Scaling Access and Excellence: Default Course Placements and Progression in Advanced Math*

Cameron Conrad† Thomas Dee‡ Elizabeth Huffaker§ Maranna Yoder¶
George Zuo||

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Abstract

Access to advanced coursework plays a central role in shaping students' academic trajectories, yet placement often depends on discretionary decisions by schools and families. We study one of the earliest known district adopters of *automatic enrollment*, where most schools initially used one of two achievement cutoffs—either the 50th or 75th percentile—to automatically enroll rising middle school students in advanced math. Using regression discontinuity designs, we find that automatic enrollment significantly expanded access to advanced math while producing durable gains in progression and course completion. At the lower cutoff, the policy more than doubled initial advanced math enrollment and eventual AP math passing on an intent-to-treat basis, with substantially larger implied effects among students whose placement changed. At the higher cutoff, effects were similar through early high school but attenuated thereafter.

JEL Classification: I21, I24, I28, H75

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†RAND Corporation (cconrad@umd.edu).
‡Stanford Graduate School of Education and Stanford Institute for Economic Policy Research (tdee@stanford.edu).
§University of Florida College of Education (ehuffaker@ufl.edu).
¶Indiana University O'Neill School of Public and Environmental Affairs (marayode@iu.edu).
||RAND Corporation (gzuo@rand.edu).

Each year, schools make high-stakes judgments about which students are ready for more advanced coursework. In math, a single placement decision in middle school can shape which courses students reach before graduation, with lasting consequences for their postsecondary and career paths. Yet there is still little consensus over how schools should make these allocation decisions. In practice, more than 90 percent of U.S. high schools that track students by achievement use teacher recommendations, family requests, or other discretionary inputs to inform these decisions (Steiner, Morales and Mulhern, 2024). These systems often leave academically prepared students out of advanced coursework, particularly those from disadvantaged backgrounds (Long, Kuhfeld and Peters, 2025; Covelli, Kaufman and Ozek, 2024; Francis and Darity, 2021). However, eliminating selectivity altogether creates a separate risk: in several prominent settings, broad acceleration policies increased course failure or reduced math achievement, with particularly adverse effects for less-prepared students (Allensworth et al., 2009; Domina et al., 2015; Clotfelter, Ladd and Vigdor, 2015).

Automatic enrollment offers a potential way through this tension by preserving selectivity while reducing the role of discretion in determining access to advanced math. Under automatic enrollment, schools use explicit achievement criteria to identify eligible students and enroll them in advanced coursework by default, while allowing families to opt out if desired. These policies, which as of spring 2026 have been enacted or introduced as legislation in nearly twenty states (Zuo et al., 2026), apply a central insight from the behavioral economics literature: when qualified students can be missed because of family advocacy barriers or discretionary screening, changing the default can increase participation without removing choice (Thaler and Sunstein, 2021).

Advanced coursework, however, adds a design problem that is largely absent from canonical default settings. Rather than applying the default to the full population, policymakers must decide how broadly it should extend across the achievement distribution. A higher threshold reduces the risk of over-placement but leaves more students subject to the same access frictions the policy is designed to address in the first place. A lower threshold broadens access but increases the risk of accelerating students who may not be prepared to succeed. Automatic enrollment therefore introduces a new design margin into the economics of defaults: how broadly can the default extend before the benefits of expanded access give way to the risks of over-placement?

We study automatic enrollment in the context where it is arguably most consequential: middle

school math, where a single placement decision in sixth grade shapes future exposure to college-level math and STEM courses, and may carry ramifications for even longer-run outcomes. We focus on Dallas Independent School District (Dallas ISD), which began automatically enrolling incoming sixth graders into advanced math in 2019-20 based on fifth-grade standardized test scores. Dallas provides a uniquely informative setting for two reasons. First, the district adopted automatic enrollment far earlier than most others, allowing us to follow the inaugural cohort through 11th grade and observe whether initial placement effects persist into advanced high school coursework. Second, the district's initial rollout generated variation in the central policy parameter for automatic enrollment: schools set eligibility thresholds at either the 50th or 75th percentile of the prior-achievement distribution. Comparing effects at these two margins lets us study whether a broader default expands access while still producing durable gains for students with lower prior achievement. We view the lower threshold as particularly informative for current policy: among states that have adopted or are considering automatic-enrollment mandates, none has set an eligibility threshold as low as the 50th percentile (Zuo et al., 2026).

Using regression discontinuity methods, we find that automatic enrollment substantially increased advanced math participation and that these effects persisted through high school. At the lower 50th percentile threshold, crossing the eligibility cutoff increased average sixth grade advanced math enrollment in the district by 17 percentage points, more than doubling the below-cutoff baseline of 11 percent (163% increase). Students above the threshold were subsequently 12 percentage points more likely to reach Algebra I or higher by eighth grade (76% increase) and 7.8 percentage points more likely to be enrolled in an AP math course in eleventh grade (159% increase). These enrollment gains also translated into substantial gains in course completion at every stage; for example, students above the threshold were 7.3 percentage points more likely to pass an AP math course (178% increase). The AP math effects are particularly notable because they capture progression into coursework that is not required for graduation and can provide college credit or advanced placement.

The effects appear broadly similar in magnitude at the higher 75th percentile threshold, despite large differences in students' baseline likelihood of enrolling in advanced math. Crossing the higher cutoff increased sixth-grade advanced math enrollment by 19 percentage points (49%) and Algebra I or higher by eighth grade by 15 percentage points (39%). Effects remain comparable

across thresholds through the early high school sequence but begin to attenuate for more advanced outcomes in tenth and eleventh grade. By eleventh grade, AP math enrollment increased by a marginally significant 5 percentage points (32%).

Together, our results show that extending the default from roughly the 75th to the 50th percentile reached students who were far less likely to access advanced math under the discretionary system, while producing effects of similar magnitude through most of the subsequent course sequence. We find little evidence that this broader reach came at the expense of attendance, discipline, or district retention. In heterogeneity analyses, we show that these gains also extend to economically disadvantaged students, who experience large and durable effects at both thresholds that closely mirror those for the full sample. Finally, we show that our results are also very stable across alternative specifications and bandwidth choices.

Although extensive research shows that default rules boost participation across many domains (Madrian and Shea, 2001; Johnson and Goldstein, 2003; Jachimowicz et al., 2019; Mertens et al., 2022), causal evidence on automatic enrollment into advanced coursework remains scarce.[1] Two studies analyze Washington's Academic Acceleration policy, which automatically enrolls academically proficient 11th- and 12th-graders into AP, IB, or dual-credit courses. Austin et al. (2025) find a 5 percentage point increase in advanced course enrollment overall and increased advanced math participation among proficient students, while Felegi (2024) finds an 8 percentage point increase in relevant dual-credit enrollment at the English cutoff but no math-cutoff effect—possibly because students encountered the policy too late to modify multi-year math sequences. The closest middle school evidence comes from a synthetic control analysis of Dallas's policy, which finds that automatic enrollment increased eighth grade Algebra I enrollment by 13 percentage points and narrowed the Hispanic-White enrollment gap in DISD when compared against a weighted combination of other Texas districts (Vargas Castaño, Antoine and Miller, 2026).

Our paper makes two contributions to this emerging literature. First, we extend causal evidence on middle-school automatic enrollment beyond initial course placement by tracing its effects through the subsequent math sequence. We show whether students induced into advanced math

[1] A robust, related literature examines universal screening, objective placement rules, gifted and upper-track assignment, targeted acceleration, and broad adjustments to math pathways. However, none of these interventions constitutes automatic enrollment with explicit default placement (Card and Giuliano, 2016*b*,*a*; Dougherty et al., 2015, 2017; Card and Giuliano, 2024; McEachin, Domina and Penner, 2020; Brummet et al., 2025; Allensworth et al., 2009; Clotfelter, Ladd and Vigdor, 2015; Domina et al., 2015).

continue into increasingly advanced coursework, whether they pass the courses they are induced to take, and whether those effects persist through eleventh grade. Second, we exploit placement discontinuities near both the middle and upper quartile of the prior-achievement distribution to study threshold choice within the same policy environment. This design allows us to compare the effects of automatic enrollment for students with substantially different levels of prior preparation while holding the broader institutional setting fixed.

1 Background

1.1 Automatic enrollment in the national context

Automatic enrollment has expanded rapidly over the past five years, driven almost entirely by state policy. As of spring 2026, three states had statewide mandates in operation, seven more had enacted mandates that were soon phasing in, and seven others had introduced legislation. Three additional states had adopted softer approaches based on guidance or incentives. District-led adoption remains rare outside these state efforts: fewer than 1 percent of the 500 largest districts in non-mandate states had independently adopted automatic-enrollment policies (Zuo et al., 2026).

States vary substantially in how they design these policies, particularly in where they set eligibility thresholds, which grades they target, and which subjects they cover. For example, among the three states with active mandates, North Carolina and Texas target rising middle school students at the 90th and 60th percentile, respectively, whereas Washington focuses on automatically enrolling later-stage high school students across a variety of subjects. These differences determine both how many students the default affects and how early it can reshape their subsequent course trajectories.

Beyond these core design choices, many states' policies incorporate additional features to soften reliance on a single high-stakes test for determining acceleration. Many policies allow students below the threshold to opt into advanced coursework, and other policies also allow students to qualify via local exams or course grades. While these policies further broaden access, they also re-introduce elements of discretion that automatic enrollment is designed to eliminate in the first place.

1.2 Automatic enrollment in Dallas

In Dallas ISD (DISD), middle school students proceed along either a standard or accelerated three-year math sequence. Students on the standard track take the end-of-year State of Texas Assessments of Academic Readiness (STAAR) for their corresponding grade. Students on the accelerated track take the same sixth-grade exam, but then proceed to take the *eighth grade* exam in grade 7, and the Algebra I end-of-course exam (a high school exam) in grade 8.

Beginning with the 2019-20 school year, DISD phased in automatic enrollment for middle-school advanced math. Parents could opt out by submitting a written request to their counselor (Dallas Independent School District, 2019). In the first year, schools varied in how broadly they implemented the policy. Most importantly for our design, schools clustered around two achievement thresholds: some automatically enrolled students who met the fifth-grade "Meets Grade Level" STAAR standard, near the 50th percentile of the achievement distribution, while others used the more selective "Masters Grade Level" standard, near the 75th percentile. Figure 1 shows the corresponding jumps in sixth-grade advanced-math enrollment at both thresholds.

This variation reduces the number of schools contributing to identification at either threshold, but it also allows us to estimate the effects of automatic enrollment at two substantively different readiness margins within the same district and cohort. We also observe a small number schools at the two extremes of implementation. Some schools did not accelerate any incoming sixth graders, consistent with capacity constraints that prevented them from adding an accelerated math track in the first year. At the other extreme, a subset of "Promise" campuses accelerated all incoming sixth graders regardless of prior achievement.

The COVID-19 pandemic paused STAAR exams for incoming sixth grade students for the 2020-2021 school year, disrupting the implementation of automatic enrollment for the next cohort of middle school students. When automatic enrollment resumed in the 2021-2022 school year, DISD had changed the policy substantially: the district extended automatic enrollment to other core subjects (English, Science, and Social Studies), and began to purposely accelerate large numbers of seventh grade students outside of the initial automatic enrollment assignment. These changes weakened the treatment contrast in later grades and made it more difficult to isolate the effects of the original sixth-grade math policy. We therefore focus on the inaugural 2019-2020 cohort, which

provides the cleanest setting for tracing the effects of automatic enrollment in sixth-grade math through the subsequent course sequence.

1.3 Conceptual Framework

Let $A_i \in \{0, 1\}$ denote student i's initial course placement, where $A_i = 1$ denotes advanced coursework and $A_i = 0$ denotes standard coursework. Let $U_i(a)$ denote the student's payoff under placement $a \in \{0, 1\}$, which reflects both benefits from learning and costs from overplacement. We define the student's net causal gain from advanced placement as

$$g_i \equiv U_i(1) - U_i(0), \qquad \bar{g}(\theta) \equiv \mathbb{E}[g_i \mid \theta_i = \theta], \qquad \bar{g}'(\theta) > 0,$$

where θ_i denotes latent academic readiness. Expected gains rise with readiness, but students with the same readiness may experience different gains from acceleration. Absent capacity constraints or spillovers, the first-best rule accelerates student i if and only if $g_i \geq 0$.

The district and school observe a noisy test-score signal $S_i = \theta_i + \varepsilon_i$. School staff also observe non-score information X_i, such as grades, classroom performance, motivation, work habits, and teacher observations. The school's perceived gain from acceleration is:

$$g_i^{\text{sch}} = G^{\text{sch}}(S_i, X_i, W_i; \lambda_j),$$

where W_i collects other factors that may systematically or idiosyncratically shift school judgment, and λ_j captures how strongly school j relies on the test-score signal relative to other information. The family forms its own perceived gain,

$$g_i^{\text{fam}} = G^{\text{fam}}(S_i, Z_i),$$

where Z_i collects private information, beliefs, and preferences that shape the family's valuation of advanced coursework. Let $k_i^{\text{in}} \geq 0$ denote the information, transaction, and advocacy costs of requesting placement, and let $k_i^{\text{out}} \geq 0$ denote the cost of declining a placement initiated by either

the school or district. Define

$$R_i = \mathbf{1}\left\{g_i^{\text{sch}} \geq 0\right\}, \qquad I_i = \mathbf{1}\left\{g_i^{\text{fam}} \geq k_i^{\text{in}}\right\}, \qquad K_i = \mathbf{1}\left\{g_i^{\text{fam}} \geq -k_i^{\text{out}}\right\},$$

where R_i indicates that the school recommends accelerating student i, I_i indicates that the family opts into advanced placement (even when not recommended), and K_i indicates that the family keeps an advanced placement (i.e., they do not opt out). We assume that families make the same acceptance decision under both school-initiated placement and automatic enrollment. Because both action costs are nonnegative, $I_i \leq K_i$.

Discretionary placement: Under discretion, a student enters advanced coursework either through a school recommendation that the family accepts or through a family request when the school does not recommend placement:

$$A_i^D = R_i K_i + (1 - R_i) I_i. \tag{1}$$

Automatic enrollment: The district assigns the automatic-placement default to students satisfying

$$E_i(c) = \mathbf{1}\{S_i \geq c\},$$

where c denotes the score cutoff. Assuming that families can still request to opt-in even if not auto-enrolled (as in the DISD context), the placement rule is given by:

$$\begin{aligned} A_i^{AE}(c) &= A_i^D + \left(1 - A_i^D\right) E_i(c) K_i \\ &= A_i^D + E_i(c) M_i, \qquad M_i \equiv K_i \left(1 - A_i^D\right). \end{aligned} \tag{2}$$

where M_i identifies a default mover: a student whom the school does not recommend, whose family would keep an advanced placement, but whose family would not incur the cost of requesting it. Because $I_i \leq K_i$, the default-mover indicator simplifies to

$$M_i = K_i(1 - R_i)(1 - I_i).$$

Thus, automatic enrollment changes placement for students whom the school does not recommend,

whose families do not request placement, but whose families keep the automatic-placement default.

Who benefits from switching regimes? Automatic enrollment changes student i's payoff by

$$\begin{aligned} B_i^{AE}(c) &\equiv g_i \left[A_i^{AE}(c) - A_i^D \right] \\ &= g_i \mathbf{1}\{S_i \geq c\} \mathbf{1}\{g_i^{\text{sch}} < 0\} \mathbf{1}\{-k_i^{\text{out}} \leq g_i^{\text{fam}} < k_i^{\text{in}}\}. \end{aligned} \tag{3}$$

Equation (3) yields several comparative statics. Holding g_i fixed, greater policy reach raises realized benefits for newly reached students with $g_i > 0$ and lowers realized benefits for newly reached students with $g_i < 0$.

- **School assessment.** More favorable school assessments increase discretionary placement and reduce the incremental reach of automatic enrollment. The information available to schools, X_i and W_i and the weight λ_j that schools place on test scores relative to other information determine these assessments. Greater reliance on test scores will therefore change policy reach depending on how score-based assessments differ from schools' other information.

- **Family assessment.** The family assessment $g_i^{\text{fam}} = G^{\text{fam}}(S_i, Z_i)$ depends on the score and on private information, beliefs, and preferences collected in Z_i. Changes in S_i or Z_i that raise the family's perceived gain have a nonmonotonic effect: they first move families from declining placement to keeping the default (increasing policy reach), but eventually induce families to request placement without automatic enrollment (reducing reach).

- **Family action costs.** Higher opt-in costs k_i^{in} expand the set of families that keep advanced placement but do not request it. Higher opt-out costs k_i^{out} expand the set that keeps the default rather than declining it. Both increase policy reach among students whom schools do not recommend.

- **Readiness and score noise.** Greater readiness tends to raise the true gain from acceleration and the probability of exceeding the cutoff, but it may also produce more favorable school and family estimates of g_i, reducing the likelihood that automatic enrollment changes placement. Greater score noise weakens the relationship between readiness and eligibility and may change school recommendations and family decisions in indeterminate ways. We therefore

cannot sign the effects of readiness or score noise without additional assumptions.

Threshold choice welfare: We first define two terms:

$$\pi(s) \equiv \Pr(M_i = 1 \mid S_i = s), \qquad \tau_g(s) \equiv \mathbb{E}[g_i \mid M_i = 1, S_i = s].$$

Here, $\pi(s)$ denotes the default-mover share at score s, while $\tau_g(s)$ denotes movers' average gain from advanced placement. The aggregate student benefit from a cutoff c is

$$\begin{aligned} \mathcal{B}(c) &= \int_c^{\infty} f_S(s)\pi(s)\tau_g(s)\, ds, \\ \frac{d\mathcal{B}(c)}{dc} &= -f_S(c)\pi(c)\tau_g(c), \end{aligned} \tag{4}$$

where $f_S(s)$ denotes the score density. Lowering the cutoff adds students at the margin; its marginal benefit therefore equals the number of students at that score times the share whose placement changes times their average gain from placement.

Empirical implications: This conceptual model gives the regression discontinuity first stage a behavioral interpretation. Because $A_i(1) - A_i(0) = M_i$, the discontinuity in advanced-course placement identifies $\pi(c) = \Pr(M_i = 1 \mid S_i = c)$, which is the share of students at the cutoff whom schools would not recommend and families would not request placement for, but whose families would keep the automatic-placement default. Heterogeneity in $\pi(c)$ across thresholds or subgroups therefore reflects differences in the prevalence of the placement frictions captured by school assessment, family valuation, and action costs. Comparing thresholds allows us to further connect the empirical estimates to policy design: differences in $\pi(c)$ capture changes in policy reach, while differences in effects per induced placement capture how the returns to expanding advanced coursework vary across margins of the score distribution. Together, these objects correspond to the mover share and gains per mover that determine the benefits of lowering or raising the automatic-enrollment threshold in Equation (4).

2 Data

We use restricted-access administrative records from the Texas Education Agency, accessed through the Texas Education Research Center (ERC). The ERC records follow students longitudinally across

Texas public schools and include enrollment, demographics, statewide assessment results, course enrollment and completion, attendance, and disciplinary records.

2.1 Threshold construction

Texas measures student achievement through the State of Texas Assessments of Academic Readiness (STAAR), a statewide standardized testing program aligned with the Texas Essential Knowledge and Skills. The state maps STAAR scale scores into four ordered performance categories: Did Not Meet Grade Level, Approaches Grade Level, Meets Grade Level, and Masters Grade Level. Approaches represents the minimum passing standard; Meets indicates that a student has a high likelihood of success in the next grade or course; and Masters indicates that a student is expected to succeed with little or no academic intervention (Texas Education Agency, n.d.). Each category reflects a state-defined scale-score cutoff that varies across grades, subjects, and years.

The Texas ERC provides each student's scale score,[2] and we overlay the known thresholds from the 2019 administration of the STAAR exam for fifth grade math. We normalize our cohort's test scores based on the *statewide* mean and standard deviation. The two thresholds of interest, "Meets" and "Masters" were set at the 48th and 76th percentile of the statewide distribution, corresponding to -0.12 and $+0.46$ standard deviations relative to the statewide mean.

2.2 Sample Construction

Our analytic sample includes all fifth grade DISD students with a valid spring 2019 math STAAR score.[3] If students were enrolled in multiple districts in the same year, their primary district was assigned based on the district where they were enrolled for the most days.[4] Because the ERC follows students across Texas public schools, we can observe subsequent enrollment and statewide assessment outcomes for students who leave Dallas ISD but remain enrolled in Texas public schools.[5]

[2] In the 2019-2020 cohort, each scale score corresponds one-to-one with the number of questions that were answered correctly.

[3] We included students who repeat 5th grade. We exclude students who took STAAR Alternate 2 in fifth grade, the version Texas administers to students with significant cognitive disabilities.

[4] If multiple districts were tied on most days in membership, and one of the districts was Dallas ISD, the Dallas ISD record was retained. Further ties were broken by selecting the district with the highest attendance rate, and if still tied, choosing the district with the first district ID in numerical order.

[5] The sample begins with 12,532 students enrolled in fifth grade in Dallas ISD in spring 2019. Of these, 11,734 have a valid fifth-grade Math STAAR score; 11,490 subsequently enrolled in a Texas public school in sixth grade; 9,605 enrolled in Dallas ISD in sixth grade; and 6,307 remained enrolled in Dallas ISD in 2024-25, when students progressing on time would be in eleventh grade.

Appendix Table A2 summarizes the full cohort and the threshold-specific samples. The full cohort includes 11,734 students, of whom 71 percent are Hispanic, 21 percent are Black, and 89 percent are economically disadvantaged. Female students account for 49 percent of the cohort, and 51 percent of students have limited English proficiency. Columns (3) and (5) describe the samples within one standard deviation of the Meets and Masters thresholds, respectively, with each sample restricted to exclude the other placement discontinuity. These samples contain 6,369 and 5,654 students before accounting for outcome availability. Both samples consist largely of economically disadvantaged students, although their demographic composition differs. Compared with the Masters sample, the Meets sample includes larger shares of Black students (24 versus 16 percent) and economically disadvantaged students (92 versus 86 percent).

Observed course-taking differs substantially across these samples. Among students with nonmissing outcomes, 22 percent of the Meets sample enrolled in sixth-grade advanced math, compared with 59 percent of the Masters sample. By eighth grade, 25 and 57 percent, respectively, had enrolled in Algebra I. These differences extend into high school: in eleventh grade, 14 percent of the Meets sample enrolled in Precalculus or higher, compared with 34 percent of the Masters sample. Outcome availability varies across grades and measures. We observe sixth-grade advanced math placement for 11,034 students and eleventh-grade course outcomes for 10,141 students, corresponding to approximately 94 and 86 percent of the initial cohort. These counts include students who leave Dallas ISD but remain observable elsewhere in the Texas public school system. Table A2 reports the number of nonmissing observations for each outcome. In robustness tests, we examine whether outcome availability changes discontinuously at either placement threshold.

2.3 Outcomes

We preregistered all confirmatory outcomes prior to this study (Huffaker et al., 2025). These outcomes focus on the inaugural 2019–20 sixth-grade cohort and trace whether automatic enrollment changed students' progression through the advanced math sequence. In middle school, we identify advanced courses using the "population served" field, which designates Honors or Advanced instruction. Sixth-grade advanced math enrollment captures the first-stage effect of automatic enrollment, while seventh-grade advanced math enrollment and Algebra I enrollment by eighth grade measure short-run persistence.

For high school, we use Texas course codes and state guidance on mathematics prerequisites and course sequencing to map courses onto an ordered progression. For courses outside the traditional sequence, we use state prerequisite rules and course content to determine their position. Appendix Table A1 documents this crosswalk. Because both Geometry and Algebra II require Algebra I, but Geometry is not a prerequisite for Algebra II, students may take either course first after Algebra I. We therefore define the high school sequence as Geometry or Algebra II by ninth grade, both Geometry and Algebra II by tenth grade, and Precalculus or higher by eleventh grade.[6] For each stage, the ERC also records whether students passed the course and received credit, allowing us to distinguish course-taking from successful completion.

Two common achievement measures are unavailable for this cohort: comparable test scores and course grades. Students on the standard and advanced tracks do not share a common end-of-year assessment after sixth grade, and the sixth-grade STAAR exam that would normally provide a common measure was canceled because of COVID-19. The ERC also does not report course grades for this cohort. We therefore measure academic success using course passing and credit receipt rather than test scores or grades.

3 Empirical Strategy

3.1 Regression Discontinuity Design

We estimate regression discontinuity (RD) models that exploit the fifth-grade math STAAR thresholds associated with advanced math placement. For the 2019-20 DISD sixth grade cohort, we estimate separate discontinuities based on the proximity of their fifth grade math test scores to both the Meets and Masters thresholds. Let S_{ik} denote student i's fifth-grade math STAAR score, recentered at threshold $k \in \{\text{Meets}, \text{Masters}\}$ and expressed in standard-deviation units, and let $D_{ik} = \mathbf{1}\{S_{ik} \geq 0\}$ indicate that the student scored at or above the relevant threshold.

For each outcome Y_i, we estimate the following linear spline separately at each threshold:

$$Y_i = \alpha_k + \tau_k D_{ik} + \beta_k S_{ik} + \delta_k (D_{ik} \times S_{ik}) + \mathbf{X}_i' \gamma_k + \lambda_{s(i),k} + \varepsilon_{ik}, \tag{5}$$

where X_i contains indicators for gender, race and ethnicity, economic disadvantage, and English

[6] Enrollments lasting fewer than 30 days are coded as 0 in course-taking outcomes.

learner status, and $\lambda_{s(i),k}$ denotes fifth-grade campus fixed effects. Our primary specification includes both student covariates and campus fixed effects, though we also report baseline estimates without either set of controls as well as estimates that include student covariates alone.

The model permits the relationship between fifth-grade achievement and the outcome to have different slopes on either side of the threshold: β_k gives the slope below the cutoff and $\beta_k + \delta_k$ gives the slope above it. The coefficient of interest, τ_k, measures the discontinuous change in the outcome at the relevant threshold. We apply the same specification to initial advanced math enrollment and to subsequent academic and schooling outcomes. Within each bandwidth, we estimate equation (5) and give equal weight to all observations in the estimation window, equivalent to using a rectangular kernel.

When sixth-grade advanced math enrollment is the dependent variable, τ_k measures the first-stage change in realized placement at the threshold. For subsequent outcomes, τ_k gives the intent-to-treat effect of crossing the threshold and becoming eligible for automatic enrollment. Identification requires potential outcomes to evolve smoothly with fifth-grade achievement through each threshold in the absence of the discontinuous change in placement. Under this assumption, students immediately below and above the threshold provide the relevant counterfactual comparison. Student covariates and fifth-grade campus fixed effects account for residual observable and school-level differences and improve precision. We report heteroskedasticity-robust standard errors.

3.2 Estimation Samples and Bandwidth Selection

We construct separate estimation samples for the Meets and Masters thresholds so that each regression isolates a single placement margin. For the Meets analysis, we exclude students who scored at or above the Masters Grade Level, leaving students below Meets and students who scored at Meets but below Masters. For the Masters analysis, we retain only students who scored at least at the Meets Grade Level, leaving students between Meets and Masters and students at or above Masters. These restrictions prevent the estimation sample for either threshold from spanning the other discontinuity.

We note that the proximity of the two thresholds, combined with the discreteness of STAAR scores, constrains our approach to bandwidth choice. Only 4 distinct scale score values lie at or

above Meets and below Masters (see Figure 1), limiting the available support *above* the Meets cutoff and *below* the Masters cutoff. Conventional data-driven bandwidth selectors are less well suited to settings with so few support points (Cattaneo, Idrobo and Titiunik, 2024).[7]

Because we exclude observations spanning the other threshold, our bandwidth choice often leads to asymmetric estimation windows with differing numbers of scale score bins on each side of the threshold. We present estimates from Equation (5) across a variety of bandwidth choices. In our tables, we first show estimates using the full available support of the relevant threshold-specific sample. We then restrict the sample to observations within one standard deviation of the cutoff, $|S_{ik}| < 1$, and within one-half of a standard deviation, $|S_{ik}| < 0.5$. Finally, we estimate an outcome-specific MSE-optimal bandwidth and retain observations satisfying $|S_{ik}| < h_k^{\text{MSE}}$, where h_k^{MSE} is the optimal bandwidth, which is selected separately for each outcome and threshold. In Figures A2 through A6, we show all of our results at each possible discrete bandwidth.

3.3 Validity Checks

The RD design requires that, absent the discontinuous change in placement, potential outcomes would evolve smoothly through each threshold. We assess this assumption using features of the assignment process and four empirical checks.

First, precise sorting around either cutoff is unlikely by design. Texas administers and scores STAAR statewide and sets the scale-score cutoffs for the Meets Grade Level and Masters Grade Level standards. Dallas ISD therefore neither determines students' exact fifth-grade scores nor chooses the score values that define the thresholds. We nevertheless test formally for discontinuities in the density of fifth-grade math scores at each cutoff following Cattaneo, Jansson and Ma (2020) and inspect the corresponding score distributions. [PLACEHOLDER: Density test results to be uploaded after disclosure review.]

Second, we test whether predetermined student characteristics change discontinuously at either threshold. We estimate separate RD models using indicators for gender, race and ethnicity, economic disadvantage, English learner status, and whether the student took the fifth-grade STAAR in Spanish as outcomes. Appendix Figure A1 reports these estimates using a 1-SD bandwidth. We

[7] We also note that the What Works Clearinghouse also recommends at least four unique running variable values on each side of the cutoff for eligibility for review under its regression discontinuity standards (What Works Clearinghouse, 2022)).

find no systematic evidence of imbalance at either cutoff.

Third, we examine whether crossing either threshold changes whether students remain observable in the data used to construct our outcomes. Appendix Table A3 shows no detectable discontinuity in the availability of sixth-grade course records at either threshold. We also find no discontinuity in whether students remain enrolled in Dallas ISD in sixth grade. For the longer-run outcomes, Appendix Table A4 tests whether course outcomes are observed in grades 9 through 11. These estimates are small and statistically indistinguishable from zero at both thresholds in every grade. Differential attrition therefore does not appear to account for our subsequent course-taking estimates.

Finally, we inspect the graphical evidence underlying the RD estimates. Figures 1 through 4 plot binned outcome means against the centered fifth-grade math score for initial placement and subsequent middle- and high-school outcomes, for each threshold. These plots provide a visual check that the estimated effects correspond to localized breaks at the placement thresholds rather than broader nonlinear relationships between prior achievement and later outcomes.

4 Results

4.1 First Stage: Advanced Math Placement at the Cutoff

Figure 1 provides visual evidence of the first stage, and Table 1 presents the corresponding regression estimates. We present estimates across three specifications: a baseline RD without controls, a version with student-level covariates, and a version with fifth-grade school fixed effects in addition to student covariates. Point estimates are broadly similar across specifications, while adding school fixed effects modestly improves precision. For these estimates, we also show how our results vary across four different bandwidth choices: the full ITT sample (i.e., no bandwidth restriction), 1 SD, 0.5 SD, and bandwidth chosen by the optimal bandwidth approach. In our discussion of results to follow, we primarily describe findings from the fully controlled specification with a 1-SD bandwidth.

We find that crossing the Meets threshold generated a sharp, large increase in sixth-grade advanced math enrollment that is both significant and visually clear. Students just above the threshold were 17.4 percentage points more likely to be placed in advanced math than students just

below it (SE: 1.8 pp), more than doubling the below-cutoff baseline of 10.7 percent. The estimated effect remains large, positive, and statistically significant across specifications and bandwidth choices. At the Masters threshold, the first-stage effect is comparable in magnitude: students just above the cutoff were 19.3 percentage points more likely to be enrolled in advanced math (SE: 3.0 pp), though baseline advanced enrollment was also much higher at this threshold (39.4 percent).

We specifically highlight the large differences in baseline advanced enrollment across the two cutoffs. At the Meets threshold—roughly the median of the achievement distribution—only one in ten students just below the threshold had accessed advanced math under the discretionary system before automatic enrollment. Although our point estimates are similar across thresholds in percentage point terms, the proportional increase is far larger at the Meets threshold (a 163% increase vs. a 49% increase). At the same time, our findings suggest underplacement for high achievers in this cohort.[8]

4.2 Persistence Through the Math Sequence

We next examine whether the initial placement gains persisted through the multi-year math sequence, and whether increased enrollment translated into successful completion of increasingly advanced coursework. Figures 5 and 6 provide an overview of this progression at the Meets and Masters thresholds, plotting below-cutoff means alongside our estimated effects on enrollment and passing from sixth-grade placement through eleventh-grade coursework.

4.2.1 Middle school advanced math

At the Meets threshold, the sixth-grade enrollment gain carried forward through middle school largely intact (Table 2; Figure 2). Enrollment in seventh-grade advanced math rose by 13.5 percentage points (SE = 2.0 pp), against a below-cutoff mean of 27.4 percent. By eighth grade, enrollment in Algebra I or higher rose by 12.3 percentage points (SE = 2.0 pp), against a below-cutoff mean of 16.1 percent. The probability of taking the Algebra I end-of-course examination rose correspondingly by 12.7 percentage points (SE = 1.9 pp), relative to a below-cutoff mean of 14.8 percent. The initial placement effect therefore translated into a sustained shift through the accelerated middle-school sequence.

[8] DISD gradually increased placement rates across the district in future years.

The middle-school effects at the Masters threshold are similarly large. Seventh-grade advanced math enrollment rose by 13.0 percentage points (SE = 3.1 pp), against a below-cutoff mean of 47.1 percent. Algebra I enrollment by eighth grade rose by 15.0 percentage points (SE = 3.1 pp), against a baseline of 38.9 percent, while the probability of taking the Algebra I end-of-course examination rose by 16.2 percentage points (SE = 3.1 pp). These results show that automatic enrollment continued to shift course progression even among relatively high-achieving students near the Masters threshold.

4.2.2 High school course-taking and passing

We next examine whether the middle-school enrollment gains carried forward into high school and translated into successful course completion. Tables 3 and 4 report enrollment and passing outcomes at the Meets and Masters thresholds, respectively; Figures 3 and 4 provide the corresponding graphical evidence.

At the Meets threshold, students above the cutoff were 12.3 percentage points more likely to enroll in Geometry or Algebra II or higher by ninth grade (SE = 2.0 pp) and 10.0 percentage points more likely to have passed one of those courses (SE = 1.9 pp), against below-cutoff means of 15.1 and 11.5 percent, respectively. By tenth grade, enrollment in both Geometry and Algebra II rose by 9.8 percentage points (SE = 2.0 pp), while passing both rose by 6.0 percentage points (SE = 1.8 pp). By eleventh grade, enrollment in Precalculus or higher rose by 7.4 percentage points (SE = 1.8 pp), and passing rose by 6.6 percentage points (SE = 1.7 pp), relative to below-cutoff means of 8.4 and 7.2 percent.

These results show that the policy's effects extended beyond course enrollment. A large share of the increase in advanced course-taking at the Meets threshold translated into increased course completion, and passing effects remain sizable and statistically significant through eleventh grade. The pattern provides little evidence that the broader default simply moved students into advanced courses that they were unable to complete.

At the Masters threshold, enrollment effects also persist into high school, but course-completion effects attenuate more in later grades. Enrollment in Geometry or Algebra II or higher by ninth grade rose by 15.1 percentage points (SE = 3.2 pp), while passing rose by 11.1 percentage points (SE = 3.1 pp), against below-cutoff means of 36.9 and 30.9 percent. By tenth grade, enrollment in both

Geometry and Algebra II increased by 10.1 percentage points (SE = 3.3 pp), while the estimated effect on passing was 5.0 percentage points (SE = 3.1 pp) and was not statistically significant. By eleventh grade, enrollment in Precalculus or higher increased by 5.5 percentage points (SE = 3.0 pp), while the passing estimate was 3.7 percentage points (SE = 2.9 pp); neither estimate is statistically significant at the five-percent level. Thus, although automatic enrollment continued to shift course-taking near the Masters threshold, the corresponding gains in course completion became smaller and less precisely estimated as students progressed into more advanced high school mathematics.

Comparing enrollment and passing gains relative to their below-cutoff means provides another way to descriptively assess whether expanded access translated into successful completion. At the Meets threshold, the estimated increase in passing generally matches or exceeds the increase in enrollment in proportional terms. For Algebra I by eighth grade, the passing effect is 90 percent of the enrollment effect, compared with a passing-to-enrollment ratio of 80 percent below the cutoff. For AP math in eleventh grade, the corresponding ratios are 94 and 84 percent. The exception is completion of both Geometry and Algebra II by tenth grade, where the effect ratio is somewhat lower than the baseline ratio (62 versus 67 percent). At the Masters threshold, passing gains generally fall short of this proportional benchmark: the corresponding ratios are 50 versus 77 percent for both Geometry and Algebra II and 67 versus 88 percent for Precalculus or higher, although AP math gains closely track the benchmark. These descriptive comparisons suggest that the large enrollment gains at the lower cutoff were generally accompanied by commensurate gains in successful completion.[9]

4.2.3 AP course outcomes

The effects of automatic enrollment also extended into AP coursework by eleventh grade. Table 5 reports enrollment and passing outcomes for AP mathematics and AP courses in any subject. At the Meets threshold, students above the cutoff were 7.8 percentage points more likely to enroll in an

[9]These ratios do not directly identify pass rates among students induced to enroll. That interpretation would require additional assumptions, including that eligibility does not reduce enrollment in the relevant course and does not affect passing among students who would enroll regardless. Earlier advanced placement could improve preparation among these students. The below-cutoff ratios also describe a selected group of enrollees within the estimation window, rather than the counterfactual success of newly enrolled students or a fitted limit at the cutoff. Finally, we caution that these comparisons use point estimates without a formal test of proportionality.

AP math course in eleventh grade (SE = 1.5 pp), relative to a below-cutoff mean of 4.9 percent. They were also 7.3 percentage points more likely to pass an AP math course (SE = 1.4 pp), relative to a baseline of 4.1 percent. These effects are large relative to baseline: AP math enrollment increased by roughly 160 percent, while AP math passing increased by roughly 180 percent.

The effects also extended beyond mathematics. Students above the Meets cutoff were 8.3 percentage points more likely to enroll in any AP course in eleventh grade (SE = 2.3 pp), relative to a below-cutoff mean of 21.2 percent, and 8.3 percentage points more likely to pass an AP course (SE = 2.2 pp), relative to a baseline of 18.4 percent. Thus, the longer-run effects of automatic enrollment were not limited to progression through the advanced math sequence.

At the Masters threshold, the AP effects are smaller and less precisely estimated. AP math enrollment increased by 5.0 percentage points (SE = 2.7 pp), relative to a below-cutoff mean of 15.8 percent, while AP math passing increased by 4.3 percentage points (SE = 2.5 pp), relative to a baseline of 13.5 percent. Both estimates are marginally significant at the 10-percent level. Effects on enrollment and passing in any AP course are smaller—3.7 and 4.0 percentage points, respectively—and are not statistically significant. The contrast across thresholds is consistent with the much higher baseline rates of AP participation near the Masters cutoff.

4.2.4 Implied effects among students induced into advanced math

The ITT estimates capture the effect of eligibility for automatic enrollment, including students whose placement would not change upon crossing the threshold. To gauge the effects among students induced into sixth-grade advanced math, we divide each subsequent outcome estimate by the initial enrollment effect: 0.174 at Meets and 0.193 at Masters. Under standard fuzzy RD assumptions, this rescaling yields implied average effects for students near each cutoff whose initial placement changed.[10]

At the Meets threshold, the implied effects on Algebra I enrollment and passing by eighth grade are approximately 71 and 64 percentage points, respectively. The corresponding gains at Masters are similar, at 78 and 62 percentage points. By eleventh grade, however, the point estimates

[10]This interpretation requires that crossing the threshold affects later outcomes only through initial advanced math placement and does not move any students out of advanced math. Because estimation samples vary across outcomes, ratios using the reported first stage are approximate; formal IV estimates would require estimating the first stage within each outcome's sample and accounting for uncertainty in both estimates.

diverge: the implied increases in AP math enrollment and passing are approximately 45 and 42 percentage points at Meets, compared with 26 and 22 percentage points at Masters.

These magnitudes suggest that initial placement changed academic trajectories substantially for the students affected. At the lower cutoff, the implied AP math passing effect amounts to roughly four additional successful completions for every ten students induced into sixth-grade advanced math, relative to how those students would otherwise have progressed. The smaller late-high-school estimates at Masters concern a different group of students and remain less precisely estimated, so the comparison across cutoffs is descriptive.

4.3 Broader Schooling Outcomes

We next examine whether the shift into more advanced coursework affected broader measures of school engagement and district attachment. Appendix Tables A5, A6, and A7 report effects on attendance, disciplinary incidents, and continued enrollment in Dallas ISD through eleventh grade.

We find little evidence of adverse effects on these outcomes at the Meets threshold. Attendance effects are close to zero and statistically insignificant in grades 9 through 11. We likewise find no detectable effect on continued enrollment in Dallas ISD in any grade. Estimates for disciplinary incidents are also nonpositive: the ninth-grade estimate is essentially zero, while incidents decline by 2.2 percentage points in tenth grade and 3.0 percentage points in eleventh grade, with the latter marginally significant at the 10-percent level.

The pattern is similar at the Masters threshold. Attendance remains statistically unchanged in all three grades, and we find no evidence that automatic enrollment reduced continued enrollment in Dallas ISD. If anything, district retention is somewhat higher by eleventh grade, although the 5.5 percentage point estimate is only marginally significant. Disciplinary incidents decline by 4.0 percentage points in ninth grade, relative to a below-cutoff mean of 13.0 percent, but this estimate is also only marginally significant and does not persist into eleventh grade.

Together, these results provide no evidence that the sustained increase in advanced math participation came at the expense of attendance or continued enrollment in the district. Combined with the course-passing results in Section 4.2.2, they also provide little indication that broader placement into advanced math generated academic disengagement among students near the Meets threshold.

4.4 Heterogeneity by Student Subgroup

Tables 6 and 7 report ITT estimates separately by gender, race and ethnicity, and economic disadvantage at the Meets and Masters thresholds, respectively. The broad pattern of persistence remains visible across several of the largest student subgroups, although estimates become less precise for smaller groups. Because these tables estimate each subgroup separately rather than formally testing equality of coefficients across groups, we treat differences in point estimates across subgroups as descriptive.

At the Meets threshold, Black and Hispanic students experienced first-stage enrollment gains of 16.7 percentage points (SE = 4.2 pp) and 17.1 percentage points (SE = 2.1 pp), respectively, close to the pooled estimate of 17.4 percentage points. For Hispanic students, these gains persisted through the sequence: Algebra I enrollment by eighth grade increased by 11.1 percentage points (SE = 2.4 pp), Geometry or Algebra II enrollment by ninth grade by 11.1 percentage points (SE = 2.4 pp), and Precalculus or higher by eleventh grade by 7.0 percentage points (SE = 2.2 pp). Estimates for Black students are also positive through high school but become smaller and less precise in later grades: Algebra I enrollment by eighth grade increased by 8.6 percentage points (SE = 4.1 pp), while the eleventh-grade Precalculus estimate is 3.4 percentage points (SE = 3.5 pp). Economically disadvantaged students, who comprise most of the Meets sample, closely track the pooled estimates throughout the sequence.

The pattern is similar at the Masters threshold for Hispanic and economically disadvantaged students. Hispanic students experienced a 22.2 percentage point first-stage increase in advanced math enrollment (SE = 3.5 pp), followed by gains of 15.4 percentage points in Algebra I enrollment by eighth grade and 15.5 percentage points in Geometry or Algebra II enrollment by ninth grade. The estimate declines to 9.7 percentage points by tenth grade and 5.0 percentage points by eleventh grade, with the latter no longer statistically significant. Economically disadvantaged students show similarly persistent effects: advanced math enrollment rises by 20.5 percentage points in sixth grade, Algebra I enrollment by 14.8 percentage points by eighth grade, and Precalculus or higher by 6.2 percentage points by eleventh grade. Estimates for Black students near the Masters threshold are generally positive but considerably less precise, reflecting the smaller sample around this cutoff.

Gender differences are more mixed. At the Meets threshold, female students had a somewhat

smaller first-stage effect than male students (15.5 versus 19.6 percentage points), but larger point estimates later in the sequence. By ninth grade, Geometry or Algebra II enrollment increased by 15.8 percentage points for female students compared with 8.1 percentage points for male students; by eleventh grade, the corresponding Precalculus-or-higher estimates were 8.7 and 6.0 percentage points. This pattern does not carry over cleanly to the Masters threshold. There, middle-school and early high-school effects are broadly similar by gender, while by eleventh grade the Precalculus-or-higher estimate is 0.9 percentage points for female students and 9.2 percentage points for male students.

One suggestive gender difference appears in disciplinary outcomes. At the Meets threshold, ninth-grade disciplinary incidents decline by 4.7 percentage points for female students but increase by 4.5 percentage points for male students. At the Masters threshold, disciplinary incidents decline by 6.1 percentage points for female students and are essentially unchanged for male students. These estimates suggest that the pooled disciplinary results may mask different responses by gender, although we do not interpret the subgroup differences as definitive without formal tests of equality across groups.

5 Conclusion

Schools face a persistent tension in deciding who should enter advanced mathematics. Discretionary placement can overlook prepared students, while broad acceleration can place others in courses they are not ready to complete. Automatic enrollment offers a middle path: schools use explicit achievement criteria to make advanced placement the default while preserving families' ability to opt out. This approach has gained substantial policy traction. By spring 2026, nearly twenty states had enacted or proposed statewide automatic-enrollment mandates.

However, this momentum has outpaced evidence on whether automatic enrollment in middle school changes students' subsequent academic trajectories. Our study examines Dallas ISD's early adoption of automatic enrollment, allowing us to follow students through eleventh grade and estimate effects at two common achievement benchmarks that schools implemented, near the 50th and 75th percentiles. These placement margins provide evidence directly relevant to the question of how broadly states should extend the default. The lower cutoff is especially informative because none of the state mandates documented in our policy review Zuo et al. (2026) sets eligibility as low

as the 50th percentile.

We find that automatic enrollment substantially expanded access to advanced math and generated durable gains in course progression and completion. At the lower cutoff, the policy more than doubled initial advanced math enrollment and eventual AP math passing, with substantially larger implied effects among students whose initial placement changed. Effects at the higher cutoff were similar through early high school but became smaller and less precisely estimated in later coursework. We find little evidence that these gains came at the expense of attendance or continued enrollment in the district.

Longer follow-up will allow us to assess whether these gains extend to graduation and post-secondary outcomes, while evidence from other districts—including Texas's statewide automatic enrollment mandate that began in 2024-25—will hopefully shed light on the extent to which effects depend on local implementation. As states put automatic-enrollment mandates into practice, our findings from the Dallas experience suggest that students well beyond the highest-achieving groups can benefit substantially from earlier access to advanced math, and sustain that progress well into high school.

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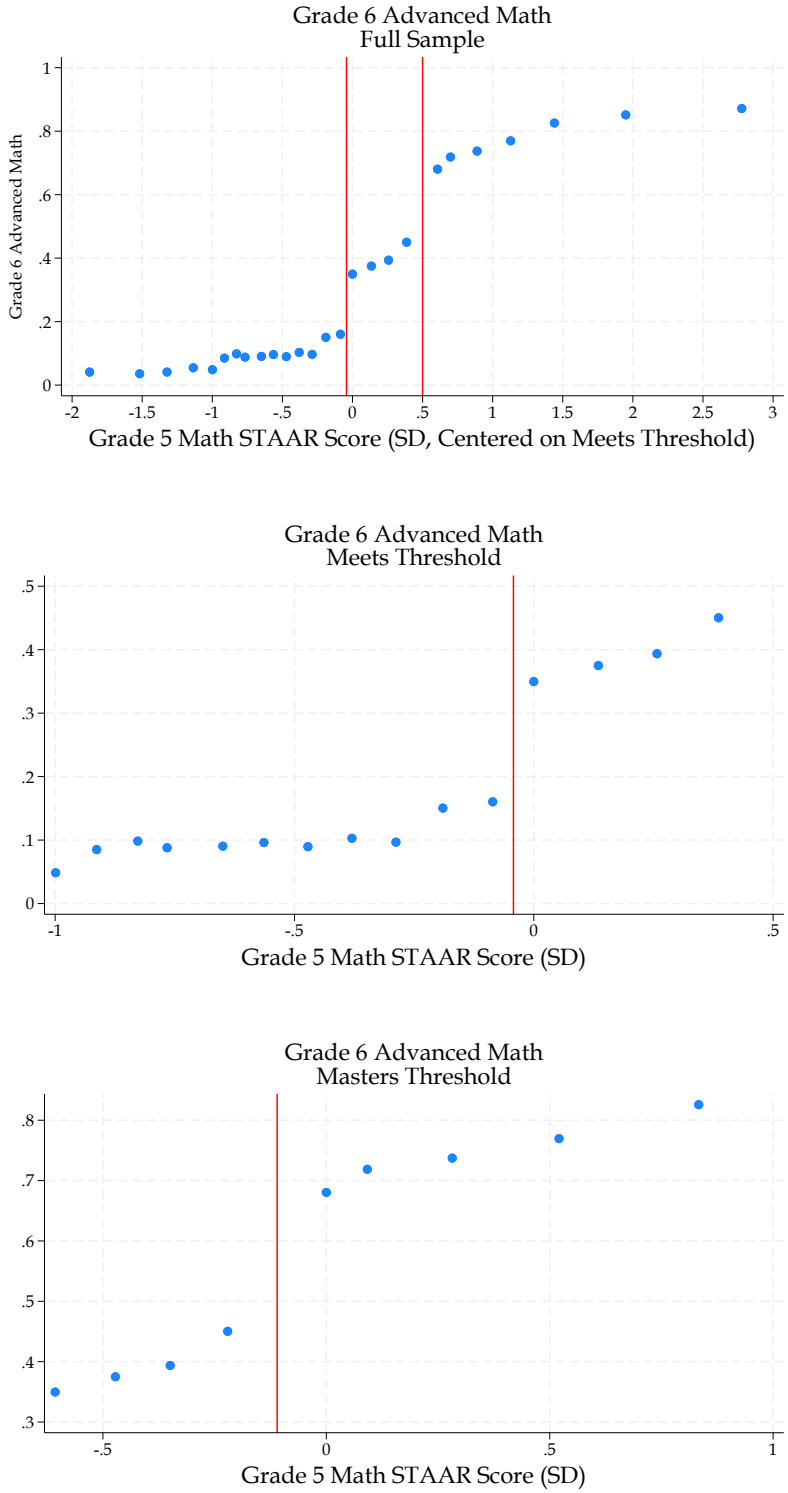
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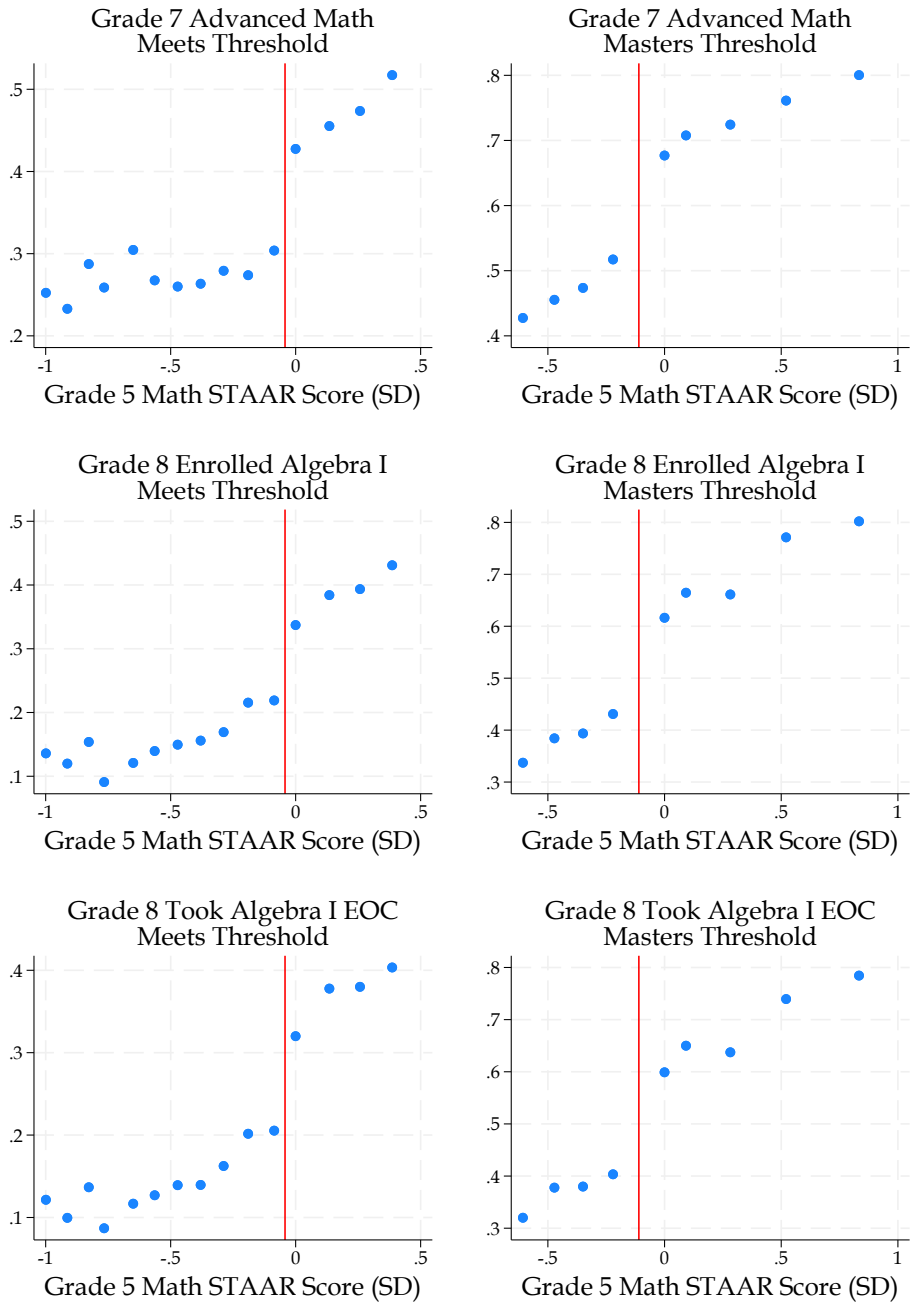
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Figure 1: First-Stage Effects on 6th-Grade Advanced Math Enrollment, by Achievement Margin (2019-20 Cohort)



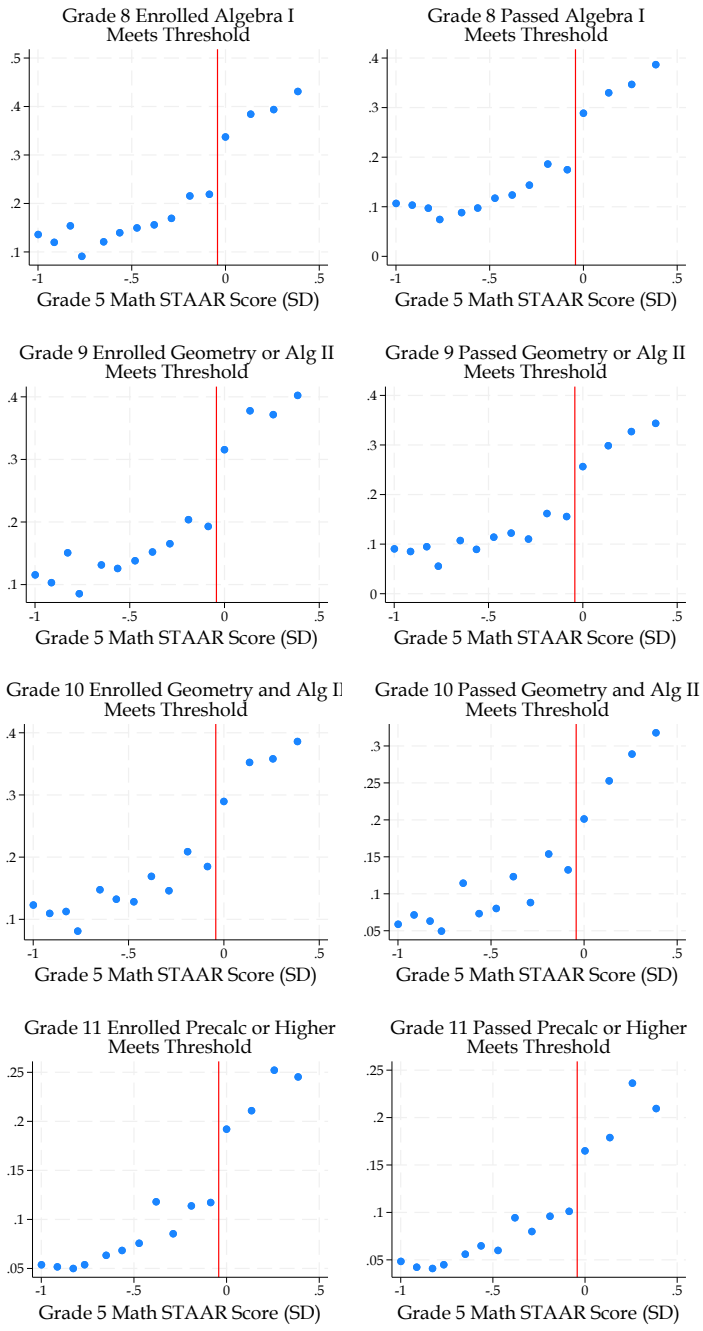
Note: Figure plots binned means of sixth-grade advanced math enrollment against standardized fifth-grade math STAAR scores for the 2019–20 sixth-grade cohort. The top panel shows the full sample, with scores centered on the Meets threshold and red vertical lines marking the Meets and Masters eligibility boundaries. The lower panels show the threshold-specific samples used to examine each discontinuity. The Meets sample excludes scores at or above Masters; the Masters sample excludes scores below Meets. Enrollment rates are expressed as proportions.

Figure 2: Binned Scatterplots on Middle School Advanced Math Enrollment, by Achievement Margin (2019-20 Cohort)



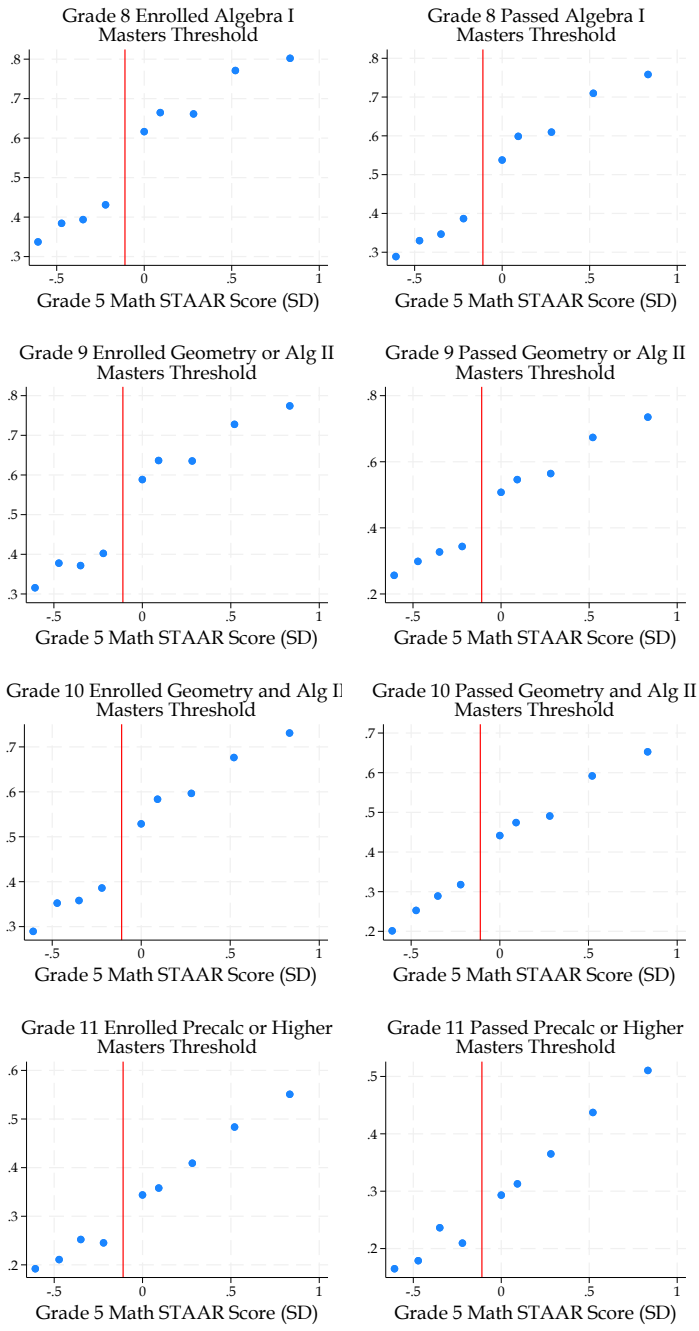
Note: Figure plots binned means of seventh-grade advanced math enrollment, Algebra I enrollment by eighth grade, and Algebra I end-of-course examination participation in eighth grade against standardized fifth-grade math STAAR scores for the 2019–20 sixth-grade cohort. Left panels show the Meets threshold; right panels show the Masters threshold. Red vertical lines mark the relevant eligibility boundaries. Each threshold-specific sample excludes the other placement discontinuity. Outcomes are expressed as proportions.

Figure 3: Binned Scatterplots on High School Course Outcomes, Meets Threshold (2019-20 Cohort)



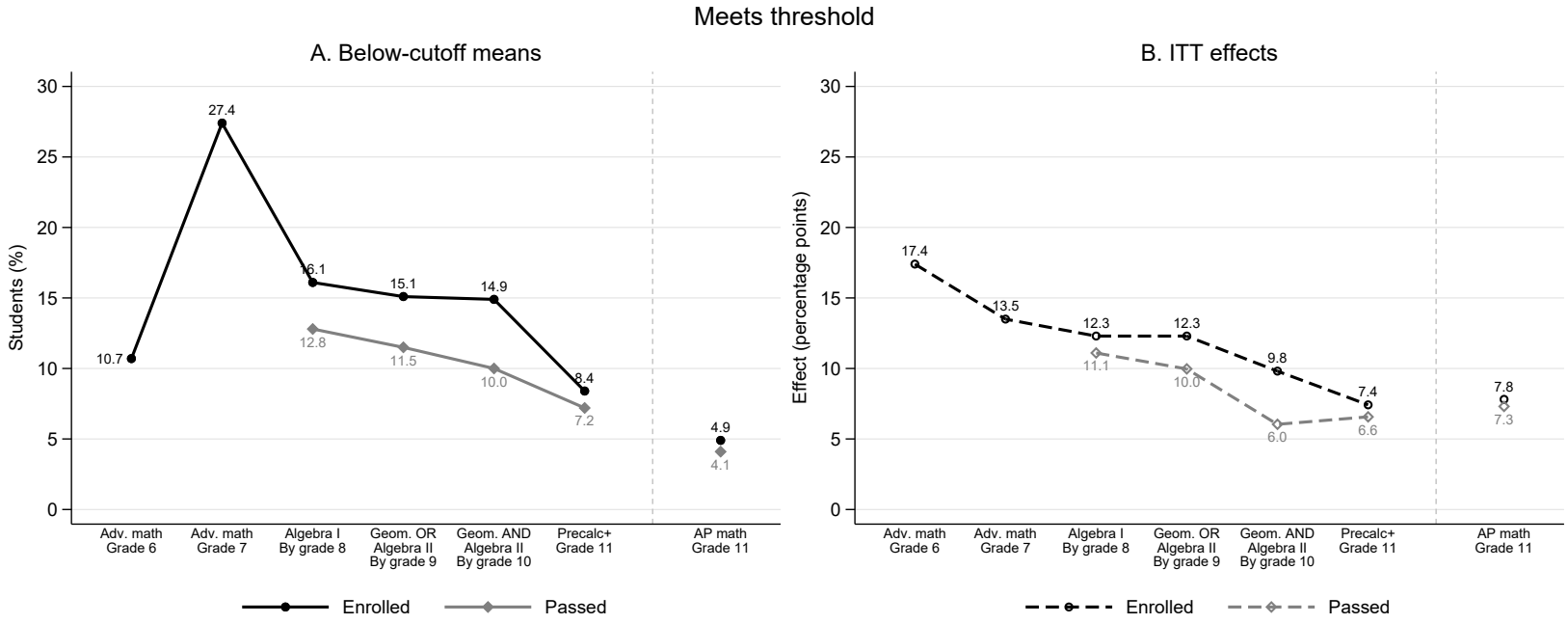
Note: Figure plots binned means of course enrollment and passing outcomes near the Meets Grade Level threshold for the 2019–20 sixth-grade cohort. Left panels show enrollment; right panels show passing, measured unconditionally on enrollment. The horizontal axes show standardized fifth-grade math STAAR scores, and red vertical lines mark the Meets eligibility boundary. Samples exclude scores at or above the Masters threshold. Outcomes are expressed as proportions and correspond to the course milestones reported in Table 3.

Figure 4: Binned Scatterplots on High School Course Outcomes, Masters Threshold (2019-20 Cohort)



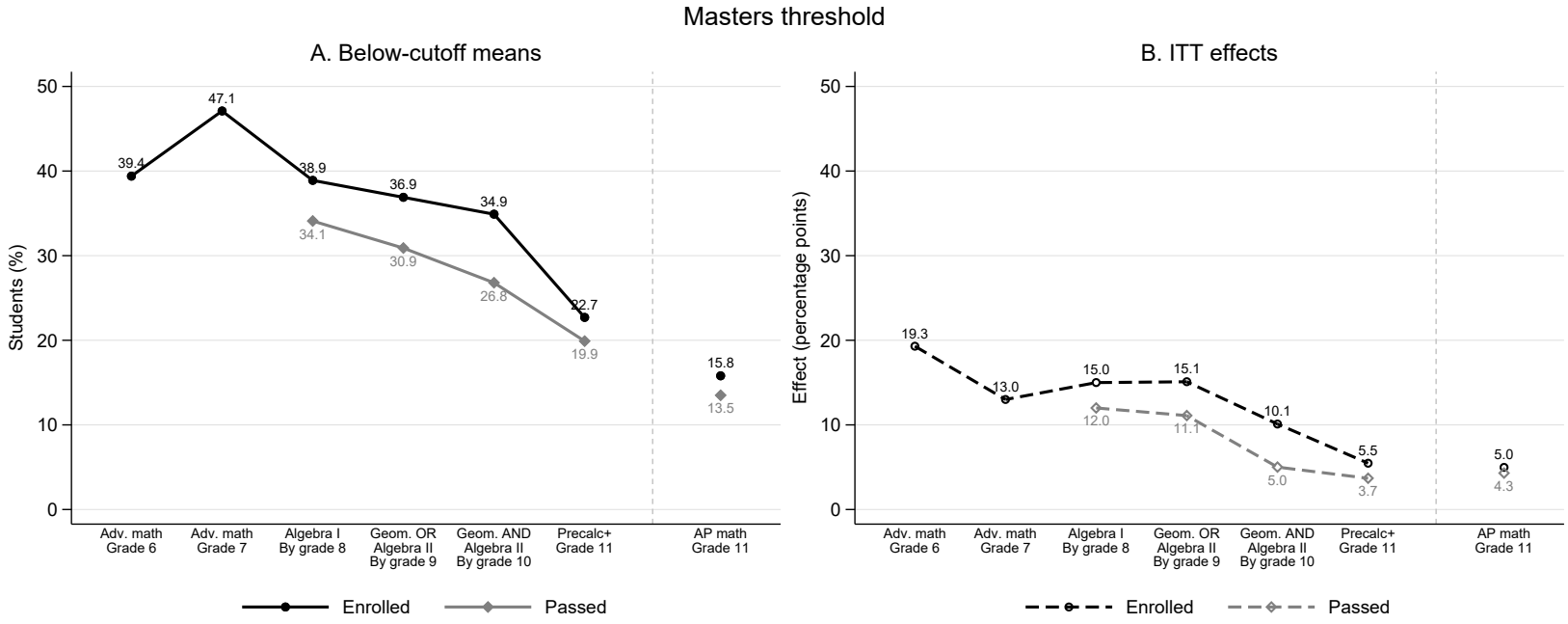
Note: Figure plots binned means of course enrollment and passing outcomes near the Masters Grade Level threshold for the 2019–20 sixth-grade cohort. Left panels show enrollment; right panels show passing, measured unconditionally on enrollment. The horizontal axes show standardized fifth-grade math STAAR scores, and red vertical lines mark the Masters eligibility boundary. Samples exclude scores below the Meets threshold. Outcomes are expressed as proportions and correspond to the course milestones reported in Table 4.

Figure 5: Enrollment and course completion at the Meets threshold



Note: Panel A reports below-cutoff means (percent), and Panel B reports intent-to-treat effects (percentage points) for the 2019–20 Dallas ISD sixth-grade cohort. Black circles indicate enrollment; gray diamonds indicate passing, measured unconditionally on enrollment. Estimates use the fully controlled regression discontinuity specification with a one standard deviation bandwidth, student covariates, and fifth-grade campus fixed effects. AP math appears separately because it overlaps with Precalculus or higher. Samples vary across outcomes. Tables 1, 2, 3, and 5 report the underlying estimates and standard errors.

Figure 6: Enrollment and course completion at the Masters threshold



Note: Panel A reports below-cutoff means (percent), and Panel B reports intent-to-treat effects (percentage points) for the 2019–20 Dallas ISD sixth-grade cohort. Black circles indicate enrollment; gray diamonds indicate passing, measured unconditionally on enrollment. Estimates use the fully controlled regression discontinuity specification with a one-standard-deviation bandwidth, student covariates, and fifth-grade campus fixed effects. AP math appears separately because it overlaps with Precalculus or higher. Samples vary across outcomes. Tables 1, 2, 4, and 5 report the underlying estimates and standard errors.

Table 1: First-Stage Effects on 6th-Grade Advanced Math Enrollment, Meets and Masters Thresholds (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Full ITT Sample						
RD Estimate	0.204***	0.202***	0.187***	0.214***	0.208***	0.208***
	(0.0192)	(0.0191)	(0.0170)	(0.0321)	(0.0320)	(0.0296)
Below-cutoff mean	0.091	0.091	0.091	0.394	0.394	0.394
N	7,295	7,295	7,295	6,066	6,066	6,066
Panel B: BW = 1 SD						
RD Estimate	0.192***	0.191***	0.174***	0.195***	0.191***	0.193***
	(0.0205)	(0.0204)	(0.0182)	(0.0329)	(0.0328)	(0.0303)
Below-cutoff mean	0.107	0.107	0.107	0.394	0.394	0.394
N	6,005	6,005	6,005	5,343	5,343	5,342
Panel C: BW = 0.5 SD						
RD Estimate	0.165***	0.168***	0.154***	0.178***	0.175***	0.166***
	(0.0244)	(0.0243)	(0.0216)	(0.0435)	(0.0433)	(0.0401)
Below-cutoff mean	0.124	0.124	0.124	0.408	0.408	0.408
N	4,415	4,415	4,415	3,661	3,661	3,661
Panel D: CCT Optimal Bandwidth						
RD Estimate	0.157***	0.160***	0.151***	0.196***	0.191***	0.189***
	(0.0305)	(0.0304)	(0.0271)	(0.0335)	(0.0334)	(0.0308)
Below-cutoff mean	0.138	0.138	0.138	0.394	0.394	0.394
N	3,047	3,047	3,047	4,815	4,815	4,815
Bandwidth (SD)	0.308	0.308	0.308	0.832	0.832	0.832

Note: Table reports regression discontinuity estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on sixth-grade advanced math enrollment for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Meets samples exclude scores at or above Masters, and Masters samples exclude scores below Meets. Panels A-D use the full threshold-specific sample, a 1-SD bandwidth, a 0.5-SD bandwidth, and the MSE-optimal bandwidth, respectively. Regressions allow separate linear slopes on either side of the cutoff and weight observations equally. Within each threshold, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Below-cutoff means are observed means within the corresponding estimation samples. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: ITT Effects on Middle-School Advanced Math Course Participation (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: 7th grade advanced math enrollment						
RD Estimate	0.133***	0.132***	0.135***	0.124***	0.122***	0.130***
	(0.0230)	(0.0230)	(0.0202)	(0.0332)	(0.0331)	(0.0310)
Below-cutoff mean	0.274	0.274	0.274	0.471	0.471	0.471
N	6,066	6,066	6,066	5,453	5,453	5,453
Panel B: Enrolled Algebra I by 8th grade						
RD Estimate	0.126***	0.126***	0.123***	0.146***	0.147***	0.150***
	(0.0217)	(0.0215)	(0.0199)	(0.0331)	(0.0327)	(0.0314)
Below-cutoff mean	0.161	0.161	0.161	0.389	0.389	0.389
N	5,921	5,921	5,921	5,377	5,377	5,377
Panel C: Algebra I EOC test taken in 8th grade						
RD Estimate	0.128***	0.129***	0.127***	0.156***	0.157***	0.162***
	(0.0211)	(0.0209)	(0.0194)	(0.0326)	(0.0322)	(0.0311)
Below-cutoff mean	0.148	0.148	0.148	0.372	0.372	0.372
N	6,073	6,073	6,073	5,450	5,450	5,450

Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on middle-school course participation for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Outcomes indicate seventh-grade advanced math enrollment, enrollment in Algebra I or higher by eighth grade, and taking the Algebra I end-of-course examination in eighth grade. All regressions use a 1-SD bandwidth, exclude the other placement discontinuity, and allow separate linear slopes on either side of the cutoff. Within each threshold, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Below-cutoff means are observed means within the estimation samples. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: ITT Effects on High School Advanced Math Course Outcomes - Meets Threshold (2019–20 Cohort)

	Enrolled in Course			Passed Course		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Algebra I by 8th grade						
RD Estimate	0.126***	0.126***	0.123***	0.111***	0.111***	0.111***
	(0.0217)	(0.0215)	(0.0199)	(0.0206)	(0.0204)	(0.0191)
Below-cutoff mean	0.161	0.161	0.161	0.128	0.128	0.128
N	5,921	5,921	5,921	5,921	5,921	5,921
Panel B: Geometry OR Algebra II by 9th grade						
RD Estimate	0.127***	0.126***	0.123***	0.104***	0.103***	0.0997***
	(0.0215)	(0.0213)	(0.0199)	(0.0200)	(0.0198)	(0.0186)
Below-cutoff mean	0.151	0.151	0.151	0.115	0.115	0.115
N	5,754	5,754	5,754	5,754	5,754	5,754
Panel C: Geometry AND Algebra II by 10th grade						
RD Estimate	0.103***	0.103***	0.0981***	0.0623***	0.0623***	0.0604***
	(0.0216)	(0.0214)	(0.0202)	(0.0190)	(0.0189)	(0.0184)
Below-cutoff mean	0.149	0.149	0.149	0.100	0.100	0.100
N	5,510	5,510	5,510	5,510	5,510	5,510
Panel D: Advanced Math Course (Precalc+) in 11th grade						
RD Estimate	0.0707***	0.0714***	0.0742***	0.0626***	0.0636***	0.0657***
	(0.0183)	(0.0182)	(0.0180)	(0.0172)	(0.0171)	(0.0169)
Below-cutoff mean	0.084	0.084	0.084	0.072	0.072	0.072
N	5,454	5,454	5,454	5,454	5,454	5,454

Note: Table reports intent-to-treat estimates at the Meets Grade Level threshold for the 2019-20 sixth-grade cohort. Columns (1)-(3) report effects on course enrollment; columns (4)-(6) report effects on course passing. Panels A-C measure progression through the specified course milestones by the end of the indicated grade. Panel D measures enrollment in or passing Precalculus or higher in eleventh grade. Passing outcomes are measured unconditionally on enrollment. All regressions use a 1-SD bandwidth, exclude scores at or above the Masters threshold, and allow separate linear slopes on either side of the Meets cutoff. Within each outcome, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Below-cutoff means are observed means within the estimation samples. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: ITT Effects on High School Advanced Math Course Outcomes - Masters Threshold (2019–20 Cohort)

	Enrolled in Course			Passed Course		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Algebra I by 8th grade						
RD Estimate	0.146***	0.147***	0.150***	0.116***	0.116***	0.120***
	(0.0331)	(0.0327)	(0.0314)	(0.0326)	(0.0321)	(0.0309)
Below-cutoff mean	0.389	0.389	0.389	0.341	0.341	0.341
N	5,377	5,377	5,377	5,377	5,377	5,377
Panel B: Geometry OR Algebra II by 9th grade						
RD Estimate	0.149***	0.149***	0.151***	0.111***	0.108***	0.111***
	(0.0333)	(0.0329)	(0.0319)	(0.0324)	(0.0319)	(0.0311)
Below-cutoff mean	0.369	0.369	0.369	0.309	0.309	0.309
N	5,265	5,265	5,265	5,265	5,265	5,265
Panel C: Geometry AND Algebra II by 10th grade						
RD Estimate	0.103***	0.101***	0.101***	0.0525	0.0495	0.0501
	(0.0336)	(0.0332)	(0.0326)	(0.0319)	(0.0316)	(0.0314)
Below-cutoff mean	0.349	0.349	0.349	0.268	0.268	0.268
N	5,123	5,123	5,123	5,123	5,123	5,123
Panel D: Advanced Math Course (Precalc+) in 11th grade						
RD Estimate	0.0509*	0.0482	0.0547*	0.0341	0.0312	0.0369
	(0.0304)	(0.0301)	(0.0301)	(0.0290)	(0.0286)	(0.0286)
Below-cutoff mean	0.227	0.227	0.227	0.199	0.199	0.199
N	5,073	5,073	5,073	5,073	5,073	5,073

Note: Table reports intent-to-treat estimates at the Masters Grade Level threshold for the 2019-20 sixth-grade cohort. Columns (1)-(3) report effects on course enrollment; columns (4)-(6) report effects on course passing. Panels A-C measure progression through the specified course milestones by the end of the indicated grade. Panel D measures enrollment in or passing Precalculus or higher in eleventh grade. Passing outcomes are measured unconditionally on enrollment. All regressions use a 1-SD bandwidth, exclude scores below the Meets threshold, and allow separate linear slopes on either side of the Masters cutoff. Within each outcome, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Below-cutoff means are observed means within the estimation samples. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: ITT Effects on 11th Grade AP Course Outcomes (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Grade 11 Enrolled in AP Course (Any Subject)						
RD Estimate	0.0768***	0.0789***	0.0826***	0.0288	0.0275	0.0370
	(0.0231)	(0.0229)	(0.0230)	(0.0344)	(0.0342)	(0.0343)
Below-cutoff mean	0.212	0.212	0.212	0.370	0.370	0.370
N	5,454	5,454	5,454	5,073	5,073	5,073
Panel B: Grade 11 Passed AP Course (Any Subject)						
RD Estimate	0.0780***	0.0800***	0.0832***	0.0356	0.0336	0.0402
	(0.0222)	(0.0221)	(0.0220)	(0.0336)	(0.0333)	(0.0335)
Below-cutoff mean	0.184	0.184	0.184	0.333	0.333	0.333
N	5,454	5,454	5,454	5,073	5,073	5,073
Panel C: Grade 11 Enrolled in AP Math Course						
RD Estimate	0.0756***	0.0759***	0.0781***	0.0441	0.0424	0.0498*
	(0.0156)	(0.0155)	(0.0154)	(0.0271)	(0.0270)	(0.0269)
Below-cutoff mean	0.049	0.049	0.049	0.158	0.158	0.158
N	5,454	5,454	5,454	5,073	5,073	5,073
Panel D: Grade 11 Passed AP Math Course						
RD Estimate	0.0706***	0.0711***	0.0731***	0.0392	0.0373	0.0430*
	(0.0145)	(0.0145)	(0.0144)	(0.0253)	(0.0251)	(0.0250)
Below-cutoff mean	0.041	0.041	0.041	0.135	0.135	0.135
N	5,454	5,454	5,454	5,073	5,073	5,073

Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on eleventh-grade AP course enrollment and passing for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Each outcome indicates enrollment in or passing at least one AP course in the specified subject category. "Any subject" includes mathematics. Passing refers to course completion, not AP examination performance, and is measured unconditionally on enrollment. All regressions use a 1-SD bandwidth, exclude the other placement discontinuity, and allow separate linear slopes on either side of the cutoff. Within each threshold, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Heterogeneity by Student Subgroup, Meets Grade Level Threshold (2019–20 Cohort)

	(1) All	(2) Female	(3) Male	(4) Black	(5) Hispanic	(6) White/Other	(7) Disadv.	(8) Not Disadv.
Middle School Course Participation								
6th grade Advanced Math	0.174***	0.155***	0.196***	0.167***	0.171***	0.163	0.175***	0.136*
	(0.0182)	(0.0259)	(0.0258)	(0.0415)	(0.0212)	(0.100)	(0.0189)	(0.0743)
7th grade Advanced Math	0.135***	0.147***	0.121***	0.0982**	0.136***	0.277***	0.117***	0.274***
	(0.0202)	(0.0287)	(0.0291)	(0.0475)	(0.0233)	(0.100)	(0.0209)	(0.0823)
Algebra I by 8th grade	0.123***	0.154***	0.0853***	0.0864**	0.111***	0.239**	0.111***	0.221***
	(0.0199)	(0.0286)	(0.0285)	(0.0409)	(0.0238)	(0.106)	(0.0207)	(0.0801)
High school course-taking								
Geometry OR Alg 2 Enrollment by 9th	0.123***	0.158***	0.0812***	0.0785**	0.111***	0.288***	0.108***	0.279***
	(0.0199)	(0.0288)	(0.0283)	(0.0398)	(0.0239)	(0.103)	(0.0206)	(0.0805)
Geometry AND Alg 2 Enrollment by 10th	0.0981***	0.137***	0.0599**	0.0523	0.0918***	0.160	0.0841***	0.233***
	(0.0202)	(0.0294)	(0.0286)	(0.0401)	(0.0246)	(0.106)	(0.0209)	(0.0851)
Advanced Math Course (Precalc+) in 11th grade	0.0742***	0.0867***	0.0595**	0.0335	0.0699***	0.161	0.0756***	0.0538
	(0.0180)	(0.0259)	(0.0260)	(0.0353)	(0.0220)	(0.115)	(0.0185)	(0.0829)
Broader schooling outcomes								
9th grade attendance rate	0.00195	0.00448	0.00155	0.0124	0.00211	-0.0282	0.00251	0.00965
	(0.00672)	(0.00972)	(0.00971)	(0.0139)	(0.00794)	(0.0305)	(0.00716)	(0.0189)
9th grade disciplinary incident	-0.00282	-0.0474**	0.0450*	-0.0744	0.0217	-0.136	-0.00615	-0.00742
	(0.0169)	(0.0226)	(0.0262)	(0.0471)	(0.0179)	(0.0855)	(0.0176)	(0.0688)
9th grade enrolled in Dallas ISD	0.00771	0.0183	-0.00603	0.0783	-0.0103	0.0885	0.00761	-0.0247
	(0.0221)	(0.0314)	(0.0322)	(0.0507)	(0.0257)	(0.110)	(0.0233)	(0.0828)
Student covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
5th grade school FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (ITT)	6,369	3,140	3,229	1,534	4,465	370	5,839	530

Note: Note: Table reports intent-to-treat estimates at the Meets Grade Level threshold for the 2019-20 sixth-grade cohort. Column (1) reports pooled estimates; the remaining columns report estimates for the indicated student subgroups. Each cell comes from a separate regression for the specified outcome and sample. All regressions use a 1-SD bandwidth, exclude scores at or above the Masters threshold, and include separate linear slopes on either side of the cutoff, student covariates, and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status, as applicable within each subgroup. Differences between subgroup estimates are not formal tests of differences in effects. Heteroskedasticity-robust standard errors appear in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Heterogeneity by Student Subgroup, Masters Grade Level Threshold (2019–20 Cohort)

	(1) All	(2) Female	(3) Male	(4) Black	(5) Hispanic	(6) White/Other	(7) Disadv.	(8) Not Disadv.
Middle School Course Participation								
6th grade Advanced Math	0.193***	0.178***	0.206***	0.126	0.222***	0.0998	0.205***	0.138
	(0.0303)	(0.0420)	(0.0446)	(0.0781)	(0.0352)	(0.111)	(0.0325)	(0.0914)
7th grade Advanced Math	0.130***	0.107**	0.144***	0.00579	0.160***	0.0820	0.143***	0.103
	(0.0310)	(0.0434)	(0.0453)	(0.0799)	(0.0357)	(0.119)	(0.0329)	(0.0948)
Algebra I by 8th grade	0.150***	0.164***	0.128***	0.110	0.154***	0.0919	0.148***	0.210**
	(0.0314)	(0.0445)	(0.0454)	(0.0816)	(0.0366)	(0.123)	(0.0335)	(0.0965)
High school course-taking								
Geometry OR Alg 2 Enrollment by 9th	0.151***	0.155***	0.131***	0.0921	0.155***	0.116	0.150***	0.199*
	(0.0319)	(0.0451)	(0.0461)	(0.0794)	(0.0374)	(0.134)	(0.0340)	(0.103)
Geometry AND Alg 2 Enrollment by 10th	0.101***	0.109**	0.0756	0.0223	0.0969**	0.119	0.0973***	0.190*
	(0.0326)	(0.0459)	(0.0474)	(0.0789)	(0.0384)	(0.139)	(0.0347)	(0.107)
Advanced Math Course (Precalc+) in 11th grade	0.0547*	0.00936	0.0918**	0.0248	0.0502	0.150	0.0621**	-0.0484
	(0.0301)	(0.0429)	(0.0444)	(0.0733)	(0.0354)	(0.156)	(0.0316)	(0.114)
Broader schooling outcomes								
9th grade attendance rate	0.00387	-0.00196	0.0101	0.00554	0.00340	-0.0404	0.00212	0.0250
	(0.00835)	(0.0116)	(0.0124)	(0.0202)	(0.00951)	(0.0357)	(0.00898)	(0.0222)
9th grade disciplinary incident	-0.0397*	-0.0613**	-0.00920	-0.0548	-0.0310	-0.0173	-0.0441*	-0.0259
	(0.0216)	(0.0278)	(0.0341)	(0.0716)	(0.0231)	(0.0656)	(0.0230)	(0.0654)
9th grade enrolled in Dallas ISD	0.0308	0.0400	0.0310	0.0572	0.0366	0.0886	0.0291	0.0519
	(0.0303)	(0.0430)	(0.0438)	(0.0826)	(0.0344)	(0.127)	(0.0321)	(0.101)
Student covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
5th grade school FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (ITT)	5,654	2,842	2,812	891	4,231	532	4,855	799

Note: Note: Table reports intent-to-treat estimates at the Masters Grade Level threshold for the 2019-20 sixth-grade cohort. Column (1) reports pooled estimates; the remaining columns report estimates for the indicated student subgroups. Each cell comes from a separate regression for the specified outcome and sample. All regressions use a 1-SD bandwidth, exclude scores below the Meets threshold, and include separate linear slopes on either side of the cutoff, student covariates, and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status, as applicable within each subgroup. Differences between subgroup estimates are not formal tests of differences in effects. Heteroskedasticity-robust standard errors appear in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.

Appendix A: Appendix Tables and Figures

Table A1: Classification of Advanced High School Mathematics Coursework

Outcome	Courses included
Algebra I or higher	Algebra I; Integrated Mathematics I
Geometry or higher	Geometry; Integrated Mathematics II
Algebra II or higher	Algebra II; Integrated Mathematics III
Geometry and Algebra II or higher	Completion of both Geometry (or Integrated Mathematics II) and Algebra II (or Integrated Mathematics III)
Post-Algebra II coursework or higher	AP Statistics; Advanced Quantitative Reasoning; Elements of Data Science; Discrete Mathematics; Linear Algebra; Number Theory; Linear Programming
Any AP mathematics	AP Statistics; AP Precalculus; AP Calculus AB; AP Calculus BC
AP Statistics	AP Statistics
Precalculus or higher	Precalculus; Integrated Mathematics IV; AP Precalculus; IB Mathematics: Analysis and Interpretation (Standard Level); IB Mathematical Studies (Standard Level)
AP Precalculus	AP Precalculus
Above Precalculus	AP Calculus AB; AP Calculus BC; Multivariable Calculus; IB Mathematics: Analysis and Interpretation (Higher Level); IB Mathematics: Analysis and Approaches (Standard or Higher Level); IB Mathematics (Standard or Higher Level); IB Further Mathematics
AP Calculus	AP Calculus AB; AP Calculus BC

Note: This table presents a crosswalk depicting how courses were classified using Texas course codes, state course descriptions, and prerequisite guidance.

Table A2: Analytic Sample Characteristics

	(1) Full Sample			(2) Meets Sample (Full)			(3) Meets Sample (1 SD)			(4) Masters Sample (Full)			(5) Masters Sample (1 SD)		
	Mean	(SD)	N	Mean	(SD)	N	Mean	(SD)	N	Mean	(SD)	N	Mean	(SD)	N
Sample Characteristics															
Female	0.49	(0.50)	11,734	0.48	(0.50)	7,749	0.49	(0.50)	6,369	0.50	(0.50)	6,451	0.50	(0.50)	5,654
Hispanic	0.71	(0.45)	11,734	0.68	(0.47)	7,749	0.70	(0.46)	6,369	0.76	(0.43)	6,451	0.75	(0.43)	5,654
Black	0.21	(0.41)	11,734	0.26	(0.44)	7,749	0.24	(0.43)	6,369	0.15	(0.35)	6,451	0.16	(0.36)	5,654
White/Other Race	0.08	(0.27)	11,734	0.06	(0.23)	7,749	0.06	(0.23)	6,369	0.10	(0.30)	6,451	0.09	(0.29)	5,654
Economic Disadvantage	0.89	(0.31)	11,734	0.92	(0.27)	7,749	0.92	(0.28)	6,369	0.85	(0.36)	6,451	0.86	(0.35)	5,654
Limited English Proficiency	0.51	(0.50)	11,734	0.49	(0.50)	7,749	0.50	(0.50)	6,369	0.53	(0.50)	6,451	0.52	(0.50)	5,654
5th Grade Math STAAR Taken in Spanish	0.01	(0.11)	11,734	0.02	(0.13)	7,749	0.01	(0.11)	6,369	0.01	(0.08)	6,451	0.01	(0.08)	5,654
5th Grade Math STAAR (Standardized)	-0.03	(0.99)	11,734	-0.59	(0.61)	7,749	-0.37	(0.41)	6,369	0.67	(0.69)	6,451	0.47	(0.44)	5,654
Middle School Course Placement															
Advanced Math Grade 6	0.38	(0.49)	11,034	0.19	(0.39)	7,295	0.22	(0.41)	6,005	0.62	(0.48)	6,066	0.59	(0.49)	5,343
Advanced Math Grade 7	0.48	(0.50)	11,208	0.33	(0.47)	7,337	0.35	(0.48)	6,066	0.65	(0.48)	6,228	0.62	(0.49)	5,453
Enrolled Algebra I by 8th	0.40	(0.49)	10,997	0.22	(0.41)	7,159	0.25	(0.43)	5,921	0.60	(0.49)	6,152	0.57	(0.50)	5,377
Confirmatory Outcomes															
Passed Algebra I by 8th	0.36	(0.48)	10,997	0.18	(0.39)	7,159	0.21	(0.41)	5,921	0.55	(0.50)	6,152	0.51	(0.50)	5,377
Enrolled Geometry or Algebra II by 9th	0.38	(0.49)	10,641	0.21	(0.41)	6,883	0.24	(0.42)	5,754	0.58	(0.49)	6,019	0.54	(0.50)	5,265
Passed Geometry or Algebra II by 9th	0.34	(0.47)	10,641	0.17	(0.37)	6,883	0.19	(0.39)	5,754	0.52	(0.50)	6,019	0.48	(0.50)	5,265
Enrolled Geometry and Algebra II by 10th	0.37	(0.48)	10,213	0.20	(0.40)	6,542	0.23	(0.42)	5,510	0.54	(0.50)	5,865	0.50	(0.50)	5,123
Passed Geometry and Algebra II by 10th	0.30	(0.46)	10,213	0.15	(0.35)	6,542	0.17	(0.37)	5,510	0.46	(0.50)	5,865	0.42	(0.49)	5,123
Enrolled Advanced Math Course (Precalc+) in 11th	0.24	(0.43)	10,141	0.12	(0.33)	6,493	0.14	(0.35)	5,454	0.37	(0.48)	5,812	0.34	(0.47)	5,073
Passed Advanced Math Course (Precalc+) in 11th	0.22	(0.41)	10,141	0.11	(0.31)	6,493	0.12	(0.33)	5,454	0.33	(0.47)	5,812	0.30	(0.46)	5,073
Passed Algebra I by 9th [Standard Track]	0.82	(0.39)	10,668	0.76	(0.43)	6,855	0.79	(0.41)	5,739	0.89	(0.31)	6,077	0.88	(0.33)	5,305
Passed Geometry or Algebra II by 10th [Standard Track]	0.80	(0.40)	10,313	0.73	(0.44)	6,598	0.75	(0.43)	5,558	0.88	(0.32)	5,925	0.87	(0.34)	5,172
Passed Geometry and Algebra II by 11th [Standard Track]	0.64	(0.48)	9,774	0.57	(0.50)	6,154	0.59	(0.49)	5,233	0.71	(0.45)	5,733	0.70	(0.46)	4,998
Attendance Rate Grade 9	0.90	(0.14)	11,023	0.88	(0.15)	7,221	0.89	(0.14)	5,975	0.92	(0.11)	6,116	0.92	(0.12)	5,361
Attendance Rate Grade 10	0.89	(0.14)	10,818	0.87	(0.16)	7,059	0.88	(0.15)	5,857	0.91	(0.12)	6,044	0.91	(0.12)	5,291
Attendance Rate Grade 11	0.89	(0.14)	10,406	0.87	(0.15)	6,724	0.88	(0.14)	5,620	0.91	(0.12)	5,887	0.90	(0.12)	5,144
Disciplinary Incident Grade 9	0.14	(0.34)	11,023	0.17	(0.38)	7,221	0.16	(0.37)	5,975	0.09	(0.29)	6,116	0.10	(0.30)	5,361
Disciplinary Incident Grade 10	0.14	(0.35)	10,818	0.17	(0.38)	7,059	0.16	(0.37)	5,857	0.10	(0.30)	6,044	0.11	(0.31)	5,291
Disciplinary Incident Grade 11	0.12	(0.32)	10,406	0.15	(0.35)	6,724	0.14	(0.34)	5,620	0.08	(0.27)	5,887	0.09	(0.28)	5,144
Remain in DISD Grade 9	0.66	(0.47)	11,734	0.63	(0.48)	7,749	0.64	(0.48)	6,369	0.71	(0.46)	6,451	0.70	(0.46)	5,654
Remain in DISD Grade 10	0.62	(0.49)	11,734	0.57	(0.49)	7,749	0.59	(0.49)	6,369	0.67	(0.47)	6,451	0.66	(0.47)	5,654
Remain in DISD Grade 11	0.57	(0.50)	11,734	0.51	(0.50)	7,749	0.54	(0.50)	6,369	0.64	(0.48)	6,451	0.63	(0.48)	5,654
Enrolled AP Course in 11th (Any Subject)	0.36	(0.48)	10,141	0.25	(0.43)	6,493	0.27	(0.45)	5,454	0.49	(0.50)	5,812	0.46	(0.50)	5,073
Passed AP Course in 11th (Any Subject)	0.33	(0.47)	10,141	0.22	(0.41)	6,493	0.24	(0.43)	5,454	0.46	(0.50)	5,812	0.43	(0.49)	5,073
Enrolled AP Math Course in 11th	0.17	(0.38)	10,141	0.08	(0.27)	6,493	0.09	(0.29)	5,454	0.27	(0.44)	5,812	0.24	(0.43)	5,073
Passed AP Math Course in 11th	0.15	(0.36)	10,141	0.07	(0.25)	6,493	0.08	(0.27)	5,454	0.24	(0.43)	5,812	0.21	(0.41)	5,073
Observations			11,734			7,749			6,369			6,451			5,654

Note: Table reports means, standard deviations, and numbers of nonmissing observations for student characteristics and study outcomes. Column (1) includes all Dallas ISD fifth graders with a valid spring 2019 math STAAR score. Column (2) includes students scoring below the Masters threshold; column (3) additionally restricts this sample to scores within 1 SD of the Meets cutoff. Column (4) includes students scoring at or above the Meets threshold; column (5) additionally restricts this sample to scores within 1 SD of the Masters cutoff. Test scores are standardized using the statewide mean and standard deviation. Outcome means use observations with nonmissing values and include students observed elsewhere in the Texas public school system after leaving Dallas ISD. Passing outcomes are measured unconditionally on course enrollment. The final row reports sample sizes before accounting for outcome availability.

Table A3: ITT Estimates of 6th Grade Attrition (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Valid 6th Grade FS (Any Course Data) - Full ITT Sample						
RD Estimate	-0.00607	-0.00431	-0.00513	0.0142	0.0150	0.0132
	(0.00972)	(0.00967)	(0.00973)	(0.0145)	(0.0144)	(0.0142)
Below-cutoff mean	0.940	0.940	0.940	0.944	0.944	0.944
N	7,749	7,749	7,749	6,451	6,451	6,451
Panel B: Valid 6th Grade FS (Any Course Data) - BW = 1 SD						
RD Estimate	-0.00788	-0.00609	-0.00536	0.0147	0.0155	0.0172
	(0.0107)	(0.0107)	(0.0107)	(0.0147)	(0.0147)	(0.0146)
Below-cutoff mean	0.942	0.942	0.942	0.944	0.944	0.944
N	6,369	6,369	6,369	5,654	5,654	5,654
Panel C: In DISD in 6th - Full ITT Sample						
RD Estimate	0.0124	0.0122	0.00153	0.00632	0.00600	0.0128
	(0.0166)	(0.0165)	(0.0164)	(0.0244)	(0.0243)	(0.0241)
Below-cutoff mean	0.805	0.805	0.805	0.817	0.817	0.817
N	7,749	7,749	7,749	6,451	6,451	6,451
Panel D: In DISD in 6th - BW = 1 SD						
RD Estimate	0.0117	0.0123	0.000130	0.00599	0.00559	0.0122
	(0.0185)	(0.0184)	(0.0182)	(0.0251)	(0.0250)	(0.0248)
Below-cutoff mean	0.804	0.804	0.804	0.817	0.817	0.817
N	6,369	6,369	6,369	5,654	5,654	5,654

Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on sixth-grade outcome availability and Dallas ISD enrollment for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. In Panels A and B, the outcome equals one if sixth-grade course records permit measurement of advanced math placement. In Panels C and D, it equals one if the student is enrolled in Dallas ISD in sixth grade. Positive estimates therefore indicate greater observability or district retention. Panels A and C use the full threshold-specific samples; Panels B and D use a 1-SD bandwidth. All regressions exclude the other placement discontinuity and allow separate linear slopes on either side of the cutoff. Specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A4: ITT Estimates on Missing 9th-11th Course Outcomes (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Grade 9 Course Outcome Non-Missing						
RD Estimate	-0.0193	-0.0173	-0.0150	0.0153	0.0175	0.0212
	(0.0134)	(0.0133)	(0.0134)	(0.0179)	(0.0178)	(0.0176)
Below-cutoff mean	0.895	0.895	0.895	0.917	0.917	0.917
N	6,369	6,369	6,369	5,654	5,654	5,654
Panel B: Grade 10 Course Outcome Non-Missing						
RD Estimate	-0.0132	-0.0108	-0.0108	0.00186	0.00403	0.00987
	(0.0156)	(0.0155)	(0.0155)	(0.0202)	(0.0201)	(0.0199)
Below-cutoff mean	0.850	0.850	0.850	0.890	0.890	0.890
N	6,369	6,369	6,369	5,654	5,654	5,654
Panel C: Grade 11 Course Outcome Non-Missing						
RD Estimate	-0.0135	-0.0105	-0.00833	0.0106	0.0130	0.0223
	(0.0162)	(0.0161)	(0.0162)	(0.0214)	(0.0212)	(0.0213)
Below-cutoff mean	0.843	0.843	0.843	0.878	0.878	0.878
N	6,369	6,369	6,369	5,654	5,654	5,654

Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on course-outcome availability for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Each outcome equals one if the relevant course outcome is nonmissing in the indicated grade, so positive estimates indicate greater observability. All regressions use a 1-SD bandwidth, exclude the other placement discontinuity, and allow separate linear slopes on either side of the cutoff. Within each threshold, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A5: ITT Effects on High School Attendance Rates (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: 9th grade attendance rate						
RD Estimate	0.00280	0.00270	0.00195	0.00464	0.00381	0.00387
	(0.00673)	(0.00669)	(0.00672)	(0.00839)	(0.00836)	(0.00835)
Below-cutoff mean	0.876	0.876	0.876	0.900	0.900	0.900
N	5,975	5,975	5,975	5,361	5,361	5,361
Panel B: 10th grade attendance rate						
RD Estimate	0.00241	0.00262	0.00129	0.0149	0.0144	0.0146
	(0.00709)	(0.00708)	(0.00711)	(0.00927)	(0.00921)	(0.00914)
Below-cutoff mean	0.865	0.865	0.865	0.893	0.893	0.893
N	5,857	5,857	5,857	5,291	5,291	5,291
Panel C: 11th grade attendance rate						
RD Estimate	0.000694	0.00114	0.00206	0.00581	0.00552	0.00651
	(0.00666)	(0.00664)	(0.00669)	(0.00841)	(0.00837)	(0.00846)
Below-cutoff mean	0.866	0.866	0.866	0.891	0.891	0.891
N	5,620	5,620	5,620	5,144	5,144	5,144

Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on attendance rates in grades 9-11 for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Attendance rates are expressed as proportions, so an estimate of 0.01 represents a one-percentage-point change. All regressions use a 1-SD bandwidth, exclude the other placement discontinuity, and allow separate linear slopes on either side of the cutoff. Within each threshold, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Samples include students with observed attendance outcomes in the indicated grade. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A6: ITT Effects on High School Disciplinary Incidents (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: 9th grade disciplinary incident						
RD Estimate	-0.00100	-0.000828	-0.00282	-0.0393*	-0.0420*	-0.0397*
	(0.0172)	(0.0168)	(0.0169)	(0.0222)	(0.0220)	(0.0216)
Below-cutoff mean	0.178	0.178	0.178	0.130	0.130	0.130
N	5,975	5,975	5,975	5,361	5,361	5,361
Panel B: 10th grade disciplinary incident						
RD Estimate	-0.0214	-0.0219	-0.0217	-0.0337	-0.0363*	-0.0349
	(0.0174)	(0.0171)	(0.0173)	(0.0223)	(0.0219)	(0.0218)
Below-cutoff mean	0.185	0.185	0.185	0.130	0.130	0.130
N	5,857	5,857	5,857	5,291	5,291	5,291
Panel C: 11th grade disciplinary incident						
RD Estimate	-0.0295*	-0.0310*	-0.0302*	-0.000837	-0.00589	-0.00477
	(0.0166)	(0.0162)	(0.0165)	(0.0202)	(0.0198)	(0.0199)
Below-cutoff mean	0.157	0.157	0.157	0.105	0.105	0.105
N	5,620	5,620	5,620	5,144	5,144	5,144

Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on disciplinary incidents in grades 9-11 for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Each outcome indicates whether the student had at least one recorded disciplinary incident in the indicated grade. All regressions use a 1-SD bandwidth, exclude the other placement discontinuity, and allow separate linear slopes on either side of the cutoff. Within each threshold, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Samples include students with observed disciplinary outcomes in the indicated grade. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A7: ITT Effects on Enrollment in Dallas ISD (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Enrolled in Dallas ISD in 9th grade						
RD Estimate	0.0104	0.0144	0.00771	0.0246	0.0275	0.0308
	(0.0226)	(0.0222)	(0.0221)	(0.0309)	(0.0304)	(0.0303)
Below-cutoff mean	0.628	0.628	0.628	0.665	0.665	0.665
N	6,369	6,369	6,369	5,654	5,654	5,654
Panel B: Enrolled in Dallas ISD in 10th grade						
RD Estimate	0.00428	0.00810	0.00105	0.0136	0.0164	0.0210
	(0.0231)	(0.0227)	(0.0226)	(0.0318)	(0.0311)	(0.0311)
Below-cutoff mean	0.574	0.574	0.574	0.623	0.623	0.623
N	6,369	6,369	6,369	5,654	5,654	5,654
Panel C: Enrolled in Dallas ISD in 11th grade						
RD Estimate	0.0136	0.0178	0.00846	0.0460	0.0489	0.0553*
	(0.0235)	(0.0232)	(0.0231)	(0.0324)	(0.0318)	(0.0317)
Below-cutoff mean	0.511	0.511	0.511	0.575	0.575	0.575
N	6,369	6,369	6,369	5,654	5,654	5,654

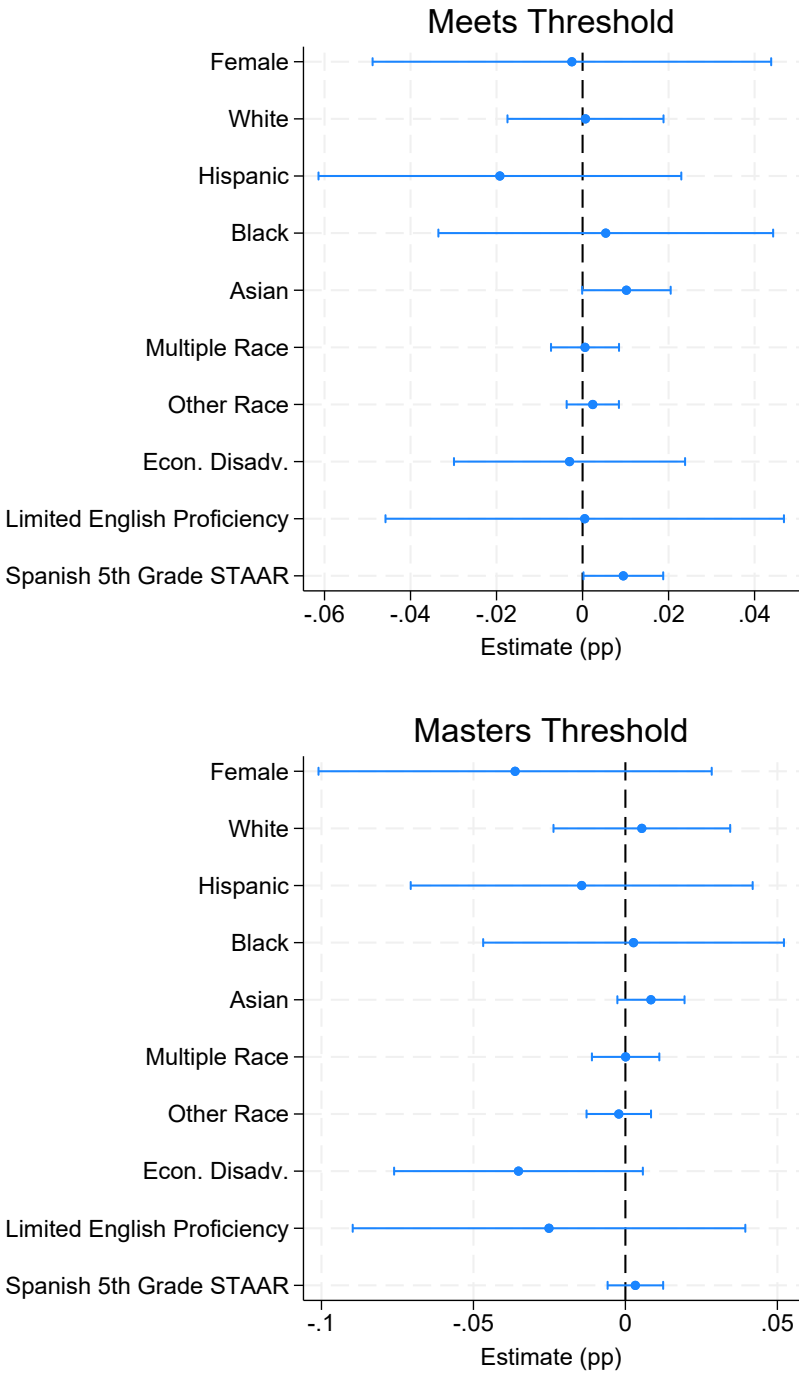
Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on continued enrollment in Dallas ISD for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Each outcome indicates enrollment in Dallas ISD in the school year corresponding to the cohort's ninth, tenth, or eleventh grade. All regressions use a 1-SD bandwidth, exclude the other placement discontinuity, and allow separate linear slopes on either side of the cutoff. Within each threshold, specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Samples retain all students in the corresponding baseline estimation window, including those who subsequently leave Dallas ISD. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A8: ITT Effects on High School "Standard Track" Completion (2019–20 Cohort)

	Meets Grade Level Threshold			Masters Grade Level Threshold		
	(1) Baseline	(2) +Covariates	(3) +School FE	(4) Baseline	(5) +Covariates	(6) +School FE
Panel A: Passed Algebra I by 9th grade						
RD Estimate	0.00884	0.00855	0.0116	-0.0163	-0.0175	-0.0176
	(0.0196)	(0.0195)	(0.0194)	(0.0239)	(0.0238)	(0.0235)
Below-cutoff mean	0.751	0.751	0.751	0.845	0.845	0.845
N	5,739	5,739	5,739	5,305	5,305	5,305
Panel B: Passed Geometry OR Algebra II by 10th grade						
RD Estimate	0.00673	0.00687	0.00507	0.0277	0.0274	0.0287
	(0.0211)	(0.0210)	(0.0212)	(0.0264)	(0.0263)	(0.0262)
Below-cutoff mean	0.711	0.711	0.711	0.811	0.811	0.811
N	5,558	5,558	5,558	5,172	5,172	5,172
Panel C: Passed Geometry AND Algebra II by 11th grade						
RD Estimate	-0.0119	-0.0111	-0.00431	-0.0223	-0.0244	-0.0260
	(0.0254)	(0.0253)	(0.0256)	(0.0338)	(0.0336)	(0.0338)
Below-cutoff mean	0.553	0.553	0.553	0.636	0.636	0.636
N	5,233	5,233	5,233	4,998	4,998	4,998

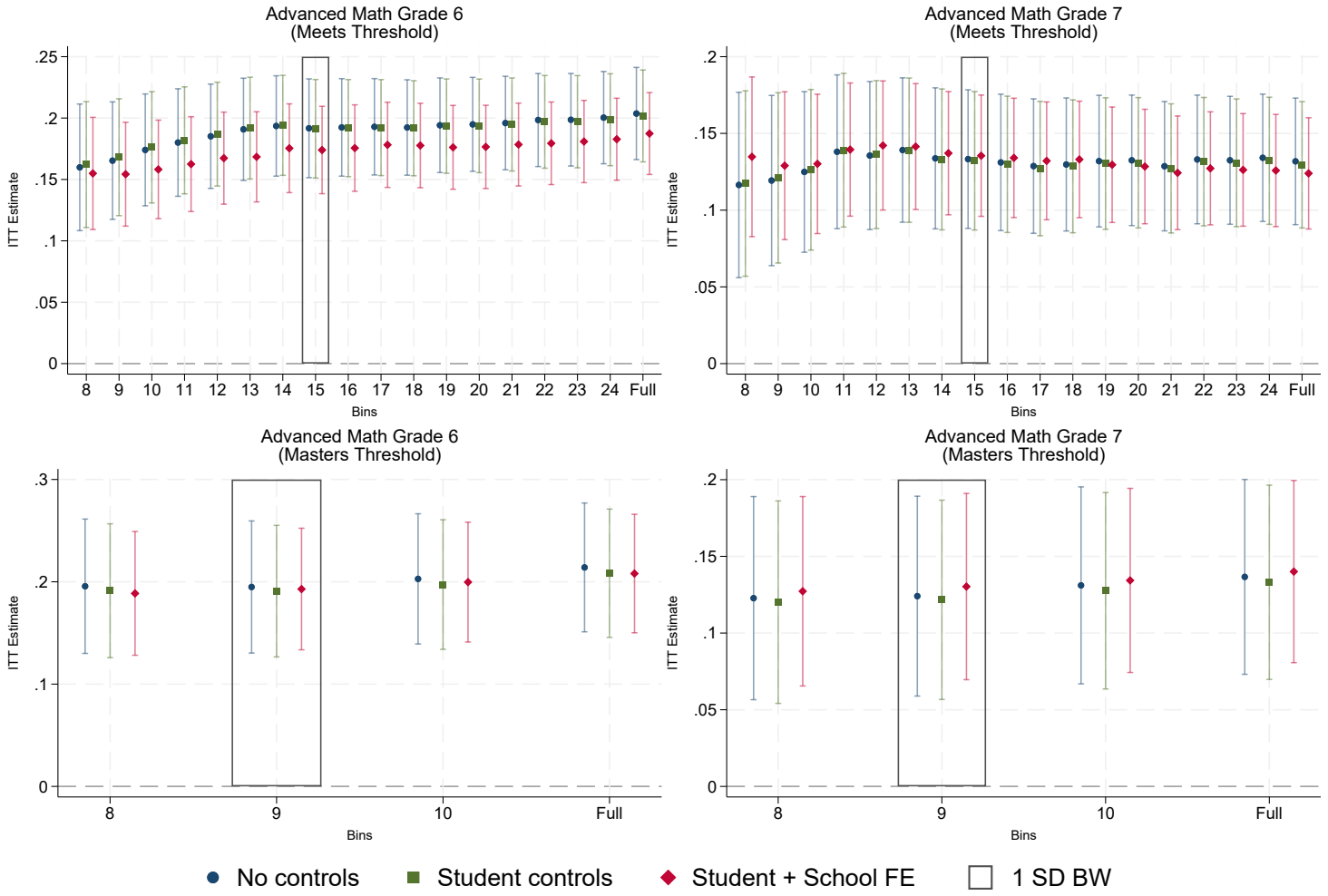
Note: Table reports intent-to-treat estimates of the effect of crossing the fifth-grade math STAAR eligibility threshold on completion of standard-track math milestones for the 2019-20 sixth-grade cohort. Columns (1)-(3) report estimates at the Meets Grade Level threshold; columns (4)-(6) report estimates at the Masters Grade Level threshold. Outcomes indicate passing Algebra I by ninth grade, Geometry or Algebra II by tenth grade, and both Geometry and Algebra II by eleventh grade. "Standard track" describes the timing of these milestones; samples are not restricted to students placed on the standard track. Passing outcomes are measured unconditionally on course enrollment. All regressions use a 1-SD bandwidth, exclude the other placement discontinuity, and allow separate linear slopes on either side of the cutoff. Specifications successively add student covariates and fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Heteroskedasticity-robust standard errors appear in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A1: Balance Test on Student Characteristics (2019-20 Cohort)



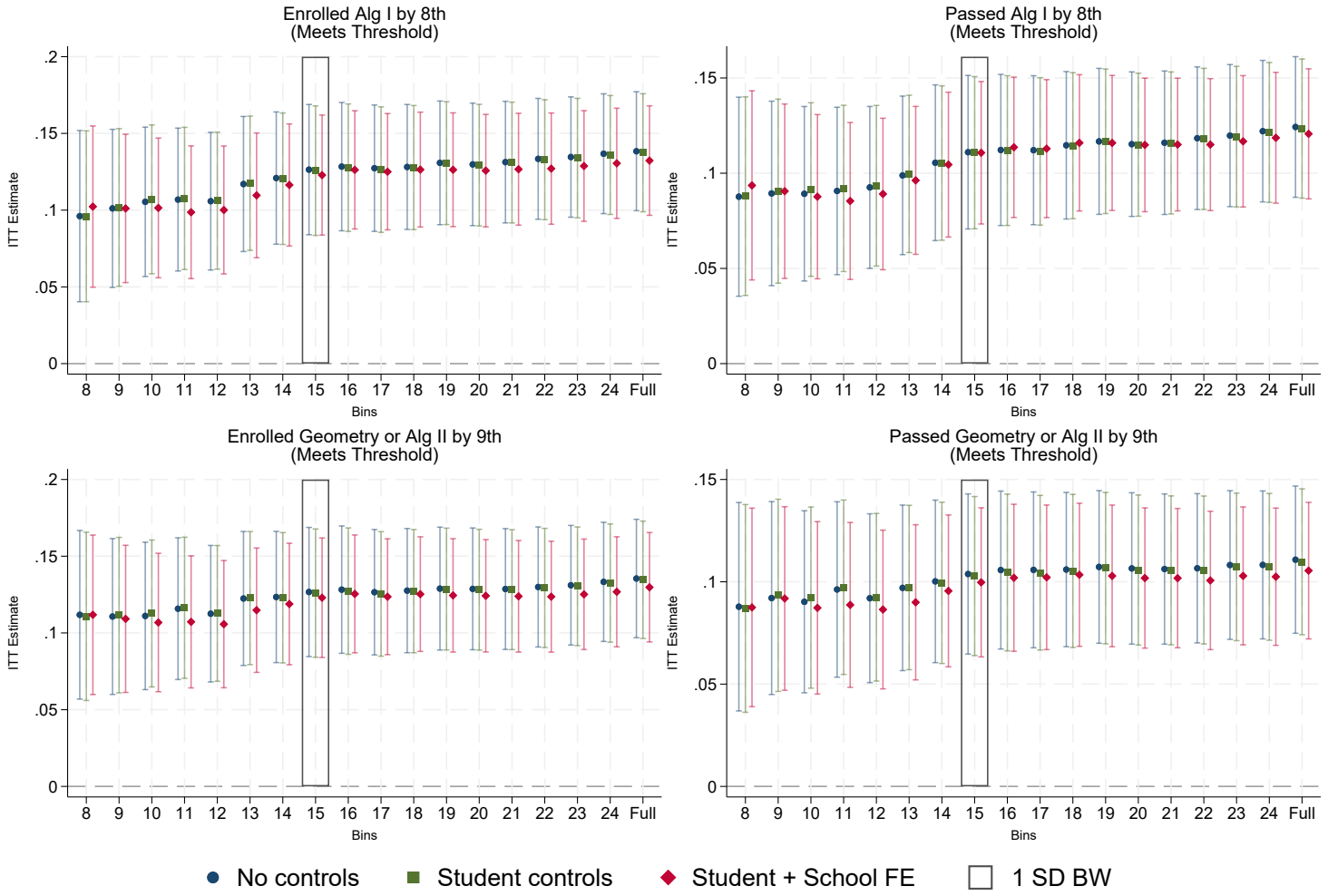
Note: Figure reports regression discontinuity estimates for predetermined student characteristics at the Meets and Masters Grade Level thresholds for the 2019–20 sixth-grade cohort. Each estimate comes from a separate regression with the indicated characteristic as the outcome. Regressions use a 1-SD bandwidth and exclude the other placement discontinuity. Estimates are expressed in proportion units, so 0.01 represents one percentage point. Whiskers indicate confidence intervals based on heteroskedasticity-robust standard errors.

Figure A2: Bandwidth Sensitivity Plots on 6th-7th Grade Course Outcomes (2019-20 Cohort)



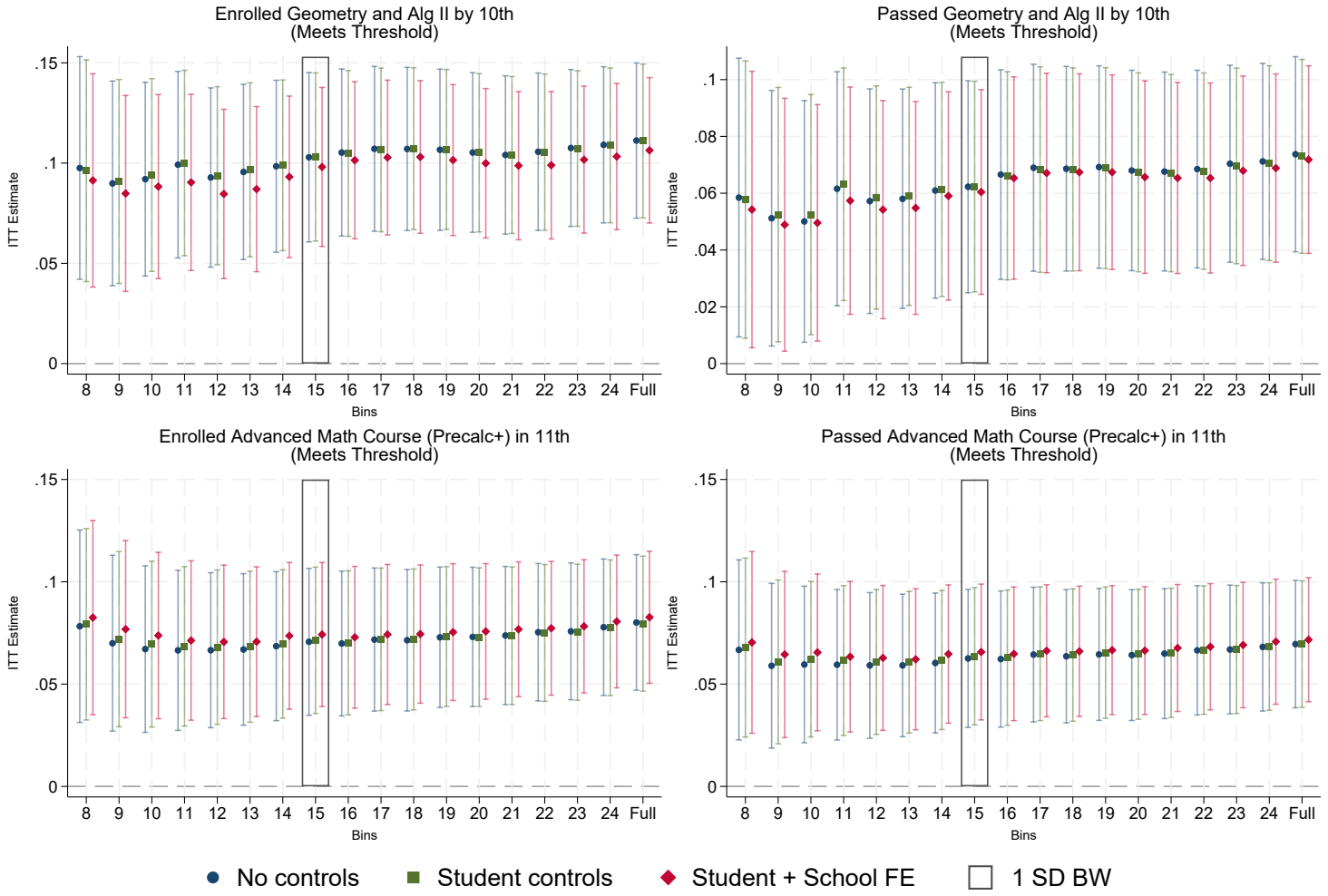
Note: Figure reports the sensitivity of regression discontinuity estimates to bandwidth choice for the 2019–20 sixth-grade cohort. The horizontal axis gives the number of distinct fifth-grade math STAAR score values included in each estimation window; "Full" uses all available scores in the threshold-specific sample. Every specification excludes the other placement discontinuity and allows separate linear slopes on either side of the cutoff. Series distinguish regressions without controls, with student covariates, and with student covariates plus fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Boxes identify the 1-SD bandwidth used in the main analysis. Estimates are expressed in proportion units; whiskers indicate confidence intervals based on heteroskedasticity-robust standard errors.

Figure A3: Bandwidth Sensitivity Plots on 8th-9th Grade Course Outcomes, Meets Threshold (2019-20 Cohort)



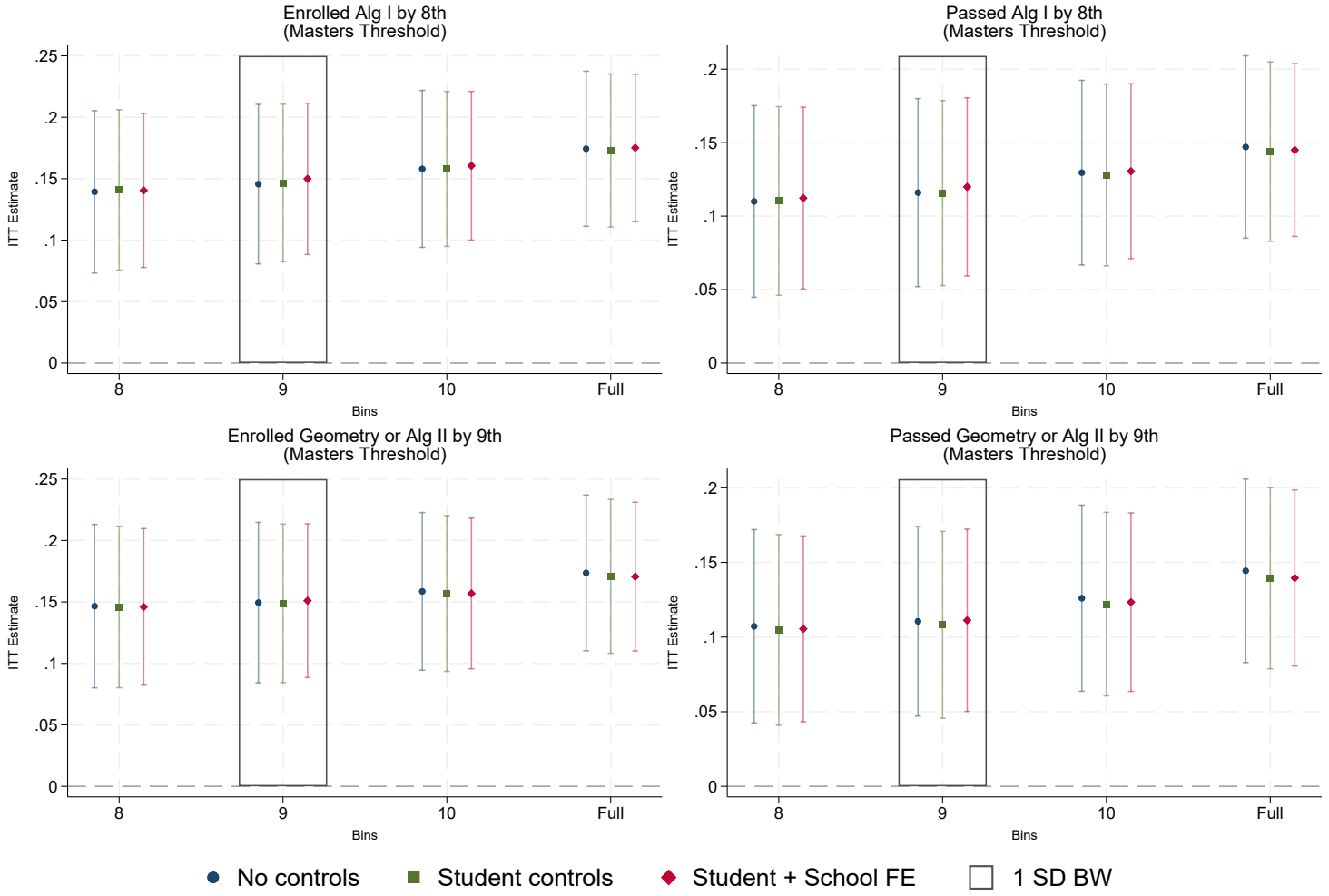
Note: Figure reports the sensitivity of regression discontinuity estimates to bandwidth choice for the 2019–20 sixth-grade cohort. The horizontal axis gives the number of distinct fifth-grade math STAAR score values included in each estimation window; "Full" uses all available scores in the threshold-specific sample. Every specification excludes the other placement discontinuity and allows separate linear slopes on either side of the cutoff. Series distinguish regressions without controls, with student covariates, and with student covariates plus fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Boxes identify the 1-SD bandwidth used in the main analysis. Estimates are expressed in proportion units; whiskers indicate confidence intervals based on heteroskedasticity-robust standard errors.

Figure A4: Bandwidth Sensitivity Plots on 10th-11th Grade Course Outcomes, Meets Threshold (2019-20 Cohort)



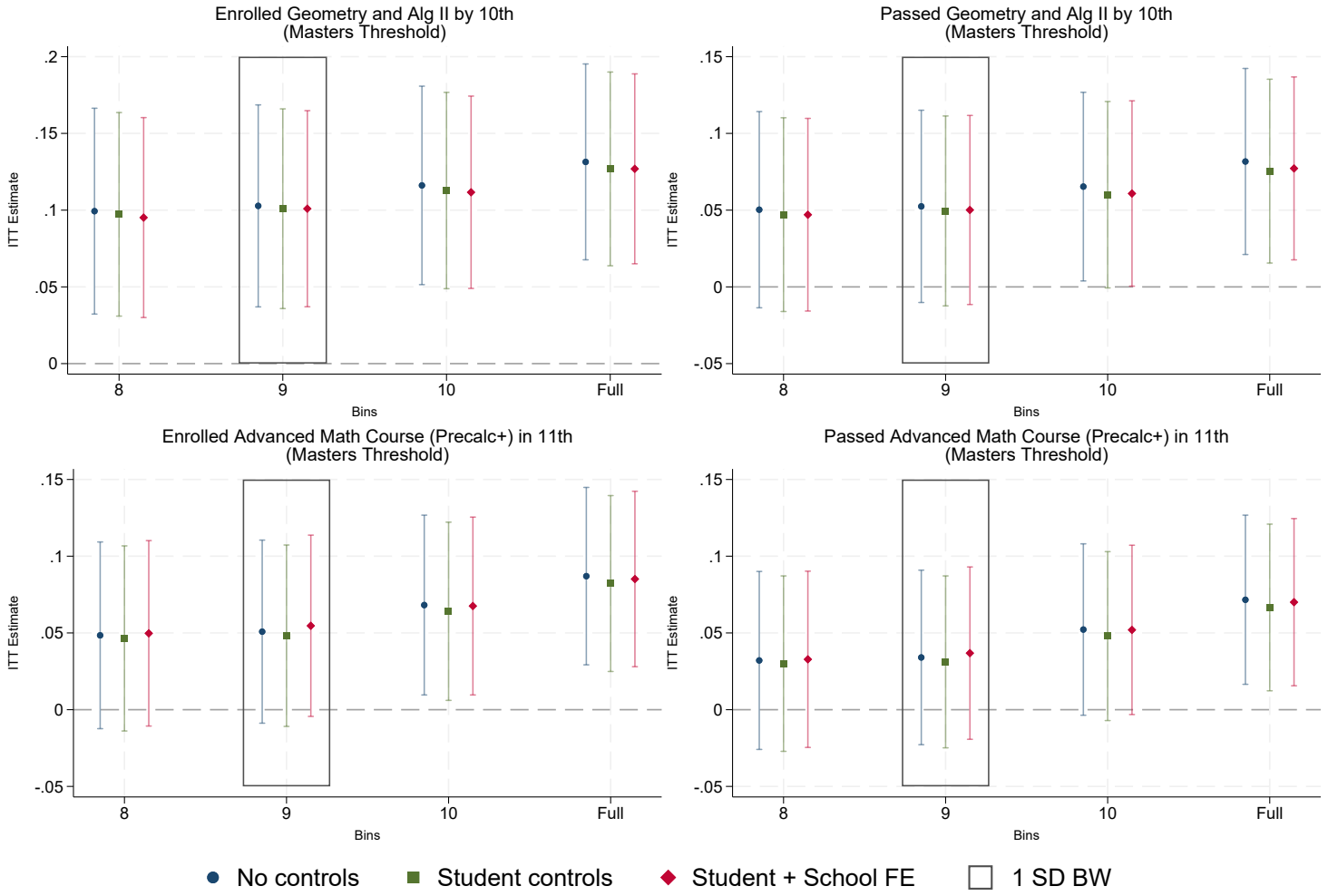
Note: Figure reports the sensitivity of regression discontinuity estimates to bandwidth choice for the 2019–20 sixth-grade cohort. The horizontal axis gives the number of distinct fifth-grade math STAAR score values included in each estimation window; "Full" uses all available scores in the threshold-specific sample. Every specification excludes the other placement discontinuity and allows separate linear slopes on either side of the cutoff. Series distinguish regressions without controls, with student covariates, and with student covariates plus fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Boxes identify the 1-SD bandwidth used in the main analysis. Estimates are expressed in proportion units; whiskers indicate confidence intervals based on heteroskedasticity-robust standard errors.

Figure A5: Bandwidth Sensitivity Plots on 8th-9th Grade Course Outcomes, Masters Threshold (2019-20 Cohort)



Note: Figure reports the sensitivity of regression discontinuity estimates to bandwidth choice for the 2019–20 sixth-grade cohort. The horizontal axis gives the number of distinct fifth-grade math STAAR score values included in each estimation window; "Full" uses all available scores in the threshold-specific sample. Every specification excludes the other placement discontinuity and allows separate linear slopes on either side of the cutoff. Series distinguish regressions without controls, with student covariates, and with student covariates plus fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Boxes identify the 1-SD bandwidth used in the main analysis. Estimates are expressed in proportion units; whiskers indicate confidence intervals based on heteroskedasticity-robust standard errors.

Figure A6: Bandwidth Sensitivity Plots on 10th-11th Grade Course Outcomes, Meets Threshold (2019-20 Cohort)



Note: Figure reports the sensitivity of regression discontinuity estimates to bandwidth choice for the 2019–20 sixth-grade cohort. The horizontal axis gives the number of distinct fifth-grade math STAAR score values included in each estimation window; "Full" uses all available scores in the threshold-specific sample. Every specification excludes the other placement discontinuity and allows separate linear slopes on either side of the cutoff. Series distinguish regressions without controls, with student covariates, and with student covariates plus fifth-grade campus fixed effects. Student covariates include gender, race and ethnicity, economic disadvantage, and English learner status. Boxes identify the 1-SD bandwidth used in the main analysis. Estimates are expressed in proportion units; whiskers indicate confidence intervals based on heteroskedasticity-robust standard errors.