.005	0.000	0.006	-0.017
Robust BC 95% CI	[-.045 ; .032]	[-.065 ; .051]	[-.057 ; .047]	[-.073 ; .042]	[-.051 ; .051]	[-.047 ; .054]	[-.079 ; .034]
Robust BC p-value	0.737	0.807	0.842	0.601	0.991	0.891	0.438
Total Observations	5,542	2,426	3,116	2,925	2,617	3,725	1,817
Final HS GPA	0.008	0.050	0.029	0.001	0.011	0.014	0.008
Robust BC 95% CI	[-.035 ; .157]	[-.101 ; .188]	[-.051 ; .207]	[-.111 ; .15]	[-.055 ; .22]	[-.031 ; .201]	[-.136 ; .208]
Robust BC p-value	0.214	0.554	0.236	0.771	0.240	0.151	0.679
Total Observations	5,542	2,426	3,116	2,925	2,617	3,725	1,817
Any Public College	-0.028	-0.031	-0.025	-0.032	-0.025	-0.012	-0.056
Robust BC 95% CI	[-.11 ; .046]	[-.145 ; .081]	[-.139 ; .075]	[-.141 ; .069]	[-.151 ; .082]	[-.123 ; .064]	[-.165 ; .118]
Robust BC p-value	0.415	0.582	0.0556	0.500	0.565	0.535	0.740
Total Observations	5,542	2,426	3,116	2,925	2,617	3,725	1,817
Any Remedial ELA	-0.058	-0.018	-0.083	-0.095	-0.025	-0.042	-0.088
Robust BC 95% CI	[-.204 ; .052]	[-.203 ; .223]	[-.292 ; .029]	[-.353 ; .009]	[-.172 ; .192]	[-.195 ; .117]	[-.370 ; .073]
Robust BC p-value	0.247	0.928	0.108	0.062	0915	0.625	0.188
Total Observations	1,969	788	1,181	969	1,000	1,242	727
Attempted Credits	-3.411	-2.963	-3.728	-3.279	-3.609	-2.048	-5.726
Robust BC 95% CI	[-8.84 ; -.611]	[-8.205 ; 5.674]	[-11.846 ; -1.817]	[-9.383 ; 3.436]	[-11.421 ; -.856]	[-8.183 ; 2.158]	[-14.735 ; -.965]
Robust BC p-value	0.024	0.721	0.007	0.363	0.023	0.253	0.0254
Total Observations	1,940	773	1,167	951	989	1,226	714
Earned Credits	-3.302	-3.482	-3.176	-3.145	-3.561	-1.488	-6.177
Robust BC 95% CI	[-10.039 ; -.895]	[-9.828 ; 5.05]	[-13.138 ; -1.638]	[-12.604 ; 1.188]	[-11.393 ; .865]	[-8.548 ; 3.117]	[-17.702 ; -2.934]
Robust BC p-value	0.019	0.529	0.012	0.105	0.092	0.361	0.006
Total Observations	1,969	788	1,181	969	1,000	1,242	727
Joint Hypothesis Test *p*-value	0.194	0.389	0.191	0.467	0.684	0.576	0.099

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Each coefficient is the result of a separate estimation. Sample includes students who took the SBAC ELA exam in high school in the sample districts. Student-level data comes from the ERDC database. The table presents reduced-form effects of the policy and has not been scaled by the first-stage using a uniform weighting scheme.

Table A3: First-Stage Results With Outcomes Sample

	Entire Sample	White	Non-White	Ever FRPL	Never FRPL	Men	Women
RDD Estimate	0.113	0.167	0.076	0.133	0.065	0.164	0.044
Robust BC 95% CI	[.042 ; .251]	[.082 ; .384]	[-.05 ; .227]	[.058 ; .312]	[-.124 ; .248]	[.088 ; .368]	[-.105 ; .206]
Robust BC p-value	0.006	0.003	0.210	0.004	0.515	0.001	0.526
Observations Left	1,707	735	972	1,178	529	935	772
Observations Right	2,142	957	1,185	1403	739	1,134	1,008

Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each coefficient is the result of a separate estimation. Sample includes students who took the SBAC ELA exam in high school in the sample districts and can be followed after high school. The outcome variable is defined as taking at least one relevant dual-credit class. Student-level data comes from the ERDC database.