NBER WORKING PAPER SERIES

ALGEBRA FOR 8TH GRADERS: EVIDENCE ON ITS EFFECTS FROM 10 NORTH CAROLINA DISTRICTS

Charles T. Clotfelter
Helen F. Ladd
Jacob L. Vigdor

Working Paper 18649
http://www.nber.org/papers/w18649

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2012

We are grateful to the Institute for Education Sciences and American Institutes for Research, through the Center for the Analysis of Longitudinal Data in Education Research (CALDER), for financial support and to Alexandra Oprea and Kyle Ott for research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Algebra for 8th Graders: Evidence on its Effects from 10 North Carolina Districts
Charles T. Clotfelter, Helen F. Ladd, and Jacob L. Vigdor
NBER Working Paper No. 18649
December 2012, Revised February 2013
JEL No. H75,I21

ABSTRACT

This paper examines the effects of policies that increase the number of students who take the first course in algebra in 8th grade, rather than waiting until 9th grade. Extending previous research that focused on the Charlotte-Mecklenberg school system, we use data for the 10 largest districts in North Carolina. We identify the effects of accelerating the timetable for taking algebra by using data on multiple cohorts grouped by decile of prior achievement and exploiting the fact that policy-induced shifts in the timing of algebra occur at different times in different districts to different deciles of students. The expanded data make it possible to examine heterogeneity across students in the effect of taking algebra early. We find negative effects among students in the bottom 60% of the prior achievement distribution. In addition, we find other sources of heterogeneity in effects.

Charles T. Clotfelter
Sanford Institute of Public Policy
Duke University
Box 90245
Durham, NC 27708
and NBER
charles.clotfelter@duke.edu

Jacob L. Vigdor
Sanford School of Public Policy
Box 90245
Duke University
Durham, NC 27708
and NBER
jacob.vigdor@duke.edu

Helen F. Ladd
Sanford School of Public Policy
Box 90245 Duke University
Durham, NC 27708
hladd@duke.edu

ALGEBRA FOR 8TH GRADERS: EVIDENCE ON ITS EFFECTS FROM 10 NORTH CAROLINA DISTRICTS[1]

Charles T. Clotfelter
Helen F. Ladd
Jacob L. Vigdor

During the past few decades, many states and school districts have been aggressively pushing more 8th graders to take algebra, a course that historically has been offered primarily to high school students in the United States. Although it has always been common for some high-ability math students to take algebra by 8th grade, between 1996 and 2008, the percentage of 13 year olds taking algebra increased from 16% to 30% (Rampey et al. 2009) and by far higher percentages in some areas. The reasons for this push for algebra by 8th grade include a new focus on STEM courses and the recognition that algebra serves as a gateway course to the higher level math courses needed for college and other careers. It has also become an issue of social justice, based on concerns that some groups are being unfairly denied access to early algebra. The policy remains controversial: critics argue that evidence for a causal impact of algebra timing on later outcomes is slight, and that some students struggle if placed in advanced coursework before they are ready (Loveless, 2008).

In a prior paper based on data mainly from a single North Carolina district (Clotfelter, Ladd, and Vigdor 2012, hereafter CLV 2012), we documented that some of the concerns raised by critics appear to be valid. In particular we found that in the Charlotte-Mecklenburg district, which had pursued a well-documented policy to place more students into early algebra in the 2003 and 2004 school years and then subsequently retracted that policy, students who were

[1] We are grateful to the Institute for Education Sciences and American Institutes for Research, through the Center for the Analysis of Longitudinal Data in Education Research (CALDER), for financial support and to Alexandra Oprea and Kyle Ott for research assistance.

pushed to take algebra by 8th grade performed less well in subsequent math courses, especially geometry, as they progressed through high school. Our prior work is limited, however, because our estimated effects are local to those students actually affected by the Charlotte-Mecklenburg policy shift. In the current paper, we circumvent that limitation by extending the analysis to 10 large North Carolina districts.

This expansion allows us to enrich the analysis and explore a wider range of effects. In particular we are able to estimate effects for high-performing students – who were not affected by the Charlotte-Mecklenburg policy shift – and to examine heterogeneous impacts by the student's race, and by the income and education status of the student's parents. Following the same methodology as in the earlier paper, we identify the effects of accelerating algebra by using data on multiple cohorts of students disaggregated by their prior math achievement and exploiting the fact that shifts in algebra timing typically apply only to students in certain segments of the math achievement distribution, and occur in different years in different districts. With the use of instrumental variables based on the inferred policy changes by district we are able to isolate the causal effects of the policy interventions on a number of subsequent math outcomes. As in our previous paper, we find that the overall effect of taking algebra by 8th grade is to increase the probability that students will pass Algebra I by 10th grade, but to depress their performance on the Algebra I test and decrease the likelihood they will pass Geometry by 11th grade. In addition, we find significant heterogeneity in effects.

In the next two sections of the paper, we provide the policy context and outline the model we employ to infer the effects of taking algebra in middle school on several important outcomes. Section III describes our data and the steps we take to trim the sample in an effort to increase our confidence that our estimates are useful and not subject to omitted variable bias. In section IV we

present instrumental variables estimates of the causal effect of teaching algebra to middle school students, and in sections V and VI we consider effects on taking calculus and pre-calculus courses and on repeating Algebra I. Section VII concludes the paper.

I.US and North Carolina Context

One of the seemingly most uncomplicated school reforms to take hold in the United States in the last two decades has been the push to have more 8th graders take algebra, a course traditionally taught in high school. Noting the success that other countries have had in teaching the course to students at this age, a chorus of scholars and blue ribbon panels have urged the U.S. to follow suit.[2] For some proponents, expanding access to algebra in middle school can help the country to regain global leadership in STEM training. For others, it is a social justice issue: scholar Alan Schoenfeld (1995) called algebra "a new literacy requirement for citizenship, and civil rights veteran Bob Moses (1995) dubbed it "the new civil right."[3] To proponents, what makes algebra so important is its "gateway" role: "The earlier a student proceeds successfully through algebra, and then on to courses such as geometry and algebra II, the more opportunities he or she has for reaching higher level mathematics courses (e.g., trigonometry, precalculus, and calculus) in high school…." (Walston and McCarroll 2011). This call for algebra in middle school has been taken up with enthusiasm in many parts of the country.

This push to teach algebra to 8th graders has not been without its critics, however. Naysayers point out the logical problems in drawing causal inferences from observed differences

[2] See, for example, Schoenfeld (1995), Schmidt (2004), and National Mathematics Advisory Panel (2008). The panel urged that math courses in elementary and middle school be adjusted to prepare more students to be able to take algebra by 8th grade.

[3] Schoenfeld (p. 11, 1995) elaborates: "Algebra today plays the role that reading and writing did in the industrial age. If one does not have algebra, one cannot understand much of science, statistics, business, or today's technology. Thus, algebra has become an academic passport for passage into virtually every avenue of the job market and every street of schooling. With too few exceptions, students who do not study algebra are therefore relegated to menial jobs and are unable often even to undertake training programs for jobs in which they might be interested. They are sorted out of the opportunities to become productive citizens in our society."

among students who take algebra at varying ages, and have worried that accelerating the math timetable in this way would create more problems than opportunities for many students ill-equipped to deal with the abstractions of algebra in middle school. Loveless (2008) presents NAEP data to show that thousands of 8th graders taking algebra or geometry do not know basic arithmetic, leaving them unable to profit from these courses and taking up the time and attention of teachers who might otherwise be helping students with stronger math backgrounds. In Clotfelter, Ladd and Vigdor (2012), we examine the effect of one district's short-lived policy of teaching Algebra I to large numbers of its 8th graders. Our findings suggest that at least some of the critics' concerns are well-founded. We found that, on average, students who were subjected to the push to take algebra earlier in the Charlotte-Mecklenberg school district scored lower on the statewide end-of-course algebra test and were less likely than other similar students subsequently to pass the follow-on course in geometry by 11th grade.

Because it relied on policy changes and data primarily from just one school district, that study (CLV 2012) left several pressing questions unanswered. Crucially, our research design limited us to assessing the impact of algebra acceleration for the set of students affected by the policy initiative, namely those in the middle of the prior math test score distribution. That ruled out, for example, analyzing the effect of the policy change in Charlotte-Mecklenburg on high-achieving students (as defined by being near the top of the 6th grade test score distribution) because virtually all such students took algebra by 8th grade both before and after the policy change. Moreover, the absence of meaningful variation for high-performers implied that we also had little chance of observing a hypothetical positive impact of early algebra taking on progression to calculus or other advanced courses in high school.

In the current paper, we combine data for the 10 largest school districts in North Carolina

over a period of six academic years to infer the effects on students of taking algebra by 8th grade, rather than later or never at all. For all of the districts, we observe significant changes over time in the probability that students in at least some deciles of prior math achievement – independent of measurable characteristics of those students – will take the state's standard Algebra I course. We use these temporal variations in probability as an instrument in equations predicting the score on the state's end-of-course test in Algebra I and success in passing Algebra I, Geometry, and Algebra II. As in our previous paper, we find that the broad effect of taking algebra by 8th grade is to increase the chance that students will pass Algebra I by 10th grade, but to depress their performance on the Algebra I test and decrease the likelihood they will pass Geometry by 11th grade. In addition, we find significant heterogeneity in effects. Supporting the suspicions of Loveless and other skeptics, we find that the effect of early algebra-taking differs by students' previous math achievement, with the deleterious effects being the most pronounced for students with the weakest previous achievement. We also find differences in policy impact by gender, free lunch eligibility, and parents' education, some of which differences are unexpected.

II. Model

Our objective is to assess the effects of variations in school policies and practices that manifest themselves in differing rates with which otherwise similar students are assigned to take Algebra I in middle school. That is, we examine the effect on students of changing the timing, not the content, of the first course in algebra. So far as we know, the content of the Algebra I course in North Carolina did not change during our period of study.[4]

We examine three kinds of possible effects on students. The first is on the student's

[4] In 2007, the state changed the scoring and passing standard of the standardized end-of-course test in Algebra I. the proportion of students failing the EOC test increased substantially relative to previous years. To our knowledge, this change in test scoring did not coincide with a change in curriculum for the course. In our specifications below we account for the change in scoring scale and passing standard by defining "passing" the test as scoring above the 20th percentile, which approximates the pre-2007 standard.

knowledge of algebra, as indicated by performance on the state's mandatory test administered to all Algebra I students at the completion of the course. By design, this test is intended to assess understanding of the course material, and we can be confident that students and teachers took the test seriously, since at least a quarter of the course grade must be based on its outcome. The second possible effect of taking algebra early is on how well the student succeeds in passing not only Algebra I itself, but also the two other basic courses in the state's mathematics sequence: Geometry and Algebra II. The third kind of outcome we examine is whether students took courses beyond Algebra II, including calculus. For reasons explained below, our identification strategy is not particularly well suited to estimating the impact of algebra timing on calculus-taking; instead we offer estimates in the spirit of a "bounds" analysis (Manski, 1990). This outcome is important because one of the arguments for moving algebra to 8th grade is that it opens up for students the opportunity to take more advanced coursework in mathematics during high school. In CLV (2012), we were unable to examine this last outcome because any effects on enrollment in math courses beyond Algebra II are most likely concentrated among higher-performing students, for whom we observed very little policy variation in our initial single-district study.

In devising our estimation strategy, we have endeavored to produce estimates that reflect causation, not simply correlation. Logic and experience suggest that, in deciding whether a 7th or 8th grader should be assigned to take algebra rather than another pre-algebra course, a school may well take numerous student-level factors into consideration, not all of which are reflected in measures contained in administrative data sets such as the one we use. Thus any attempt to assess the effect of early assignment to algebra based on observational, within-cohort-and-district variation will inevitably be subject to omitted variable, or selection, bias.

To combat that statistical challenge, our estimating models use instrumental variables estimation, along the same lines as the model we employed in CLV (2012). The larger number of districts used in this study means that a wider range of students were subjected to shifts in algebra placement policy over time. As in the initial study, we use this policy-induced variation to assess the impact of algebra timing on student outcomes. This task is complicated in the present study by the absence of clear documentation of any official shift in policy in some districts. We infer that a policy shift has occurred in those circumstances where the across-cohort variation in Algebra I placement patterns is too large to be based on random fluctuations in student background characteristics alone.

We begin with data on students from six successive cohorts in the 10 largest school districts in North Carolina. We stratify the sample by student prior achievement, as measured by the student's average scores on 6th and 7th grade standardized math tests. We then reduce the sample, using a procedure outlined below, to those district, cohort, and prior achievement decile cells that exhibit significant variation in placement patterns across cohorts.

Our model takes the form of conventional instrumental variables estimation. We estimate several different two-stage models of the form:

(1) $T_{idcs} = \gamma_c + \gamma_d + \gamma_s + \bar{T}_{dcs} + \nu_{idcs}$

(2) $Y_{idcs} = \alpha_c + \alpha_d + \alpha_s + \beta \hat{T}_{dcs} + \varepsilon_{idcs}$

where Y_{idcs} is the outcome of interest for student *i* in prior achievement decile *d* in cohort *c* enrolled in school district *s*, α_d, α_c, and α_s are decile, cohort, and school district fixed effects, T_{idcs} is a treatment indicator, where the treatment is taking Algebra I no later than 8th grade, $\bar{T}_{dcs}$, the instrument, is the decile-cohort-school district cell average of the treatment, and $\hat{T}_{dcs}$

is the predicted value obtained from equation (1) – and ν_{idcs} and ε_{idcs} are independent and identically distributed error terms. Cohort fixed effects account for policy changes or other contemporaneous effects that had an influence on all students in a cohort across the state, decile fixed effects account for broad differences in outcome trajectories for students with differing prior achievement, and school district fixed effects account for systematic policy and other differences across districts. By using decile fixed effects rather than a linear control for test score, we are able to account for potentially nonlinear effects of prior achievement on subsequent outcomes.

In effect, this estimation strategy associates across-cohort/decile/school district variation in the propensity to take Algebra I by 8th grade with across-cohort/decile/school district variation in the outcome of interest. We attribute a positive (or negative) effect to acceleration if students subjected to a higher risk of earlier algebra than others in the same prior achievement decile and district in another cohort exhibit better (or worse) subsequent outcomes of interest – performance in Algebra I, passing that and the two following math courses, taking a math course beyond Algebra II. We also examine the probability of repeating Algebra I. Because the identifying variation in algebra timing is at the cohort-by-decile-by-school district level, we cluster standard errors at that level.

Equations (1) and (2) highlight a potentially serious criticism of our identification strategy. By instrumenting for a student's own placement experience with the average experience of students in her cohort/decile/district cell, we risk replacing an individual-level variable that is subject to concerns about unobservable factors with a cell-level average variable that is subject to a different set of concerns about unobserved factors. For the approach to be successful, we must have some confidence that differences in the cell averages reflect differences

in placement policy rather than differences in unobserved student characteristics. In order to describe our strategy for attaining this degree of confidence, we must first describe our data in greater detail.

III. Data

We use student-level data for the 10 largest North Carolina school districts. Shown in Figure 1, these districts are: Charlotte-Mecklenburg, Wake (containing Raleigh), Guilford (Greensboro), Cumberland (Fayetteville), Winston-Salem/Forsyth, Gaston (Gastonia), Durham, Union (Monroe), Johnston (Smithfield), and Cabarrus (Concord). Of these districts, three (Gaston, Union, and Cabarrus) contain suburban overflow from Mecklenburg County, and Johnston similarly contains some bedroom suburbs adjacent to Wake County. In order to study students who experienced different policy regimes regarding the aggressiveness in placing students in Algebra I by 8th grade, we use information for six successive cohorts of students, beginning with those who were 7th graders for the first time in the fall of 1999 and ending with those who arrived in 7th grade in the fall of 2004. Students in these cohorts who made normal progress in school would have graduated from high school in the years 2005 to 2010, but we track students whether or not they experienced normal grade progression.

Our data represent longitudinally matched records on students derived from administrative records housed in the North Carolina Education Research Data Center.[5] When evaluating the effect of taking Algebra I by 8th grade, we focus on those students enrolled in one of our 10 districts in the year after they began 7th grade. We restricted the sample to students with valid scores on the state's standardized 6th and 7th grade mathematics end-of-grade assessments. For each student, we averaged those two scores, to reduce possible concerns with

[5] The Data Center supplies unique identifying numbers that allow researchers to link student records in different data sets while protecting the identities of individuals.

measurement error in test scores, and used that average to assign them to deciles in order to stratify them by prior math performance.[6] Students were assigned to districts based on their 8th grade enrollment. We then tracked their progress through college-preparatory math courses using information from the state's end-of-course examinations in Algebra I, Geometry, and Algebra II.

We employ several outcome measures based on students' math achievement and course-taking. Ideally, we would have estimated the impact of taking Algebra I by 8th grade on actual knowledge gained, as measured by performance on the test designed for that course and on the tests for subsequent math courses. The approach is frustrated, however, because some students never take Algebra I, and many more never take the follow-up courses. These facts create two sources of sample selection bias. The more serious one is that so many students never take Geometry or Algebra II. If we were to use as outcome measures the scores on the tests for those two courses, we would have to restrict ourselves to those select students brave or accomplished enough to take the courses at all. To avoid the obvious selection bias that would invite, we adopt as our outcome measure simply whether or not students passed those courses, an outcome we can measure for all students given that those who do not take a course by definition cannot pass it. The second source of selection bias arises in analyzing the end-of-course test score in Algebra I. Because we can observe performance only for those students who actually take the course, our analysis may lead us to overstate the negative effects of the acceleration policy. That outcome will occur to the extent that a policy of accelerating students into 8th grade algebra correlates with efforts to expand the set of students ever taking algebra, causing marginally-performing students to be swept into the sample only during years of acceleration. To deal with this second selection problem, we estimate alternative models using quantile regression methods. For the

[6] If a student took Algebra I before 8th grade, the fact was noted, and the student was included with students who took Algebra I by 8th grade. If the student took it in a different district from his or her 8th grade district, he was dropped from the sample.

quantile regressions, we impute test scores for non-algebra takers, under the presumption that those students who do not take the course would have scored below the median conditional on their observed characteristics (Neal and Johnson 1997).

In contrast to our analysis of Charlotte-Mecklenburg (CLV 2012) – where we had direct evidence that that district had undertaken an explicit policy of placing more 8th graders in algebra classes – we have little documentary proof for the districts studied in the current paper of formal policy directives about offering algebra to 8th graders. Moreover, as noted above, our identification strategy will yield biased results to the extent that across-cell variation in placement patterns reflects across-cell variation in unobservables rather than policy. Our strategy for distinguishing policy-induced variation from random fluctuations in placement patterns attributable to student unobservables rests on the assumption that there is no reason to expect systematic variation in unobservables across cohorts. If all variation in mean unobserved characteristics across cohorts is idiosyncratic, then we can use standard statistical tests to determine whether the degree of observed variation is too large to be explained by idiosyncratic factors alone. In practice, we use two rounds of statistical tests. The first is a simple Chi-squared test for significant variation in placement patterns across cohorts within a district/decile cell. The second is an *F*-test for significant residual variation in placement patterns after controlling for observed student characteristics.

To appreciate the need for the first of these exclusions, consider the bar graphs in Figures 2 to 4. These graphs show the percentage of students (grouped by quintile rather than decile, for ease of presentation) who took Algebra I by 8th grade in five selected districts. For example, Figure 2, which tracks middle schoolers who took Algebra I in Charlotte-Mecklenburg, clearly illustrates the effects of that district's bold algebra acceleration policy. Successive cohorts in the

first three quintiles experienced a marked jump, then drop, in the risk of taking Algebra I by 8th grade. In contrast, there was almost no change over the period for students in the highest quintile – almost all of whom took algebra by 8th grade. Figure 3, for Wake County, reveals a very different pattern, with virtually no change over time in the treatment of students in the bottom three quintiles.

The Chi-squared tests for the 100 decile-district subsamples test the null hypothesis that the probability of taking algebra for those students remained the same for all six cohorts – i.e., that any observed variation is attributable to random fluctuation rather than any systematic change in policy.[7] For 35 of the 100 subsamples, we could not at the 5% level of confidence reject the hypothesis of no difference in probability across the six cohorts, leading us to exclude those cells from our sample.[8] For example, none of the bottom seven deciles in Wake County showed significant variation across cohorts, reflecting the near uniformity that is evident in the pattern of bars for the bottom three quintiles in Figure 3. For Charlotte-Mecklenburg, the lack of variation in the top quintile shown in the figure turns out to reflect a lack of variation primarily in the top decile alone, so we excluded that decile. In contrast, for Durham, the hypothesis of no variation could be rejected for all 10 deciles, a result that is not surprising, considering the patterns evident in Figure 4.

The second set of tests eliminates decile-district subsamples for which student characteristics appear to explain a significant portion of the variation in algebra-taking across the cohorts. For our entire sample of students, we estimated a linear probability model regressing an indicator for 8th grade Algebra I placement on gender, year of birth, categorical variables for race/ethnicity, free lunch status, and parental education, as well as fixed effects for cohort,

[7] Note that seven of the decile-district subsamples had no students who had taken Algebra I by 8th grade.
[8] The p-values from the Chi-squared tests are shown in Appendix Table A1.

decile, district, and all their interactions. For each of the 100 decile-district subsamples, we performed an *F*-test for the hypothesis that the cohort fixed effects were equal.[9] For six decile-district subsamples that had not previously been excluded by our first test of variation, we were unable to reject this hypothesis, leaving us with 59 remaining decile-district subsamples, containing a total of 124,505 students.[10]

For this trimmed sample, Table 1 shows, by district, the number of students and the means and standard deviations of our measure of prior achievement, as well as four of our main outcome measures. As noted above, we sorted students into deciles of prior math achievement, based on the average of each student's 6th and 7th grade standardized end-of-grade math scores. The remaining four variables are measures of outcomes and are all based on the state's mandatory end-of-course tests . In addition to the score on the Algebra I end-of-course exam, we also track whether students took and passed that course and the two follow-on courses – Geometry, and Algebra II.[11] Owing to our exclusion of designated decile-district subsamples, the two largest districts, Charlotte-Mecklenburg and Wake County, contribute fewer students to the final sample than the third largest district, Guilford. The average student characteristics reflect the deciles that remained after trimming. The averages for Wake and Cabarrus, which had most or all of their lowest seven deciles excluded, reflect the characteristics of their remaining relatively high-scoring students. The mean for Charlotte-Mecklenburg, in contrast, shows the effects of having some of its highest deciles dropped. Within each of the districts, the average

[9] The p-values for these *F*-tests are shown in Appendix Table A2.
[10] The excluded decile-district subsamples are identified Appendix Table A3.
[11] Our definition of a passing grade on the Algebra I and Algebra II end-of-course tests is based on the proficiency standard in place for most of the years in our sample, which was roughly equal to the 20th percentile of the statewide distribution for both tests. In 2007, the state adopted stricter grading standards on both end-of-course tests, placing the passing threshold closer to the 40th percentile of the statewide distribution. By using a uniform standard based on a specific point in the distribution, we assume that there is no meaningful change in the statewide distribution of Algebra I or Algebra II test scores over time. As there is no substantial shift in standards on the Geometry EOC test, no comparable adjustment is necessary.

rate of passage for Algebra I by 10th grade is higher than those for Geometry by 11th and Algebra II by 12th.

Before turning to estimates of the effect of taking Algebra by 8th grade, we summarize the correlates of our four main outcome measures. Table 2 presents estimates based on four OLS regressions explaining the four measures – the student's performance on the first Algebra I end-of-course test and the three binary indicators for taking and passing Algebra I, Geometry, and Algebra II, as described above. These results should not be assigned a causal interpretation, as they make use of observational variation in algebra timing, which presumably correlates with unobserved determinants of math achievement. The table reports estimated coefficients on an indicator for taking Algebra I by 8th grade as well as cohort, district fixed effects, and decile fixed effects. Most noteworthy are the coefficients for taking algebra by the 8th grade. They demonstrate beyond any reasonable doubt that, although students who take algebra in middle school tend to perform poorly on the algebra end-of-course test, they are more likely than other students to take and pass Algebra I, Geometry, and Algebra II. They are more than 5 percentage points more likely than other students to take and pass Geometry, and the differences are even greater for the two algebra courses.

As for other correlates of taking algebra in middle school, the regression reveals few statistically significant differences by cohort. By district, Wake County, the omitted one, consistently bests most of the others, reflecting in part its more affluent makeup. The decile indicators have the expected pattern, with all coefficients increasing monotonically through the first eight deciles. Not surprisingly, the best predictor of math achievement is prior math achievement.

IV. The Causal Effects of 8th Grade Algebra

Following the approach we take in CLV (2012), we employ instrumental variables methods to estimate the causal impact of taking algebra by 8th grade. As described in section II above, we used fitted values from a first stage regression (1) as an instrument for the likelihood that a student of cohort *c*, whose prior achievement puts her in decile *d*, and whose residence assigns her to school district *s*, will be put into an Algebra I class by 8th grade. Because we have excluded all students who were in decile-district subsamples for which this likelihood of taking algebra by 8th grade either did not vary significantly over time or for which across-cohort variation could be attributed to student characteristics, we can view the remaining students as facing exogenously varying probabilities of receiving this treatment, with those probabilities being entirely a function of year of birth, that is, of the cohort into which the student found herself in 7th grade.

We implemented three variants of instrumental variables estimation. First, we estimated simple two-stage least squares, for both equations explaining the score on the Algebra I test and the linear probability models explaining course passage. Second, we used binomial probit as an alternative to the 2SLS linear probability models because the latter, while producing coefficients easy to interpret in terms of probabilities, do not conform to the necessary distributional assumptions in either the first or second stage. Third, we applied a quantile regression version of I.V. using imputation methods to deal with sample selection in the students who take Algebra I.

Basic Results

Table 3 reports instrumental variables estimates applying to students at large, with none of the interactions we focus on below. The first equation employs IVQR estimation, with imputed test scores for students not taking Algebra I at all. The other equations use as outcomes

taking and passing the three math courses, with a two-stage least squares and binomial probit used for each of those outcomes. Each model controls for prior achievement (based on 6th and 7th grade end-of-grade math tests), cohort and district fixed effects, and the predicted value of Algebra I enrollment by 8th grade derived from first-stage equations.

The estimated equations paint a largely negative picture. Students who were enrolled in years when their districts were more aggressive about teaching Algebra I to 8th graders scored lower on their end-of-course test (some 37% of a standard deviation lower) and were less likely to take and pass Geometry by 11th grade by some 6.6 percentage points (based on the 2SLS model). Nor were they more likely to take and pass Algebra II by 12th grade; in fact the 2SLS model implies that they were a little less likely to. The only ray of sunshine in these results comes from the probability of passing Algebra I itself. We take this positive finding to be a direct consequence of the opportunity an early algebra class affords a student who does not pass the course the first time around to retake it. These findings closely mirror those we obtained in our earlier analysis of Charlotte-Mecklenburg.[12]

Effects by Prior Achievement

Analyzing outcomes in 10 different districts provides us an opportunity to examine heterogeneity across the student population in the effects of taking algebra by 8th grade. It is especially important to determine whether there are differences by prior achievement, since much of the discussion about the advisability of teaching algebra to 8th graders concerns middle schoolers' readiness for the course. Few would dispute that at least some 8th graders are ready to take algebra. The question is, how many? To address this question, we estimated equations of the

[12] In CLV (2012, Table 4), for example, the estimated coefficient in the corresponding IVQR equation is -0.324***, compared to the -0.374*** in the current paper. The estimated coefficients in the 2SLS equations in the previous paper are 0.069**, -0.095***, and -0.002 for Algebra I, Geometry and Algebra II, respectively, compared to the 0.091***, -0.066***, and -0.026* in Table 3 of the current paper.

form shown in Table 3 with interactions by quintile of prior achievement. These are presented in Table 4b. For ease of interpretation, we have dropped the bivariate probit specifications and added a 2SLS specification for the Algebra I test score, the estimates from which may well be subject to sample selection bias, as noted above.

The most striking set of estimates is for the effect of passing Geometry. The estimated coefficients make it clear that the overall negative effect of taking algebra by 8th grade comes entirely from the deleterious effects on students in the lowest three prior-year achievement quintiles. For those students – occupying the middle and bottom portions of the distribution – algebra by 8th grade reduces by at least 8 percentage points the chance that a student will take and pass geometry by 11th grade. For students in the top quintile, however, taking algebra by 8th grade *increases* the chance of success in geometry. For students in the fourth quintile, there is no effect one way or the other. As for passing Algebra I, something of a U-shaped effect is evident. We interpret the large positive coefficient in the lowest quintile to be an enabling effect: for those most likely to struggle in algebra the best shot at passing the course eventually is to start early. The effects on Algebra I test scores are negative for all students. The RFQR estimates, which impute poor performance to those with missing test scores in a quantile regression model, show that performance by students in quintiles 2 and 3 is harmed the most and that by students at the top is harmed the least. As for passing Algebra II, the faintly negative effect observed in the overall effects shows up in the Table 4b estimates only in the second quintile, and with a point estimate suggesting a decline of 4 percentage points. For Algebra II it is impossible to reject the hypothesis that all the quintile coefficients are equal.

Effects by Other Characteristics

We estimated three additional models with interactions for three other student characteristics: gender, free lunch status, and parental education. Taken together, these models yield several quite unexpected results. The gender interactions are shown in Table 5. Holding constant their previous math achievement, boys score lower than girls do and are less likely to pass the three math courses. The interaction with algebra by 8th grade is positive, implying the negative effects of early algebra are more pronounced among girls. This implies that the gender gap is much more pronounced among students who do not take algebra by 8th grade. The percentage point gap in passage rates separating boys and girls is 5.1 for those who did not take algebra by 8th grade but only 0.6 for those who did. For Algebra II the corresponding gender gaps are 6.8 and 3.0 percentage points. The estimated coefficients for Algebra I test scores tell a similar story: boys are lagging behind girls in high school math, but the gap is smaller among those who took algebra in middle school.

Table 6 reports interaction effects for free and reduced-price lunch, a common but imperfect proxy for low income. Unsurprisingly, subsidized lunch receipt associates with poorer academic outcomes overall. Interaction terms indicate that the effects of algebra acceleration are more pronounced among disadvantaged students in some cases, but not others. Students on free and reduced lunch suffer a more pronounced negative effect on standardized Algebra I test scores, but more encouraging results on all other outcomes. The positive effect of acceleration on Algebra I passage is stronger among subsidized lunch recipients. For passing Geometry and Algebra II, the penalty associated with taking algebra in middle school was partially or wholly erased for those getting free and reduced price lunch. This puzzling pattern might result from tracking practices in high schools. Students who do not take Algebra I in middle school face a

mathematics placement decision in high school, with options including the college-preparatory sequence beginning with Algebra I as well as other options. Disadvantaged students, for a variety of reasons, may be more likely to choose or be steered into less rigorous tracks in high school. Completing Algebra I in middle school, by contrast, clearly marks a student as being selected for the college preparatory track, even if the student's performance in the course is relatively poor.

The last set of interactions is summarized in Table 7, where the characteristic of interest is parental education, specifically, whether either parent had a bachelor's degree or more. Not surprisingly, students with college-educated parents did better than others on every one of our outcome measures. Interaction terms show that the children of highly educated parents appear relatively impervious to the effects of algebra acceleration – both positive and negative. Results are consistent with the view that highly educated parents buffer the impacts of education policies.

The effects of algebra acceleration are clearly heterogeneous. Generally speaking, students who begin in a relatively advanced position – perhaps thanks to their family background, or to their rate of learning in earlier grades – appear to suffer no long term effects when steered towards taking the course in 8th rather than 9th grade. The story differs for students who begin at an educational disadvantage. Altogether, patterns indicate that a policy of mandating 8th grade algebra for all students runs the risk of exacerbating educational inequalities in the high school years, with the possible exception of disadvantaged student for whom acceleration implies a possible track switch into college-preparatory coursework.

V. Taking Calculus and Other Advanced Math Courses

As articulated by Walston and McCarroll (2011) above, one of the strongest selling

points for teaching algebra to more 8th graders is the increased opportunities it should provide for students to take courses beyond Algebra II, including calculus. Ideally, we would have liked to analyze the effect of taking algebra in middle school on students' enrollment and success in these more advanced math courses. However, North Carolina has no end-of-course tests for these advanced math courses like those it mandates for Algebra I and II and Geometry. The only information on enrollment in these courses is a relatively new data set summarizing student transcripts, which just covers 7th grade cohorts beginning with 2002/03. The restriction to just three age cohorts deals a significant blow to our identification strategy, which relies upon the existence of variation in placement patterns across cohorts that is too large to be explained by idiosyncratic factors. As a result, we abandon that strategy here and focus on a form of bounds analysis, presenting simple statistics with clearly signed biases. This strategy can yield informative results in certain circumstances, such as when documenting that the upper bound on a coefficient with a clear upward bias is small.

Figure 6 shows the share of two groups of students, by level of prior achievement, who took calculus by 12th grade: those who had taken Algebra I by 8th grade and those who had not. Not surprisingly, taking calculus was more common among students in the algebra-by-8th grade group. There are two reasons for this difference. One is a causal inference: taking algebra early places students in a superior position to proceed to calculus. The other reason is that the two groups are different: even conditional on 6th and 7th grade performance, early algebra-takers are likely to be more promising students in ways not captured by test scores alone. We therefore expect simple comparisons such as the ones shown here to be affected by positive selection bias. The difference between the rates of calculus-taking, shown by the vertical distance between the curves, is therefore an upper bound on the effect of a policy to teach Algebra I in middle school.

In the lower half of the prior achievement distribution, this gap is small, suggesting that, for a majority of students, taking Algebra I in middle school is not going to have a big impact on the probability of taking calculus in high school. For students near the median in middle school, access to Algebra I in 8th grade raises the chances of taking calculus in high school by at most 10 percentage points. Note that some students manage to take calculus even after waiting to take Algebra I until high school, presumably because they take two math courses simultaneously in one or more years. Not until we get into the top quintile of students (by prior achievement) is the raw difference in rates of calculus-taking as much as 20 percentage points. In short, arguments to accelerate algebra on the grounds that it significantly enables high school calculus-taking are best applied to high-performing students, and not moderate or low performers. [13]

VI. Repeating Algebra I

The last consequence of teaching algebra in middle school that we trace is a negative one: repeating the first algebra course. We have seen that one consequence of the practice is lower performance on the Algebra I end-of-course test. Although taking the course in middle school does indeed boost the chance of passing it eventually, for many students passing the course requires taking it more than once. Here we look directly at repeating as an outcome of the push to teach algebra in middle school.

[13] A stronger case can be made for the enabling effect of accelerating algebra on the opportunity to take pre-calculus courses. Comparisons similar to that shown in Figure 6 between students who did and did not take Algebra I in 8th grade indicate larger differences in the share of students who took at least one pre-calculus course, including analytical geometry and courses entitled "pre-calculus." Compared to those in the previous graph, these lines are farther apart, suggesting an upper bound net enabling effect amounting to almost 20 percentage points at the middle of the prior achievement distribution and more than 40 percentage points at the 90th percentile. Although the comparisons shown in Figure 6 suggest that moving algebra to 8th grade is unlikely to increase the share of students who take calculus in high school, except for the top students, there does appear to be some real scope for an enabling effect to operate for pre-calculus courses. The pre-calculus courses included in these latter comparisons were math course numbered 2031(Analytical Geometry), 2070 (Pre-Calculus), and 2071 (IB Math Methods I). The findings are quite similar if we define pre-calculus more broadly, in include in addition math course numbered 2041 (Trigonometry), 2054 (Integrated Math IV), 2065 (Probability and Statistics), 2066 (AP Statistics, 2070), and 2078 (Math HL I IB).

In this case, we expect a negative bias in simple estimates. Early algebra takers are positively selected and should therefore be less disposed to negative outcomes such as retaking. Any simple comparison will therefore likely understate the effect of acceleration on the propensity to retake the course.

Using axes like the previous two figures, Figure 7 shows the percentage of students, by prior achievement level, who had to repeat Algebra I. As expected, this share tends to be smaller for students with higher prior math achievement. That said, the students who took Algebra I by 8th grade were more likely to repeat the course. For students at the 20th percentile score, for example, the early algebra takers were more than 20 percentage points more likely to repeat the course. Given the expected bias in these estimates, for roughly the bottom half of the distribution, the effect of taking Algebra I by 8th grade is *at least* a 20 percentage point increase in the risk of repeating the course. For the median student, and all those below, acceleration appears to be more likely to lead to course repetition than to calculus.

VII. Conclusion

This paper examines a widely-espoused policy in math education: getting more students to take algebra in 8th grade, rather than waiting until 9th grade, when most students have traditionally taken the first course in algebra. Offering algebra early allows students more time in high school to take advanced math courses, but critics complain that most students are not ready for algebra in 8th grade. In a prior paper we examined the effect of early algebra, using data mainly from the Charlotte-Mecklenberg Schools, a district that had pursued a well-documented policy to place more students into early algebra in the early 2000's. We found that some of the concerns raised by critics appear to be valid. In particular we found that students who were pushed to take algebra by 8th grade performed less well in subsequent math courses, especially

geometry, as they progressed through high school. That study is limited, however, because the effects we obtained apply only to those students actually affected by the Charlotte-Mecklenburg policy shift. To see how generalizable these estimates are, in the current paper we extend the analysis to the 10 largest North Carolina school districts. This expansion allows us to enrich the analysis and explore a wider range of effects. In particular we are able to estimate effects for high-performing students – who were not affected by the Charlotte-Mecklenburg policy shift – and to examine heterogeneous impacts by the student's race, and by the income and education status of the student's parents.

Following the same basic methodology as in the earlier paper, we identify the effects of accelerating algebra by using data on multiple cohorts of students disaggregated by their prior math achievement and exploiting the fact that shifts in the timing of algebra occur at different times in different districts to different deciles of students. Our aim is to assess the effects of variations in school policies and practices that manifest themselves in differing rates with which otherwise similar students are assigned to take Algebra I in middle school. We examine three kinds of possible effects on students: on their performance on the state's mandatory test administered to all Algebra I students; on how well they succeed in passing Algebra I, Geometry and Algebra II; and on whether they took courses beyond Algebra II, including calculus.

Our estimation strategy is designed to yield estimates that reflect causation, not simply correlation. Since assignment of any student to take algebra in 7^{th} or 8^{th} grade is likely to depend on numerous student-level factors, any attempt to assess the effect of early assignment to algebra based on observational, within-cohort-and-district variation will inevitably be subject to omitted variable, or selection, bias. We therefore use instrumental variables estimation. The large number of districts used in this study means that a wider range of students were subjected to shifts in

algebra placement policy over time. As in our previous study, we use this variation to assess the impact of algebra timing on student outcomes. We infer that a policy shift has occurred in those circumstances where the across-cohort variation in Algebra I placement patterns is too large to be based on random fluctuations in student background characteristics alone. We use data on students from six successive cohorts in the 10 districts, stratifying the sample by students' prior achievement, as measured by each student's average scores on 6th and 7th grade standardized math tests. We then reduce the sample to those district and prior achievement decile cells that exhibit significant variation in placement patterns across cohorts.

As in our previous paper, we find that the overall effect of taking algebra by 8th grade is to increase the probability that students will pass Algebra I by 10th grade, but to depress their performance on the Algebra I test and decrease the likelihood they will pass Geometry by 11th grade. In addition, we find significant heterogeneity in effects. The most important form of heterogeneity we observe is based on prior achievement. We find that the harmful effects are almost entirely confined to students in the bottom 60% of the prior achievement distribution, lending support to the argument of critics that only the best prepared students are ready to take algebra in 8th grade. Other sources of heterogeneity are less predictable. As might be expected, girls do better on our math outcomes, as do those not receiving free lunch and those with college educated parents. But the interactions with early algebra were anything but expected. Boys were harmed less by taking algebra early. Students on free lunch, while being harmed more by early algebra on Algebra I test scores, actually were harmed less by other measures. And for students with college-educated parents the effect of early algebra had varying effects but in general these differences did little to affect their overall superior performance.

For two other outcomes, we are not able to use the instrumental variables approach,

because of limited data. We therefore offer estimates in the spirit of a "bounds" analysis. The first outcome is taking calculus and other advanced math courses. This outcome is important because a prime argument for moving algebra to 8th grade is that it opens up for students the opportunity to take more advanced coursework in mathematics during high school. For calculus taking, we compared students who did and did not take algebra I by 8th grade. The differences reflect the course and selection. Our bounds analysis suggests that there can be little effect in the bottom half of the prior achievement distribution because so few students take calculus, whether or not they took algebra in 8th grade. It is only in the top fifth that the differences are as great as 20 percentage points. As for repeating Algebra I, all of the action is at the bottom of the prior achievement scale, the potential effects are quite large. For those students at or near the bottom, taking Algebra I early increases the likelihood of re-taking the course by at least 20 percentage points.

As is the case with our previous study of accelerated algebra, it is important to end with a caveat emphasizing at least one conclusion that cannot be drawn from our work. The present paper, like our previous study focusing on Charlotte-Mecklenberg, addresses a policy of changing the timing of the conventional first course in algebra. We ask whether it was a good idea to take the existing Algebra I course and increase the number of 8th graders taking it. We cannot address the effects of proposals that would take concepts from algebra and introduce them to students in earlier grades to an extent not previously done, that is, a thorough-going reform of the mathematics curriculum. Regarding the desirability of such a reform, our research is silent.

References

Clotfelter, Charles T., Helen F. Ladd, and Jacob L. Vigdor (2012).”The Aftermath of Accelerating Algebra: Evidence from a District Policy Initiative.” NBER Working Paper18161, June.

Dulaney, Chuck, “Should Students Take Algebra in Middle School?” (1996) *Eyes on Evaluation,* Evaluation and Research Department, Wake County Public School System, April; http://www.wcpss.net/evaluation-research/reports/earlier_years/9608students_algebra_midsc.pdf, 6/17/12.

Moses, Bob. (1995). “Algebra, The New Civil Right,” in Carole Lacampagne, William Blair, and Jim Kaput (eds.) *The Algebra Initiative Colloquium: Papers Presented at a Conference on Reform in Algebra*. Washington: U.S. Department of Education, Office of Educational Research and Improvement, May.

Loveless, Tom. (2008) *The Misplaced Math Student: Lost in Eighth-Grade Algebra*. Washington: Brookings Institution Brown Center Report on American Education, September.

National Mathematics Advisory Panel. (2008). *Foundations for Success: The Final Report of the National Mathematics Advisory Panel.* U.S. Department of Education. Washington, DC.

Pope, Devin G., and Justin R. Sydnor. 2010. "Geographic Variation in the Gender Differences in Test Scores." *Journal of Economic Perspectives*, 24(2): 95–108.

Rampey, Bobby D., Gloria S. Dion, and Patricia L. Donahue. 2009. *The Nation's Report Card: Trends in Academic Progress in Reading and Mathematics 2008*, April. http://nces.ed.gov/nationsreportcard/pubs/main2008/2009479.asp#section5, 6/17/12.

Schmidt, W.H. (2004). “A Vision for Mathematics.” *Educational Leadership, 61*(5): 6–11.

Schoenfeld, Alan. (1995) “Report of Working Group 1.” in Carole Lacampagne, William Blair, and Jim Kaput (eds.) *The Algebra Initiative Colloquium: Papers Presented at a Conference on Reform in Algebra*. Washington: U.S. Department of Education, Office of Educational Research and Improvement, May.

Walston, J. and J.C. McCarroll (2010) “Eighth Grade Algebra: Findings from the Eighth-Grade Round of the Early Childhood Longitudinal Study, Kindergarten Class of 1998-99 (ECLS-K).”

National Center for Education Statistics Publication 2010-016.

List of Figures and Tables

Figures

1.Map of NC with districts shown
Bar charts before exclusions, demonstrating differences
2.CMS
3.Wake
4.Forsyth
5.Durham
6.Taking Calculus: Algebra by 8th versus Algebra after 8th
7.Repeating Algebra I: Algebra by 8th , Algebra in 9th, and Algebra After 9th

Tables

1.Sample Sizes and Selected Summary Statistics, After Trimming
2a, 2b Correlates of Math Success: OLS Estimates (a=districts; b=deciles)
3.IV Estimates of the Impact of Acceleration into Algebra I by 8th Grade
4.Quitile Interaction Effects
5.Gender Interaction Effects
6.Free/Reduced Price Lunch Interaction Effects
7.Parental Education Interaction

Appendix Tables

A1. Chi-squared Tests for Variation in Risk for Algebra I
A2. *F*-tests for Unexplained Variation in Risk for Algebra I
A3. Decile-district Subsamples Excluded
A4. Sample Sizes and Selected Summary Statistics, Before Trimming

Figure 1. North Carolina Ten Largest Districts

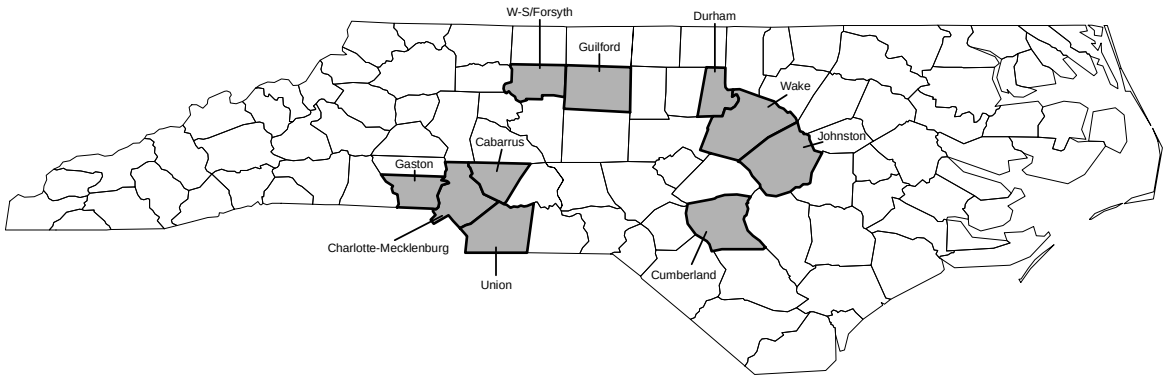

Figure 2. Probability of Taking Algebra by 8th Grade, Charlotte-Mecklenburg

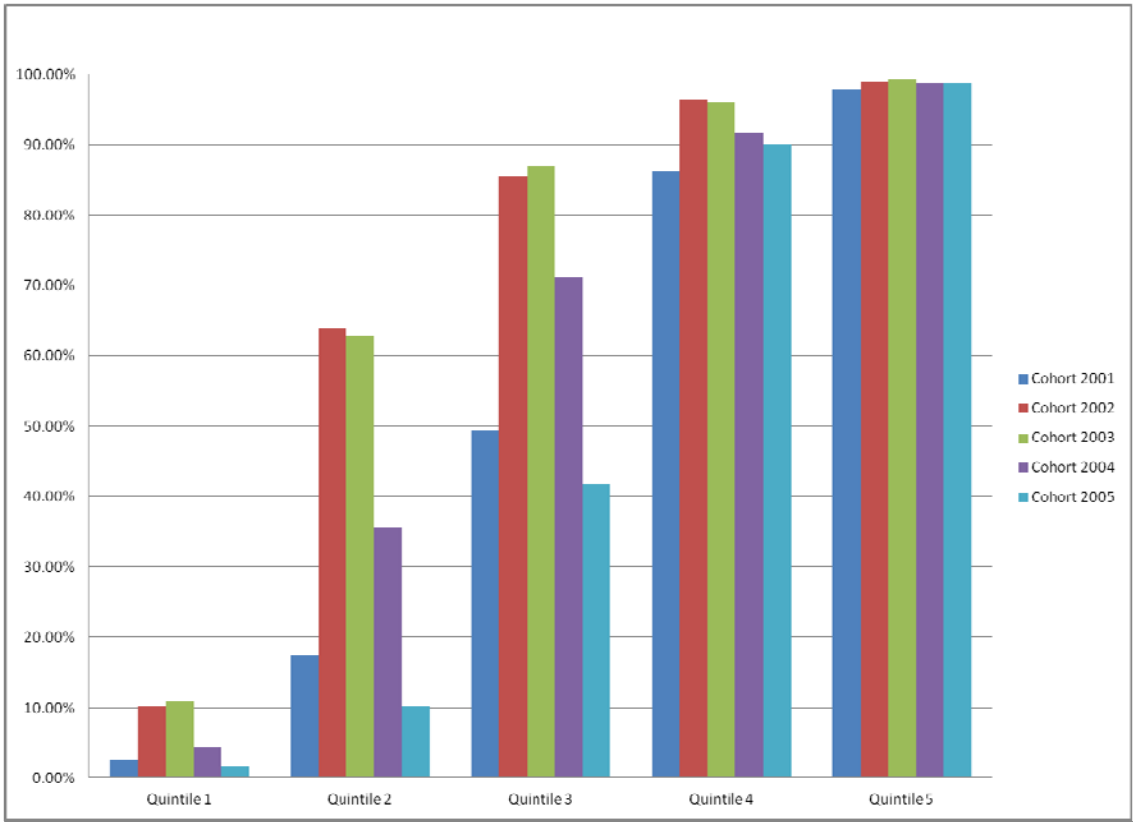

Figure 3. Probability of Taking Algebra by 8th Grade, Wake

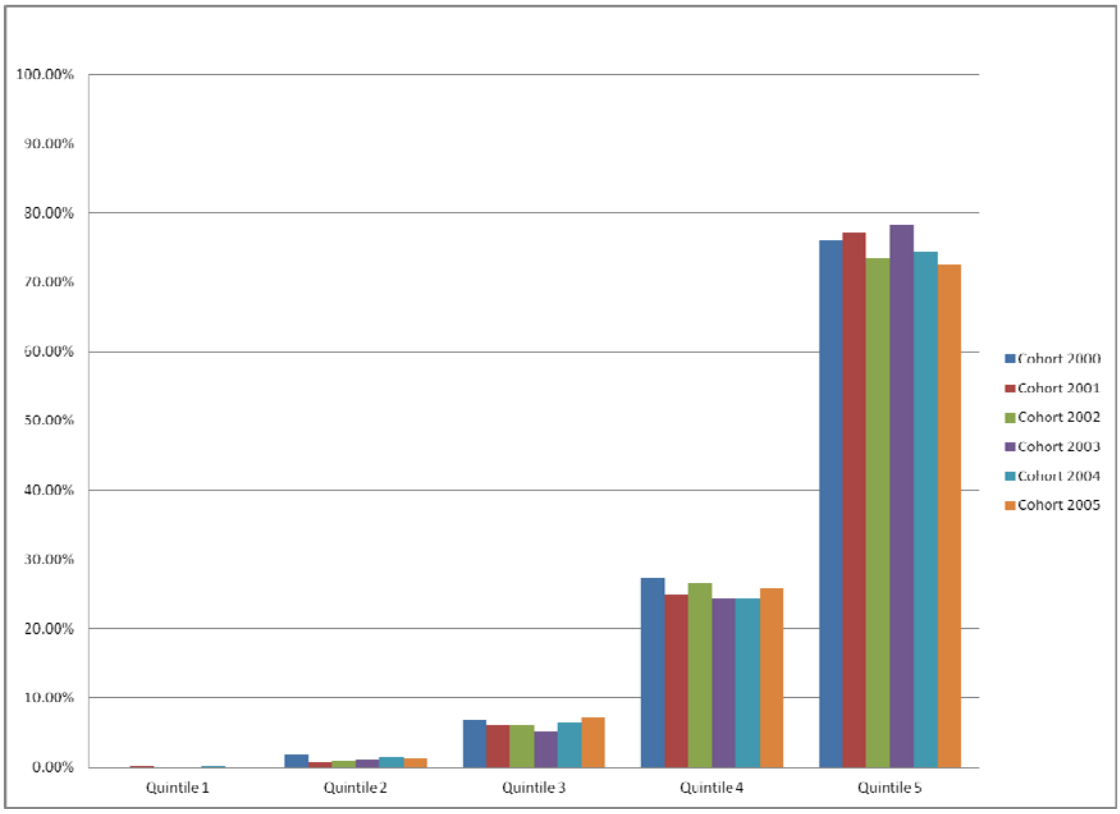

Figure 4. Probability of Taking Algebra by 8th Grade, Forsyth

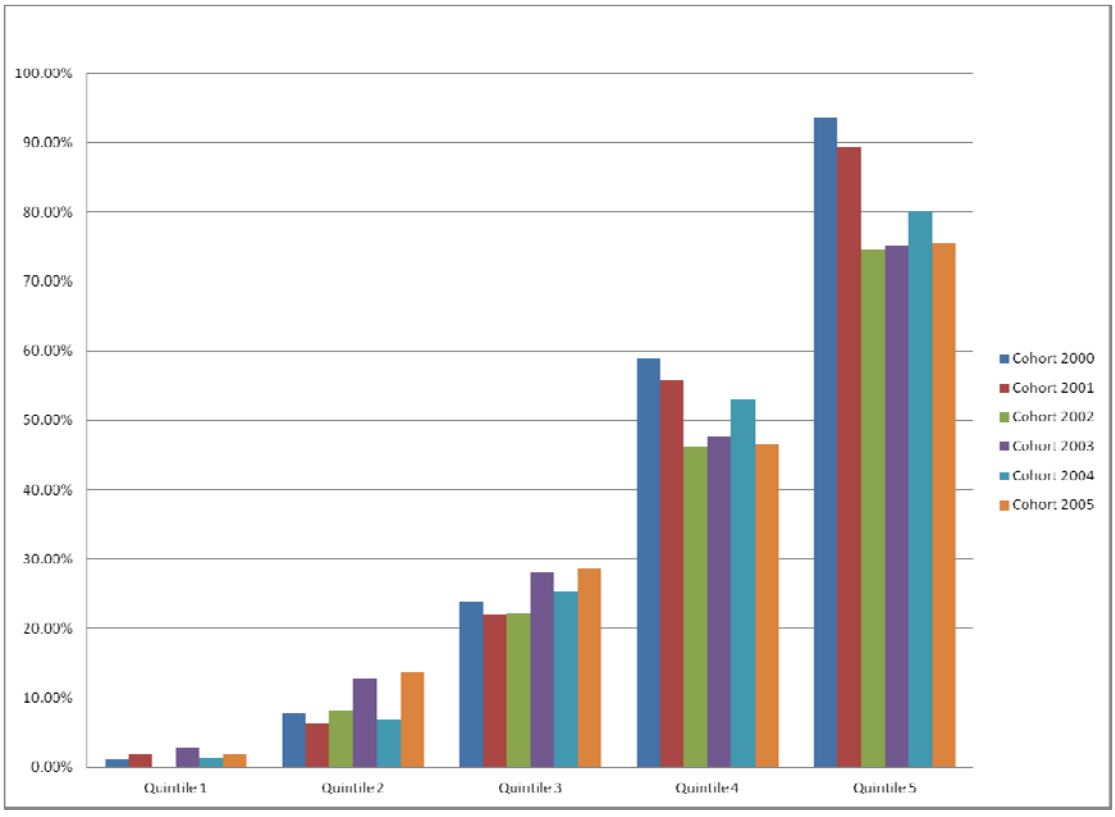

Figure 5. Probability of Taking Algebra by 8th Grade, Durham

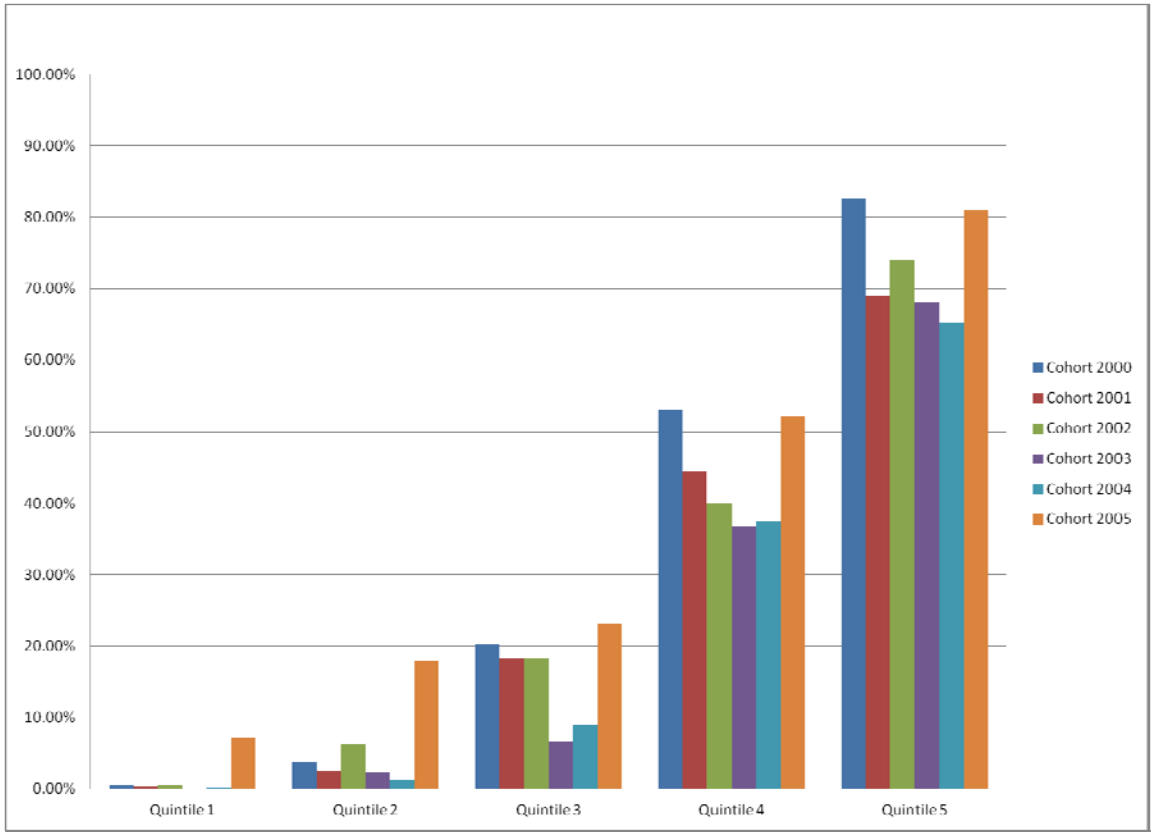

Figure 6. Taking Calculus: Algebra by 8th versus Algebra after 8th

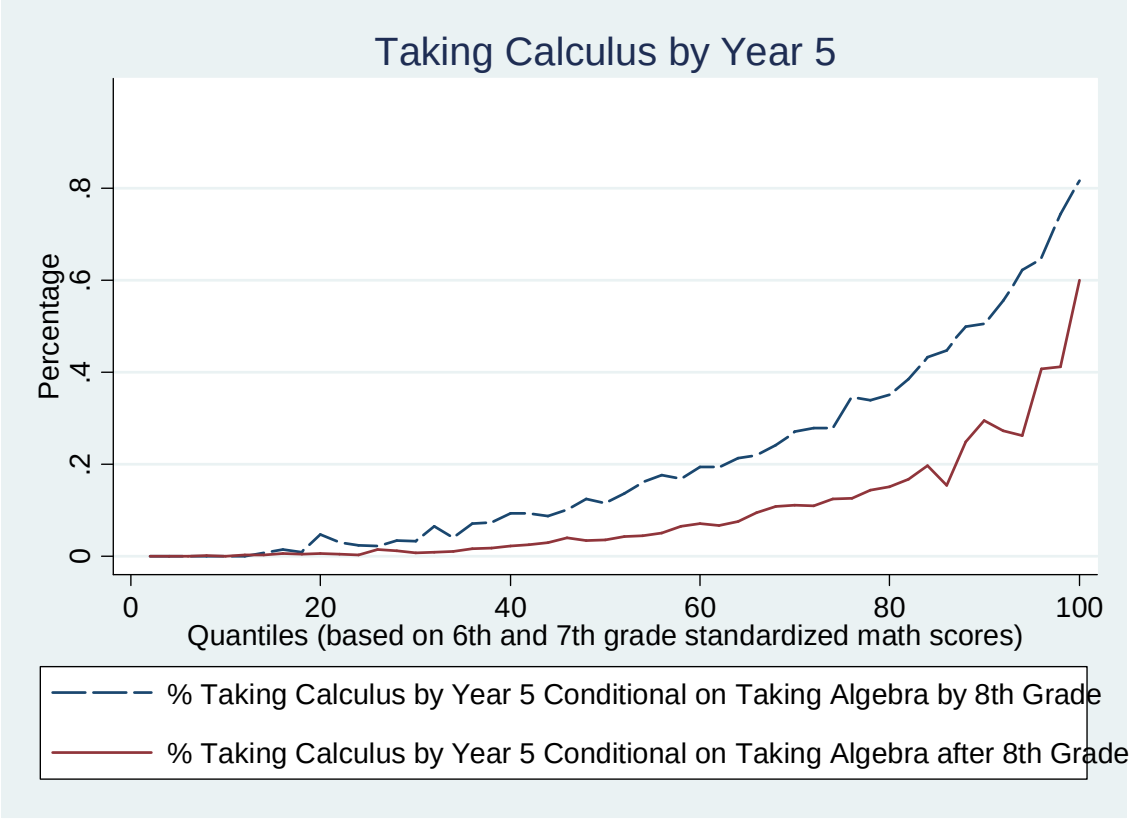

Figure 7. Repeating Algebra I: Algebra by 8th , Algebra in 9th, and Algebra After 9th

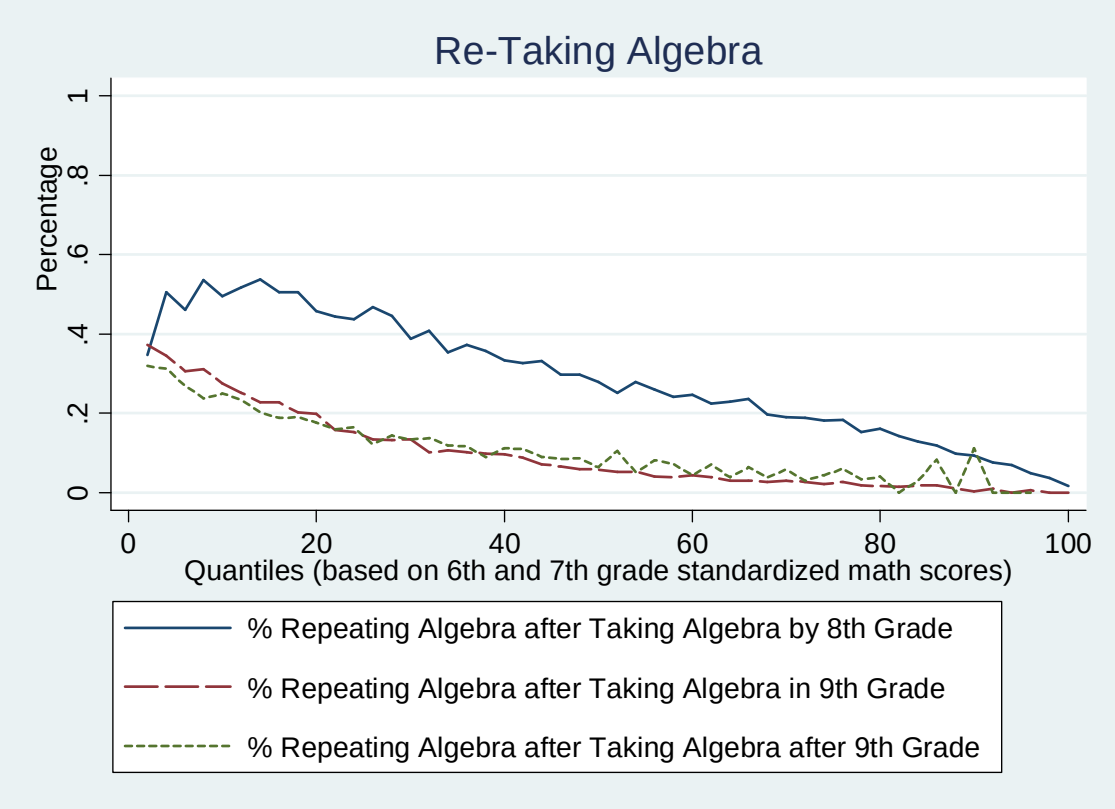

Table 1. Samples Sizes and Selected Summary Statistics, After Trimming

School Districts	Largest City	District Enrollment	Average 6th and 7th Grade EOG Math Scores	Algebra I Test Scores	Pass Algebra I by 10th Grade	Pass Geometry by 11th Grade	Pass Algebra II by 12th Grade	N (Sample)
Wake	Raleigh	141,194	1.052 (0.711)	1.013 (0.828)	93.1	84.5	84.0	21,367
CMS	Charlotte	134,121	-0.306 (0.900)	-0.408 (0.932)	57.1	35.1	41.3	24,512
Guilford	Greensboro	71,079	0.021 (0.994)	-0.224 (1.029)	73.2	48.3	55.0	25,691
Cumberland	Fayetteville	53,264	0.051 (0.819)	-0.026 (0.899)	66.0	44.0	47.4	14,697
Forsyth	Winston Salem	51,526	0.395 (1.019)	0.234 (1.056)	74.9	57.4	59.8	10,798
Union	Monroe	39,200	0.912 (0.744)	0.765 (0.807)	93.6	80.9	80.1	4,878
Johnston	Smithfield	32,063	0.067 (0.721)	0.067 (0.721)	70.9	39.3	42.5	3,928
Durham	Durham	31,867	-0.210 (1.005)	-0.259 (0.965)	56.3	33.5	46.7	8,688
Gaston	Gastonia	32,169	0.700 (0.781)	0.500 (0.850)	86.4	60.6	60.7	5,322
Cabarrus	Concord	28,127	1.170 (0.470)	1.072 (0.686)	97.6	93.1	90.4	3,107

Note: The sample in each district covers the decile groups not excluded by tests of variability across cohorts. Each is restricted to students observed in the district during the year after their first year in 7th grade that can be assigned to a decile based on 6th and 7th grade math test scores.. The district enrollment totals were obtained from http://www.ncpublicschools.org/fbs/accounting/data/ and are shown for the school year 2009-2010. Means and standard deviations are reported for test scores, sample proportions for all other variables. All test scores have been standardized.

Table 2: Correlates of Math Success Measures: OLS Estimates

Independent variable	Algebra I Test Scores	Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Enrolled in Algebra I by 8th Grade	-0.0850***	0.137***	0.0519***	0.0866***
	(0.021)	(0.012)	(0.006)	(0.006)
Year entered 7th grade (2000 omitted)				
2001	0.0505	0.0232	0.0154	0.0262*
	(0.027)	(0.013)	(0.012)	(0.012)
2002	0.0300	0.0113	0.00727	0.0148*
	(0.027)	(0.008)	(0.011)	(0.006)
2003	-0.00155	0.0254*	0.00990	0.00274
	(0.025)	(0.010)	(0.012)	(0.006)
2004	-0.00410	0.00177	0.00223	0.00489
	(0.025)	(0.008)	(0.013)	(0.008)
2005	0.0758*	-0.0544**	0.0140	0.00432
	(0.029)	(0.020)	(0.012)	(0.008)
School District (Wake omitted)				
Mecklenburg	-0.344***	-0.0539***	-0.0605***	-0.0680***
	(0.024)	(0.009)	(0.012)	(0.009)
Guilford	-0.374***	-0.00202	-0.0372**	-0.032**
	(0.021)	(0.008)	(0.012)	(0.010)
Cumberland	-0.211***	-0.0387***	-0.0495***	-0.072***
	(0.017)	(0.009)	(0.010)	(0.009)
Forsyth	-0.264***	-0.0159	-0.0338**	-0.048***
	(0.019)	(0.009)	(0.011)	(0.010)
Union	-0.158***	0.0315***	0.012	0.00498
	(0.023)	(0.007)	(0.007)	(0.007)
Johnston	-0.0314	-0.0186	-0.091***	-0.120***
	(0.031)	(0.019)	(0.020)	(0.017)
Durham	-0.316***	-0.0516***	-0.075***	-0.0137
	(0.018)	(0.009)	(0.015)	(0.010)
Gaston	-0.230***	-0.00696	-0.108***	-0.131***
	(0.017)	(0.012)	(0.022)	(0.021)
Cabarrus	-0.0692***	0.0333***	0.0226**	0.0216**
	(0.014)	(0.007)	(0.007)	(0.007)
Average of 6th and 7th grade math test score decile (lowest omitted)				
Second lowest	0.266***	0.157***	0.0330***	0.0856***
	(0.030)	(0.022)	(0.003)	(0.008)

Third lowest	0.495*** (0.031)	0.319*** (0.027)	0.104*** (0.006)	0.177*** (0.008)
Fourth lowest	0.709*** (0.032)	0.458*** (0.023)	0.200*** (0.011)	0.280*** (0.014)
Fifth lowest	0.954*** (0.028)	0.590*** (0.018)	0.364*** (0.009)	0.403*** (0.007)
Sixth lowest	1.129*** (0.043)	0.646*** (0.018)	0.493*** (0.022)	0.499*** (0.016)
Seventh lowest	1.416*** (0.044)	0.711*** (0.018)	0.656*** (0.011)	0.600*** (0.015)
Eighth lowest	1.717*** (0.033)	0.741*** (0.021)	0.776*** (0.008)	0.699*** (0.008)
Ninth lowest	2.084*** (0.033)	0.735*** (0.022)	0.837*** (0.009)	0.744*** (0.009)
Highest	2.683*** (0.033)	0.695*** (0.021)	0.837*** (0.014)	0.749*** (0.014)
N	113738	124505	124505	124505
Adjusted R^2	0.720	0.485	0.571	0.442

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage for Algebra I and Algebra II is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution. Course passage for Geometry is defined as obtaining an achievement level at or above 3 on the test. Grade-retained students are kept with their original cohort. District fixed effects exist but are not shown in this table.
*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 3: Instrumental Variable Estimates of the Impact of Acceleration into Algebra I in 8th Grade

	Algebra I Test Score	Pass Algebra I by 10th grade		Pass Geometry by 11th grade		Pass Algebra II by 12th grade	
Independent variable	IVQR w/imputation	2SLS	BP	2SLS	BP	2SLS	BP
Enrolled in Algebra I by 8th Grade	-0.374***	0.091***	0.400***	-0.066***	-0.152**	-0.026*	-0.038
	(0.006)	(0.021)	(0.086)	(0.014)	(0.056)	(0.012)	(0.046)
N	113,738	124,505	124,505	124,505	124,505	124,505	124,505
Adjusted R^2		0.484		0.564		0.436	

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage for Algebra I and Algebra II is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution. Course passage for Geometry is defined as obtaining an achievement level at or above 3 on the test. Grade-retained students are kept with their original cohort. All models control for average 6th and 7th grade math test score decile, cohort and district fixed effects, and instrument for Algebra I enrollment by 8th grade using an indicator representing the probability of taking Algebra I by 8th grade within your decile-cohort-district cell. Columns headed "2SLS" are estimated by two-stage least squares; columns headed "BP" are estimated by bivariate probit. Column headed "IVQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 10,767 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method.
*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 4a: Quintile Interaction Effects of the Impact of Acceleration into Algebra I in 8th Grade

	Algebra I Test Score		Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Independent variable	OLS	QR w/imputation	OLS	OLS	OLS
Quintile 1 Student * Enrolled in Algebra I by 8th Grade	-0.183***	0.028***	0.192***	0.0202*	0.0935***
	(0.035)	(0.002)	(0.018)	(0.009)	(0.008)
Quintile 2 Student * Enrolled in Algebra I by 8th Grade	-0.197***	-0.156***	0.140***	0.016	0.089***
	(0.036)	(0.001)	(0.014)	(0.011)	(0.015)
Quintile 3 Student * Enrolled in Algebra I by 8th Grade	-0.185***	-0.160***	0.091***	0.0249	0.108***
	(0.031)	(0.001)	(0.012)	(0.015)	(0.013)
Quintile 4 Student * Enrolled in Algebra I by 8th Grade	-0.098**	-0.046***	0.075***	0.0767***	0.0903***
	(0.030)	(0.001)	(0.006)	(0.007)	(0.009)
Quintile 5 Student * Enrolled in Algebra I by 8th Grade	0.058	0.136***	0.130***	0.0736***	0.0721***
	(0.037)	(0.001)	(0.027)	(0.007)	(0.006)
N	113,738	124,505	124,505	124,505	124,505
Adjusted R^2	0.721	0.376	0.425	0.572	0.443

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage for Algebra I and Algebra II is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution. Course passage for Geometry is defined as obtaining an achievement level at or above 3 on the test. Grade-retained students are kept with their original cohort. All models control for average 6th and 7th grade math test score decile, cohort and district fixed effects. The main effect is divided into five interaction effects by quintile. Column headed "QR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 10,767 non-Algebra I-takers and estimating using the quantile regression method.

*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 4b: Instrumented Quintile Interaction Effects of the Impact of Acceleration into Algebra I in 8th Grade

	Algebra I Test Score		Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Independent variable	2SLS	RFQR w/imputation	2SLS	2SLS	2SLS
Quintile 1 Student * Enrolled in Algebra I by 8th Grade	-0.479***	-0.240***	0.221***	-0.108***	-0.0627
	(0.080)	(0.015)	(0.037)	(0.031)	(0.048)
Quintile 2 Student * Enrolled in Algebra I by 8th Grade	-0.456***	-0.397***	0.0921***	-0.081***	-0.0401*
	(0.034)	(0.006)	(0.022)	(0.012)	(0.016)
Quintile 3 Student * Enrolled in Algebra I by 8th Grade	-0.429***	-0.398***	0.0356*	-0.085***	-0.0174
	(0.034)	(0.006)	(0.015)	(0.018)	(0.020)
Quintile 4 Student * Enrolled in Algebra I by 8th Grade	-0.324***	-0.260***	0.0462**	-0.0129	-0.015
	(0.048)	(0.006)	(0.014)	(0.018)	(0.017)
Quintile 5 Student * Enrolled in Algebra I by 8th Grade	-0.306***	-0.140***	0.096***	0.0687**	0.011
	(0.092)	(0.010)	(0.022)	(0.026)	(0.023)
N	113738	124505	124505	124505	124505
Adjusted R^2	0.712	0.376	0.424	0.568	0.436

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage for Algebra I and Algebra II is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution. Course passage for Geometry is defined as obtaining an achievement level at or above 3 on the test. Grade-retained students are kept with their original cohort. All models control for average 6th and 7th grade math test score decile, cohort and district fixed effects, and instrument for Algebra I enrollment by 8th grade using an indicator representing the probability of taking Algebra I by 8th grade within your decile-cohort-district cell. Columns headed "2SLS" are estimated by two-stage least squares. Column headed "RFQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 10,767 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method.

*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 5: Instrumented Gender Interaction Effects of the Impact of Acceleration into Algebra I in 8th Grade

	Algebra I Test Score		Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Independent variable	2SLS	RFQR w/imputation	2SLS	2SLS	2SLS
Enrolled in Algebra I by 8th Grade	-0.416***	-0.398***	0.0695**	-0.0678***	-0.0447***
	(0.028)	(0.011)	(0.022)	(0.014)	(0.013)
Male	-0.064***	-0.118***	-0.051***	-0.0159***	-0.068***
	(0.009)	(0.005)	(0.006)	(0.004)	(0.007)
Enrolled in Algebra I by 8th Grade * Male	0.014	0.070***	0.045***	0.0042	0.0379***
	(0.021)	(0.008)	(0.007)	(0.006)	(0.011)
N	113738	124505	124505	124505	124505
Adjusted R^2	0.710	0.377	0.485	0.564	0.439

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage for Algebra I and Algebra II is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution. Course passage for Geometry is defined as obtaining an achievement level at or above 3 on the test. Grade-retained students are kept with their original cohort. All models control for average 6th and 7th grade math test score decile, cohort and district fixed effects, and instrument for Algebra I enrollment by 8th grade using an indicator representing the probability of taking Algebra I by 8th grade within your decile-cohort-district cell. Columns headed "2SLS" are estimated by two-stage least squares. Column headed "RFQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 10,767 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method.

*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 6: Instrumented Free/Reduced Lunch Interaction Effects of the Impact of Acceleration into Algebra I in 8th Grade

	Algebra I Test Score		Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Independent variable	2SLS	RFQR w/imputation	2SLS	2SLS	2SLS
Enrolled in Algebra I by 8th Grade	-0.330***	-0.315***	0.0467*	-0.0765***	-0.0534***
	(0.034)	(0.012)	(0.019)	(0.018)	(0.015)
On Free/Reduced Lunch	-0.0752***	-0.141***	-0.0835***	-0.0831***	-0.103***
	(0.011)	(0.006)	(0.007)	(0.010)	(0.010)
Enrolled in Algebra I by 8th Grade * On Free/Reduced Lunch	-0.124***	-0.039***	0.0884***	0.0308*	0.0621***
	(0.020)	(0.010)	(0.010)	(0.014)	(0.013)
N	113738	124505	124505	124505	124505
Adjusted R^2	0.714	0.378	0.485	0.567	0.439

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage for Algebra I and Algebra II is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution. Course passage for Geometry is defined as obtaining an achievement level at or above 3 on the test. Grade-retained students are kept with their original cohort. Free/Reduced Lunch is defined as having ever been observed as receiving a free or reduced-price lunch during the students' enrollment in NC. All models control for average 6th and 7th grade math test score decile, cohort and district fixed effects, and instrument for Algebra I enrollment by 8th grade using an indicator representing the probability of taking Algebra I by 8th grade within your decile-cohort-district cell. Columns headed "2SLS" are estimated by two-stage least squares. Column headed "RFQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 10,767 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method.
*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 7: Instrumented Parent Education Interaction Effects of the Impact of Acceleration into Algebra I in 8th Grade

	Algebra I Test Score		Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Independent variable	2SLS	RFQR w/imputation	2SLS	2SLS	2SLS
Enrolled in Algebra I by 8th Grade	-0.547***	-0.358***	0.128***	-0.0955***	-0.0375*
	(0.030)	(0.010)	(0.022)	(0.014)	(0.016)
Parent with College Degree or More	0.021*	0.159***	0.077***	0.0534***	0.0869***
	(0.009)	(0.004)	(0.006)	(0.008)	(0.009)
Enrolled in Algebra I by 8th Grade * Parent with College Degree or More	0.202***	0.028**	-0.0512***	0.0456***	0.020
	(0.024)	(0.008)	(0.010)	(0.012)	(0.015)
N	113737	124504	124504	124504	124504
Adjusted R^2	0.712	0.378	0.487	0.567	0.441

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage for Algebra I and Algebra II is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution. Course passage for Geometry is defined as obtaining an achievement level at or above 3 on the test. Grade-retained students are kept with their original cohort. Parental education is defined as the highest level of education achievement by the more educated parent during the period in which the student was observed. Completion of a four year college or graduate degree is necessary to be included in the category "Parent with College Degree or More". Community College or Trade School does not qualify. All models control for average 6th and 7th grade math test score decile, cohort and district fixed effects, and instrument for Algebra I enrollment by 8th grade using an indicator representing the probability of taking Algebra I by 8th grade within your decile-cohort-district cell. Columns headed "2SLS" are estimated by two-stage least squares. Column headed "RFQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 10,767 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method.
*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

A1. Chi-squared Tests for Variation in Risk for Algebra I

	Bottom Decile	Decile 2	Decile 3	Decile 4	Decile 5	Decile 6	Decile 7	Decile 8	Decile 9	Top Decile
Wake	**0.339**	**0.547**	**0.857**	**0.154**	0.048	**0.141**	**0.788**	0.044	0.017	0.000
CMS	0.028	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	**0.744**
Guilford	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Cumberland	**0.261**	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.000
Forsyth	**0.634**	0.004	0.000	**0.057**	**0.727**	0.030	**0.063**	0.000	0.000	0.000
Union	**N/A**	**N/A**	**0.465**	**0.683**	0.024	**0.158**	0.003	0.006	0.003	0.000
Johnston	**N/A**	**0.070**	0.039	0.007	**0.308**	0.003	**0.281**	0.004	**0.069**	**0.148**
Durham	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.019	0.000	0.000
Gaston	**0.565**	**0.664**	**0.487**	0.045	0.050	0.025	**0.067**	**0.249**	0.000	0.002
Cabarrus	**N/A**	**N/A**	**N/A**	**N/A**	**0.640**	**0.755**	**0.424**	0.003	0.000	0.035

A2. F-tests for Unexplained Variation in Risk for Algebra I

	Bottom Decile	Decile 2	Decile 3	Decile 4	Decile 5	Decile 6	Decile 7	Decile 8	Decile 9	Top Decile
Wake	0.001	0.000	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000
CMS	0.000	0.000	0.001	0.000	0.000	**0.084**	**0.078**	0.000	0.000	0.000
Guilford	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Cumberland	0.000	0.000	0.000	0.000	0.001	**0.449**	0.000	0.004	0.003	0.000
Forsyth	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Union	**0.314**	0.001	0.000	0.000	0.000	**0.116**	0.000	**0.242**	0.000	0.000
Johnston	0.000	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Durham	0.000	0.000	0.000	0.000	0.000	**0.175**	0.000	0.000	0.000	0.000
Gaston	0.000	0.000	0.000	**0.634**	0.000	0.004	0.000	0.004	0.000	0.000
Cabarrus	0.000	0.000	0.000	0.000	**0.299**	0.000	0.000	0.000	0.000	0.000

A3. Decile-district Subsamples Excluded

	Bottom Decile	Decile 2	Decile 3	Decile 4	Decile 5	Decile 6	Decile 7	Decile 8	Decile 9	Top Decile
Wake	X	X	X	X		X	X			
CMS						X	X			X
Guilford										
Cumberland	X					X				
Forsyth	X			X	X		X			
Union	X	X	X	X		X		X		
Johnston	X	X			X		X		X	X
Durham						X				
Gaston	X	X	X	X			X	X		
Cabarrus	X	X	X	X	X	X	X			

A4. Sample Sizes and Selected Summary Statistics, Before

School Districts	Largest City	District Enrollment	Average 6th and 7th Grade	Algebra I Test Scores	Pass Algebra I by 10th Grade	Pass Geometry	Pass Algebra II by 12th	N (Sample)

			EOG Math Scores			by 11th Grade	Grade	
Wake	Raleigh	141,194	0.369 (0.989)	0.514 (0.984)	78.6	63.0	65.1	40,978
CMS	Charlotte	134,121	0.067 (1.046)	-0.090 (1.053)	68.2	47.2	52.2	35,117
Guilford	Greensboro	71,079	0.021 (0.994)	-0.224 (1.029)	73.2	48.3	55.0	25,691
Cumberland	Fayetteville	53,264	-0.138 (0.894)	-0.143 (0.919)	60.3	39.4	42.9	19,048
Forsyth	Winston Salem	51,526	0.056 (1.010)	-0.035 (1.026)	66.9	47.0	50.9	17,731
Union	Monroe	39,200	0.292 (0.951)	0.351 (0.893)	79.2	60.3	60.9	10,119
Johnston	Smithfield	32,063	0.336 (0.911)	0.336 (0.911)	72.8	46.8	49.7	9,267
Durham	Durham	31,867	-0.183 (0.968)	-0.246 (0.939)	58.4	34.5	48.1	11,114
Gaston	Gastonia	32,169	0.071 (0.937)	0.106 (0.894)	67.0	40.9	42.2	12,953
Cabarrus	Concord	28,127	0.223 (0.903)	0.342 (0.940)	78.4	59.1	58.5	8,573

Note: In each district, sample is restricted to students observed in the district during year 1 that can be assigned to a decile based on 6th and 7th grade math test scores. The district enrollment data has been obtained from http://www.ncpublicschools.org/fbs/accounting/data/ and is shown for the school year 2009-2010. Mean and standard deviation reported for test scores, sample proportion for all other variables. All test scores have been standardized.