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

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Abstract

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 regression discontinuity design to evaluate subsequent education outcomes. I find that students just qualifying for AAP increase their likelihood of taking a relevant dual credit course by 8 percentage points, with stronger results for boys. However, qualification for AAP does not significantly alter education outcomes. As policymakers continue these programs, it's important to understand whether simply expanding access is sufficient in improving outcomes.

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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 undergraduate students reported taking a remedial course upon entering postsecondary education (National Center for Education Statistics, 2023). One solution proposed by policy makers to address this issue is to raise the rigor of high school coursework through greater participation in dual credit classes.[1] However, there are significant gaps in 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 (Avery and Goodman, 2022; 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 switched 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 empirically address this question 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 regression discontinuity design 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 average number of credits attempted/earned per term 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.[2] Interestingly, the increase in dual credit course taking is driven by boys. Boys

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

[2] Caution should be used when interpreting the magnitude of the effect since the baseline mean represents the

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, suggesting that AAP may be serving as a catch-up mechanism. Additional heterogeneity results suggest 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$^{\text{th}}$ 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 0.5-0.6 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

average for those just below the cutoff. This average is comprised of both compliers and always-takers.

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) and attempt/earn a similar number of credits in their first year of college. Together, my results suggest that while AAP qualification was successful in increasing dual credit participation, it did not translate into better educational outcomes. However, my results also suggest that qualification for AAP did not negatively impact students on these outcomes. This is an important conclusion because it suggests that the academic rigor of advanced coursework was not detrimental, assuaging concerns that the program is targeting students who are unprepared.

This paper contributes to two distinct literatures. First, it speaks to the growing 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) and Liu et al. (2024) also implement a regression discontinuity design to evaluate the effects of dual enrollment courses on students who just pass the cutoff to participate. Speroni (2011) finds no effect of dual enrollment on high school or college outcomes (except for those students qualifying to participate in college algebra), while Liu et al. (2024) finds that students just above the GPA cutoff to participate in dual enrollment classes were more likely to apply and be admitted into selective, four-year colleges. Compared to the programs studied in Speroni (2011) and Liu et al. (2024), 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 an intervention that changes the default and is targeted at the average student during their high school career can alter their educational outcomes.

Austin et al. (2022) also examines the effectiveness of the Academic Acceleration Program in Washington state. The authors use a staggered difference-in-difference design and find that students in adopting districts increased their participation in any advanced course enrollment by 5.3 percentage points.[3] This paper builds on their results in several ways, beginning with the identification strategy. The effects estimated from my regression discontinuity design are specific to the marginal student who just qualified to participate. Understanding the impacts for this specific group of students may be of particular interest to policymakers since there are concerns that expanding access to lower achieving students may set them up for failure (Bailey and Karp, 2003). Furthermore, this paper includes analyses on subsequent education outcomes beyond taking advanced coursework. Understanding the impact of qualifying for AAP on these outcomes is critical to assess whether the program is effective.

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 (AAP) 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 as a part of the accountability requirements for public schools.[4] For both English Language Arts (ELA) and Mathematics, if the student scored above a certain threshold

[3] These estimates should be interpreted with caution as the authors present evidence of a violation in the parallel trends assumption.

[4] 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.

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 goal of the program 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 a student's high school career. All but Running Start courses are taught during the regular class time and generally serve as a substitute for another course.[5] 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 surrounding higher education as well (Jackson, 2010).

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 (De La Rosa, 2024). However, there remains the question of whether automatic enrollment in these courses might set up students for failure, if the average high school student is not prepared to handle college-level work. This paper empirically addresses this question by evaluating the educational impacts of the Academic 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

[5] The fact that these dual credit courses are offered during regular school hours is what distinguishes them from dual enrollment (DE) courses. DE courses are often offered at local community colleges, where students must travel.

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 since these students are much more likely to be on the margin of deciding whether to matriculate into college (Zimmerman, 2014).

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.[6] 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, 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.[7] 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 16,757 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 A4 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.

[6] 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.

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

Table 2 presents the summary statistics of students in different subsets of the sample. In the 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, with the unconditional average of number of dual credit classes equaling 1.9.[8] 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 per term in their first year of college (14.23 & 11.24 versus 13.89 & 10.83). 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^{th} or 11^{th} grade.[9] The Smarter Balanced Assessment takes the correct answers a student completes and converts it to a scale score

[8]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.

[9]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. The grade a student was required to take the SBA changed from the 10^{th} to 11^{th} grade during my sample period.

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 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.

Simply comparing students that take dual credit courses with those who did not would give a biased estimate of the effectiveness of the Academic Acceleration Program. Any observed difference between the two groups of students may stem from differences in student characteristics rather than the impact of the program itself. An ordinary least squares (OLS) regression on observable characteristics would not fully address this issue. If there are unobserved factors, such as personal motivation or family support, that influence both dual credit course taking and educational outcomes these factors would bias the estimated program effects. To address this issue, I estimate the causal impact of dual credit course enrollment (and AAP participation) on education outcomes using a regression discontinuity design. I compare students who are just above and just below the eligibility cutoffs minimizing the potential influence of omitted variable bias, since students are in as-good-as random order within a small window of points on the exam (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 the exam is a random draw from a student's underlying ability distribution since students cannot precisely control their score on the test.

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, 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 cutoff 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). For a all of my results, I focus on the first-stage and reduced-form impacts of qualifying for AAP. 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 begin by estimating the following first stage:

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

where AAP_{ist+K} is an indicator for a student i's enrollment in a relevant dual credit class at school s in any year $t + K$ after taking the SBAC exam, Gap_{ist} measures the distance to the eligibility threshold in standard deviations, and $Above_{ist}$ is an indicator variable for being above the threshold in a given year t. I separately estimate the results using the ELA and Math cutoffs.

I then generate reduced-form estimates of the impact of qualifying for AAP on student outcomes using the following local linear regression:

$$Y_{ist+K} = \alpha_0 + \alpha_1 Above_{ist} + \alpha_2 Gap_{ist} + \alpha_3 Gap_{ist} \cdot Above_{ist} + \epsilon_{ist} \tag{2}$$

The coefficient of interest α_1 measures the effect of a student being just above the test score cutoff on the outcome of interest, Y_{ist+K}, in some year, $t + k$ after taking the exam. The main outcomes of interest include on-time high school graduation, final high school grade point average (HS GPA), matriculation into any (2- or 4-year) public college, attempt of any remedial ELA course, and average number of credits attempted/earned per semester during the first year of college. We can interpret the results from this estimation strategy as the intent-to-treat impact.

In my preferred specification, I employ a triangular kernel weighing function and set the bandwidth to 0.3 standard deviations on either side of the cutoff.[10] In later sections, I test the robustness of my findings to several additional bandwidths and weighting schemes. For statistical inference, I report the robust bias-corrected confidence intervals and p-values (Calonico et al., 2014). This confidence interval has been adjusted for an estimated bias term and is thus often not centered around the RDD point estimate. It is important to consider that when discussing the magnitude of the results (i.e., scaling the treatment effect by some control or baseline mean) we would ideally report the mean of compliers. However, it is not possible to identify them in the data.

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.[11] 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 11th 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 A3 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.

[10] I chose 0.3 as the bandwidth by first estimating the bandwidths that minimize the mean square error (MSE) of the RDD estimate as suggested in Calonico et al. (2014) and Cattaneo et al. (2019). I then took the average across the outcome variables of interest in order to have a consistent sample. Appendix Figure A1 shows that my results are robust to the specific choice in bandwidth length.

[11] The number of observations included in the first-stage sample are reported in Appendix Table A2.

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 for AAP by race and free/reduced-price lunch status in Appendix Table A2. I find that qualifying for AAP has a stronger impact on the likelihood a student takes a relevant dual credit course for FRPL students (17% increase) compared to never-FRPL students (statistically insignificant 12% 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 (34% increased) compared to students that identify as non-White (statistically insignificant 8% 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.[12]

Given that students qualifying for AAP are more likely to take relevant dual credit courses, I examine whether the increase in participation is driven by any particular class type. Specifically, 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). Appendix Table A3 reports

[12] In Appendix Table A2, I show that the null effect using the math cutoff holds does not differ across subgroups.

the results of Equation 1 for each dual credit 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 IB, 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.[13] 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^{th} and 12^{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^{th} and 12^{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 suggest that automatic enrollment has an similar effect size when compared to programs that expand the availability of these courses. However, it is unlikely that the two types of programs expand access to the same group of students.

V.A.1 Validity Checks

The previous section shows that qualification for the Academic Acceleration program is associated with an increased 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 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, Panel A of Table 3 reports the first-stage results at four placebo cutoffs. This falsification test replaces the true cutoff by another value (0.5 and 0.6 standard deviations below and above the actual cutoff) at which treatment status does not really

[13] Among 9-12 schools, 76 percent offer AP courses, 22 percent offer pre-AP courses, and 5 percent offer IB courses (De La Rosa, 2024).

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 the estimates 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. Panels B and C of Table 3, therefore, presents the results of a series of falsification tests to examine the sensitivity of my findings. Panel B 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 B of Table 3 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 specifications, 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 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. To assuage concerns over the specific bandwidth choice, Appendix Figure A1 reports the results of my first-stage estimates when I vary the length of the bandwidth from 0.1 to 0.48 in 0.02 increments. I still find consistent evidence that qualification for the Academic Acceleration Program lead to an increase in dual credit course taking.

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. Appendix Figure A2 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 4 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.[14]

The results of Table 4 show that 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.[15] However, it is important to note that while high school final grade point average was not improved by the qualification for AAP, students just above the cutoff did not see declines in this outcome. Specifically, I can rule out any effect size larger than .07 (off a base mean of 2.63). This result is important because if the rigor of advanced coursework was inappropriate for the group of students just qualifying, it is likely we would see a negative effect on grades.

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 whether the student identifies as White, has ever qualified for subsidized lunch, or identifies as Male. I calculated these estimates by splitting the sample by the particular subgroup. Columns (2)-(4) of Table 4 displays the results of the heterogeneity analysis by student subgroup for each of educational outcomes of interest. Similar to the results shown in Column (1) of Table 4, there does not seem to be systematic evidence that qualifying for AAP improves educational outcomes for any particular subgroup.

[14] I also examine whether qualification for AAP leads to changes in the institutional quality of enrolled postsecondary institutions. I define quality as the graduation rate within 150% of normal time for the student's identified race at enrolled institutions. Similarly to the main results, I find no effect of AAP qualification on this outcome as shown in Appendix Figure A5.

[15] Appendix Figure A4 presents the corresponding plots for the reduced-form analysis.

My findings are 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

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. However, the 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. It is important to consider the potential drawbacks of these programs, such as the possible extra cost for students to participate (Washington Student Achievement Council, 2017) and the potential for these courses to place extra stress on students (Suldo and Shaunessy-Dedrick, 2013). As policymakers continue to discuss the expansion of these programs, its important to understand for which groups of students these classes are beneficial.

There are several possible explanations for why automatic enrollment into advanced coursework does not alter educational outcomes. First, previous research suggests that simply expanding access to these courses is not sufficient (Hemelt and Swiderski, 2021), additional support is likely necessary to improve outcomes. Second, it is possible that the benefits of advanced coursework are only realized when the benchmark for college credit is met (Smith et al., 2017; Gurantz, 2021). Therefore, greater access may only be beneficial if it increases the student's likelihood of receiving college credit. Lastly, where the cutoff is for qualification matters and the program may be more effective if targeted at a different group of 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.

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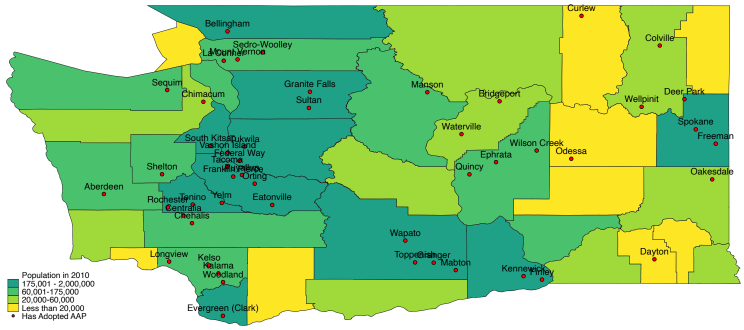
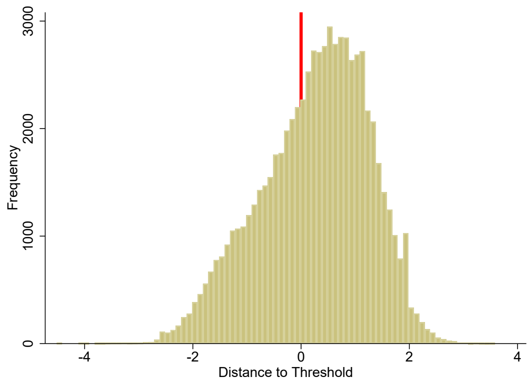
Figure 1: Adopting School Districts between 2012-2018



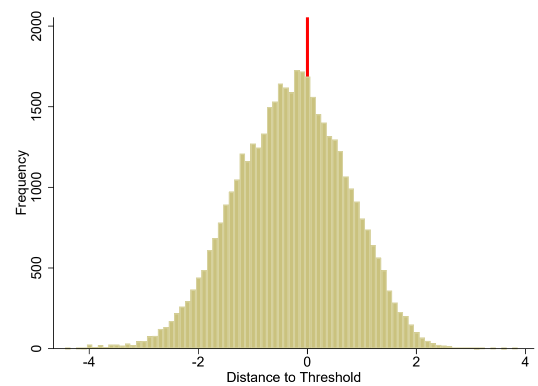
Notes: This map presents the locations of each school district that had adopted an Academic Acceleration Program prior to the enactment of HB 1599 against the 2010 population of each county in the state. Implementation dates for AAP was provided by the non-profit Stand for Children. 2010 population counts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

Figure 2: Scaled Test Score Histograms

(a) English Language Arts



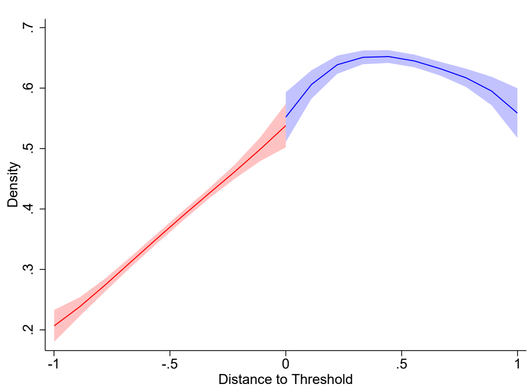
(b) Mathematics



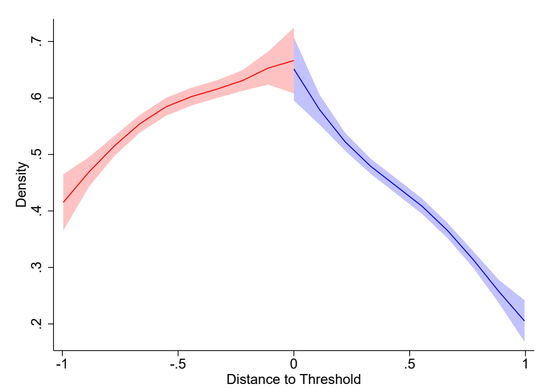
Notes: This figure presents the distributions of the English Language Arts and Mathematics SBA scores over the sample period. The solid, red line highlights the cutoff for eligibility into the Academic Acceleration Program. Data on students' test scores and cutoff for eligibility come from the ERDC.

Figure 3: McCrary (2008) Density Test Results

(a) English Language Arts



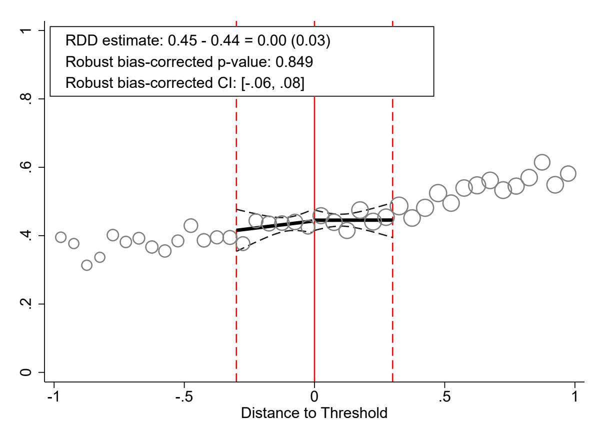
(b) Mathematics



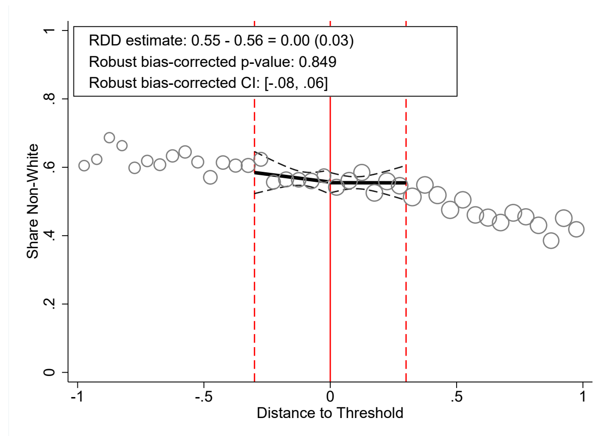
Notes: This figure presents the results of the McCrary (2008) density test for ELA (Panel A) and Math (Panel B) test scores across the eligibility threshold. The red lines and confidence intervals indicates the observations below the threshold, while the blue lines and confidence intervals indicate the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

Figure 4: Covariate Balance Checks

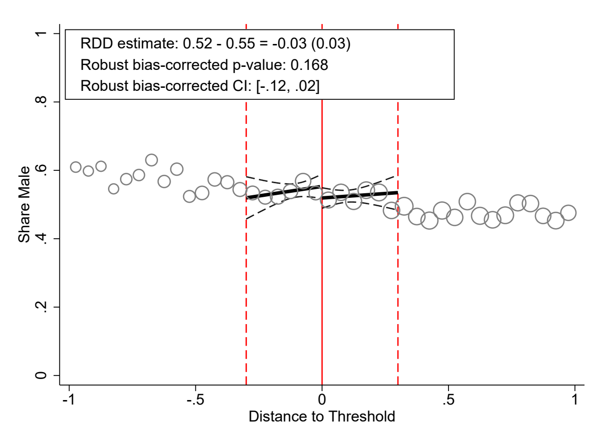
(a) Share White



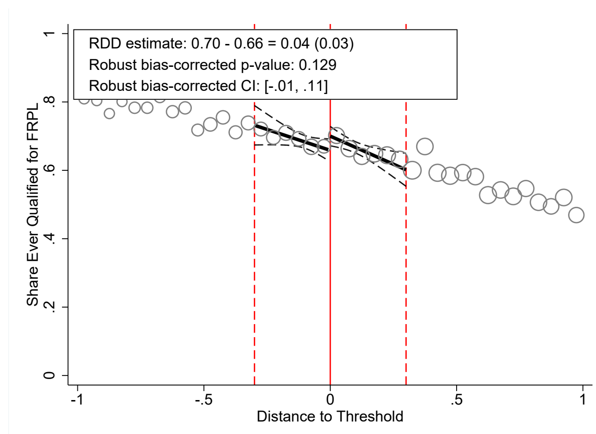
(b) Share Non-White



(c) Share Male



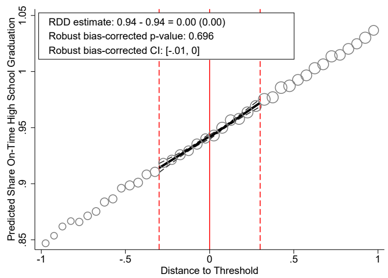
(d) Share Ever FRPL



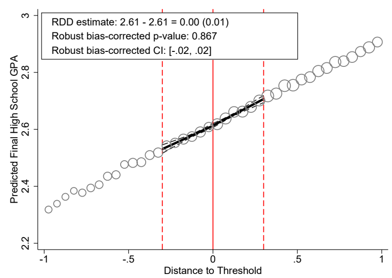
Notes: This figure shows descriptive characteristics of students by the running variable for students from 2014-2015 through 2002-2021. I impose a linear fit on either side of the threshold. Each dot represents the average of the descriptive characteristic for the bins of width 0.05 standard deviations. Data on student characteristics comes from the ERDC. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

Figure 5: Predicted Outcomes

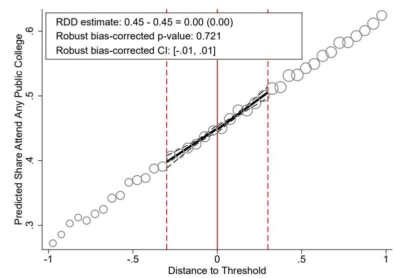
(a) On-Time Graduation



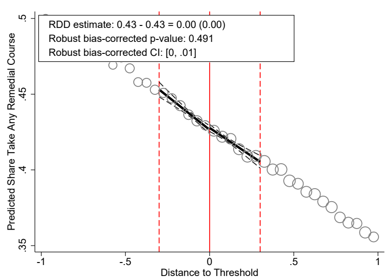
(b) Final HS GPA



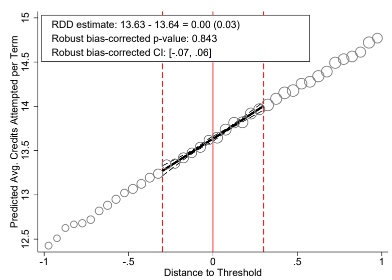
(c) Attend Any Public College



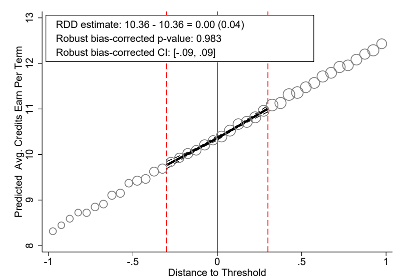
(d) Any Remedial ELA Courses



(e) Average Credits Attempted



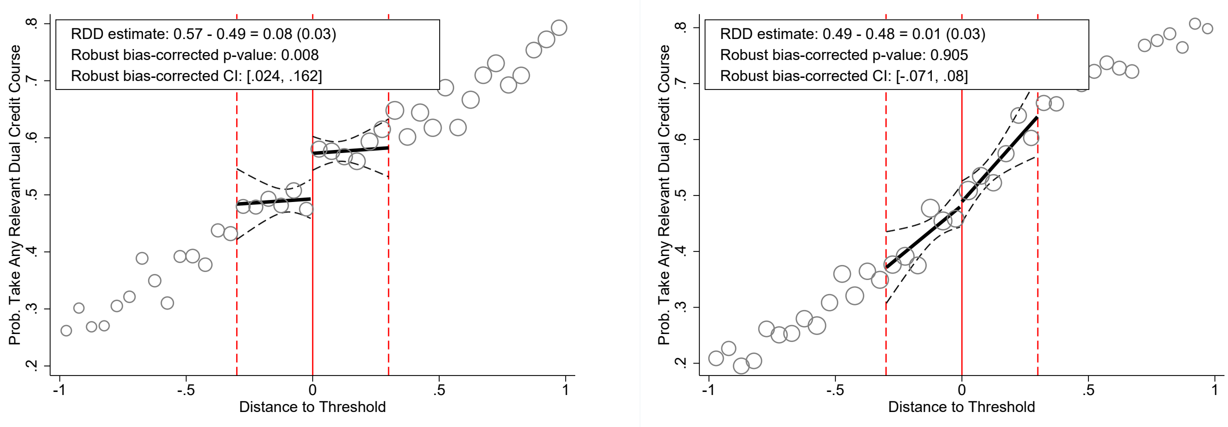
(f) Average Credits Earned



Notes: This figure shows average predicted educational outcomes for evenly-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 outcome for the bins of width 0.05 standard deviations. 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. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

Figure 6: First-Stage Results of AAP Eligibility

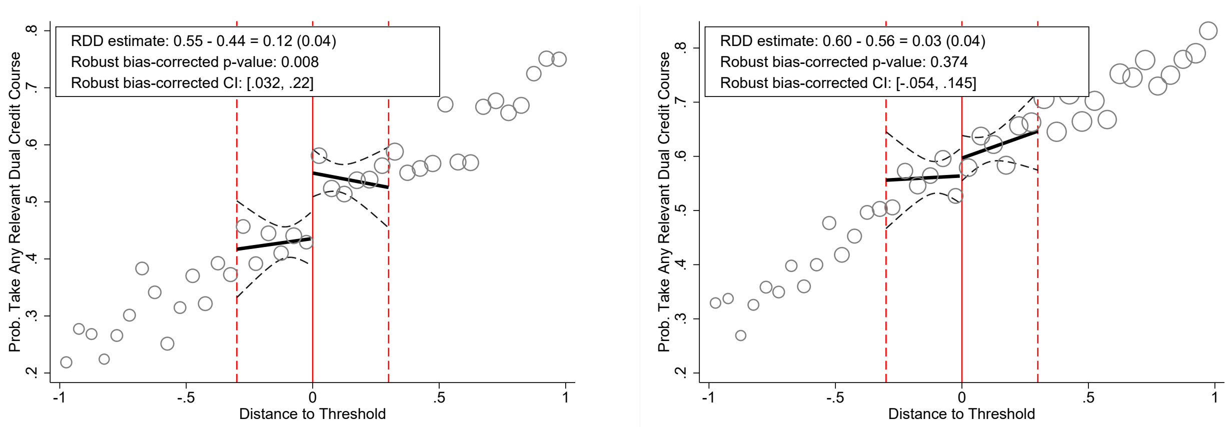
(a) English Language Arts (b) Mathematics



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 outcome for the bins of width 0.05 standard deviations. 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. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

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 outcome for the bins of width 0.05 standard deviations. Data on courses and test scores comes from the ERDC. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

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	0.523
	(0.500)	(0.500)	(0.499)
White	0.467	0.435	0.439
	(0.499)	(0.496)	(0.496)
Ever Subsidized Lunch	0.642	0.621	0.672
	(0.480)	(0.485)	(0.470)
Panel B: AAP Participation			
Take Any Dual Credit Course	0.673	1.00	0.653
	(0.469)	(0.000)	(0.476)
Number of Dual Credit Courses	1.908	2.937	1.675
	(1.973)	(1.775)	(1.779)
Panel C: High School Milestones			
On-Time Graduation	0.938	0.966	0.936
	(0.242)	(0.181)	(0.245)
Final Grade Point Average	2.761	2.898	2.663
	(0.633)	(0.600)	(0.604)
Panel D: Postsecondary Outcomes			
Attend Any Public College	0.354	0.389	0.355
	(0.478)	(0.487)	(0.478)
Average Credits Attempted Per Term in Year 1	13.893	14.230	13.583
	(6.424)	(6.281)	(6.305)
Average Credits Earned Per Term in Year 1	10.829	11.236	10.255
	(7.006)	(6.998)	(6.918)
Number of Observations	16,757	11,272	5,543

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 0.3 around the eligibility threshold. Student-level data comes from the ERDC database.

Table 3: First-Stage Results Robustness Checks

Panel A: Placebo Cutoffs				
	Cutoff at -0.6	Cutoff at -0.5	Cutoff at 0.5	Cutoff at 0.6
RDD Estimate	-0.061	0.004	0.023	0.031
Robust BC 95% CI	[-.154 ; .013]	[-.078 ; .08]	[-.028 ; .09]	[-.02 ; .099]
Robust BC p-value	0.100	0.980	0.300	0.191
Observations Left	1,840	2,421	5,305	5,434
Observations Right	3,564	3,800	5,147	4,092
Panel B: Donut Estimation				
	Donut Size 0.01	Donut Size 0.02	Donut Size 0.03	Donut Size 0.04
RDD Estimate	0.067	0.079	0.079	0.079
Robust BC 95% CI	[.002 ; .16]	[.012 ; .185]	[.007 ; .197]	[0 ; .208]
Robust BC p-value	0.0450	0.0259	0.0355	0.0496
Observations Left	3,725	3,646	3,572	3,508
Observations Right	5,224	5,150	5,068	4,979
Panel C: Kernel Type				
		Triangular	Uniform	Epanechnikov
RDD Estimate	-	0.080	0.066	0.076
Robust BC 95% CI	-	[.024 ; .162]	[.013 ; .142]	[.022 ; .158]
Robust BC p-value	-	0.008	0.019	0.009
Observations Left	-	3,800	3,800	3,800
Observations Right	-	5,389	5,389	5,389

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. The bandwidth for each estimate is set at 0.3 standard deviations away from the cutoff.

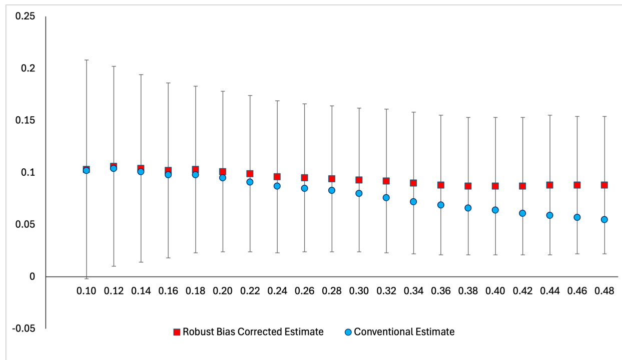
Table 4: Reduced-Form Effects of AAP Eligibility on Education Outcomes

	Entire Sample	White	Ever FRPL	Men
	(1)	(2)	(3)	(4)
On-Time Graduation				
Estimate	0.002	-0.002	0.009	-0.017
Robust BC 95% CI	[-.035 ; .067]	[-.037 ; .12]	[-.037 ; .095]	[-.074 ; .074]
Robust BC p-value	0.543	0.295	0.383	0.997
Final HS GPA				
Estimate	-0.015	-0.025	0.044	-0.043
Robust BC 95% CI	[-.072 ; .108]	[-.159 ; .116]	[-.056 ; .163]	[-.177 ; .069]
Robust BC p-value	0.695	0.760	0.337	0.391
Any Public College				
Estimate	-0.040	-0.047	-0.022	-0.041
Robust BC 95% CI	[-.113 ; .035]	[-.153 ; .067]	[-.11 ; .069]	[-.139 ; .057]
Robust BC p-value	0.297	0.448	0.653	0.409
Any Remedial ELA				
Estimate	-0.069	-0.005	-0.051	-0.125
Robust BC 95% CI	[-.18 ; .033]	[-.179 ; .158]	[-.174 ; .089]	[-.306 ; -.006]
Robust BC p-value	0.176	0.903	0.527	0.0421
Attempted Credits				
Estimate	0.131	0.594	0.623	-0.290
Robust BC 95% CI	[-1.431 ; 1.876]	[-2.401 ; 3.609]	[-1.118 ; 3.058]	[-2.549 ; 2.117]
Robust BC p-value	0.792	0.694	0.363	0.856
Earned Credits				
Estimate	-0.016	0.796	0.543	-0.228
Robust BC 95% CI	[-1.632 ; 1.765]	[-1.976 ; 3.985]	[-1.382 ; 2.984]	[-2.645 ; 2.037]
Robust BC p-value	0.939	0.509	0.472	0.799

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. The bandwidth for each estimate is set at 0.3 standard deviations away from the cutoff.

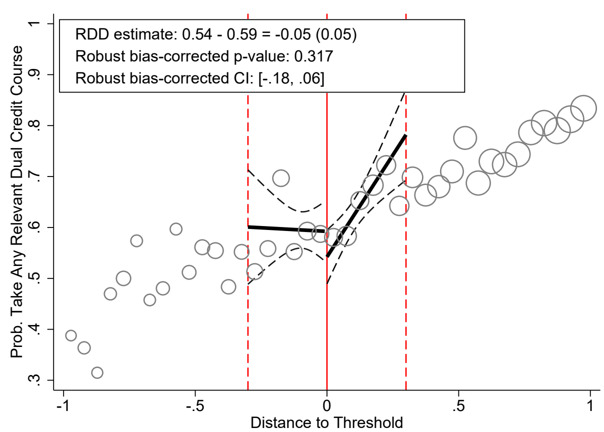
A1 Appendix Figures and Tables

Figure A1: First-Stage Robustness to Bandwidth Choice



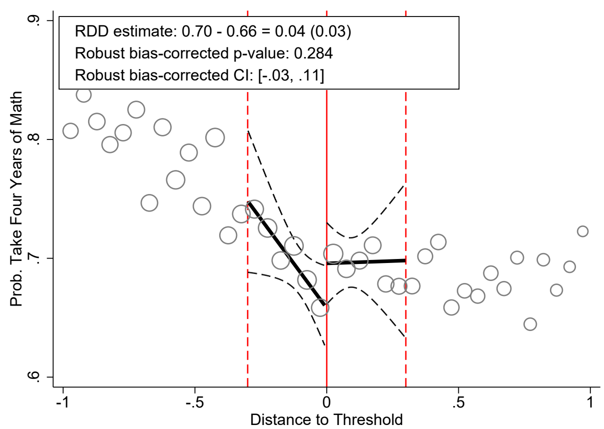
Notes: This figure shows the first-stage results of AAP eligibility on dual credit course taking when varying the bandwidth from 0.1 standard deviations away from the cutoff to 0.48 standard deviations. The red boxes report the robust bias corrected treatment effects, which are not otherwise reported. They are plotted here since the confidence intervals are centered around these estimates. The blue dots report the conventional estimates, which are reported throughout the paper.

Figure A2: 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 outcome for the bins of width 0.05 standard deviations. Data on courses and test scores comes from the ERDC. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

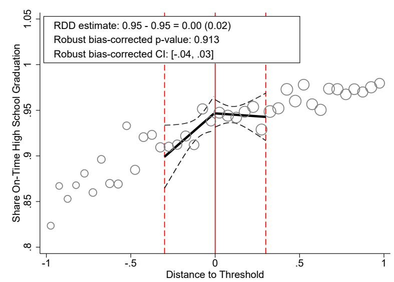
Figure A3: AAP Eligibility on Taking Four Years of Math



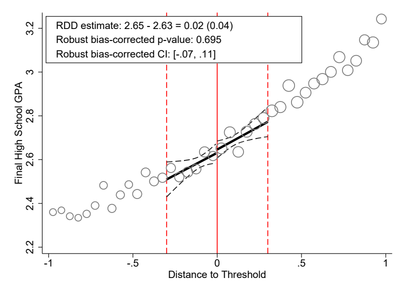
Notes: This figure shows the average likelihood of taking four years of math classes between the 2014-2015 and 2020-2021 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the outcome for the bins of width 0.05 standard deviations. Data on courses and test scores comes from the ERDC. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

Figure A4: Reduced-Form Results of AAP Eligibility

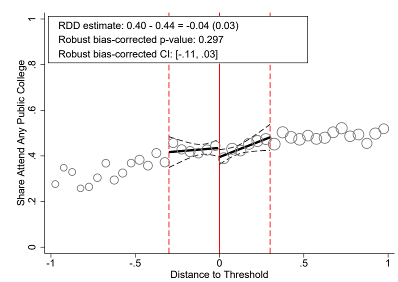
(a) On-Time Graduation



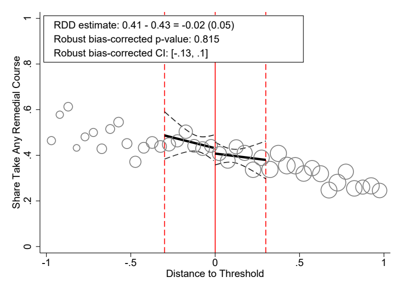
(b) Final HS GPA



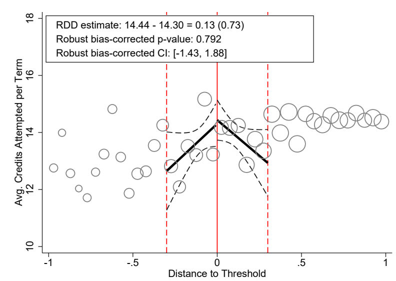
(c) Attend Any Public College



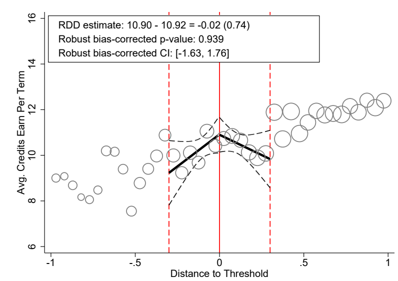
(d) Any Remedial ELA Courses



(e) Average Credits Attempted

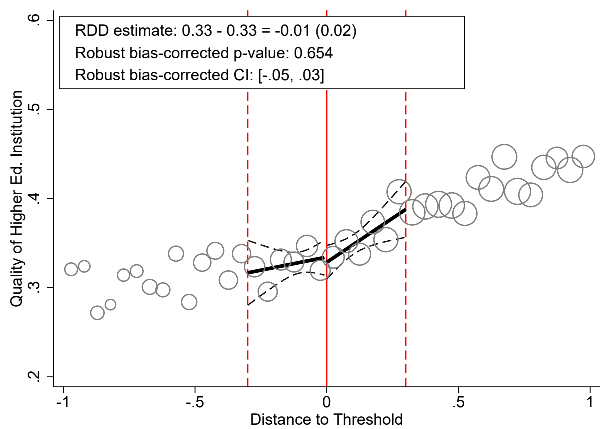


(f) Average Credits Earned



Notes: This figure shows the reduced-form effects of AAP eligibility on high school and college outcomes for students between the 2014-2015 and 2020-2021 acedmic years. A linear fit is imopsed on either side of the threshold. Each dot represents the average of the outcome for the bins of width 0.05 standard deviations. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

Figure A5: AAP Eligibility on Institutional Quality



Notes: This figure shows the average quality of the institution attended by the running variable for students between the 2014-2015 and 2020-2015 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the outcome for the bins of width 0.05 standard deviations. Data on courses and test scores comes from the ERDC. The bandwidth for each figure is set at 0.3 standard deviations away from the cutoff and there are 20 bins on either side.

Table A1: Summary Statistics Comparing First-Stage and Outcomes 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
	(0.499)	(0.499)
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	16,757	13,591

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: First-Stage Results by Student Characteristics

	Full Sample	White	Non-White	Ever FRPL	Never FRPL	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ELA Cutoff							
RDD Estimate	0.080	0.116	0.053	0.087	0.057	0.115	0.033
Robust BC 95% CI	[.024 ; .162]	[.032 ; .234]	[-.025 ; .153]	[.023 ; .19]	[-.068 ; .179]	[.032 ; .22]	[-.054 ; .145]
Robust BC p-value	0.008	0.010	0.157	0.012	0.380	0.009	0.374
Observations Left	3,800	1,587	2,213	2,692	1,108	2,065	1,735
Observations Right	5,389	2,500	2,889	3,428	1,961	2,699	2,690
Control Mean	0.49	0.35	0.61	0.50	0.48	0.44	0.56
Panel B: Math Cutoff							
RDD Estimate	0.007	0.012	0.008	0.001	0.011	-0.022	0.039
Robust BC 95% CI	[-.071 ; .08]	[-.091 ; .115]	[-.108 ; .116]	[-.105 ; .103]	[-.101 ; .125]	[-.127 ; .084]	[-.072 ; .140]
Robust BC p-value	0.905	0.820	0.946	0.982	0.839	0.689	0.527
Observations Left	3,457	1,781	1,676	2,029	1,428	1,701	1,756
Observations Right	2,894	1,721	1,173	1,397	1,497	1,428	1,466
Control Mean	0.48	0.43	0.55	0.45	0.52	0.46	0.50

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. Control mean is defined as the average of the outcome variable for the group of student just below the cutoff. The bandwidth for each estimate is set at 0.3 standard deviations away from the cutoff.

Table A3: ELA First-Stage Results by Course Type

	Any ELA Tech Prep	Any ELA IB	Any ELA CHS	Any ELA AP	Any ELA RS	Any ELA Cambridge
	(1)	(2)	(3)	(4)	(5)	(6)
RDD Estimate	-0.002	0.041	0.009	0.051	0.022	-0.005
Robust BC 95% CI	[-.012 ; .009]	[.017 ; .079]	[-.043 ; .057]	[-.005 ; .124]	[.001 ; .049]	[-.032 ; .02]
Robust BC p-value	0.731	0.003	0.782	0.071	0.039	0.635
Observations Left	3,800	3,800	3,800	3,800	3,800	3,800
Observations Right	5,389	5,389	5,389	5,389	5,389	5,389

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. The bandwidth for each estimate is set at 0.3 standard deviations away from the cutoff.

Table A4: First-Stage Results Using Outcomes Sample

	Full Sample	White	Non-White	Ever FRPL	Never FRPL	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ELA Cutoff							
RDD Estimate	0.108	0.154	0.077	0.124	0.069	0.162	0.036
Robust BC 95% CI	[.053 ; .205]	[.067 ; .288]	[-.002 ; .195]	[.058 ; .243]	[-.06 ; .211]	[.082 ; .286]	[-.058 ; .163]
Robust BC p-value	<0.001	0.002	0.0540	0.001	0.275	<0.001	0.352
bservations Left	3,140	1,315	1,825	2,217	923	1,717	1,423
Observations Right	4,406	2,058	2,348	2,781	1,625	2,226	2,180
Panel B: Math Cutoff							
RDD Estimate	0.003	-0.019	0.040	0.003	-0.003	0.020	-0.008
Robust BC 95% CI	[-.08 ; .094]	[-.139 ; .101]	[-.081 ; .176]	[-.114 ; .128]	[-.131 ; .131]	[-.093 ; .148]	[-.131 ; .113]
Robust BC p-value	0.878	0.757	0.469	0.911	>0.999	0.657	0.881
Observations Left	2,478	1,264	1,214	1,436	1,042	1,215	1,263
Observations Right	2,134	1,295	839	1,013	1,121	1,079	1,055

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. The bandwidth for each estimate is set at 0.3 standard deviations away from the cutoff.