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 12 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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The research presented here uses confidential data from the Education Research and Data Center (ERDC) located within the Washington Office of Financial Management (OFM). ERDC's data system is a statewide longitudinal data system that includes de-identified data about people's preschool, educational, and workforce experiences. The views expressed here are those of the authors and do not necessarily represent those of OFM or other data contributors. Any errors are attributable to the authors.

I Introduction

The lack of academic preparation of college-going students is an ongoing concern. During the 2003-2004 academic year 34.8% of first-year undergraduate students were required to take a remedial class upon entering tertiary education. That number has since grown, reaching 43% in the 2014-2015 academic year (National Center for Education Statistics, 2018). Policymakers have suggested that one solution to this problem 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 in the literature on 401(k) savings (Madrian and Shea, 2001) and organ donations (Li et al., 2013), is to change the default option. Since 2012, school districts across Washington state have adopted a program, Academic Acceleration (AAP), that switches participation in advanced coursework to the default based on students' test scores. AAP, 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 12 percentage points (p.p.) more likely to ever take a relevant dual-credit course. Off a base mean of 44 percent, this result suggests that AAP increased participation in dual-credit classes by 27.3 percent. Interestingly, the increase in dual-credit course taking is driven by boys. Boys who score just above the ELA threshold for AAP are 16 p.p. more likely to take a relevant dual-credit course compared to a statistically insignificant increase of 7 p.p. for just qualifying girls.

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

The baseline dual-credit participation rate for boys is nearly 10 p.p. lower than that of girls, which suggests that AAP may be serving as a catch-up mechanism for boys. 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 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 attempted and earned credits in their first year enrolled in college. The reduced-form result suggests that on average, those just qualifying for AAP, attempt 5.42 and earn 5.95 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. However, one must consider that the education outcomes sample relies solely on the earliest cohorts. Future work will expand the set of students included in the analysis.

I also show that the 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 show that the magnitude and significance of my first-stage results are robust to bandwidth and kernel choices. Second, 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.

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 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. In Section II, I provide 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 preliminary results of the program, which include the first-stage results, heterogeneity analysis, the validity checks, and the reduced-form results. Section VI contains the results for choice schools. Section VII discusses some limitations to the findings, and Section VII 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, History and Humanities. For Math, they qualified to take advanced math 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. 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 GPA, 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 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.

[2] Within my sample period the year students take the SBA switched from the 10th to 11th grade.

From 2012 to 2018, nearly 50 other 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 including Federal Way, Franklin Pierce, South Kitsap, Spokane, Sultan, Tacoma, Tukwila, and Yelm School Districts that had adopted AAP before 2016. 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, 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$^{\text{th}}$-12$^{\text{th}}$ 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 high shares of low-income students compared to adopting districts, but otherwise are comparable.

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 Math and their corresponding cutoffs. The cutoff for eligibility into the program is in the middle of both distributions, suggesting that AAP is targeting average students, which may be of particular interest to policy makers considering expanding these programs to include a much larger group of students.

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 9$^{\text{th}}$ through 12$^{\text{th}}$ grades between the 2014-2015 and 2018-2019 academic years. Each of the students included in the sample took the Smarter Balanced Assessment either in the 10$^{\text{th}}$ or 11$^{\text{th}}$ 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[3], 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 11,750 students.[4]

Table 2 presents the summary statistics of students in different subsets of the sample. In the full sample, as shown in Column 1, 10.9 percent of students are Black, 49.5 percent are White, and 61.4 percent ever qualified for subsidized lunch. Compared to the full sample, students that enroll in a dual-credit class are slightly less male, more non-White and more likely to ever qualify for subsidized lunch. About 40 percent of students in the sample ever take a dual-credit class, with the unconditional average of number of dual-credit classes equaling 0.77. Conditional on taking one dual-credit class, the average student takes around 2 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 more non-White and more likely to 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 (97.1% versus 94.1%) and enroll in public colleges at higher rates (47.7% versus 45.9%). However, on average, students who take a dual-credit class attempt and earn fewer credits in their first year of college (31.38 & 26.30 versus 32.01 & 26.91). 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

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

[4]However, nearly half of my sample is not included in the reduced-form estimation because the students had not graduated from high school yet. In Appendix Table A1, I show how the summary statistics compare for students in the entire sample, first-stage sample and outcome sample.

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 Reduced-Form 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[5]. 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 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

[5]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.

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 Math 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[6] (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:

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

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

[6]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.

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 current specification estimates local linear regression with a triangular kernel of bandwidth around 0.3 on either side of the program cutoff. The triangular kernel weights points near the threshold more heavily that those farther from the threshold. I show robustness of my first-stage estimates to several bandwidth and kernel choices in Appendix Tables A2 and A3 (Calonico et al., 2017, 2018).

V Preliminary Results

V.A Effects of Qualification on Dual-Credit Participation

First-stage estimates of AAP for ELA and Math are presented in Figure 6.[7] 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 12 percentage points more likely to enroll in a relevant course. Off a base mean of 44 percent, this suggests that the Academic Acceleration program increased participation in dual-credit classes by 27.3 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.[8] 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-

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

[8] All students are required to take four years of English.

credit classes at higher rates (Burns and Leu, 2019) and have been shown to be more responsive to interventions than boys (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 boys. The first panel shows that boys just above the ELA threshold for AAP are 16 percentage points more likely to participate in a relevant dual-credit course and off a base mean of 40 percent, the result suggest an increase of 40%. The second panel shows statistically insignificant increases for girls just above the threshold. The results by gender suggest that AAP is serving as a catch-up mechanism for boys, since their baseline participation rate is nearly 10 p.p. below that of girls.

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 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 (35% increase) compared to never-FRPL students (statistically insignificant 16% increase). These results suggest that the adoption of Academic Acceleration achieved its original goal of increasing access to dual-credit classes for this group of students. On the race dimension, qualification for AAP had similar sized effects for White and non-White students. However, the two groups differ in their baseline levels of participation which results in larger percentage increases in dual-credit course taking for White students. This result may be driven by Asian students as they have the highest levels of dual-credit participation (U.S. Department of Education, 2012), but due to data limitations I cannot confirm this statement. I 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 five types dual-credit courses offered in Washington state: Advanced Placement (AP), International Baccalaureate (IB), Cambridge International (Cambridge), College in High School (CHS), and Running Start (RS). Table 3 reports the results of Equation 1 for each dual-credit class type.

I find that compared to students who just missed qualifying for AAP, just qualified students were more likely to take ELA relevant AP, IB, and College in High School courses. Students saw the greatest increases in their likelihood to take AP courses. Off a control mean of 27 percent, an increase of 5.8 p.p. suggests an 21.4 percent increase in the likelihood that a student takes a relevant

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

ELA AP class. 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 11th and 12th 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 12 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 11th and 12th 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 dual enrollment than those in the control group. Overall, my findings suggest that qualifying for AAP has similar effects on dual-credit course taking as other programs across the country.

To ensure the first-stage results I find are due to the policy rather than some other change at the cutoff score, I estimate a falsification test using students that attended Seattle Public School District. Seattle Public School District had not adopted an Academic Acceleration program during my sample time period, but students were still required to take the Smarter Balanced Assessment. Therefore, I can test whether there is jump in the likelihood students take dual-credit 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.

V.B Reduced-Form Effects of Academic Acceleration

In this section, I present the reduced-form results of AAP eligibility on subsequent educational outcomes. Figure 10 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

[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).

just qualifying for AAP did see statistically significant decreases in the number of attempted and earned credits in their first year enrolled. The reduced-form results suggest that on average, those that just qualified for AAP decreased the number of credits they attempted in their first year of college by 5.42 and number of credits they earned by 5.95. 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 under-studied 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. Table 4 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 (1) and (2) display the reduced-form results for Men and Women, Columns (3) and (4) display the results for White and Non-White students and Columns (5) and (6) display the results for students ever qualifying for FRPL and never qualifying for FRPL.

Similar to the results shown in Figure 10, there does not seem to be systematic evidence that qualifying for AAP improves educational outcomes for any particular subgroup. In the first four panels of Table 4 only 2 of the 24 coefficients are statistically significant at the 10 percent level. However, there is some suggestive evidence that for groups where the ELA first-stage results were particularly strong (White and Ever FRPL students), qualification for AAP lead to an increased likelihood of on-time graduation, and higher final grade point averages. I take these results as evidence that once the analysis can be completed on the full sample of students, the conclusions about the program may change. The subgroup analysis on the number of credits attempted and earned in the first year of college are similar to the results for the overall sample. While the exact coefficient and significance level varies slightly across the columns of Table 4, the story that qualification for AAP reduces the number of credits attempted/earned in the first year remains consistent.

However, I refrain from concluding that qualifying for Academic Acceleration reduces the number of credits students attempted and earned for two reasons. First, when I run a joint hypothesis test for the effect of the program across all outcomes of interest within each subgroup, I fail to reject the null hypothesis that there is no effect of qualifying for AAP on subsequent educational outcomes. The results of the joint hypothesis test can be found in Appendix Table A4.[11] Second, differential attrition can be a concern because of the timing of available data. In Figure A3, I show that students just above the cutoff for qualifying for Academic Acceleration are five percentage points less likely to appear in the outcomes sample compared students just below the threshold. Therefore, the results on the number of credits attempted and earned may be a function of the differential attrition rather than the program itself.[12]

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 Limitations

There are limitations to the findings I have presented. Specifically, the conclusion that qualifying for the Academic Acceleration Program has minimal effects on subsequent educational outcomes relies solely on data from the earliest cohorts in my sample. This restriction is necessary based on the years available in my dataset. Later cohorts of students, specifically those enrolled in the 10$^{\text{th}}$ and 11$^{\text{th}}$ grades between 2016 and 2017, would have taken their SBAC ELA and Math exams (and possibly gone on to take dual-credit classes), but would not have graduated high school before the

[11] Note, to conduct this exercise I had to use a different regression specification, which slightly altered the magnitude of some of the coefficients. Specifically, this specification uses uniform weighting compared to the triangular weighting used in the preferred specification.

[12] The issue of differential attrition will be resolved when I receive data on the later cohorts of students.

2018-2019 academic year. Since students did not graduate, or complete their high school career, I could not include them in the reduced-form analysis. This data limitation reduces my sample size by nearly 50 percent. The inclusion of these students may fundamentally change my findings. I am currently in the process of receiving more data on these later cohorts and hope to incorporate new results in the next iteration of the paper.

VII 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 27 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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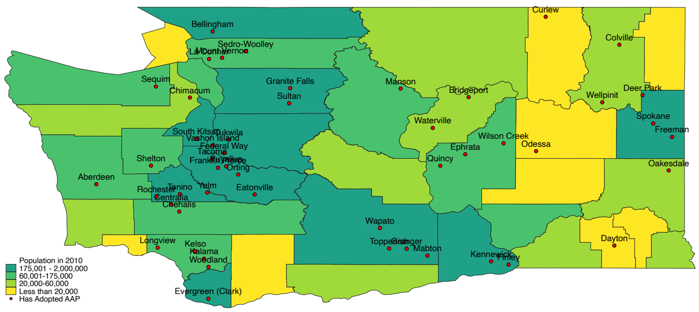
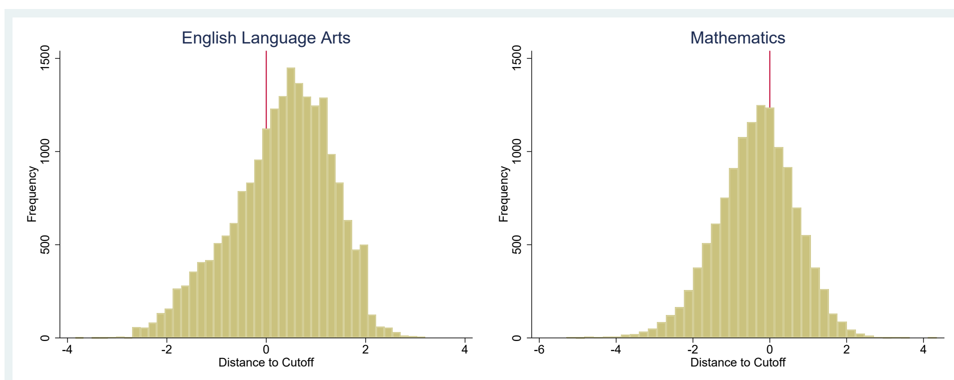
Figures

Figure 1: Adopting School Districts between 2012-2018



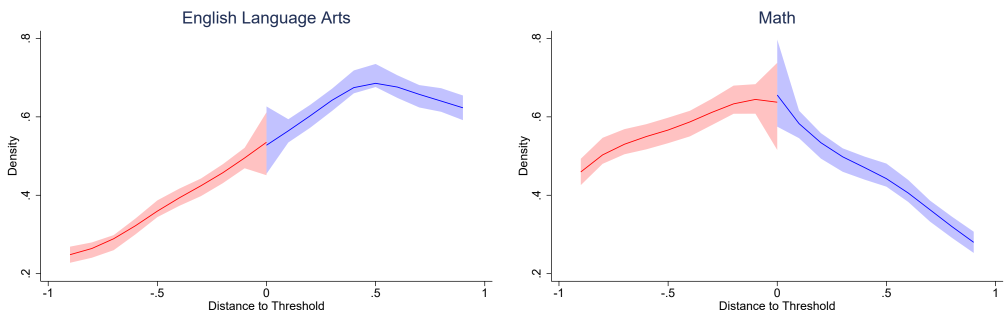
Notes: This map presents the locations of each school district that had adopted an Academic Acceleration Program prior to the enactment of HB 1599 against the 2010 population of each county in the state. Implementation dates for AAP was provided by the non-profit Stand for Children. 2010 Population count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

Figure 2: Distribution of Exam Score and Cutoff



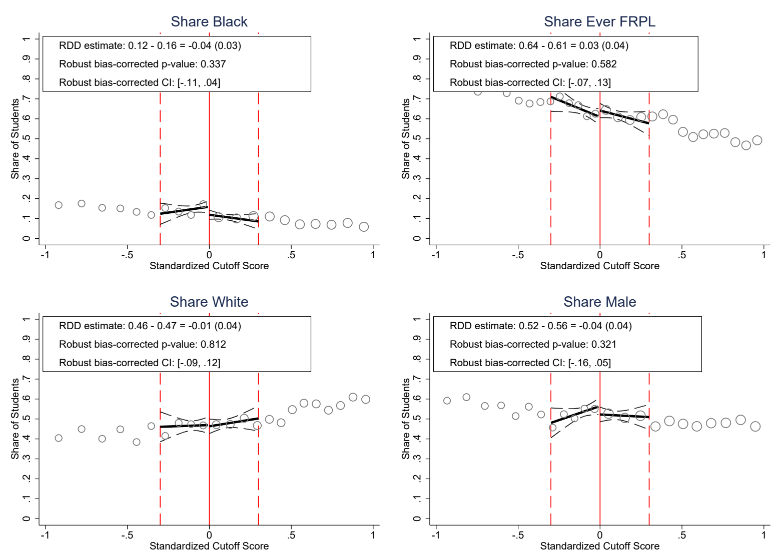
Notes: This figure presents the distributions of the English Language Arts and Mathematics SBA scores over the sample period. The solid, red line highlights the cutoff for eligibility into the Academic Acceleration Program. Data on students' test scores and cutoff for eligibility come from the ERDC.

Figure 3: McCrary (2008) Density Test



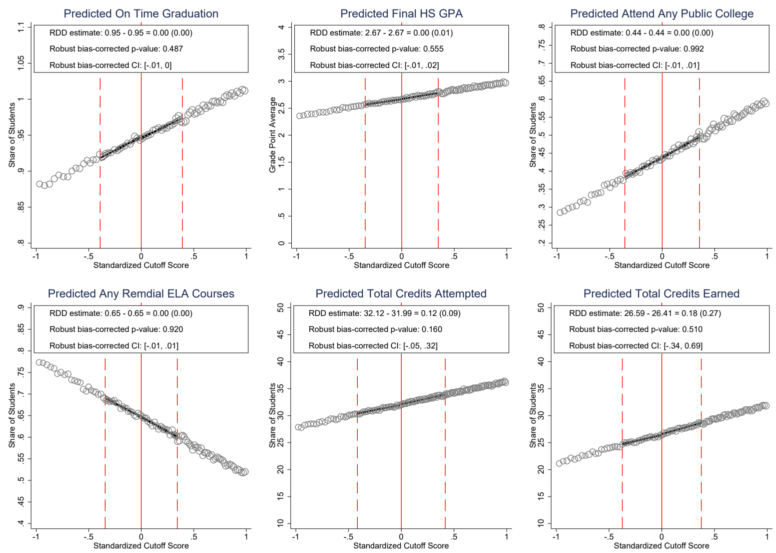
Notes: This figure presents the results of the McCrary (2008) density test for ELA (Panel A) and Math (Panel B) test scores across the eligibility threshold. The red lines and confidence intervals indicates the observations below the threshold, while the blue lines and confidence intervals indicate the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

Figure 4: Covariate Balance Checks



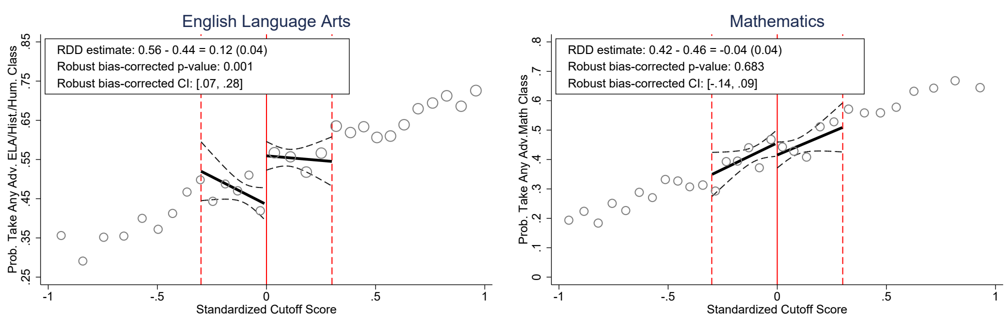
Notes: This figure shows descriptive characteristics of students by the running variable for students from 2014-2015 through 2018-2019. I impose a linear fit on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on student characteristics comes from the ERDC.

Figure 5: Predicted Outcomes



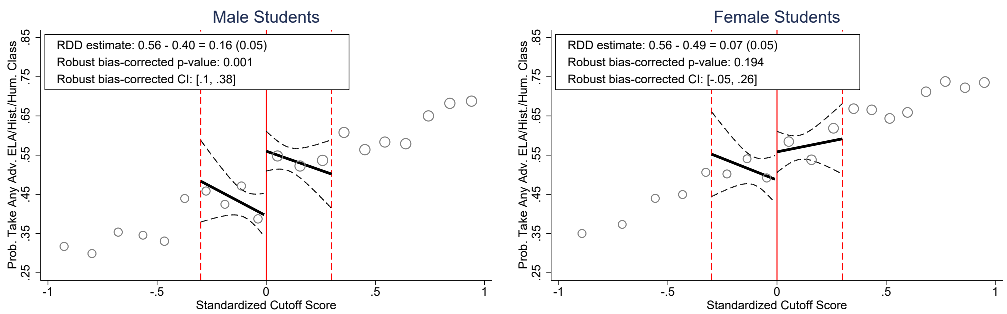
Notes: This figure shows average predicted educational outcomes for quantile-spaced bins on either side of the threshold. I impose a linear fit on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Predicted outcomes are generated 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



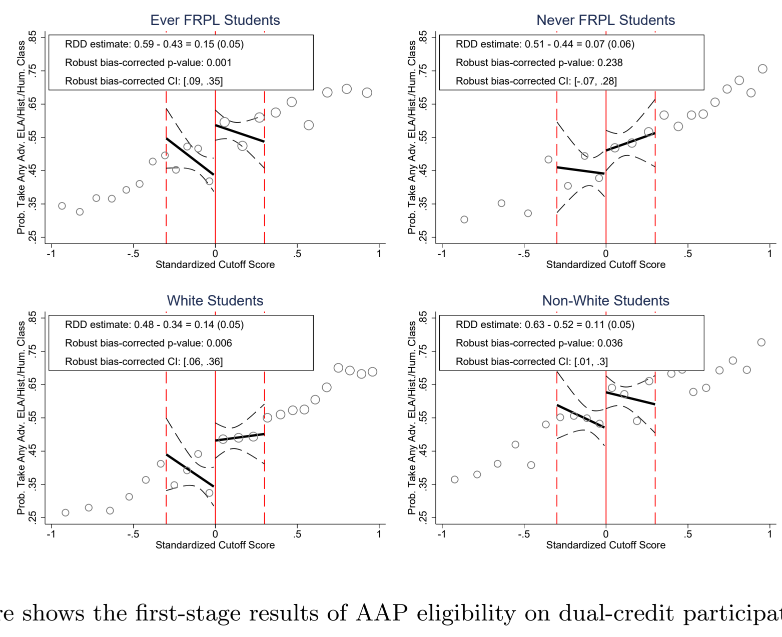
Notes: This figure shows dual-credit/AAP participation by the running variable for students between the 2014-2015 and 2018-2019 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Panel 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. $N = 1,655$ to the left of threshold and $N = 2,090$ to the right of the cutoff. Total observations in the the sample, 11,500.

Figure 7: ELA First-Stage Results by Gender



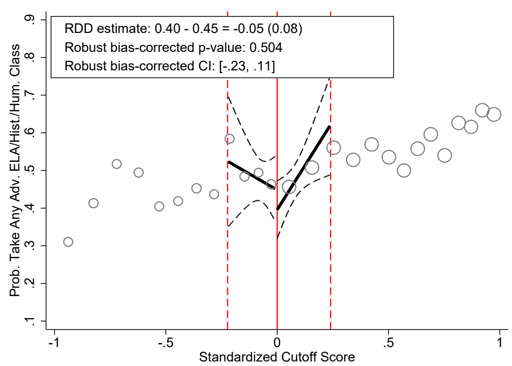
Notes: This figure shows the first-stage results of AAP eligibility on dual-credit participation by gender for students between the 2014-2015 and 2018-2019 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

Figure 8: ELA First-Stage by Subgroup



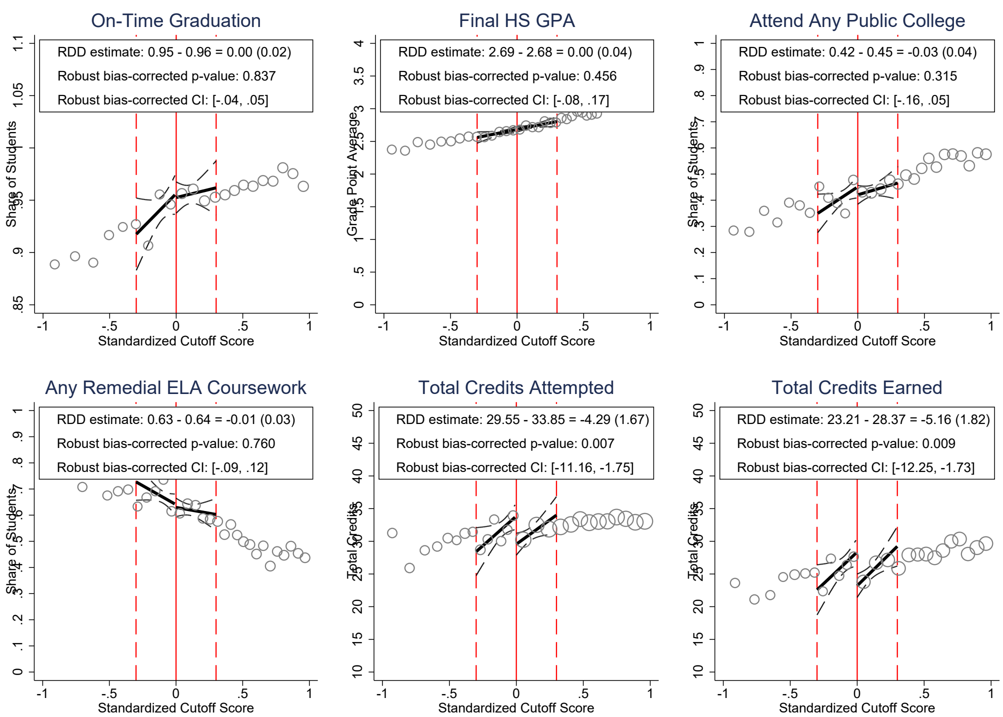
Notes: This figure shows the first-stage results of AAP eligibility on dual-credit participation by race and FRPL status for students between the 2014-2015 and 2018-2019 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic 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 2018-2019 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.

Figure 10: Reduced-Form Effects of Academic Acceleration



Notes: This figure shows average educational outcomes for students between the 2014-2015 and 2018-2019 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 educational outcomes 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^{th} Grade^b	13.19	12.15	11.46
% Met Standard on ELA SBAC- 11^{th} Grade^c	25.28	26.46	26.44

Notes a, b, c - Only includes information from districts that do not require the suppression of data. This table presents summary statistics for students attending grades 9^{th}-12^{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	0.506	0.470	0.508
	(0.499)	(0.499)	(0.499)
Black	0.109	0.134	0.119
	(0.312)	(0.341)	(0.325)
White	0.495	0.410	0.464
	(0.499)	(0.492)	(0.498)
Ever Subsidized Lunch	0.614	0.627	0.639
	(0.487)	(0.484)	(0.480)
Panel B: AAP Participation			
Take Any Adv. Class	0.400	1.000	0.397
	(0.490)	(0.000)	(0.489)
Number of Adv. Class	0.770	1.922	0.744
	(1.167)	(1.089)	(1.13)
Panel C: High School Milestones			
On-Time Graduation	0.941	0.971	0.941
	(0.234)	(0.168)	(0.236)
Final GPA	2.777	2.968	2.719
	(0.621)	(0.570)	(0.596)
Panel D: College Outcomes			
Any Public College	0.459	0.477	0.434
	(0.498)	(0.499)	(0.456)
Total Credits Attempted Year 1	32.013	31.381	31.691
	(15.404)	(15.669)	(15.393)
Total Credits Earned Year 1	26.910	26.296	26.231
	(16.199)	(16.222)	(16.091)

Mean values of each variable are shown by sample. Column (1) is the full sample of students. 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: ELA First-Stage Results

	Any ELA Dual Credit	Any ELA IB	Any ELA CHS	Any ELA RS	Any ELA AP	Any ELA Cambridge
	(1)	(2)	(3)	(4)	(5)	(6)
RDD Estimate	0.119	0.028	0.022	0.018	0.058	-0.005
Robust BC 95% CI	[.072 ; .279]	[-.014 ; .056]	[0 ; .127]	[-.06 ; .056]	[-.009 ; .18]	[-.045 ; .022]
Robust BC p-value	0.001	0.0225	0.0496	0.235	0.0775	0.507
Baseline Mean	0.44	0.03	0.10	0.02	0.27	0.04
Bandwidth	[.3; .3]	[.3; .3]	[.3; .3]	[.3; .3]	[.3; .3]	[.3; .3]
Observations Left	1655	1655	1655	1655	1655	1655
Observations Right	2090	2090	2090	2090	2090	2090

Standard errors (in parentheses) are clustered at the 11^{th} grade school district level. Sample includes students who took the SBAC ELA exam in high school in the sample districts. Each column represents a separate regression. 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.

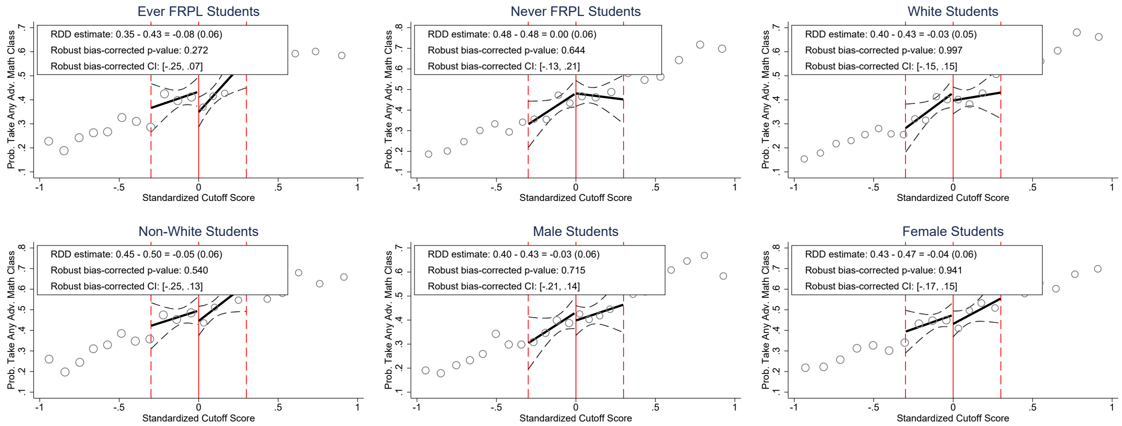
Table 4: Reduced-Form Results by Subgroup

	Men	Women	White	Non-White	Ever FRPL	Never FRPL
On-Time Graduation	-0.012	0.013	0.018	-0.016	0.011	-0.016
Robust BC 95% CI	[-.07 ; 0.05]	[-.05 ; .10]	[-.05 ; .11]	[-.07 ; .04]	[-.04 ; 0.08]	[-.09 ; .04]
Robust BC p-value	0.714	0.491	0.441	0.569	0.478	0.446
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Final HS GPA	-0.038	0.060	0.028	0.002	0.040	-0.02
Robust BC 95% CI	[-.22 ; .13]	[-.01 ; .33]	[-.09 ; .30]	[-.06 ; .24]	[-12.26 ; -1.71]	[-.20 ; .24]
Robust BC p-value	0.632	0.056	0.301	0.756	0.240	0.857
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Any Public College	-0.021	-0.054	0.004	-0.066	-0.058	0.02
Robust BC 95% CI	[-.18 ; .1]	[-.24 ; .08]	[-.13 ; .18]	[-.27 ; .02]	[-.25 ; .01]	[-.11 ; .25]
Robust BC p-value	0.560	0.315	0.745	0.086	0.07	0.436
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Any Remedial ELA	-0.022	0.008	-0.027	0.003	0.028	-0.088
Robust BC 95% CI	[-.13 ; .15]	[-.14 ; .18]	[-.20 ; .11]	[-.08 ; .21]	[-0.04 ; 0.22]	[-.30 ; .04]
Robust BC p-value	0.854	0.792	0.553	0.359	0.160	0.131
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Attempted Credits	-5.653	-2.862	-3.696	-4.823	-4.275	-4.397
Robust BC 95% CI	[-13.33 ; .104]	[-12.90 ; .33]	[-14.58 ; 0.45]	[-12.48 ; -.14]	[13.467 ; -1.75]	[-13.25 ; 3.31]
Robust BC p-value	0.505	0.063	0.0653	0.045	0.011	0.239
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Earned Credits	-6.804	-3.485	-4.900	-5.452	-5.254	-5.027
Robust BC 95% CI	[-14.99 ; 0.499]	[-13.853 ; .608]	[-15.57 ; .83]	[-14.04 ; -.01]	[-14.33 ; -.80]	[-14.98 ; 2.38]
Robust BC p-value	0.067	0.073	0.078	0.0498	0.028	0.155
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340

Standard errors are clustered at the 11^{th} grade school district level. Sample includes students who took the SBAC ELA exam in high school in the sample districts. The table presents reduced-form effects of the policy and has not been scaled by the first-stage.

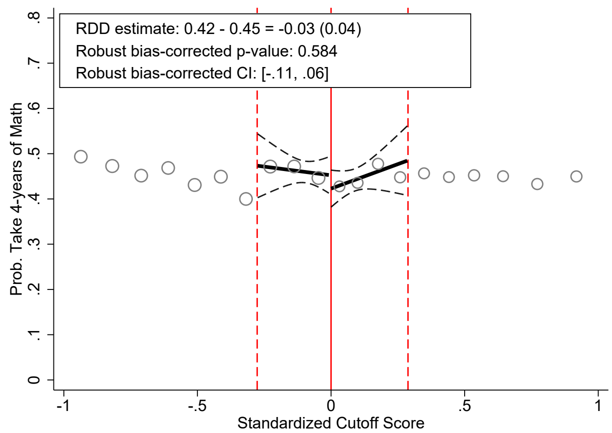
A1 Appendix Figures and Tables

Figure A1: Math First-Stage Results by Subgroup



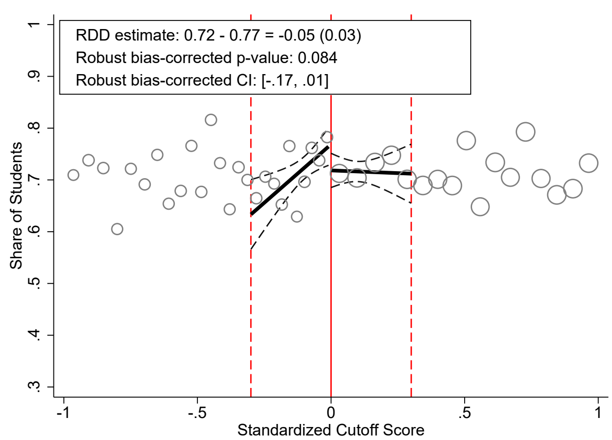
This figure shows the Math first-stage results of AAP eligibility on dual-credit participation for several subgroups between the 2014-2015 and 2018-2019 academic years. A linear fit is imposed on either side of the threshold. Each dot represents the average of the descriptive characteristic for the quantile-spaced bins. Data on courses and test scores comes from the ERDC.

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



This figure shows average likelihood for taking four years of math classes for students between the 2014-2015 and 2018-2019 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



This figure shows average likelihood of remaining in the sample across the 2014-2015 and 2018-2018 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

	Entire Sample	First-Stage Sample	Outcomes Sample
	(1)	(2)	(3)
Any ELA Dual Credit	0.586	0.552	0.511
	(0.004)	(0.005)	(0.007)
Distance to Cutoff	0.325	0.167	0.145
	(0.007)	(0.005)	(0.007)
Final HS GPA	2.899	2.781	2.834
	(0.005)	(0.006)	(0.008)
Share White	0.523	0.498	0.520
	(0.004)	(0.005)	(0.007)
Share Male	0.498	0.507	0.498
	(0.004)	(0.005)	(0.007)
Share Ever FRPL	0.568	0.611	0.580
	(0.004)	(0.004)	(0.007)
Share Black	0.101	0.109	0.110
	(0.002)	(0.003)	(0.004)
Observations	19,144	11,750	5,212

Mean values of each variable are shown by sample. Column (1) is the full sample of students. Column (2) restricts that sample to the set of students who are included in the first-stage analysis. Column(3) restricts the full sample to those I observe following their high school careers. Student-level data comes from the ERDC database.

Table A2: ELA First-Stage Robustness - Bandwidth Selection

	MSE-Optimal	Separate MSE-Optimal	CER-Optimal	Separate CER-Optimal
	(1)	(2)	(3)	(4)
RDD Estimate	0.149	0.155	0.157	0.182
Robust BC 95% CI	[.073 ; .259]	[.083 ; .271]	[.054 ; .272]	[.081 ; .301]
Robust BC p-value	0.0005	0.0002	0.0033	0.0007
Bandwidth	[.219; .219]	[.191; .285]	[.137; .137]	[.12; .178]
Observations Left	1,253	1,090	785	691
Observations Right	1,519	1,983	928	1,191

Standard errors (in parentheses) are clustered at the 11^{th} grade school district level. Sample includes students who took the SBAC ELA exam in high school in the sample districts. Each column represents a separate regression. In each specification I alter the bandwidth selection based on the recommendations in Calonico et al. (2017) and Calonico et al. (2018). Student-level data comes from the ERDC database.

Table A3: ELA First-Stage Robustness - Kernel Type

	Triangular (1)	Epanechnikov (2)	Uniform (3))
RDD Estimate	0.119	0.112	0.090
Robust BC 95% CI	[.072 ; .279]	[.075 ; .277]	[.069 ; .262]
Robust BC p-value	0.001	0.001	0.001
Bandwidth	[.3; .3]	[.3; .3]	[.3; .3]
Observations Left	1655	1655	1655
Observations Right	2090	2090	2090

Standard errors (in parentheses) are clustered at the 11^{th} grade school district level. Sample includes students who took the SBAC ELA exam in high school in the sample districts. Each column represents a separate regression. In each specification I alter the choice of kernel based on the recommendations in Calonico et al. (2017) and Calonico et al. (2018). Student-level data comes from the ERDC database.

Table A4: Reduced-Form Results by Subgroup Alternate Specification

	Men	Women	White	Non-White	Ever FRPL	Never FRPL
On-Time Graduation	-0.010	0.016	0.017	-0.011	0.014	-0.014
Robust BC 95% CI	[-.072 ; 0.041]	[-.055 ; .077]	[-.048 ; .086]	[-.078 ; .032]	[-.049 ; 0.066]	[-.077 ;.036]
Robust BC p-value	0.592	0.744	0.584	0.409	0.765	0.479
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Final HS GPA	-0.007	0.025	0.029	0.000	0.033	-0.015
Robust BC 95% CI	[-.24 ; .08]	[-.044 ; .271]	[-.152 ; .201]	[-.137 ; .161]	[-0.84 ; -0.196]	[-.228 ; .168]
Robust BC p-value	0.324	0.157	0.786	0.873	0.433	0.766
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Any Public College	-0.001	-0.025	0.010	-0.025	-0.026	0.027
Robust BC 95% CI	[-.182 ; .07]	[-.237 ; .040]	[-.148 ; .131]	[-.254 ; .011]	[-.223; -.001]	[-.149 ; .167]
Robust BC p-value	0.350	0.164	0.906	0.033	0.047	0.908
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Any Remedial ELA	-0.041	-0.012	-0.039	-.022	0.002	-0.091
Robust BC 95% CI	[-.11 ; .14]	[-.102 ; .181]	[-.144 ; .132]	[-.077 ; .171]	[-0.04 ; 0.187]	[-.242 ; .074]
Robust BC p-value	0.844	0.584	0.934	0.455	0.196	0.297
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Attempted Credits	-5.234	-1.035	-2.084	-2.672	-4.275	-4.114
Robust BC 95% CI	[-12.56 ; -.21]	[-11.33 ; .64]	[-12.96 ; 0.76]	[-11.47 ; -.128]	[-12.03 ; -1.19]	[-12.18 ; 2.60]
Robust BC p-value	0.042	0.080	0.082	0.045	0.017	0.205
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Earned Credits	-5.784	-1.81	-3.38	-4.233	-3.617	-4.298
Robust BC 95% CI	[-15.49; -1.48]	[-12.15 ; .78]	[-14.49 ; .19]	[-13.60 ; -.76]	[-13.88 ; -1.41]	[-13.82 ; 1.70]
Robust BC p-value	0.017	0.085	0.056	0.028	0.016	0.126
Total Observations	1,919	1,767	1,740	1,946	2,346	1,340
Joint Hypothesis Test *p*-value	0.154	0.715	0.528	0.310	0.454	0.170

Standard errors are clustered at the 11^{th} grade school district level.