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Early Math Coursework and College Readiness: Evidence from Targeted Middle School Math Acceleration

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Abstract

To better prepare students for college-level math and the demands of the labor market, school systems have tried to increase the rigor of students' math coursework. The failure of universal "Algebra for All" models has led recently to more targeted approaches. We study one such approach in Wake County, North Carolina, which began using prior test scores to assign middle school students to an accelerated math track culminating in eighth grade algebra. The policy has reduced the role that income and race played in course assignment. A regression discontinuity design exploiting the eligibility threshold shows that acceleration has no clear effect on test scores but lowers middle school course grades. Acceleration does, however, raise the probability of taking and passing geometry in ninth grade by over 30 percentage points, including for black and Hispanic students. Nonetheless, most students accelerated in middle school do not remain so by high school and those that do earn low grades in advanced courses. This leaky pipeline suggests that targeted math acceleration has potential to increase college readiness among disadvantaged populations but that acceleration alone is insufficient to keep most students on such a track.

1 Introduction

Skill in mathematics has long been regarded as essential for individual educational and economic success, as well as national global competitiveness. Numerous microeconomic studies have shown that math skills, high school math coursework and quantitative college majors are often predictive of individuals' labor market earnings later in life (Altonji, 1995; Grogger and Eide, 1995; Levine and Zimmerman, 1995; Brown and Corcoran, 1997; Weinberger, 1999; Rose, 2004; Altonji et al., 2012). Hanushek and Woessmann (2015) summarizes evidence that standardized measures of nations' math skills strongly predict macroeconomic growth and that this relationship is likely causal. Following the launch of Sputnik in 1957 and later the 1983 "A Nation at Risk" report, policymakers have called for increased proficiency in math among American students as a national imperative (Gardner et al., 1983; Tate, 1997).

Efforts to increase the amount and rigor of math coursework have, in recent years, focused substantially on early exposure to Algebra I. These efforts have been bolstered by a body of research suggesting that access to Algebra I is associated with future academic success (Smith, 1996; Gamoran and Hannigan, 2000; Stein et al., 2011). In particular, Algebra I is perceived as a gatekeeper course required to continue on to college-preparatory math, as it generally precedes a sequence of geometry, algebra II, pre-calculus and calculus (Adelman, 2006).[1] Completion of such coursework strongly predicts later college success (Long et al., 2012). This presents a policy challenge because not all students appear to have equal access to Algebra I, particularly at earlier grades. Traditionally, students are selected into algebra classes by some combination of input from their previous math teacher, their guidance counselor, their parents and themselves. That this decision-making process can overlook talented but disadvantaged students may be partly responsible for disparate rates of algebra coursework by race and income, though differences in academic skill and school offerings are also critical factors (Conger et al., 2009).

A number of school districts and states have tried to address these issues by implementing universal algebra policies that mandate all 9th, or even 8th, graders enroll in Algebra I (Silver, 1995). Proponents of such policies hope both to increase overall levels of math skill and to mitigate equity concerns, promoting access for students traditionally underrepresented in higher level math coursework by removing barriers to entry. Partly as a result of such policies, eighth grade algebra enrollment rates have more than doubled in the past two decades, from 15 percent in 1990 to 34 percent in 2012, with additional growth in the proportion of 8th graders completing even higher

[1] The recent and widespread adoption of the Common Core standards has begun to change this particular coursework sequence but has not affected the broader debate over whether and how to expose students to more rigorous coursework in early grades.

math courses.[2] Substantial gaps by race and income still, however, remain (Stein et al., 2011). Although it has expanded short-run access to advanced coursework, the Algebra-for-All movement has nonetheless generated substantial debate due to concerns that such access may harm the long-run outcomes of under-prepared students, both because such students may not succeed in their new, harder courses and because schools may dilute algebra curricula to adapt to the new skill-level of the average enrollee (Loveless, 2008; Schneider, 2009). Evidence we discuss below supports this criticism, namely that universal algebra policies appear to harm the very students they were designed to help. Such universal policies have thus fallen out of favor. In 2013, for example, California backed off its requirement that 8th graders all take algebra, a policy that had been in place since 2008.

School districts have begun to seek alternative ways to advance students in the math pipeline. One alternative model, which addresses the dual concerns of access and preparedness, is a targeted approach that encourages more but not all students to enroll in the college-ready track that includes 8th grade algebra. We study one such model implemented by the Wake County Public School System (WCPSS) in Wake County, North Carolina. WCPSS, concerned about both levels of and inequitable access to rigorous math coursework, instituted a new policy designed to increase advanced math course-taking for students predicted to be successful in such courses. In particular, WCPSS wanted to ensure that all students capable of success in 8th grade algebra enroll in the pipeline to that course as early as possible in their academic trajectory.

Rather than implement a universal algebra-for-all style policy, WCPSS chose a targeted enrollment strategy beginning with the 2010-11 academic year. The district announced that, starting in 6th grade, assignment to the accelerated track leading to 8th grade algebra would be based on a prediction of whether that student would likely succeed in algebra. Specifically, an algorithm based on historical data and incorporating all available prior test scores generated for each student a predicted probability of successfully passing a standardized algebra test. If that predicted probability exceeded 70 percent, WCPSS' new policy declared that the student should be assigned to the track culminating in 8th grade algebra. That threshold corresponded to roughly the 25th percentile of the WCPSS skill distribution, so that the new policy suggested that 75 percent of students should be placed in the accelerated track. The remaining 25 percent would take coursework leading to algebra in ninth grade, in contrast to universal policies prescribing that all students take the course in the same grade.

We document three important effects of this targeted middle school math acceleration policy.

[2] See Figure 33 of The National Center for Education Statistics' 2013 publication "The Nation's Report Card: Trends in Academic Progress 2012."

First, the new assignment rule, with its emphasis on standardized measures of past academic performance, substantially reduced the role that income and race played in course assignment. Prior to the policy's enactment, we observe large income and racial gaps in assignment to accelerated coursework, even among schoolmates of the same academic skill. Earlier research has shown disadvantaged students are more likely to have novice or low quality teachers because of variation across schools (Lankford et al., 2002; Boyd et al., 2005; Jackson, 2009; Sass et al., 2012) and even within schools (Clotfelter et al., 2005, 2006; Kalogrides and Loeb, 2013). These coursework gaps between schoolmates of similar skill represent another important way in which disadvantaged students receive lower quality educational services. Importantly, such coursework gaps vanish or diminish greatly upon implementation of the new policy.

This is the first example we know of documenting how increased emphasis on objective criteria can reduce gaps by race and income in an educational setting. Policies to focus decision-makers' attention on qualities most arguably relevant to those decisions have worked similarly in other settings. Symphony orchestras' use of blind auditions reduced gender gaps in hiring as screens forced juries to focus only on the quality of the music being heard (Goldin and Rouse, 2000). Relative to single-blind peer review, double-blind peer review lowered acceptance rates at one leading economic journal as reviewers focused less on authors' identities and more on the quality of the submissions, thus becoming more critical in their reviews (Blank, 1991). Conversely, attempts to combat judicial discrimination in criminal sentencing (Abrams et al., 2012; Alesina and Ferrara, 2011) by imposing federal guidelines based on objective criteria have failed to eliminate racial and gender disparities (Mustard, 2001; Sorensen et al., 2012). In educational settings, test-score based rules may remove some of the discretion allowed to schools and teachers that can lead to differential treatment by race and income. Our descriptive findings of the policy's impact on such gaps in course enrollment highlight an important and understudied issue, namely how and by whom course assignment decisions are made. The clear result of this targeted math acceleration policy is, ultimately, increased access to advanced math coursework for disadvantaged students.

Our second finding concerns the impact of math acceleration on students' short run outcomes. A regression discontinuity design comparing students just above and below the eligibility threshold shows that acceleration has little clear effect on test scores and often lowers the grades students earn in their middle school math courses. These results are consistent with prior research on other attempts to increase early exposure to algebra. In 1997, Chicago eliminated remedial coursework and required that all ninth graders take algebra. Allensworth et al. (2009) found no overall improvement in test scores and a decline in grades. Nomi (2012) argues that the reform actually lowered the test scores of high-skilled students by exposing them to lower-skilled peers in more mixed ability classrooms. California has included algebra in its 8th grade standards since 1997 and in 2008 made algebra the benchmark test for 8th grade accountability purposes. Domina (2014) show that the increased 8th grade algebra enrollment prompted by the new benchmark test led to lower 10th grade test scores, particularly in larger school districts. In perhaps the best identified research on this topic, Clotfelter et al. (2015) study two districts in North Carolina and find that accelerating algebra to 8th grade lowers course performance, particularly for the lowest-skilled students. This research and ours points to a general pattern that inducing lower-skilled students to take more advanced coursework can hurt some aspects of their academic performance, highlighting important differences between the descriptive relationships noted earlier and these causal estimates generated by quasi-experimental policy changes. What is unclear is whether lowered course grades in the short run represent a true loss of learning or are a mechanical result of grading curves made tougher by exposure to a higher-skilled peer group. That test scores are either unchanged or decrease suggests little clear gain or even harm from exposure to a more advanced curriculum that may be unsuited to a given student's skill level.

Marginal students may, however, be better off in the long run having earned a lower grade in a more challenging course than a higher grade in an easier course, particularly if the new trajectory allows them to complete later courses in a college-preparatory track. Our third finding therefore concerns the longer-run impact of middle school math acceleration on the coursework trajectory of treated students. Of students accelerated in 7th grade, we find that only three-fifths remain on that track by 8th grade (taking algebra) and only two-fifths remain on that track by 9th grade (taking geometry). One positive result this suggests is that targeted acceleration in middle school does increase substantially the probability of being on a college-ready math track in high school. For black and Hispanic students, for example, acceleration in 7th grade increases by over 30 percentage points the probability of taking and passing geometry in 9th grade. Conversely, while a decent fraction of accelerated students pass freshman geometry, few if any excel in such courses by earning As or Bs. Perhaps more importantly, leakages in this pipeline are so large that the majority of students revert back to the lower math track within two years of initial acceleration. This is consistent with Liang et al. (2012), who find that California's push for 8th grade algebra resulted in a leaky pipeline, and Clotfelter et al. (2015), who see little evidence in North Carolina that middle school math acceleration increased rates of advanced math coursework in high school. These results suggest targeted math acceleration has potential to increase college readiness among disadvantaged populations but that acceleration alone is insufficient to keep most students on such a track.

Our paper contributes to the previously mentioned literature in three ways. First, we document the impacts of a targeted math acceleration policy with a clearly defined assignment rule. Prior studies in this area examined either universal policies that induced all students to take the same coursework or less clearly defined policies that generally encouraged schools and students to increase early enrollment in algebra. We describe a type of assignment rule that is likely to become more prevalent given the waning of interest in such universal policies. Second, as a result of having a clearly defined assignment rule, we can rigorously identify both the marginal student and the impact of acceleration on that student using a regression discontinuity design. The best prior studies in this area have used interrupted time series designs that exploit the differential timing of policy reforms across school districts. Such an approach leaves open the possibility that the timing of such math reforms is correlated with the timing of other district-level initiatives. Our estimates, based on comparisons of schoolmates with nearly identical underlying skills, do not suffer from such concerns.

Third, the availability of extensive middle and high school transcripts allow us to carefully track course enrollment over time and thus precisely document leakages in the math pipeline. As such, our work complements much of the prior research, which focuses more on test scores and grades than on the coursework trajectories students follow after being accelerated. That this policy improved the high school math coursework of at least some students suggests it has the potential for longer-run impacts. A variety of recent quasi-experimental studies have demonstrated that interventions designed to expose students to more math coursework or support them in completing already required coursework can have substantial effects on high school graduation rates, college enrollment rates and labor market earnings, both for relatively low-skilled students (Goodman, 2012; Cortes et al., 2015) and relatively high-skilled ones (Joensen and Nielsen, 2009; Jackson, 2010).

The remainder of the paper is structured as follows. In Section 2, we provide a history and detailed description of the middle school math acceleration policy in WCPSS. We then describe our data and empirical strategy in Section 3. In Section 4, we present estimates of the impact of acceleration on a variety of student outcomes, including test scores, course grades and subsequent coursework. We conclude in Section 5 with a discussion of these results and implications for policy, practice and future research.

2 Math Acceleration in Wake County

District leaders in WCPSS initiated the targeted enrollment policy to respond to two key concerns. First, approximately 30 percent of WCPSS 8th graders enrolled in Algebra I and district leaders hoped to increase the overall enrollment. Second, the district had concerns that the students who did enroll in Algebra I in the 8th grade were not demographically representative of the district overall. In response, the school board, partnering with a task force focused on the experiences of economically disadvantaged students, sought a strategy to provide equitable access to appropriate and rigorous mathematics courses in the middle grades and to ensure access to Algebra I by the 8th grade for academically prepared students. In particular, the district hoped to increase the disproportionately low rates of enrollment in accelerated math coursework among students who were black, Hispanic or from low-income households. The district's theory of action assumed that increasing students' access to such coursework prior to high school would, in turn, increase their subsequent academic opportunities and, specifically, their likelihood of completing a rigorous, college-preparatory sequence of high school math courses.

The district ultimately implemented a targeted middle-grades math enrollment strategy. Starting in the 2010-11 school year, the district identified students eligible for accelerated math using a proprietary numeric criterion developed by the SAS Institute's Education Value-Added Assessment System (EVAAS). At the end of each academic year, the EVAAS model generates for each student a predicted probability of success on the North Carolina Algebra I end-of-course exam, based on all available prior standardized end-of-grade test scores.[3] The district stipulated that students with a 70% or higher predicted probability of Algebra I success would be recommended for placement in accelerated math courses. For 6th graders, such a course was called "Accelerated Math", for 7th graders "Pre-Algebra", and for 8th graders "Algebra I."

In the accelerated math course for sixth graders, the course standards included all of the sixth grade content for the non-accelerated course and roughly one half of the content for the non-accelerated seventh grade course. Similarly, in Pre-Algebra, the accelerated math course for seventh graders, the standards include the remaining content for the non-accelerated seventh grade course, and all of the content for the non-accelerated eighth grade course. The subject matter of the sixth and seventh grade advanced math courses overlaps largely with the standards that are tested on the North Carolina End-of-Grade examinations, and content review for the End-of-Grade examinations is included within each course outline. The eighth grade advanced course, which is Algebra I, includes the content typically covered in a high school first-year algebra course, but as of the 2012-13 school year, is an integrated course that is part of a three-year high school sequence comprising the material in the Common Core State Standards for Mathematics. As with the other accelerated courses, content review for the eighth grade End-of-Grade math examination is also

[3]For purposes of the policy studied here, success is defined as achieving Level III (of four levels) on the Algebra I end-of-course exam. The EVAAS model also generates predicted probabilities for a given student achieving other levels, none of which is relevant here.

included as a part of the course outline.

WCPSS leadership worried that an algebra-for-all policy might enroll students in courses for which some were not academically prepared. Use of the EVAAS predicted probability had two perceived advantages. First, it helped identify students who were thought to be well-prepared for such coursework. Second, because EVAAS is an objective measure, the district believed it could help identify students who might otherwise be overlooked as a result of variation in course grading practices and subjective beliefs about which students are capable of success in accelerated math courses.

As Figure 1 shows, the share of middle school students in accelerated math rose from 40 percent to nearly 70 percent within two years of the policy's implementation. By 2012-13, nearly all EVAAS-eligible students were enrolled in accelerated math, while acceleration rates remained largely unchanged for students deemed ineligible by the new policy. Acceleration rates rose substantially for both low income and non-low income students though a large income gap in acceleration persists in part because of the large income gap in EVAAS scores. A similar pattern is seen when comparing black and Hispanic students to white and Asian students. Both levels and trends in math acceleration look quite similar for boys and girls.[4] We now turn attention to the data and empirical strategy that inform our analysis of the causal impacts of math acceleration on student outcomes.

3 Data and Empirical Strategy

3.1 Data and Summary Statistics

Using data from the WCPSS longitudinal student information system, we follow students from the end of fifth grade, when they are assigned the EVAAS scores used to determine initial middle school math placement, through middle and high school, during which our outcomes of interest are measured. We can track students as long as they stay within WCPSS. The data include student-level EVAAS scores, generated annually for rising 6th, 7th and 8th graders as further standardized test scores are incorporated into the calculation. The data also contain information on student demographics, such as gender, free/reduced price lunch status, and race/ethnicity. We utilize such variables as controls in some regression specifications and to explore heterogeneity in program impacts.

We observe each student's complete middle school coursework transcript, as well as high

[4]See Figures A.2, A.3 and A.1 for trends by income, race and gender.

school transcripts for our earliest cohorts. We can observe the math courses in which students enroll and thus their acceleration status. Because classrooms can be uniquely identified and linked to both students and teachers, we can construct measures of peer composition, such as class size or average prior achievement, and teacher characteristics, such as years of experience or value-added. These classroom-level measures help us characterize in greater detail the various channels through which acceleration may affect student outcomes. We observe three important categories of outcomes that may be affected by math acceleration, namely standardized test scores, grades earned in middle school courses, and the high school coursework in which students later enroll. Standardized test scores come from North Carolina's end-of-grade (EOG) exams in math and reading comprehension, administered in the 3rd through 8th grades regardless of the specific courses in which the students were enrolled. That all students in a given grade receive a common assessment allows us to explore whether acceleration affected math and reading achievement at the end of 6th, 7th and 8th grade.

Because the acceleration policy under study was first implemented in the 2010-11 school year, we focus on data for the school years ending in 2011-2014. Our main analysis sample consists of WCPSS students with valid EVAAS scores who entered 6th grade in the 2009-10 through 2012-13 school years. We refer to these students collectively as the 2009-12 cohorts, named for the fall of the academic year in which they first entered 6th grade. The 2009 cohort was subject to the new policy starting only in 7th grade, while the subsequent three cohorts were subject to it starting in 6th grade.

Table 1 contains summary statistics for the main analytic sample. Here, each observation is a student-year, so that some students are represented up to three times, once each in 6th, 7th and 8th grades.[5] Column 1, which contains all students in the sample, shows that 57% of WCPSS students in these grades are white or Asian and 38% are black or Hispanic. During this time period, 74% of middle school students are in accelerated math coursework, and the average EVAAS predicted probability is more than 10 percentage points higher than the 70% eligibility threshold set by the assignment rule. In fact, the EVAAS threshold represents roughly the 25th percentile of math skill in the district, so that the accelerated track would contain about 75% of WCPSS students if the acceleration rule were followed exactly. About 95% of students pass their middle school math courses though fewer than three-fifths earn an A or a B in those courses.

Columns 2 and 3 divide the sample into students in accelerated math courses and those not. Accelerated students are substantially more likely to be white or Asian and less likely to be black

[5] Grade retention in middle school is quite rare in WCPSS, so very few students appear more than three times in the data.

or Hispanic. Accelerated students have much higher math skills, whether measured by EVAAS or by their 5th grade math exam z-score, the latter of which suggests a 1.4 standard deviation difference between the average performance of the two groups. Accelerated students' math classes have much more highly skilled peers, are roughly four students larger, and have fewer black or Hispanic peers than do the math classes of non-accelerated students. Accelerated students are nine percentage points more likely to pass their math courses and over 30 percentage points more likely to earn an A or a B. The gap in end-of-grade test scores between these two groups of students is quite similar to the fifth grade gap.

Before turning toward estimation of the impact of acceleration on later outcomes, we explore the extent to which the new policy did in fact reduce the role that demographic characteristics play in course assignment.[6] To do so, we compute for each student the fraction of observed middle school career spent in accelerated math coursework. We then regress that outcome on academic skill as measured by EVAAS, demographic controls for income, race and gender, and cohort-by-school fixed effects to ensure students are compared to their schoolmates and not across schools. We run this analysis separately for the untreated 2008 cohort, for the earliest treated 2009 and 2010 cohorts, and for the more recently treated 2011 and 2012 cohorts.

Table 2 shows the resulting estimates. For the untreated 2008 cohort, EVAAS scores are strongly predictive of math acceleration. Conditional on that measure of academic skill, low income students spend eight percent less and black and Hispanic spend four percent less of their middle school years in accelerated coursework. In other words, prior to the policy change, income and race are strong determinants of course assignment even conditional on academic skill. Interestingly, there is no apparent gender gap.

As time passed and the policy was enacted, the relationship between academic skills and acceleration increased in strength. The relationship between income, race and acceleration simultaneously diminished. For the most recent cohorts, conditional on academic skill, the income gap in acceleration was one-third the size of the gap in earlier cohorts and there was no statistically significant racial gap. Black and white students in the same school, same grade and of the same academic skill therefore appear to have equal exposure to accelerated math coursework. The new EVAAS score-based assignment rule thus succeeded in reducing the role of income and race in the math acceleration decision by emphasizing the role of academic skill.[7]

[6] See Dougherty et al. (2015) for a lengthier discussion of this issue.

[7] The policy substantially narrowed income and race gaps conditional on academic skill but had less of an impact on unconditional gaps because a high proportion of low income and minority students were below the eligibility threshold. In other words, the policy helped previously overlooked students in the top three-fourths of the skill distribution but did not address the bottom fourth, which is disproportional low income and minority.

3.2 Regression Discontinuity Design

We now turn toward estimation of the impacts of math acceleration on student outcomes. The substantial differences in academic skill and other factors between accelerated and non-accelerated students would severely bias a simple comparison of these two groups' outcomes. To cleanly identify the impact of math acceleration on test scores, course grades and course-taking outcomes, we exploit the fact that WCPSS chose an EVAAS predicted probability of 70% as the cutoff for assignment to accelerated math coursework. This fact allows us to use a regression discontinuity design to compare outcomes of students just above and below that threshold, two groups of students who are nearly identical except that the former group was recommended for acceleration while the latter was not. As such, comparison of these two groups near the threshold should yield estimates unbiased by differences in prior academic achievement or other student characteristics.

EVAAS scores are recalculated after each grade to incorporate new standardized test scores. Because math acceleration may affect such scores and thus subsequent EVAAS values, EVAAS scores calculated at the end of 6th and 7th grades may be partly endogenous to the policy itself. We therefore assign to each student the EVAAS score he had prior to being affected by the new policy. We call this his initial EVAAS score. For the 2009 cohort, this is the EVAAS score calculated at the end of 6th grade, as the policy only affected such students starting in 7th grade. For the 2010-12 cohorts, we use each student's EVAAS score calculated at the end of 5th grade, prior to the point in time when middle school math acceleration could have affected that score.

For the RD approach to yield valid causal inference, subjects must not be able to precisely control their EVAAS score relative to the threshold. Three facts support this assumption. First, while WCPSS selected the cutoff criteria of 70%, SAS was responsible for generating the probability values and the underlying model is not made public. Second, the cutoff scores are a function of performance on multiple prior standardized tests and students have neither sufficient technical knowledge of the policy nor sufficient capability to manipulate their own test performance to precisely influence their placement relative to the threshold. Third, for the earliest cohorts, students sat for standardized tests prior to the development of the prediction model or assignment policy and could not have anticipated it being implemented. The density of EVAAS scores near the threshold, shown graphically in Figure A.4, is quite smooth across the whole sample, suggesting no obvious manipulation of the EVAAS scores. Tests and figures disaggregated by grade and school year look similarly smooth.

Because, as we will show, compliance with the assignment rule is imperfect, we use a fuzzy regression discontinuity design (Imbens and Lemieux, 2008) by implementing a two-stage approach to estimate the effect of math acceleration on various outcomes. In the first stage, we use the threshold as a source of exogenous variation in the probability that a student is on the accelerated math track. We use local linear regressions so that the first stage takes the form:

$$Accelerated_{ics} = \alpha_0 + \alpha_1 Eligible_{ics} + \alpha_2 EVAAS_{ics} + \alpha_3 (Eligible * EVAAS)_{ics} + \gamma_{cs} + \mu_{ics} \tag{1}$$

Here, *Accelerated* indicates that student *i* in cohort *c* and school *s* was initially placed in an accelerated math course. The variable *Eligible* indicates whether a student was above the EVAAS eligibility threshold. The running variable *EVAAS* is that student's initial EVAAS score, re-centered around the threshold value of 70. Including that term, as well as its interaction with *Eligible*, fits straight lines of potentially different slopes on either side of the threshold. The coefficient α_1 represents the difference in acceleration probabilities between students just above and just below the eligibility threshold. Cohort-by-school fixed effects γ ensure that students are being compared to their within-school peers, to control for differences in course offerings and assignment processes across schools and time.[8]

We use predicted values from the first stage to then estimate the following second-stage equation:

$$Y_{ics} = \beta_0 + \beta_1 Accelerated_{ics} + \beta_2 EVAAS_{ics} + \beta_3 (Eligible * EVAAS)_{ics} + \lambda_{cs} + \epsilon_{ics} \tag{2}$$

Here, Y represents a variety of outcomes, including test scores, course grades and measures of subsequent coursework. The coefficient β_1 thus estimates the impact of initial math acceleration on such subsequent outcomes for compliers, those students whose acceleration status was affected by the eligibility threshold itself (Angrist et al., 1996). These estimates represent local average treatment effects for students near the 25th percentile of the math skill distribution in Wake County. We present estimates separately for the first two exposed cohorts (2009-10) whom we can observe through the start of high school and the most recent two cohorts (2011-12) whom we can only observe in middle school.

For our primary specification, we will estimate these local linear regressions using a rectangular kernel, a bandwidth of 15 EVAAS percentage points, and standard errors clustered by initial middle school.[9] We choose that bandwidth because it is quite close the first-stage and reduced form optimal bandwidths suggested by Imbens and Kalyanaraman (2012). We later show that our results

[8]Inclusion of the cohort-by-school fixed effects has little impact on the magnitude of the estimated coefficients but substantially increases their precision.

[9]EVAAS scores are computed to one-tenth of a percentage point and are thus nearly continuous, so that we have no need to cluster by score as suggested by Lee and Card (2008).

are robust to alternate bandwidths, including the Imbens-Kalyanaraman bandwidth, as well as to the inclusion of demographic covariates as controls.[10]

That inclusion of covariates does not affect our central estimates is unsurprising given that the inability to manipulate the EVAAS score suggests students' demographic characteristics should be balanced across the threshold. We confirm this in Table A.1, which tests for discontinuities in demographic characteristics at the threshold by running our first-stage specification with various covariates as outcomes. The samples appear balanced in terms of gender, race, income, special education status and age at the start of 6th grade. Only limited English proficiency shows a statistically significant imbalance, though the direction of the imbalance varies across the two pairs of cohorts. To test the joint balance of all of these covariates, we generate predicted math scores and GPA for the treated cohorts based on coefficients from a regression of those outcomes on these covariates for the untreated 2008 cohort. The last two columns of Table A.1 show no evidence of differences in predicted math scores or grades across the threshold.

3.3 First Stage Results

We test the strength of the first stage relationship between eligibility and math acceleration in Table 3, where the outcome in each row is an indicator for enrollment in the accelerated math course appropriate to the listed grade. Reassuringly, the untreated 2008 cohort shows no discontinuities in acceleration status as a function of eligibility. For the 2009-10 cohorts, no discontinuity appears in 6th grade but 7th graders just above the threshold are 22 percentage points more likely to be in accelerated math courses than their schoolmates just below the threshold. The coefficients look similar across the 2009 and 2010 cohorts separately. That the discontinuity appears first in seventh grade is due to two factors. First, the policy was enacted after the 2009 cohort had finished sixth grade. Second, the district appears not to have enforced the policy for the 2010 6th graders, perhaps because the first year of implementation was seen as potentially risky to apply to students just making the transition to middle school.

By years two and three of implementation, that was no longer the case. The 2011 and 2012 cohorts both show clear 22 percentage point discontinuities in the probability of math acceleration in 6th grade. Because of these findings, we define the endogenous variable of initial math acceleration differently across these two pairs of cohorts. For the 2009-10 cohorts, we define initial acceleration as enrollment in pre-algebra in 7th grade. For the 2011-12 cohorts, we define it as

[10]Though not shown here, the results presented below are also robust to optimal bandwidths selected by the methods proposed in Ludwig and Miller (2007) and Calonico, Cattaneo, and Titiunik (Calonico et al.). They are also robust to the use of a triangular kernel.

enrollment in accelerated math in 6th grade. Figure 2 shows graphically the reduced form version of this relationship, with a separate panel for each pair of cohorts. Both panels show a substantial and visually obvious discontinuity and both suggest the relationship between acceleration and EVAAS is fairly linear over the bandwidth used here. The F-statistics from the relevant coefficients in Table 3 both exceed 20, suggesting that eligibility is a strong source of exogenous variation in acceleration status.

We also explore, in Table A.2, first stage heterogeneity by income, race or gender. To do so, we run the first-stage specification with the eligibility indicator interacted with indicators for income, race or gender, and also include the direct effect of the given characteristic. We see no evidence that the relationship between eligibility and acceleration varied by income or gender. We do, however, see some evidence that the relationship varied by race, a fact that is statistically clearer in the 2009-10 cohorts. For those cohorts, eligibility increased acceleration rates for black students and Hispanic students by a statistically significant 26 percentage points but for white students and Asian students by an insignificant 10 percentage points. This is consistent with the possibility that white and Asian parents are more likely than black and Hispanic parents to request that schools violate the course assignment rule, though it is worth noting that such a difference is not apparent when separating students by low income status.

Before turning to the impact of math acceleration on student outcomes, we document a variety of channels potentially responsible for such impacts. The most obvious channel through which math acceleration might affect students is through exposure to a more rigorous curriculum, something we cannot measure beyond our ability to categorize courses based on their titles. We can, however, observe other aspects of the classroom experience to which students are exposed, including the characteristics of the peers and teacher in each student's math classroom. In each classroom, we can characterize the mean and standard deviation of peers' math skills as measured by 5th grade math scores, the total class size, and the fraction of students who are female, low income and black or Hispanic. For many of the students' math teachers, we can also identify value-added measures of teacher quality, years of experience, and gender.

Table 4 shows instrumental variables estimates of the effect of initial math acceleration on the characteristics of a student's initial math classroom. Panel A shows that acceleration exposes students to peers who are 1.2-1.3 standard deviations higher in math skill. The reduced form version of this difference is shown graphically in Figure A.5. Acceleration has no impact on the within-classroom variance of peer skills. It substantially increase class size, by 5-7 students, consistent with the observation by Lazear (2001) that more academically skilled students can be placed in larger classes. Accelerated students end up in math classes substantially less populated by low income and black or Hispanic students, though it has no impact on the gender composition of one's classmates. Panel B shows that acceleration exposes students to teachers who are 0.7-0.9 standard deviations higher in quality, driven in part by a 28-38 percentage point reduction in the probability of having a low quality teacher (defined as a teacher with VAM lower than one standard deviation below zero). Acceleration has no impact on the experience level or gender of one's math teacher. These results are not biased by our inability to link some students to teachers, as the final column shows that the probability of such linkages is unaffected by the eligibility threshold.

In total, these results suggest that acceleration exposes students to higher skilled peers and higher quality teachers, which might have positive effects, but also to larger class sizes, which might have negative effects. The impact of a more challenging curriculum is also theoretically ambiguous. Based on these results, it is worth noting that because of the structure of the policy, students on different sides of the EVAAS threshold had mathematics classroom experiences that differed not only in terms of curriculum and course content but also in terms of the student composition of the classroom itself. In this respect, the treatment we are assessing is multidimensional and not necessarily the exclusive effect of a more advanced mathematics curriculum.

4 Math Acceleration and Student Outcomes

4.1 Middle School Math Grades and Test Scores

Having established that the eligibility threshold provides a strong source of exogenous variation in the probability of being in the accelerated math track, we now estimate the impact of such acceleration on middle school math grades and test scores. We present two types of evidence, visual evidence of the reduced form relationship between these outcomes and EVAAS scores and instrumental variables estimates of the impact of acceleration on these outcomes.

Figure 3 shows the reduced form relationship between initial EVAAS scores and end-of-grade math test scores as measured in 7th grade for the 2009-10 cohorts and 6th grade for the 2011-12 cohorts. Unsurprisingly, prior achievement as measured by EVAAS scores has a very clear and positive relationship with subsequent achievement as measured by later test scores. There is, however, no apparent discontinuity in test scores at the eligibility threshold. Point estimates in column 1 of Table 5 confirm this, showing no statistically significant impacts of initial acceleration on math achievement in that school year. This non-result is unlikely to be driven by differential selection into test-taking, as column 2 shows no difference in testing rates across the the threshold. Acceleration in math does not appear to have spillover effects onto reading skills, given no apparent impact on end-of-grade reading test scores. Columns 4-6 suggest no statistically significant test score impacts in the year following initial acceleration.[11]

Figure 4 shows the reduced form relationship between initial EVAAS scores and initial math course grades. This relationship is noisier than that between EVAAS and test scores. There is, however, fairly clear visual evidence that students' grades just above the eligibility threshold are lower than those just below the threshold. Table 6 confirms this, presenting instrumental variables estimates of the impact of initial acceleration on several measures of course performance. The first column suggests that acceleration reduces students' GPA by 0.5 grade points for the earlier cohorts and 0.9 grade points for the later cohorts. None of this effect is driven by changes in the proportion of students passing their math courses. Instead, all of the effect comes from a roughly 40 percentage point drop in the probability of earning an A or B. There is no clear or consistent evidence of spillover impacts onto grades earned in non-math courses.

The negative impact on course grades could represent true learning losses. Alternatively, they could be the result of teachers using a grading curve, given that these effects are estimated off of students switching from classes where they would be near the top of the skill distribution to classes where they are near the bottom. Interestingly, for the earlier cohorts, this negative impact on course grades disappears in the subsequent year (8th grade). As we show shortly, two-thirds of the students accelerated in 7th grade remain accelerated in 8th grade, so that grading curves should still come into play for most such students. That the negative grade impact vanishes may suggest that students become more accustomed to their new classroom environment. The negative grade impact does, however, remain in the year after initial acceleration for the 2011 cohort, whom we observe for two years, making it harder to develop a generalized explanation for these effects.

We explore heterogeneity in these test score and course grade impacts in Table A.3, where we interact our instrumental variables specification with demographic indicators in both the first and second stages. The resulting coefficients, and tests of the differences between them, show little consistent evidence of heterogeneous impacts by low income status or by race. Panel C shows weak evidence that female students are negatively impacted by math acceleration moreso than male students. Such differences in test score impacts are statistically indistinguishable but, for the earlier cohorts, the negative impacts on grades are driven entirely by female students, a difference in magnitudes across genders that is statistically significant. In the later cohorts, women see larger drops in course grades but that effect is not significantly different from the effect on men.

In the short run, students induced into that track by the new district policy appear not to benefit

[11]The relatively large but noisy 0.2 standard deviation impact on math test scores is fairly sensitive to the bandwidth choice.

from such acceleration. As measured by end-of-grade test scores, their achievement is unchanged. Their course grades appear to suffer, at least initially. These are important findings but the question of ultimate importance is whether such acceleration succeeds in putting students on a longer term math trajectory that improves their college readiness. We now turn toward exploring that question.

4.2 The College Readiness Track

To explore how initial math acceleration affects subsequent course-taking patterns, we focus on the 2009 and 2010 cohorts whom we can observe through their first year of high school. We have focused until now on the initial acceleration decision for these students in 7th grade, namely enrollment in pre-algebra. In Table 7, we explore how enrollment in 7th grade pre-algebra affects subsequent enrollment in 8th grade algebra. To do so, we simply estimate the instrumental variables specification from equations 1 and 2, using enrollment in, passing and earning at least a B in 8th grade algebra as the outcomes. We assign zeroes to the roughly 10 percent of students who leave WCPSS between 7th and 8th grades but also study such attrition as an outcome in the final row of the table.

The resulting coefficients in the table's top row thus estimate the extent to which inducing enrollment in 7th grade pre-algebra increases the probability of enrollment in 8th grade algebra. We can alternatively interpret these coefficients as the fraction of compliers induced into 7th grade pre-algebra who then continue on the accelerated track in 8th grade.[12] Overall, it appears that 59 percent of students accelerated into pre-algebra in 7th grade continue on to algebra in 8th grade. That fraction differs greatly by income, with basically all of non-low income students continuing on the accelerated track but only 40 percent of low income students doing so. Differences by race are less stark, as 54 percent of black and Hispanic students continue on the accelerated track in 8th grade. There are no significant differences by gender. Only 12 percent of students just below the eligibility threshold enroll in 8th grade algebra, suggesting that nearly none of these marginal students would have enrolled if not for the new assignment rule. The reduced form version of this is shown in panel A of Figure 5, which shows a relatively clear discontinuity in 8th grade algebra enrollment, but one that is somewhat muted relative to the initial acceleration discontinuity shown in panel A of Figure 2.

Enrollment in 8th grade algebra does not, however, guarantee good performance in that course. The second row of coefficients suggests that 49 percent of those accelerated in 7th grade earn passing grades in 8th grade algebra. This represents 83 percent (0.49/0.59) of the students who enrolled

[12]This alternative interpretation makes the reasonable assumption that the threshold did not induce anyone to enroll in 8th grade algebra without first enrolling in 7th grade pre-algebra.

in 8th grade algebra as a result of 7th grade acceleration. This proportion does not appear to differ substantially by income or race, though is higher for female than for male students. Overall, these estimates imply that the vast majority of those who remain on the accelerated track in 8th grade are passing their algebra courses. They are not, however, excelling in such courses. Enrollment in 7th grade pre-algebra has no discernible impact on the probability of earning an A or B in an 8th grade algebra course, either overall or for any of the subgroups shown. The reduced form version of this is shown in panel B of Figure 5, which shows no discontinuity in the probability of earning at least a B in 8th grade algebra. In total, these results imply that, of the roughly three-fifths of students who remain on the accelerated track in 8th grade, nearly all are earning Cs or Ds in algebra.

One piece of positive evidence is found in the fourth row of Table 7, which shows that 7th grade acceleration increases by 26 percentage points the probability of a student passing the North Carolina Algebra I end-of-course exam by 8th grade. This represents nearly half of the students who enroll in 8th grade Algebra I due to earlier acceleration, implying that a good fraction of the marginal students accelerated are sufficiently skilled by the end of 8th grade to fulfill this component of North Carolina's high school graduation requirements.[13] Sub-group estimates are not precise enough to detect clear differences in exam impacts but it is worth noting that accelerated black and Hispanic students do see significantly increased passage rates. The final row of Table 7 suggests little evidence that eligibility affects the probability of enrollment in WCPSS in 8th grade, suggesting that differential attrition does not play a substantial role in any of the aforementioned results.[14]

We then follow students for another year to see whether they remain on the accelerated track upon starting high school, defined as taking geometry in 9th grade. Table 8 thus replicates Table 7, estimating how enrollment in 7th grade pre-algebra impacts enrollment and performance in 9th grade geometry as outcomes. Overall, 40 percent of students accelerated into pre-algebra in 7th grade continue on to geometry in 9th grade. Given that 59 percent were enrolled in 8th grade algebra, this suggests further leakage in the pipeline between 8th and 9th grade. As with 8th grade algebra, the fraction continuing on to 9th grade geometry differs greatly by income, with 86 percent of non-low income students but only 20 percent of low income students still on the accelerated track. Differences by race are smaller and noisier, with 34 percent of black and Hispanic students continuing on to 9th grade geometry.[15] Female students remain on the accelerated track

[13]This exam factors into the grade students earn in Algebra I, for which they must earn credit in order to earn a diploma.

[14]Consistent with this claim, exclusion of 8th grade attriters from the data has nearly no impact on the estimated coefficients.

[15]It is worth noting that eligibility is a weak instrument for the initial acceleration of non-low income and white and Asian students. For those sub-samples, the F-test of the excluded instrument respectively yields values of 7.0 and 1.6.

at a somewhat higher rate than male students, though the difference is statistically insignificant. Only 11 percent of students just below the eligibility threshold enroll in 9th grade geometry, again suggesting that few of these marginal students would have enrolled if not for the new assignment rule. The reduced form version of overall enrollment in 9th grade geometry is shown in panel A of Figure 6.

As with 8th grade algebra, enrollment in 9th grade geometry for the marginal student does not necessarily translate into success. The second row of coefficients suggests that nearly all of those induced to enroll in 9th grade geometry pass that course, a proportion that does not vary much income, race or gender. The marginal student is not, however, excelling in geometry, as there is generally no discernible impact on the overall probability of earning an A or B. Only female students see a marginally significant 15 percentage point rise in the proportion earning at least a B in geometry. Point estimates for low income and black and Hispanic students are fairly precisely estimated zeroes. The reduced form version of this overall result is shown in panel B of Figure 6, which shows no discontinuity in the probability of earning at least a B in 9th grade geometry. In total, these results imply that, of the roughly two-fifths of students who remain on the accelerated track in 9th grade, nearly all are earning Cs or Ds in geometry. The final row of Table 8 again shows little evidence that differential attrition plays a substantial role in these results.[16]

4.3 Robustness Checks

We have used a bandwidth of 15 EVAAS points throughout the paper so far. In Table A.4, we show how alternative bandwidths and inclusion of demographic controls affect estimates of the first stage, as well as of test score and course grade effects. In the first five rows, we vary the bandwidth between five and 25 in multiples of five, so that the third row represents our default specification previously shown. The sixth row uses the optimal bandwidth suggested by Imbens and Kalyanaraman (2012), where we choose the smaller of the two bandwidths from the first stage and reduced form specifications. That optimal bandwidth is then listed below the estimate. The final row uses the IK optimal bandwidth but adds controls for gender, race, age at start of 6th grade, low income, special education and limited English proficiency status. The first two columns suggest that our first stage estimates are remarkably stable across the variety of bandwidths shown. That inclusion of demographic controls has no impact on those estimates is consistent with earlier evidence of no sorting across the threshold.

Those estimates may be biased toward the OLS estimates for that reason.

[16] Again consistent with this claim, exclusion of 9th grade attriters from the data has nearly no impact on the estimated coefficients.

The third and fourth columns suggest that estimated effects on test scores are somewhat sensitive to bandwidth, with the default and IK bandwidths suggesting no statistically significant impact but larger bandwidths suggesting large negative impacts. No specification yields clear positive impacts on test scores. Effects on math course grades are slightly sensitive to bandwidth for the 2009-10 cohorts, with the narrower IK bandwidth generating a negative but statistically insignificant estimate. Grade impacts for the 2011-12 cohorts are large, stable and highly significant across all specifications.

Table A.5 limits the sample to the 2009-10 cohorts and tests the robustness of estimated impacts on 8th grade algebra and 9th grade geometry outcomes. All of these estimates are fairly stable across the range of specifications shown here, with the only variation coming at the most extreme bandwidths. All are consistent with the facts described previously, namely the leakage over time in the acceleration pipeline and the fact that the marginal students who remain in the pipeline largely pass their accelerated courses but do not excel in them. Finally, Table A.6 again limits the sample the 2009-10 cohorts but explores in each column whether the estimate of a given subgroup's geometry pass rate is sensitive to specification. Again, apart from a few extremely narrow or wide bandwidths, none of these estimates is affected in a substantial way by our precise choice of specification.

5 Conclusion

Concerned both about the low number and demographic composition of students prepared for rigorous math coursework in high school, WCPSS implemented a targeted policy based on prior test scores to enroll appropriately skilled middle school students in a math track culminating in 8th grade algebra. Encouragingly, the policy appears to have moved the district towards achieving these goals. The share of students successfully completing an Algebra I course in 8th grade increased substantially, while course assignment became more strongly related to prior achievement and less to demographic characteristics such as income and race. For the marginal student, acceleration in seventh grade raises the probability of taking and passing geometry in ninth grade by over 30 percentage points, including for black and Hispanic students. Most students accelerated in middle school do not, however, remain so by high school and those that do earn low grades in advanced courses. This leaky pipeline suggests that targeted math acceleration has potential to increase college readiness among disadvantaged populations but that acceleration alone is insufficient to keep most students on such a college preparatory trajectory in math.

This research raises at least four important questions. First, why is it that prior to the new rule's implementation, low income and black or Hispanic students were less likely to be placed in accelerated coursework than their schoolmates of similar academic skill? Are such disparities driven by differences across parents and students in either preferences for or information about rigorous math coursework? To what extent, if any, does implicit or explicit discrimination on the part of teachers and schools play a role? Assignment rules based on objective criteria such as test scores have the potential to remedy such disparities, but it may still be valuable to pinpoint the root cause of those disparities. It is also worth noting that, though the new rule reduced race and income gaps conditional on academic skill, it did much less to eliminate such gaps unconditional on skill. Low income and minority students are substantially more likely to be in the bottom quarter of the skill distribution at the start of middle school, and a targeted policy like the one studied here does not address such students and such disparities.

Second, why does math acceleration have little discernible impact on standardized test scores? Our estimated test score impacts are relatively imprecise so we cannot exclude the possibility of modest positive or negative impacts. Curricular differences between the advanced and non-advanced course sequences may also be more modest than course titles suggest, so that students in each track are exposed to similar material and are thus similarly prepared for the end-of-grade assessments. The end-of-grade tests may not be sensitive to the curricular differences that do exist between these two course levels, if topics covered in the advanced track but not in the lower track are not present on the exam. The fact that acceleration does improve Algebra I end-of-course exam pass rates is consistent with this last explanation, given that the end-of-course exam is more closely aligned with Algebra I content than is the end-of-grade exam. Finally, given that Clotfelter et al. (2015) find heterogeneous impacts by student skill, it may be that WCPSS set the eligibility threshold at a point in the skill distribution where the marginal student's test scores are neither helped nor harmed by this intervention.

Third, should the targeted acceleration policy be modified in order to reduce leakage from the math pipeline and, if so, in what way? Two-fifths of students accelerated into 7th grade pre-algebra do not enroll in 8th grade algebra and another one-fifth drop out of the accelerated track between 8th and 9th grades. Course grades suggest the marginal accelerated student is not excelling in his or her new course, so that one potential modification is to offer further academic support to marginal students. Anticipating this issue, WCPSS did offer additional, optional tutoring services for students with EVAAS scores between 70 and 80. Though we have little clear evidence on take-up rates, we see no evidence of discontinuities in any outcomes at the 80 threshold. This suggests that such tutoring, if utilized, had little impact on students. Another potential modification is to raise the eligibility threshold. This would reduce the rate of leakage as the marginal student would now have stronger academic skills. It would, however, come at the cost of reducing the number of students encouraged to enroll in accelerated coursework, some of whom might be able to succeed in such courses. This tension between access and the return to the marginal eligible student is highlighted in recent work by Cestau et al. (2015).

Fourth, and perhaps most importantly, what effect if any will middle school math acceleration have on students' longer-run outcomes, such as high school graduation, college completion and labor market earnings? Research cited in the introduction to this paper suggests that high school math coursework can have a substantial impact on such outcomes. The targeted acceleration rule did substantially increase the proportion of students, including black and Hispanic students, enrolled in the college-readiness math track at the start of high school. It remains to be seen whether this will translate into subsequent educational and economic success or whether the observed leakages fully diminish the policy's impacts. We plan to return to these students in a few years in the hope of providing insight into these questions.

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Figure 1: Fraction of Students Accelerated, By Year and Eligibility

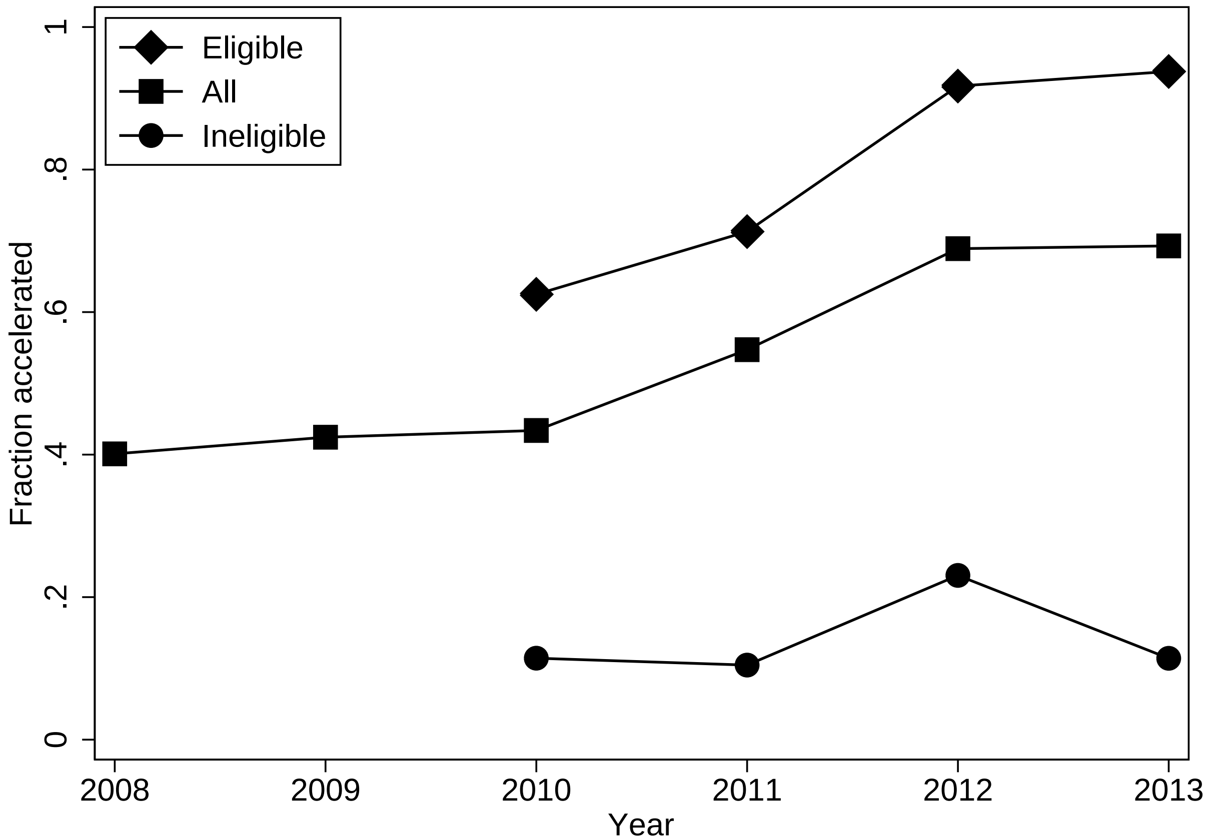


Figure 2: Initial Math Acceleration

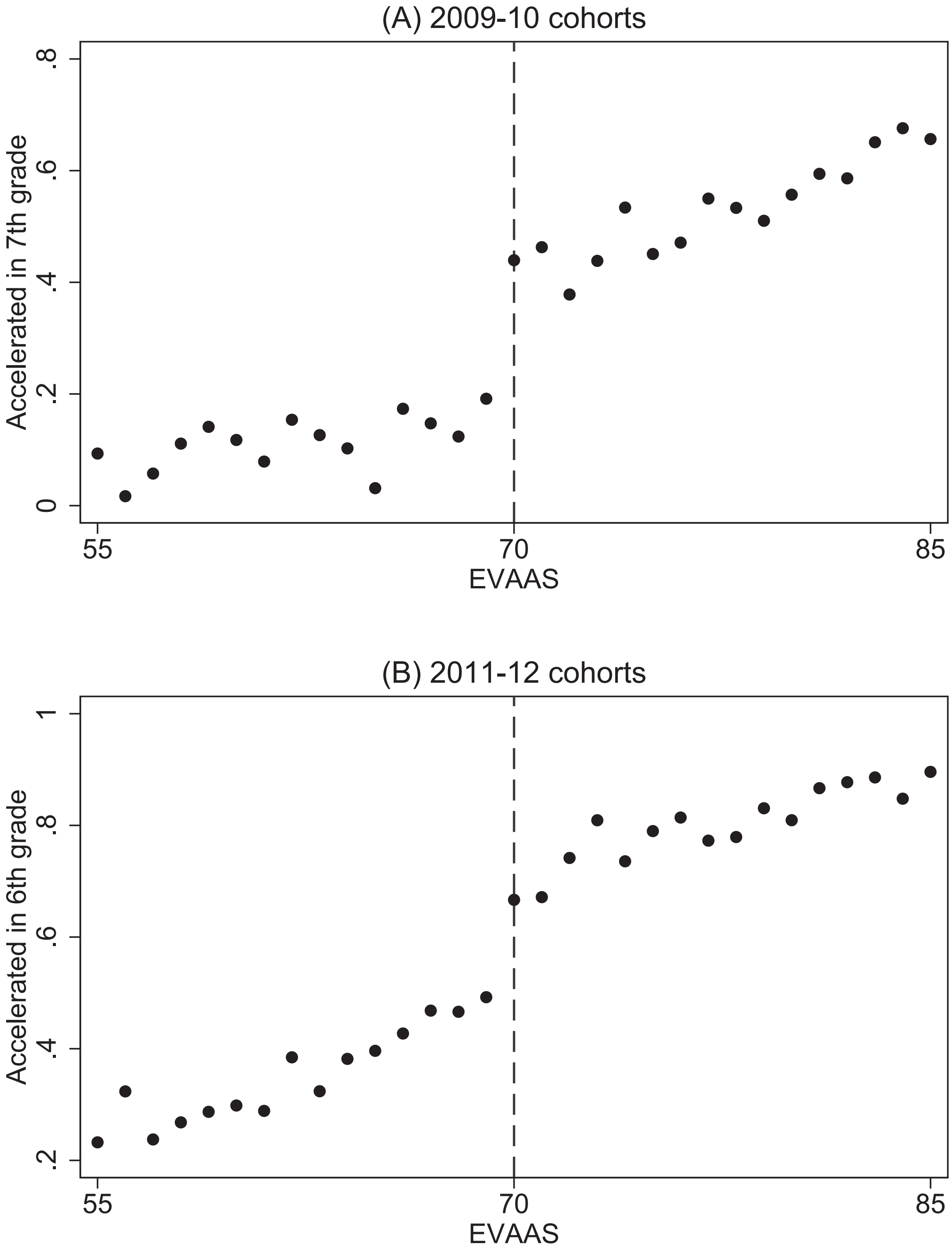


Figure 3: Math Acceleration and Math Achievement

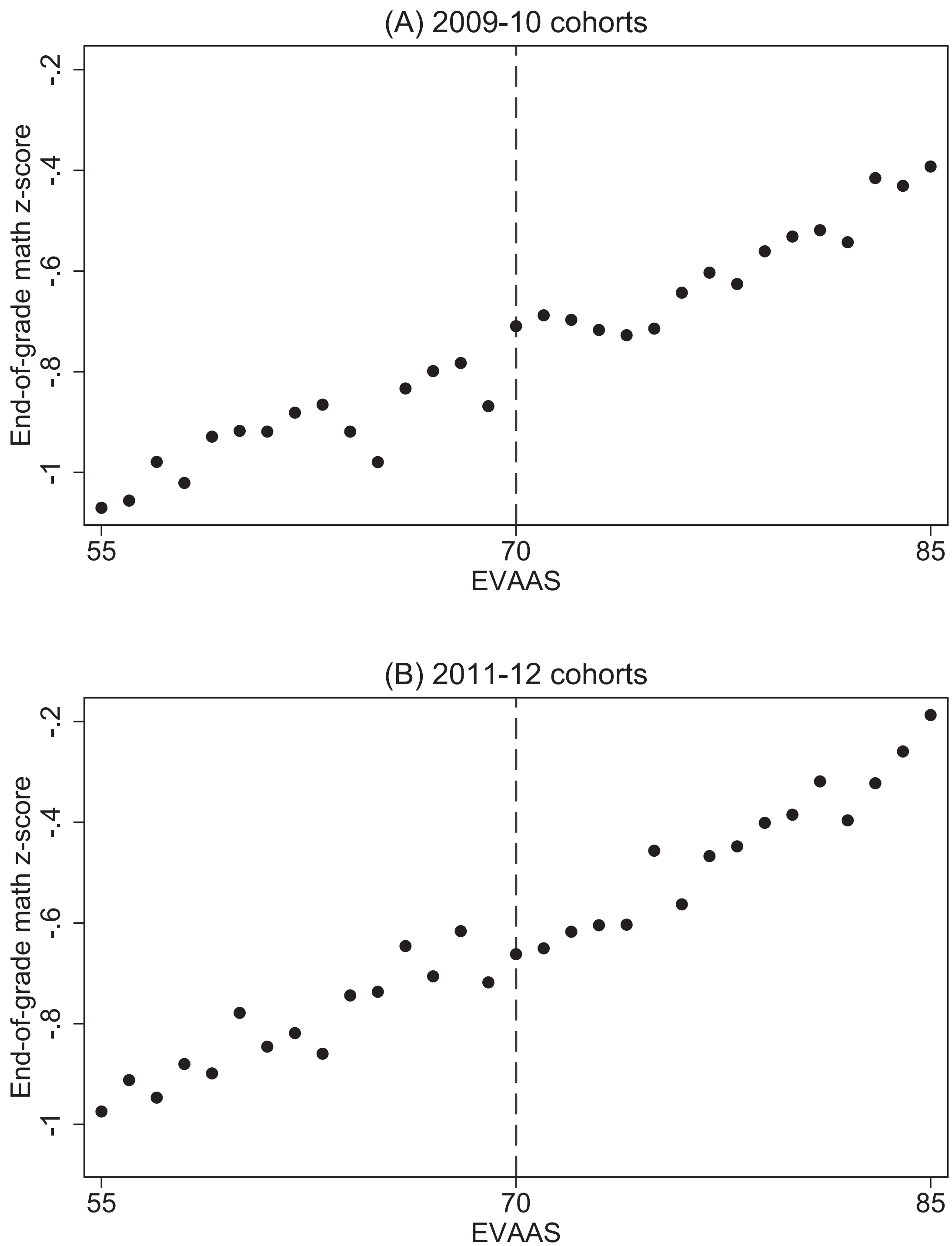


Figure 4: Math Acceleration and Math Grades

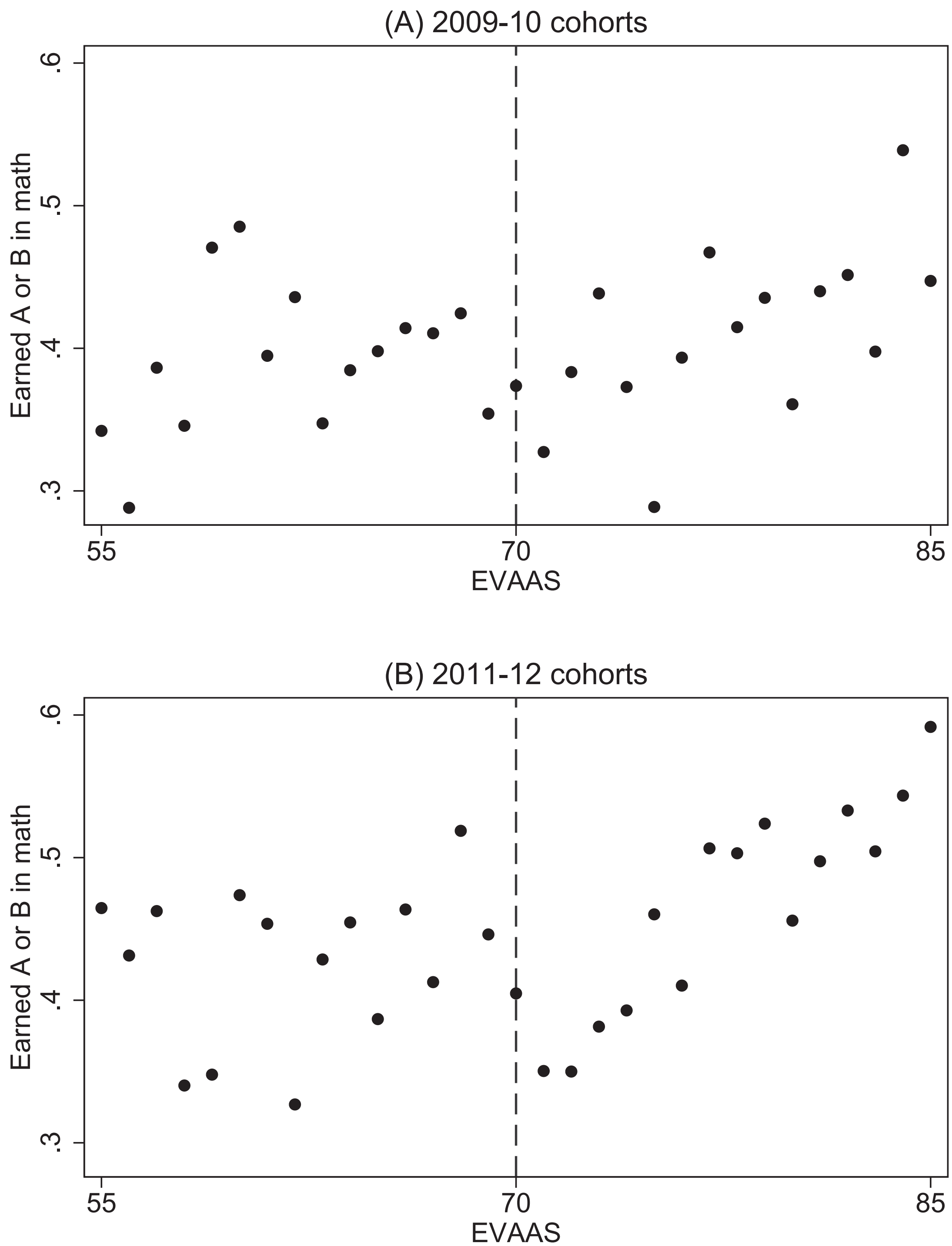


Figure 5: Math Acceleration and 8th Grade Algebra I

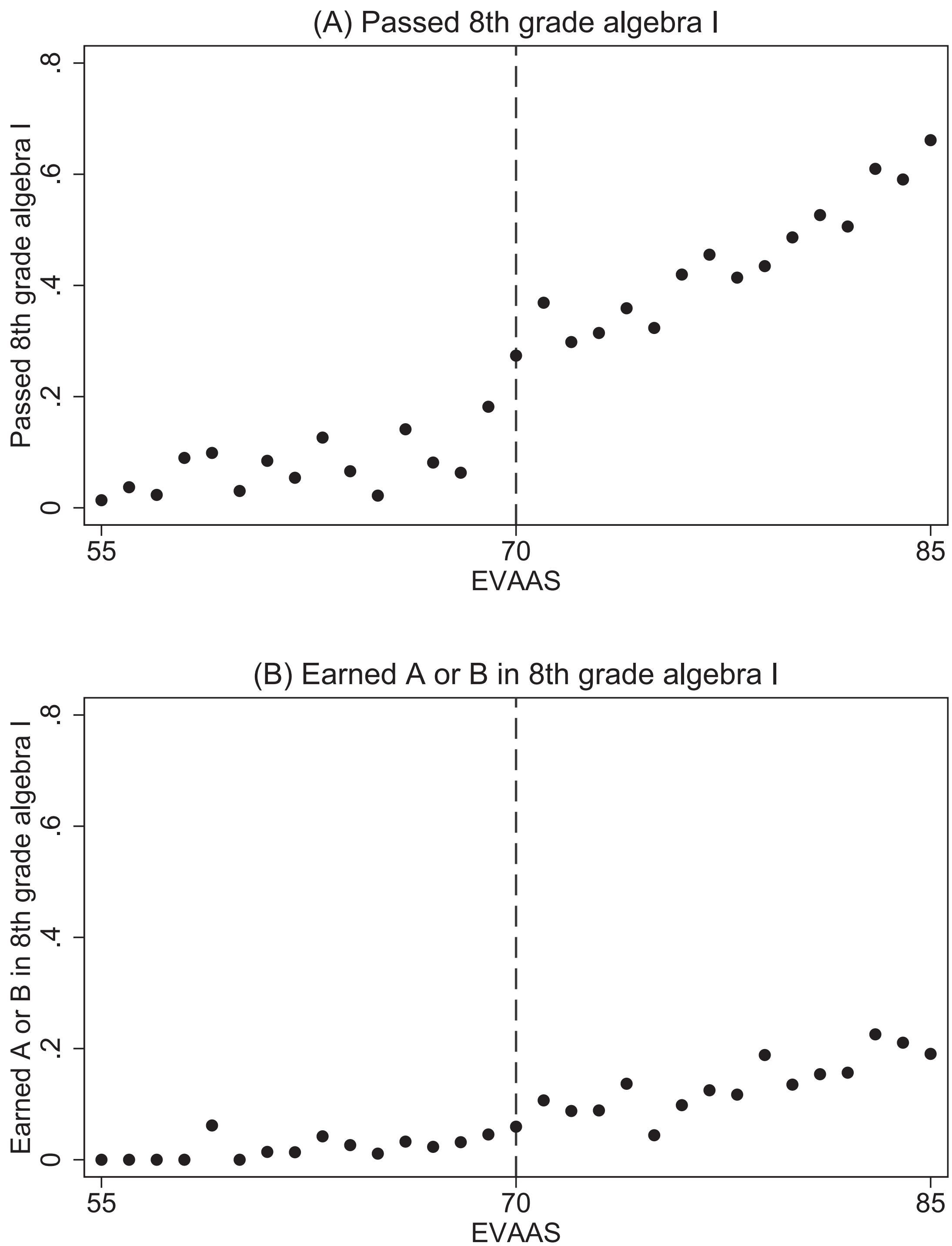


Figure 6: Math Acceleration and 9th Grade Geometry

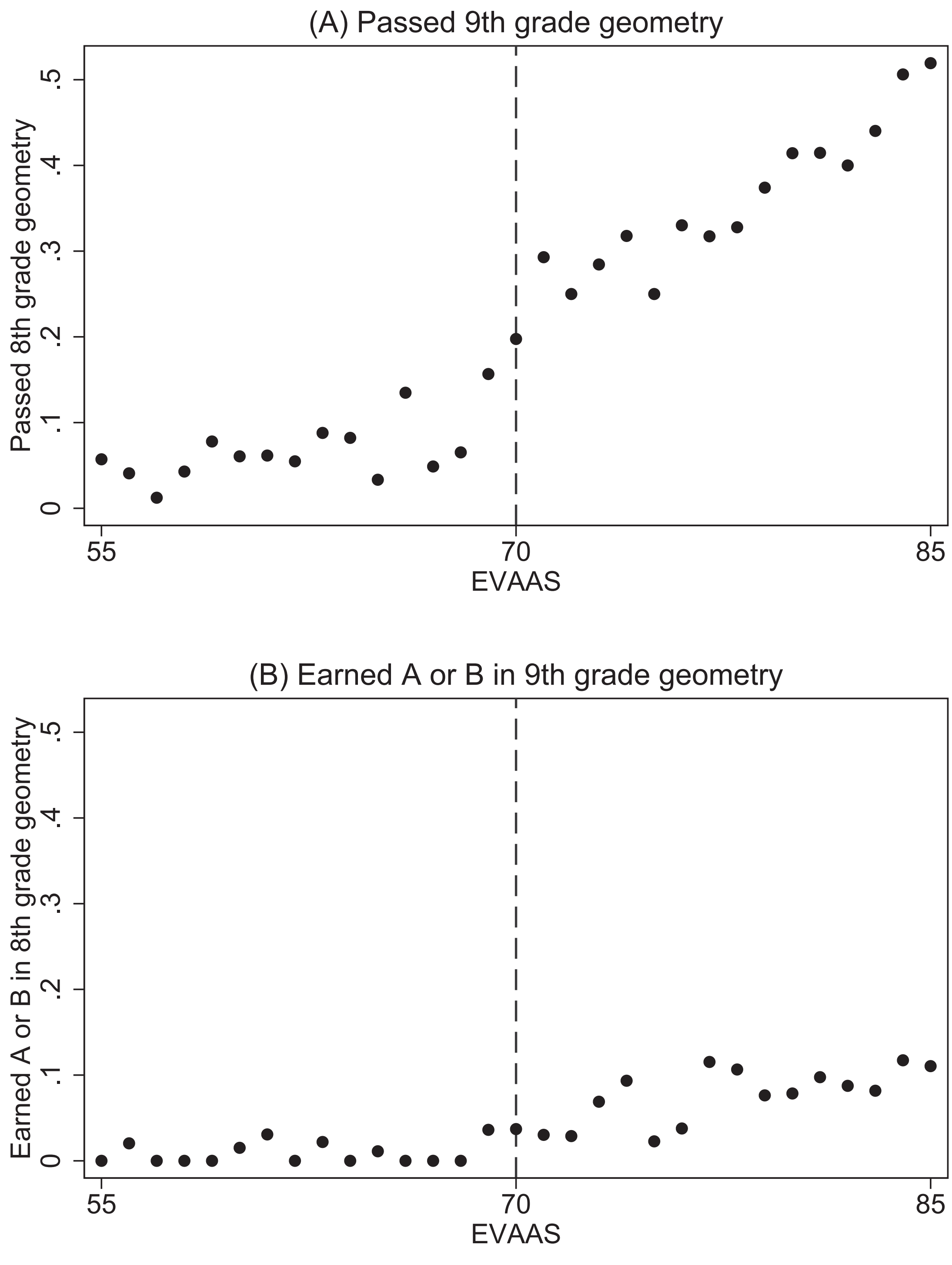


Table 1: Summary Statistics

	(1) All students	(2) Accelerated	(3) Non-accelerated
(A) Controls			
Female	0.502	0.505	0.493
White	0.506	0.589	0.272
Asian	0.063	0.078	0.023
Black	0.246	0.174	0.447
Hispanic	0.139	0.113	0.213
Other race	0.046	0.046	0.045
Low income	0.379	0.277	0.665
Special education	0.290	0.285	0.304
Limited English proficiency	0.167	0.145	0.227
Age at start of 6th grade	12.546	12.497	12.685
(B) Math course and skills			
Accelerated	0.735	1.000	0.000
Current EVAAS	80.232	91.272	49.136
Initial EVAAS	82.501	92.189	55.215
5th grade math z-score	0.024	0.386	-1.010
(C) Math course peer composition			
Mean 5th grade math z-score	0.017	0.344	-0.903
SD 5th grade math z-score	0.617	0.609	0.641
Class size	26.150	27.289	22.944
Fraction black or Hispanic	0.430	0.342	0.678
Fraction female	0.498	0.504	0.481
(D) Grade and test score outcomes			
Math GPA	2.610	2.852	1.928
Passed math class	0.948	0.971	0.883
At least B in math class	0.582	0.675	0.318
End-of-grade math z-score	0.062	0.383	-0.884
N	37,965	28,018	9,947

Notes: Mean values of key variables are shown for all students in the 2009-2012 cohorts.

Table 2: Demographics and Advanced Math Course Enrollment

	(1) 2008 cohort	(2) 2009-10 cohorts	(3) 2011-12 cohorts
Earliest EVAAS	0.010***	0.011***	0.013***
	(0.000)	(0.000)	(0.001)
Low income	-0.083***	-0.073***	-0.027***
	(0.010)	(0.008)	(0.007)
Black/Hispanic	-0.042***	-0.031***	-0.009
	(0.012)	(0.009)	(0.007)
Female	0.008	0.012**	0.011***
	(0.007)	(0.005)	(0.003)
Constant	-0.188***	-0.196***	-0.288***
	(0.026)	(0.026)	(0.043)
R^2	0.48	0.61	0.63
F(income,race)	61.5	41.9	9.3
p	0.00	0.00	0.00
N	8,123	16,946	19,342

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). Each column uses OLS to estimate the relationship between the fraction of middle school years spent in accelerated math coursework (as of the latest observable grade) and earliest observed EVAAS score, income, race and gender. Each regression also includes cohort-by-school fixed effects. Below each column are F-tests of the joint significance of the income and race coefficients, as well as the p-value associated with that F-test.

Table 3: Initial Eligibility and Math Acceleration

	(1) 2008 cohort	(2) 2009-10 cohorts	(3) 2011-12 cohorts
Accelerated in grade 6	0.029	0.009	0.219***
	(0.032)	(0.014)	(0.044)
μ	0.07	0.15	0.48
F	0.8	0.4	24.7
N	1,547	3,454	4,254
Accelerated in grade 7	-0.009	0.217***	0.308***
	(0.023)	(0.046)	(0.043)
μ	0.07	0.15	0.60
F	0.2	22.1	50.4
N	1,616	3,375	1,960
Accelerated in grade 8	0.032	0.136***	
	(0.028)	(0.034)	
μ	0.07	0.16	
F	1.4	16.3	
N	1,621	3,180	

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). First stage estimates show the impact of initial eligibility on the probability of enrollment in accelerated math coursework in the given grade. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. Initial eligibility is measured by each student's EVAAS score in the fall of 6th grade for the 2010 cohort onward, or in the fall of 2010 for earlier cohorts. Below each coefficient is the mean math acceleration rate of students just below the threshold (with initial EVAAS between 67 and 70), as well as the F-statistic associated with the eligibility instrument.

Table 4: Math Classroom Peer and Teacher Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
(A) Peers	Mean math skill	St. dev. math skill	Class size	Fraction low inc.	Fraction black/Hisp.	Fraction female
2009-10 cohorts	1.209***	-0.026	6.745***	-0.321***	-0.285***	-0.048
	(0.088)	(0.042)	(1.646)	(0.049)	(0.050)	(0.040)
N	3,368	3,368	3,368	3,368	3,368	3,368
2011-12 cohorts	1.348***	0.006	4.714**	-0.232***	-0.270***	0.022
	(0.073)	(0.042)	(1.974)	(0.036)	(0.036)	(0.036)
N	4,252	4,252	4,252	4,252	4,252	4,252
(B) Teachers	VAM estimate	Low VAM	Years of exp.	Novice teacher	Female teacher	Missing teacher
2009-10 cohorts	0.935**	-0.275**	-2.472	0.054	0.125	-0.113
	(0.405)	(0.141)	(1.865)	(0.075)	(0.127)	(0.146)
N	3,316	3,316	2,820	2,820	2,820	3,368
2011-12 cohorts	0.661	-0.378**	0.903	-0.033	0.008	-0.057
	(0.485)	(0.167)	(1.188)	(0.058)	(0.143)	(0.117)
N	3,757	3,757	3,820	3,820	3,859	4,252

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). Instrumental variables estimates show the impact of initial math acceleration on the peer and teacher characteristics for each student's initial math classroom, where acceleration is instrumented with eligibility. Classroom characteristics are measured in 7th grade for the 2009-10 cohorts and 6th grade for the 2011-12 cohorts. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. In panel B, is an indicator for having math teacher whose VAM is more than one standard deviation below average. indicates missing data for a student's math teacher.

Table 5: Math Acceleration and Standardized Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)
	Year of initial acceleration			Year after initial acceleration		
	Math z-score	Took math exam	Reading z-score	Math z-score	Took math exam	Reading z-score
2009-10 cohorts	0.032	0.047	-0.103	0.233	0.040	0.015
	(0.147)	(0.033)	(0.238)	(0.150)	(0.053)	(0.262)
μ	-0.82	0.98	-0.68	-0.81	0.89	-0.73
N	3,337	3,368	3,320	3,100	3,368	3,087
2011-12 cohorts	-0.158	-0.021	-0.121	0.210	-0.074	-0.928
	(0.105)	(0.026)	(0.194)	(0.385)	(0.087)	(0.810)
μ	-0.68	0.99	-0.55	-0.76	0.94	-0.51
N	4,205	4,252	4,183	1,933	2,102	1,928

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). Instrumental variables estimates show the impact of initial math acceleration on standardized test scores, where acceleration is instrumented with eligibility. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. Columns 1-3 measure outcomes at the end of the year of initial math acceleration, which is 7th grade for the 2010-11 cohorts and 6th grade for the 2012-13 cohorts. Columns 4-6 measure outcomes at the end of the following year, which is 8th grade for the 2010-11 cohorts and 7th grade for the 2012-13 cohorts. Columns 1 and 4 use as outcomes end-of-grade math test z-scores. Columns 2 and 5 use as outcomes indicators for taking those math exams. Columns 3 and 6 use as outcomes end-of-grade reading test z-scores. Below each coefficient is the mean of the outcome variable for students just below the threshold (with initial EVAAS between 67 and 70). In the second row, the 7th grade outcomes in columns 4-6 are available only for the 2011 cohort.

Table 6: Math Acceleration and Course Grades

	Year of initial acceleration				Year after initial acceleration			
	(1) Math GPA	(2) Passed math	(3) A or B in math	(4) Non-math GPA	(5) Math GPA	(6) Passed math	(7) A or B in math	(8) Non-math GPA
2009-10 cohorts	-0.519*	0.047	-0.367***	0.372	-0.047	-0.076	0.012	0.335
	(0.265)	(0.087)	(0.137)	(0.242)	(0.268)	(0.082)	(0.136)	(0.249)
μ	2.10	0.90	0.40	2.18	1.96	0.93	0.31	1.94
N	3,368	3,368	3,368	3,368	3,162	3,162	3,162	3,151
2011-12 cohorts	-0.871***	-0.019	-0.411***	-0.247	-2.277**	-0.342	-0.627	-0.727
	(0.324)	(0.064)	(0.144)	(0.211)	(1.045)	(0.213)	(0.427)	(0.783)
μ	2.33	0.96	0.46	2.60	2.10	0.93	0.37	2.39
N	4,252	4,252	4,252	4,252	1,954	1,954	1,954	1,954

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). Instrumental variables estimates show the impact of initial math acceleration on course grades, where acceleration is instrumented with eligibility. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. Columns 1-4 measure outcomes at the end of the year of initial math acceleration, which is 7th grade for the 2010-11 cohorts and 6th grade for the 2012-13 cohorts. Columns 5-8 measure outcomes at the end of the following year, which is 8th grade for the 2010-11 cohorts and 7th grade for the 2012-13 cohorts. Math course grades are measured regardless of the type of math course taken. Below each coefficient is the mean of the outcome variable for students just below the threshold (with initial EVAAS between 67 and 70). In the second row, the 7th grade outcomes in columns 5-8 are available only for the 2011 cohort and not the 2012 cohort.

Table 7: Math Acceleration and Algebra I in 8th Grade

	(1) All students	(2) Non-low income	(3) Low income	(4) White/ Asian	(5) Black/ Hispanic	(6) Male students	(7) Female students
Enrolled	0.593***	1.120***	0.399***	0.886	0.537***	0.633***	0.620***
	(0.133)	(0.266)	(0.136)	(0.643)	(0.117)	(0.141)	(0.205)
μ	0.12	0.14	0.11	0.17	0.10	0.12	0.12
Passed course	0.494***	0.922***	0.318**	0.679	0.445***	0.476***	0.578***
	(0.132)	(0.241)	(0.148)	(0.593)	(0.110)	(0.124)	(0.196)
μ	0.10	0.14	0.08	0.14	0.08	0.10	0.10
Earned A or B	0.006	0.139	-0.071	-0.203	0.029	0.026	0.005
	(0.075)	(0.174)	(0.084)	(0.440)	(0.086)	(0.086)	(0.141)
μ	0.03	0.05	0.02	0.05	0.02	0.03	0.03
Passed Algebra I exam	0.263**	0.433*	0.194	0.841	0.193**	0.337**	0.256*
	(0.106)	(0.239)	(0.119)	(0.778)	(0.085)	(0.156)	(0.137)
μ	0.04	0.07	0.03	0.07	0.03	0.04	0.05
Present in 8th grade	0.050	0.081	0.062	0.098	0.048	0.007	0.059
	(0.036)	(0.057)	(0.041)	(0.155)	(0.032)	(0.054)	(0.047)
μ	0.90	0.95	0.88	0.93	0.89	0.91	0.90
F	21.1	7.0	22.7	1.6	29.6	17.7	14.0
N	3,368	1,192	2,156	1,024	2,312	1,603	1,739

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Instrumental variables estimates show the impact of 7th grade math acceleration on 8th grade Algebra I outcomes, where acceleration is instrumented with eligibility. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. The sample consists of all students from the 2009-10 cohorts present in 7th grade, with outcomes assigned as zeroes for those not present in 8th grade. The first row uses as an outcome an indicator for enrollment in Algebra I in 8th grade. The second and third rows use as outcomes indicators for passing or earning an A or B in Algebra I in 8th grade. The fourth row uses as an outcome an indicator for passing the North Carolina Algebra I end-of-course exam by 8th grade. The last row uses as an outcome an indicator for being present in WCPSS in 8th grade. Below each coefficient is the mean of the outcome variable for students just below the threshold (with initial EVAAS between 67 and 70). Below each column is the F-statistic associated with the eligibility instrument.

Table 8: Math Acceleration and Geometry in 9th Grade

	(1) All students	(2) Non-low income	(3) Low income	(4) White/ Asian	(5) Black/ Hispanic	(6) Male students	(7) Female students
Enrolled	0.396***	0.856***	0.196	0.600	0.343***	0.359***	0.476**
	(0.106)	(0.250)	(0.145)	(0.653)	(0.099)	(0.108)	(0.188)
μ	0.11	0.14	0.10	0.14	0.10	0.10	0.12
Passed	0.350***	0.855***	0.151	0.461	0.314***	0.286***	0.433***
	(0.084)	(0.260)	(0.100)	(0.591)	(0.083)	(0.100)	(0.155)
μ	0.08	0.10	0.07	0.13	0.06	0.08	0.08
Earned A or B	0.065	0.207	-0.009	0.337	0.008	0.017	0.151*
	(0.053)	(0.157)	(0.041)	(0.495)	(0.040)	(0.062)	(0.085)
μ	0.01	0.02	0.00	0.03	0.00	0.01	0.01
Present in 9th grade	-0.073	0.047	-0.097	0.355	-0.127	-0.105	-0.021
	(0.100)	(0.183)	(0.109)	(0.490)	(0.084)	(0.120)	(0.136)
μ	0.87	0.91	0.86	0.87	0.87	0.86	0.88
F	21.1	7.0	22.7	1.6	29.6	17.7	14.0
N	3,368	1,192	2,156	1,024	2,312	1,603	1,739

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Instrumental variables estimates show the impact of 7th grade math acceleration on 9th grade geometry outcomes, where acceleration is instrumented with eligibility. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. The sample consists of all students from the 2009-10 cohorts present in 7th grade, with outcomes assigned as zeroes for those not present in 9th grade. The first row uses as an outcome an indicator for enrollment in geometry in 9th grade. The second and third rows use as outcomes indicators for passing or earning an A or B in geometry in 9th grade. The last row uses as an outcome an indicator for being present in WCPSS in 9th grade. Below each coefficient is the mean of the outcome variable for students just below the threshold (with initial EVAAS between 67 and 70). Below each column is the F-statistic associated with the eligibility instrument.

Figure A.1: Fraction of Students Accelerated, By Gender

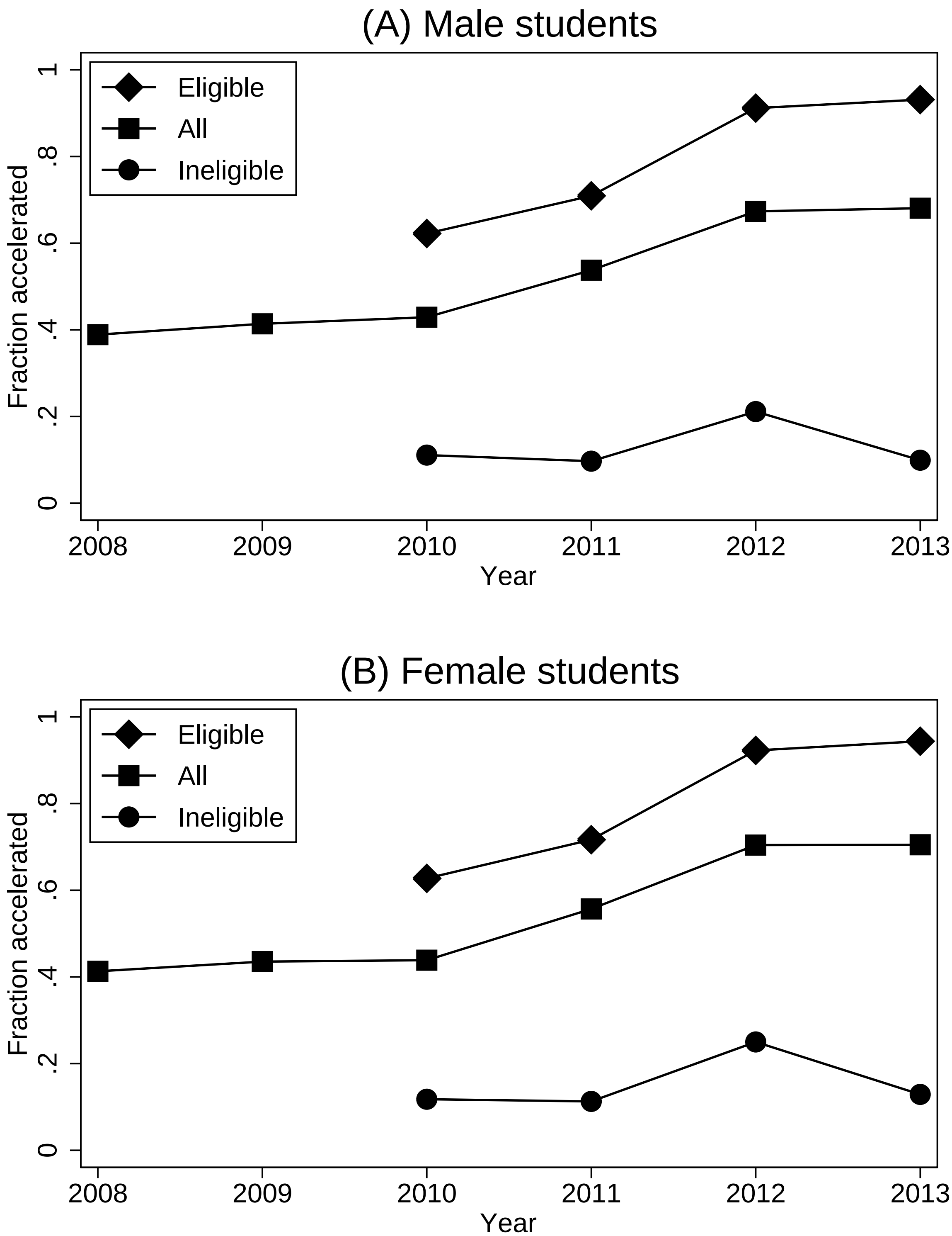


Figure A.2: Fraction of Students Accelerated, By Income

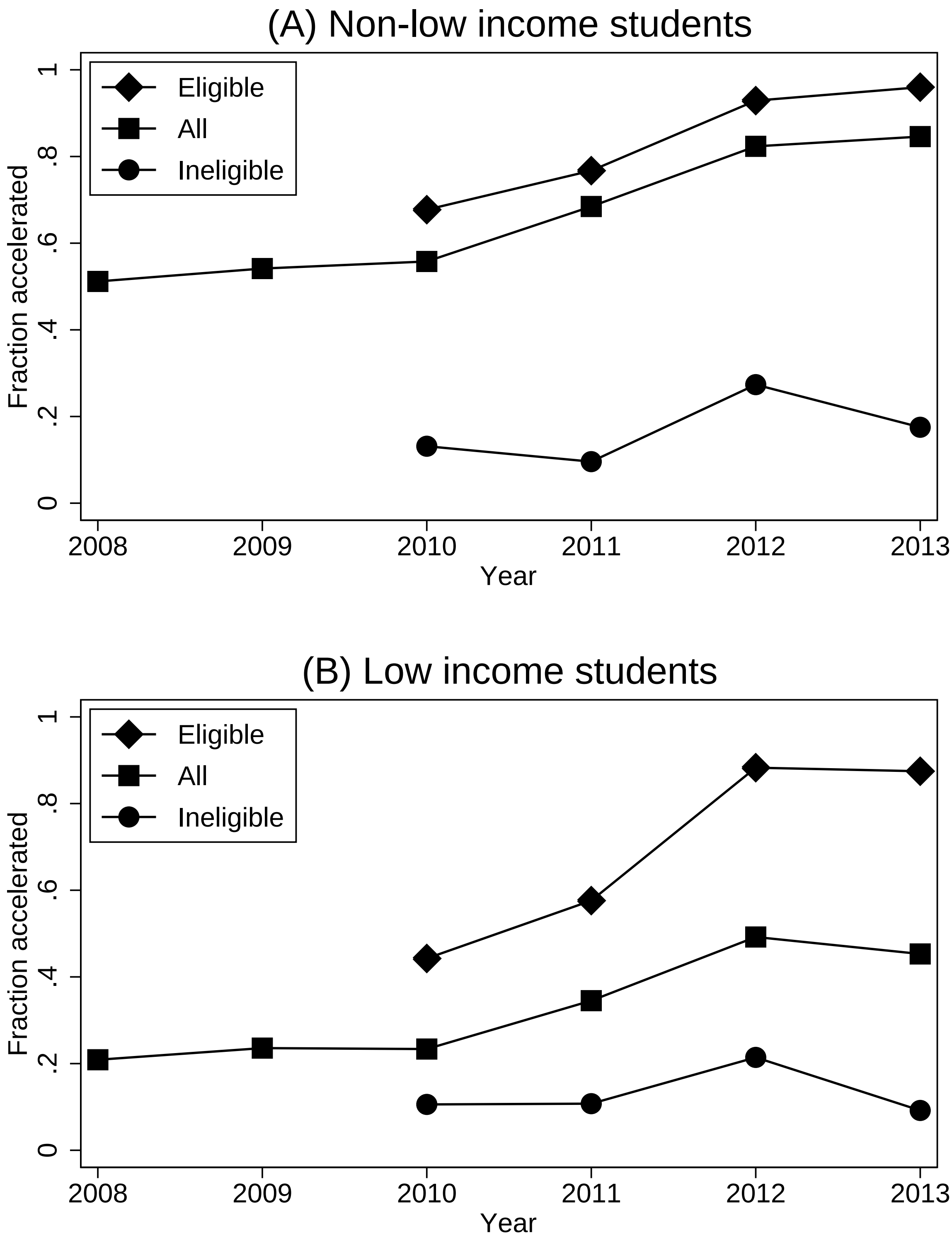


Figure A.3: Fraction of Students Accelerated, By Race

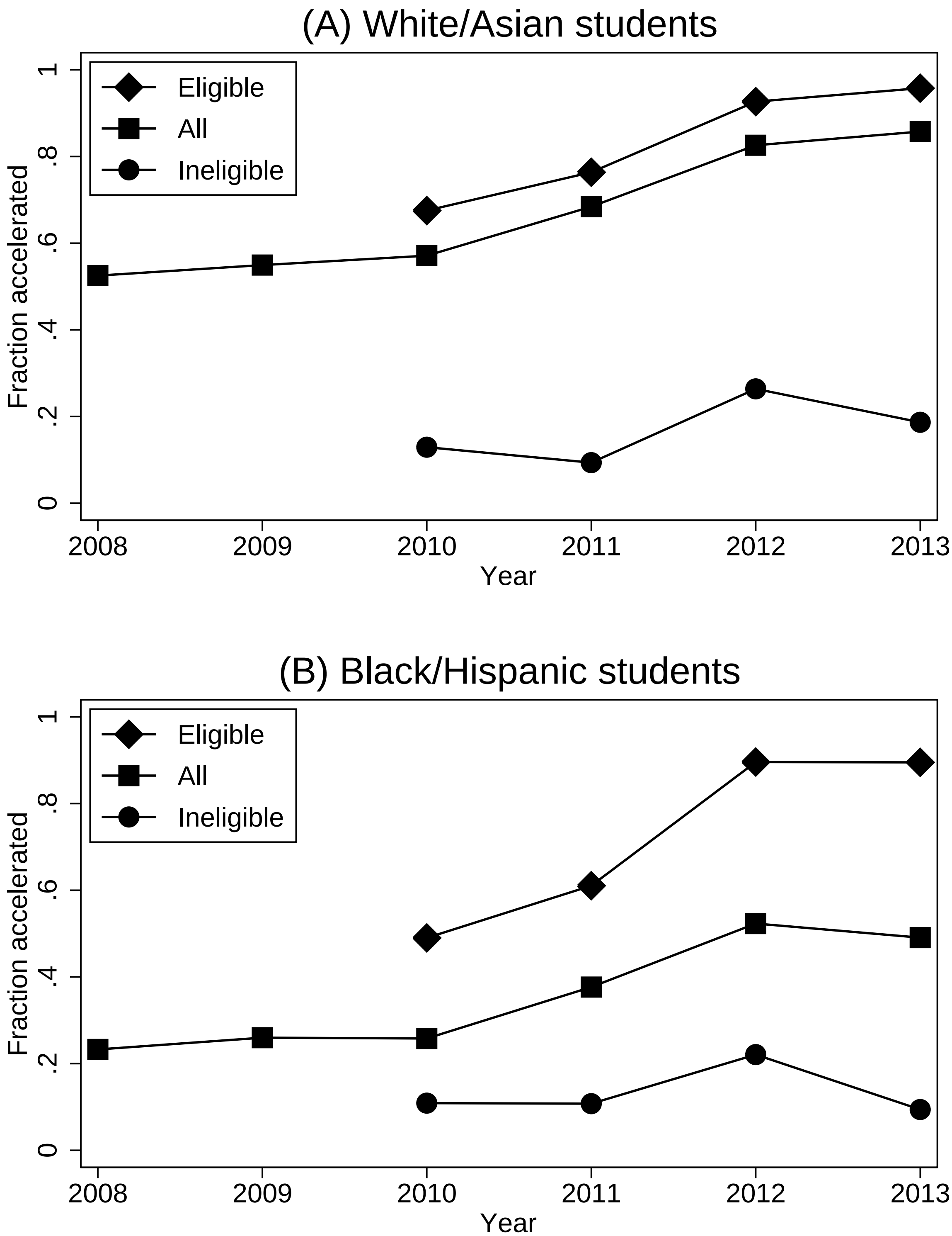


Figure A.4: Distribution of EVAAS Scores

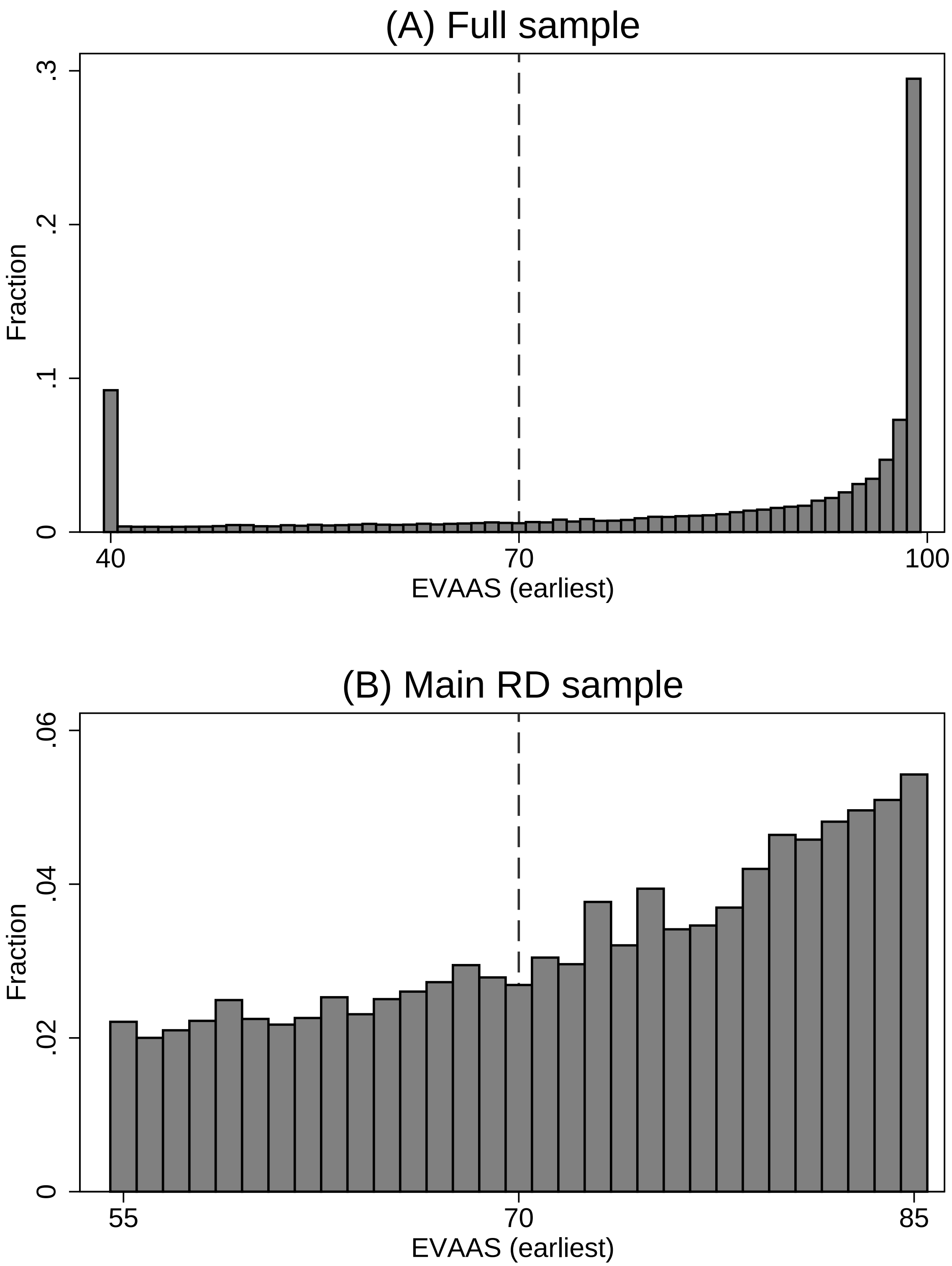
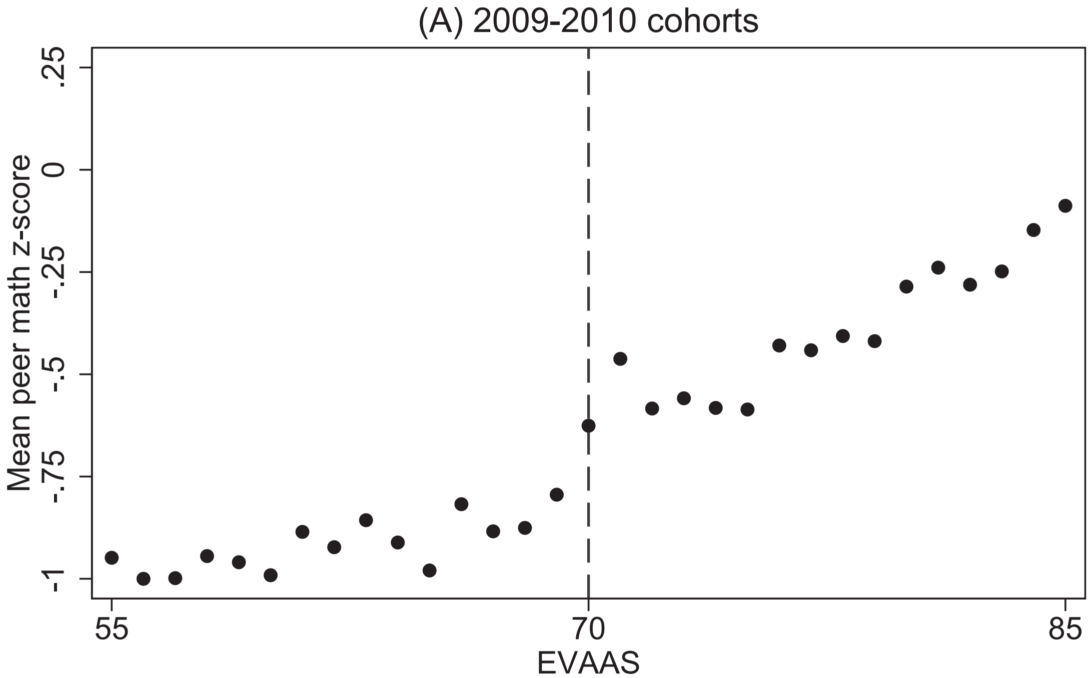


Figure A.5: Math Acceleration and Peer Characteristics



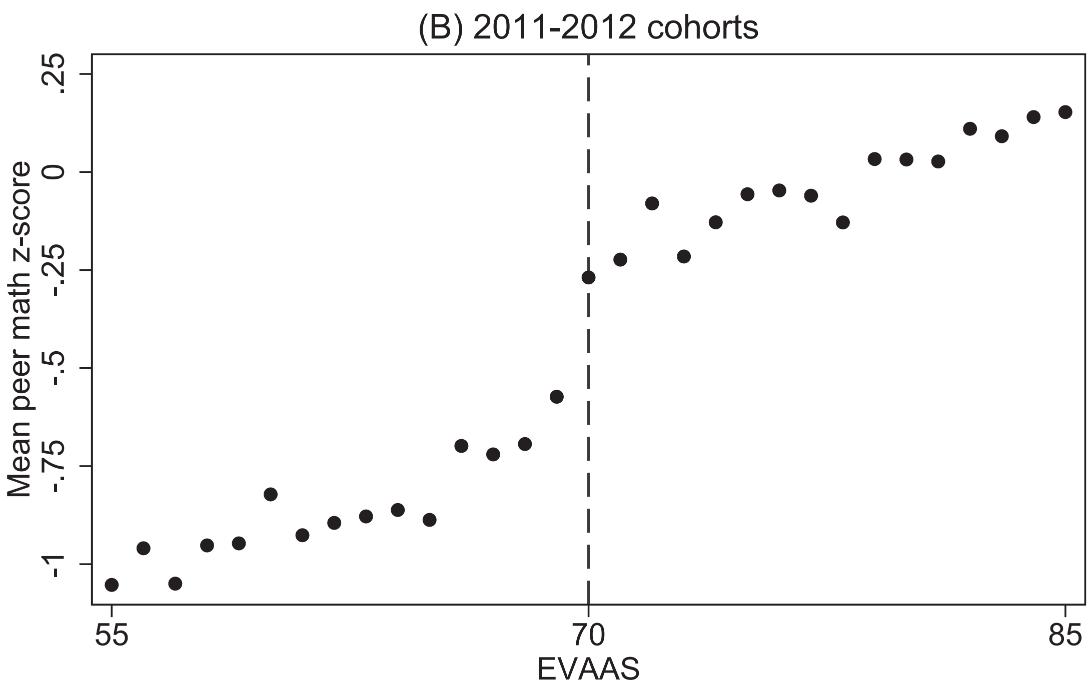


Table A.1: Covariate Balance Tests

	(1) Female	(2) Black/ Hispanic	(3) Low income	(4) Special ed.	(5) LEP	(6) Age at start of 6th grade	(7) Predicted math score	(8) Predicted math GPA
(A) 2009-10 cohorts								
Eligible	0.004	-0.008	-0.030	-0.014	0.064**	0.010	0.004	0.019
	(0.033)	(0.033)	(0.032)	(0.029)	(0.028)	(0.034)	(0.007)	(0.017)
μ	0.52	0.74	0.71	0.23	0.20	12.62	-0.61	2.04
N	3,375	3,375	3,375	3,375	3,375	3,375	3,375	3,375
(B) 2011-12 cohorts								
Eligible	-0.008	-0.024	-0.015	-0.022	-0.045*	0.040	-0.002	-0.004
	(0.033)	(0.024)	(0.027)	(0.022)	(0.025)	(0.029)	(0.007)	(0.017)
μ	0.54	0.66	0.59	0.20	0.22	12.55	-0.58	2.08
N	4,254	4,254	4,254	4,254	4,254	4,254	4,254	4,254

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Each coefficient is the reduced form estimate of the relationship between initial eligibility for acceleration and the listed covariate. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. Below each coefficient is the mean of the covariate for students just below the threshold (with initial EVAAS between 67 and 70). The first and second rows includes only the 2009-10 and 2011-12 cohorts respectively. The final two columns use as outcomes predicted math scores and grades, where predictions are generated from the untreated 2009 cohort as described in the text.

Table A.2: Heterogeneity in Initial Math Acceleration

	(1) 2009-10 cohorts	(2) 2011-12 cohorts
(A) Income		
Non-low income * Eligible	0.184***	0.189***
	(0.065)	(0.043)
Low income * Eligible	0.227***	0.232***
	(0.048)	(0.059)
p	0.47	0.49
N	3,375	4,254
(B) Race		
White/Asian * Eligible	0.102	0.160***
	(0.064)	(0.040)
Black/Hispanic * Eligible	0.261***	0.244***
	(0.048)	(0.057)
p	0.01	0.15
N	3,375	4,254
(C) Gender		
Male * Eligible	0.243***	0.222***
	(0.054)	(0.040)
Female * Eligible	0.192***	0.217***
	(0.053)	(0.061)
p	0.37	0.93
N	3,375	4,254

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). First stage estimates show the impact of initial eligibility on the probability of initial math acceleration, defined as enrollment in advanced math coursework in 7th grade for the 2009-10 cohorts or 6th grade for the 2011-12 cohorts. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. These replicate the regressions from Table 3, interacting the independent variables with indicators for income, race and gender groups, and including such indicators directly. Also shown is a p-value from an F-test of the equality of the two listed interaction coefficients.

Table A.3: Heterogeneity of Math Test Score and Grade Impacts

	(1)	(2)	(3)	(4)
	Math z-scores		Math GPA	
	2009-10 cohorts	2011-12 cohorts	2009-10 cohorts	2011-12 cohorts
(A) Income				
Non-low income * Accelerated	0.373	-0.365	-0.417	-1.243***
	(0.313)	(0.239)	(0.558)	(0.399)
Low income * Accelerated	-0.105	-0.068	-0.667**	-0.689
	(0.174)	(0.142)	(0.293)	(0.444)
μ (Non-low income)	-0.75	-0.59	2.41	2.48
μ (Low income)	-0.84	-0.74	1.98	2.22
p	0.15	0.35	0.67	0.34
N	3,337	4,205	3,368	4,252
(B) Race				
White/Asian * Accelerated	-0.425	-0.253	0.351	-0.792
	(0.584)	(0.280)	(0.763)	(0.540)
Black/Hispanic * Accelerated	0.134	-0.150	-0.692***	-0.959**
	(0.125)	(0.132)	(0.231)	(0.395)
μ (White/Asian)	-0.71	-0.65	2.26	2.34
μ (Black/Hispanic)	-0.85	-0.70	2.05	2.32
p	0.34	0.76	0.16	0.79
N	3,337	4,205	3,368	4,252
(B) Gender				
Male * Accelerated	0.209	0.055	0.148	-0.714
	(0.216)	(0.184)	(0.321)	(0.484)
Female * Accelerated	-0.131	-0.368	-1.155***	-1.002***
	(0.131)	(0.230)	(0.403)	(0.354)
μ (Male)	-0.88	-0.73	1.68	2.14
μ (Female)	-0.75	-0.64	2.49	2.48
p	0.13	0.22	0.01	0.62
N	3,337	4,205	3,368	4,252

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). Instrumental variables estimates show the impact of initial math acceleration on math test scores and course grades, where acceleration is instrumented with eligibility. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. These replicate the regressions from Tables 5 and 6, interacting the independent variables with indicators for income, race and gender groups, and including such indicators directly. Below each pair of coefficients are the subgroup means of the outcome variable among students just below the threshold (with initial EVAAS between 67 and 70). Also shown is a p-value from an F-test of the equality of the two interaction coefficients.

Table A.4: Robustness Checks, Initial Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Accelerated		Math z-score		Math GPA	
	2009-10	2011-12	2009-10	2011-12	2009-10	2011-12
Bandwidth = 5	0.194**	0.212***	0.127	0.206	-0.002	-1.411***
	(0.077)	(0.056)	(0.318)	(0.270)	(0.651)	(0.515)
Bandwidth = 10	0.240***	0.219***	0.121	-0.126	-0.216	-1.197***
	(0.056)	(0.050)	(0.167)	(0.159)	(0.348)	(0.395)
Bandwidth = 15	0.216***	0.218***	0.034	-0.160	-0.521**	-0.878***
	(0.047)	(0.044)	(0.149)	(0.106)	(0.264)	(0.326)
Bandwidth = 20	0.228***	0.241***	-0.032	-0.183**	-0.601***	-0.955***
	(0.044)	(0.040)	(0.151)	(0.081)	(0.232)	(0.270)
Bandwidth = 25	0.218***	0.262***	-0.255*	-0.352***	-0.759***	-1.111***
	(0.044)	(0.041)	(0.151)	(0.102)	(0.226)	(0.248)
IK Bandwidth	0.213***	0.221***	0.062	-0.119	-0.128	-1.177***
	(0.044)	(0.042)	(0.145)	(0.105)	(0.291)	(0.383)
BW	28.1	16.7	14.4	14.9	10.6	11.7
With controls	0.209***	0.220***	0.020	-0.088	-0.209	-1.162***
	(0.043)	(0.041)	(0.148)	(0.101)	(0.263)	(0.352)

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). Column 1 shows first stage estimates of the relationship between eligibility for math acceleration and the fraction of middle school years spent in accelerated coursework. The remaining columns show instrumental variables estimates of the impact of math acceleration on various outcomes. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. The first five rows use bandwidths from 5 to 25. The sixth row uses the smaller of the two Imbens-Kalyanaraman optimal bandwidths generated by the first stage and reduced form specifications. The seventh row adds to that controls for gender, race, age, poverty, special education and LEP status.

Table A.5: Robustness Checks, Longer-Run Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	8th Grade Algebra I			9th Grade Geometry		
	Enrolled	Passed	A or B	Enrolled	Passed	A or B
Bandwidth = 5	0.885***	0.657**	0.075	0.467**	0.486**	0.004
	(0.292)	(0.284)	(0.186)	(0.231)	(0.214)	(0.133)
Bandwidth = 10	0.626***	0.526***	0.055	0.491***	0.441***	0.032
	(0.163)	(0.167)	(0.121)	(0.145)	(0.125)	(0.077)
Bandwidth = 15	0.629***	0.531***	0.007	0.435***	0.385***	0.059
	(0.142)	(0.141)	(0.079)	(0.114)	(0.080)	(0.057)
Bandwidth = 20	0.669***	0.570***	0.030	0.480***	0.417***	0.047
	(0.136)	(0.134)	(0.070)	(0.112)	(0.087)	(0.043)
Bandwidth = 25	0.773***	0.637***	-0.106	0.496***	0.401***	-0.029
	(0.162)	(0.151)	(0.075)	(0.123)	(0.093)	(0.044)
IK Bandwidth	0.838***	0.650***	0.051	0.448***	0.392***	0.055
	(0.157)	(0.139)	(0.096)	(0.116)	(0.078)	(0.060)
BW	28.1	27.4	13.4	19.0	15.3	15.7
With controls	0.837***	0.644***	0.030	0.430***	0.359***	0.038
	(0.159)	(0.138)	(0.093)	(0.111)	(0.084)	(0.062)

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). All columns show instrumental variables estimates of the impact of 7th grade math acceleration on various outcomes, using the 2009-10 cohorts. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. The first five rows use bandwidths from 5 to 25. The sixth row uses the smaller of the two Imbens-Kalyanaraman optimal bandwidths generated by the first stage and reduced form specifications. The seventh row adds to that controls for gender, race, age, poverty, special education and LEP status.

Table A.6: Robustness Checks, Passing 9th Grade Geometry

	(1) Non-low income	(2) Low income	(3) White/ Asian	(4) Black/ Hispanic	(5) Male students	(6) Female students
Bandwidth = 5	1.066***	0.065	-0.089	0.611**	0.299	0.672
	(0.384)	(0.325)	(0.919)	(0.251)	(0.325)	(0.515)
Bandwidth = 10	0.707***	0.174	0.325	0.444***	0.373**	0.388**
	(0.198)	(0.149)	(0.589)	(0.125)	(0.188)	(0.178)
Bandwidth = 15	0.761***	0.183	0.330	0.377***	0.330***	0.399***
	(0.213)	(0.120)	(0.563)	(0.087)	(0.119)	(0.127)
Bandwidth = 20	0.733***	0.215*	0.308	0.418***	0.344***	0.446***
	(0.177)	(0.118)	(0.289)	(0.092)	(0.088)	(0.135)
Bandwidth = 25	0.656***	0.256**	0.258	0.412***	0.188**	0.548***
	(0.152)	(0.129)	(0.258)	(0.106)	(0.094)	(0.154)
IK Bandwidth	0.709***	0.190	0.274	0.390***	0.317**	0.548***
	(0.176)	(0.122)	(0.303)	(0.096)	(0.129)	(0.154)
BW	19.6	17.3	19.9	17.5	13.9	25.0
With controls	0.683***	0.199*	0.242	0.374***	0.319***	0.524***
	(0.178)	(0.116)	(0.301)	(0.094)	(0.124)	(0.156)

Notes: Heteroskedasticity robust standard errors clustered by initial middle school are in parentheses (* p<.10 ** p<.05 *** p<.01). All columns show instrumental variables estimates of the impact of 7th grade math acceleration on passing 9th grade geometry, using the 2009-10 cohorts. The coefficients shown are generated by local linear regression using a rectangular kernel of bandwidth 15, including cohort-by-school fixed effects. The first five rows use bandwidths from 5 to 25. The sixth row uses the smaller of the two Imbens-Kalyanaraman optimal bandwidths generated by the first stage and reduced form specifications. The seventh row adds to that controls for gender, race, age, poverty, special education and LEP status.