The Aftermath of Accelerating Algebra:
Evidence from District Policy Initiatives

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September 6, 2013

Abstract

The proportion of students taking a first algebra course in middle school has doubled over the past generation and there have been calls to make eighth grade algebra universal. We use significant policy shifts in the timing of algebra in two large North Carolina districts to infer the impact of accelerated entry into algebra on student performance in math courses as students progress through high school. Students affected by this acceleration initiative scored significantly lower on end-of-course tests in Algebra I, and in most specifications were either equally or less likely to pass higher-level courses on a college-preparatory timetable. Accelerating algebra to middle school appears benign or beneficial for higher-performing students, but unambiguously harmful to the lowest performers. We consider whether the effects reflect the reliance on less qualified teachers and conclude that this mechanism explains only a small fraction of the result.

• Corresponding author: jacob.vigdor@duke.edu. We gratefully acknowledge the support of the Institute for Education Sciences and American Institutes for Research through the Center for the Analysis of Longitudinal Data in Education Research. We thank seminar participants at Notre Dame, the APPAM annual meeting, the CALDER annual research conference, the Federal Reserve Bank of New York, the University of Illinois-Chicago, and the Association for Education Finance and Policy annual meeting as well as Dan Goldhaber, Nora Gordon, Henry Levin, and Gary Solon for helpful comments. Kyle Ott, Alexandra Oprea and Maria Laurito provided outstanding research assistance.

1. Introduction

In 2008, the California State Board of Education voted to require all students to enroll in algebra by 8th grade.[1] This policy initiative, yet to be actually implemented, represents the culmination of a decades-long movement toward offering algebra instruction before the traditional high school years.[2] Nationally, the proportion of 8th grade students enrolled in algebra doubled between 1988 and 2007 (Perie, Moran and Lutkus, 2005; Walston and McCarroll 2010), reaching rates over 50% in three states and the District of Columbia.[3] The movement to offer algebra instruction before high school has been inspired in large part by correlational research documenting significant differences in later-life outcomes between those students who enroll in algebra by 8th grade and those who do not.

Correlation need not imply causation, and it is unclear whether accelerated algebra enrollment – particularly when not accompanied by complementary curriculum reform in earlier grades – yields positive or negative effects (Loveless, 2008). This paper provides a quasi-experimental estimate of the causal impact of accelerating the introduction of algebra coursework. We analyze policy initiatives introduced in two large North Carolina school districts in 2002/03. These initiatives caused many students to take Algebra I earlier than they would have before the initiative, with the increase being especially abrupt for students in the middle deciles of the initial math achievement distribution. After maintaining the acceleration policy for two years, one district reversed course, reverting almost entirely to its previous

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[1] *Algebra Policy in California: Great Expectations and Serious Challenges,* EdSource, May 2009. http://www.noycefdn.org/documents/math/EdSourceReport0609.pdf, 6/19/13. Then-governor Arnold Schwarzenegger referred to algebra as "the key that unlocks the world of science, innovation, engineering, and technology." See "California to Require Algebra Taught in 8th Grade," *USA Today*, July 11, 2008. In the early 2000s, the state led the nation with 59% of all 8th grade students enrolled in Algebra (Loveless, 2008).

[2] See, for example, Usiskin (1987), who cites Japan's success in teaching algebra to 7th graders. In this paper, we use the term *algebra* to refer generically to a content area in mathematics and *Algebra I* to refer to the course traditionally taken at the beginning of a college-preparatory math sequence in North Carolina public schools. We similarly distinguish between Geometry courses and the content area known as geometry.

[3] In 2007, early algebra-taking rates exceeded 50% in California, Maryland, Utah, and the District of Columbia (Loveless, 2008).

placement pattern, whereas the other district stuck with its policy throughout the sample period.[4] We use the across-cohort variation in placement patterns created by these abrupt shifts in policy to infer the impact of acceleration, by comparing students with similar initial math achievement who were subjected to different placement policies solely on the basis of their year of birth. The analysis also incorporates, as an additional counterfactual, observations from eight additional large North Carolina school districts with no announced policy shift regarding the timing of Algebra I. Recognizing that policy changes are not always announced with fanfare, however, we estimate an alternative specification that uses "apparent" policy variation in these districts – shifts in course-taking patterns that are both uncorrelated with variation in observed student characteristics and too large to be explained by random variation – as an additional robustness check.

We assess the effect of accelerating students into Algebra I in 8th grade by looking at several kinds of outcomes. We examine whether acceleration increased the likelihood that students would stay on track to pass three college-preparatory math courses – Algebra I, Geometry, and Algebra II – within six years of beginning 7th grade. Students who do so also meet the North Carolina State Board of Education's minimum standards for a college-preparatory course of study.[5] We use standardized end-of-course tests designed by the state to assess performance in each course, rather than the grade assigned by the course instructor.[6] We also provide some basic descriptive evidence on the likelihood of progressing to a calculus

[4] As discussed below, the clear negative effects of acceleration may explain why the district reversed course.

[5] The State Board of Education permits a student to substitute a more advanced mathematics course – one using Algebra II as a prerequisite – for Geometry, or an alternative course sequence labeled Integrated Math I, II, and III in the state's official curriculum guide. In practice, the full Integrated Math sequence was not offered by any school in CMS during the period of study. Note also that admission to the 16-campus University of North Carolina system for most of the cohorts in our study required additional coursework beyond Algebra II. Thus completion of the three-course sequence was neither necessary nor sufficient for college admission. Nonetheless, failure to pass Algebra II effectively guaranteed that a student would not meet state standards for college-readiness.

[6] The state mandates that at least of the course grade in one of these courses be based on the end-of-course score. See GreatSchools, "Testing in North Carolina," http://www.greatschools.org/students/local-facts-resources/435-testing-in-NC.gs, 1/11/12.

course in high school, the traditional culmination of a college-preparatory math course sequence beginning with Algebra I in 8th grade, and on the likelihood of repeating Algebra I.

We find that acceleration initiatives lowered the Algebra I test scores of affected students. Moderately-performing students who were accelerated into Algebra I in 8th grade scored one-third of a standard deviation worse on the state end-of-course exam, compared to otherwise similar students in seventh-grade cohorts that were not subjected to acceleration.[7] Across numerous specifications, only one point estimate suggests a statistically significant positive effect on subsequent course performance; the vast majority of estimates are insignificant or significantly negative. Specifications examining apparent policy variation across the full set of 10 large North Carolina districts indicate more persistently negative effects for low-performing students, but benign or beneficial effects for high-performing students.

The impact of acceleration could conceivably vary over time; the transition to earlier algebra may introduce transition costs that mask more persistent benefits. We show, for example, that the credentials of Algebra I teachers declined in the year of acceleration. Nonetheless, we present a variety of evidence indicating that the deleterious impact of acceleration does not dissipate significantly over time.

Our findings directly contradict prior correlational research, thereby casting considerable doubt on the wisdom of teaching algebra to low-to-moderately performing students in 8th grade. Although it is undeniable that students who take algebra early tend to do better in subsequent math courses, this correlation arises because it is usually the strongest students who take algebra early. Once this selection bias is eliminated, we find that the remaining causal effect of accelerating the conventional first course of algebra into earlier grades, in the absence of other changes in the math curriculum, is never positive and in some cases significantly negative. We

[7] Clotfelter, Ladd and Vigdor (2012a) show similar sets of findings for high-performing students accelerated into 7th grade algebra, and lower-performing students accelerated into 9th grade algebra.

caution, however, that our results apply to the impact of changing the timing of the conventional first course in algebra, holding math instruction in the early grades constant. It is quite possible that more thoroughgoing reform of the math curriculum, by way of promoting readiness for algebra by 8th grade, could well prove beneficial.[8] Our results should also not be taken as evidence that math coursework in general has no value; to the contrary, Goodman (2012) shows that requiring additional math courses as high school graduation requirements – without specifying the rigor of those courses – yields tangible returns to students.

2. Origins of the Algebra Acceleration Movement

As suggested by the brief history sketched above, accelerating algebra instruction into middle school has been promoted as a strategy for improving the mathematics achievement and college-readiness of American high school students. Nationwide, the proportion of 13-year-olds enrolled in algebra courses has increased markedly, rising from 16% in 1988 to 29% in 2004 (Perie, Moran, and Lutkus, 2005). Among students in the nationally representative Early Childhood Longitudinal Survey Kindergarten cohort, just over one-third were enrolled in either algebra or a more advanced math course in 2006/07, when most of the cohort was in 8th grade (Walston and McCarroll, 2010). As noted above, there is significant variation from this average across jurisdictions.

This early algebra movement appears to have been bolstered in part by unwarranted causal inferences from correlational research. Eighth grade students enrolled in algebra consistently outscore their counterparts on 8th grade standardized math tests (Walston and McCarroll, 2010). By the time they reach 12th grade, early algebra-takers have completed more years of advanced math and attain higher scores on 12th grade math assessments (Smith, 1996).

[8] See, for example, Burris, Heubert and Levin (2006), who show significant positive effects of a math curriculum reform that began the acceleration process in 6th grade. Schoenfeld (1995) advocates spreading the teaching of algebraic concepts throughout the K-12 years.

Other research has documented higher achievement outcomes among students who enroll in algebra at any point in their secondary school career (Dossey et al., 1988; Gamoran and Hannigan, 2000). Ma (2005a; 2005b) reports that taking algebra in 8th grade is associated with the greatest improvement in math skills among the lowest-achieving students – particularly those below the 65th percentile of the 7th grade math distribution. To date, no study has attempted to address concerns regarding selection into early algebra on the basis of unobserved characteristics.[9]

That is not to say there have been no doubters. Concerns about the reliability of previous studies have provoked something of a backlash against accelerating algebra into middle school. Opponents of accelerated algebra argue that too many students enter the course unprepared for advanced work and may in fact fall behind their peers who had originally enrolled in less rigorous coursework. In a 2008 report, Tom Loveless documents the poor math performance of some students enrolled in algebra by 8th grade, and he notes the inattention to the problem of possible selection bias in prior work justifying the push to offer algebra in middle school. The Loveless report itself, however, provides no evidence on the causal question of whether early placement in algebra promotes or retards mathematics achievement.[10] The poorly-performing students he cites may have performed just as badly in a more traditional 8th grade math course. An empirical assessment of the effects of accelerating the first algebra course requires comparison with a counterfactual: otherwise identical students who take algebra on a traditional schedule. This is exactly the counterfactual provided by the rapid and uneven rollout of accelerated algebra in at least two of North Carolina's largest school districts.

[9] Ma (2005b), for example, reports that only 4% of students below the 65th percentile of the 7th grade math distribution are placed in algebra by 8th grade.

[10] Allensworth et al. (2009) provide evidence that a broad multi-subject curricular reform emphasizing placement in college-preparatory coursework in Chicago high schools led to no significant improvement in test scores or college entry rates.

3. Conceptual Framework

3.1 Algebra timing, mathematics skills, and labor productivity

From an economic perspective, algebra skills can be valued for two basic reasons. First, algebra skills may contribute directly to labor productivity.[11] Second, algebra skills might serve as inputs into the production of higher-order mathematical knowledge, which in turn may affect productivity. It is because of this second function that algebra is sometimes called a "gateway" to higher mathematics and STEM courses in general. These two effects on productivity can be summarized in this expression:

(1) $y = y(a(t_a), h(a, t_h))$,

where y is a measure of productivity, a is a measure of algebra skill, h is a measure of higher-order mathematical skill, and t_a and t_h measure the amount of time devoted to the study of algebra and higher-order topics, respectively. All three functions in equation (1) are presumed to be nondecreasing in their arguments. If students are expected to complete their human capital investment by a specific age, the case for accelerating entry into algebra is clear: initiating algebra earlier allows more time for instruction in both algebra and higher-order topics, thereby unambiguously increasing productivity.

Things get more complicated when we introduce the possibility that both algebra and higher-order math skills rely on the degree to which students have mastered lower-order topics in mathematics. Consider the formulation:

(2) $y = y(l(t_l), a(l, t_a), h(a, l, t_h))$

where l and t_l represent lower-order mathematical skill and the time devoted to learning these skills, respectively. Although we did not introduce an explicit time constraint in our initial formulation, it makes sense here to assume a fixed amount of time available between school

[11] Beyond improving labor productivity and earnings, math skills may also increase utility by promoting better consumption decisions by boundedly-rational agents (Benjamin, Brown, and Shapiro, 2006).

entry and the end of human capital investment. In this formulation, the question of optimal algebra timing means weighing the benefits of time to learn higher-order topics against the costs of insufficient mastery of pre-algebra concepts. The belief that students enter algebra too late is equivalent to an argument that too much time is devoted to lower-order subject matter.

Equation (2) implies that the optimal allocation of time across mathematical topics depends on a number of relationships: the relative importance of lower-order skills in the production of higher-order skills, the marginal impact of time on skill acquisition, and the relative importance of various types of mathematical skill on productivity. A proposal to reallocate time away from lower-order skills makes the most sense if lower-order skills significantly contribute neither to labor productivity nor production of algebra and higher-order skills.

3.2 The opportunity cost of acceleration

What kinds of topics are short-changed when algebra is accelerated? To get an idea, Table 1 describes the key competencies that North Carolina's standard course of study establishes for several pre-algebra courses, ranging from 7th Grade Math to Introductory Math, the course prescribed for students who do not take Algebra I upon entry into high school.[12]

The similarity in course objectives across 7th and 8th grade math, and the high school introductory math course, suggests the possibility of diminishing returns in lower-order mathematics instruction. The objectives of 8th grade math and Introductory Math are nearly identical, suggesting that the high school course largely repeats subject matter for students who did not master it the first time around. Furthermore, the distinctions between 7th and 8th grade math objectives are minor: eighth graders, for example, are expected to perform computations

[12] These competencies form the basis for standardized End-of-Grade tests in mathematics conducted since the early 1990s.

with irrational numbers whereas in seventh grade computation with rational numbers is sufficient.[13]

Although a perusal of these stated objectives suggests that pre-algebra courses are incremental if not redundant, it is possible that many students need repeated exposure to this subject matter. It is interesting to note, furthermore, that each of the middle-grades math courses includes significant attention to geometry. Computation of volume and surface area is a key component of the 7th grade curriculum, and the Pythagorean theorem is mentioned specifically in the 8th grade curriculum. Both topics also appear in the high school Introductory Math course, and both relate directly to subjects covered in the state's official Geometry curriculum, which focuses in part on right triangles, problems involving surface area and volume, and elementary proof-writing.

Algebra I acceleration is not the only curricular reform that has been introduced in hopes of improving mathematics achievement. California's Math A and New York's Stretch Regents curriculum exemplify reforms that target the quality of pre-algebra instruction rather than the timing of algebra course taking (White 1995; White et al. 1996; Gamoran et al. 1997).[14] Although evidence on the effectiveness of these programs is inconclusive (White et al 1996; Gamoran et al, 1997), these alternatives may offer promising avenues to improve achievement in the event that accelerating algebra is judged not to be worth the cost of forgone pre-algebra instruction (Burris et al. 2006).

Although relevant to the question of optimal time allocation, the larger question of which math subjects have the strongest effects on productivity is beyond the scope of our empirical analysis. In one study pertinent to this issue, Rose and Betts (2004) analyze transcript data from

[13] A rational number is one that can be expressed as the ratio of two integers.

[14] Math A is a high school curriculum used in certain districts used to transition lower achieving students to a college-preparatory algebra and geometry curriculum. The Stretch Regents program permits students to take New York State's rigorous Regents curriculum at a slower pace. See Gamoran (1997) for further description.

the High School and Beyond dataset, using straightforward methods to address concerns about self-selection into higher-order courses. That study suggests that the labor market return to higher-order coursework is greater than the return to coursework at the level of introductory algebra or geometry.

4. Data and Methodology

4.1 Setting

In the fall of 2002, two of the three largest school districts in North Carolina adopted unusually aggressive policies to accelerate placement of middle and high school students into Algebra I. The districts, Charlotte-Mecklenburg Schools (hereafter, CMS) and Guilford County Public Schools (encompassing the cities of Greensboro and High Point), were led by strong superintendents who championed a policy of increasing the number of 8^{th} graders taking Algebra I. The superintendent of CMS strongly believed as a matter of pedagogy that algebra should be offered to many, if not most, students in middle school, rather than waiting until they are in high school. Later described as "a bear on getting middle school kids in eighth grade to learn Algebra I," this superintendent announced at the beginning of the 2001/02 year that his goal would be to increase to 60% the portion of students in the district who were proficient in Algebra I by the end of eighth grade, as indicated by scoring at level 3 or above on the state's end-of-course test.[15] In Guilford, a new superintendent began his tenure in May 2000 forcefully advocating a policy of

[15] *Educate!*, September 16, 2001, p. 5. As evidence of the superintendent's focus on increasing the number of middle school students taking algebra, one informant described how he ordered middle school principals to overhaul schedules after the school year had commenced in order to increase the number of middle school students in algebra classes. In an interview after he stepped down as CMS superintendent, Eric Smith stated, "The middle school math piece was the gatekeeper and it is the gatekeeper. It's the definition of what the rest of the child's life is going to look like academically, not just through high school but into college and beyond. If they make it into algebra one, the likelihood of getting into the AP class and being successful on the SAT and having a vision of going on to college is dramatically enhanced. And so our pressure to make sure that kids were given that kind of access to upper level math in middle school was a critical component of our overall district strategy." [15] http://www.pbs.org/makingschoolswork/dwr/nc/smith.html, 4/5/11.

enrolling as many 8th graders as possible in Algebra I.[16] These superintendents not only broke from past patterns of course-taking but also diverged significantly from policies followed by most other districts in North Carolina. To be sure, there was widespread interest in education circles at this time in the idea of accelerating the teaching of algebra, as noted in Section 2 above. Adding additional impetus, the state of North Carolina had increased from three to four the number of math courses required for admission to the University of North Carolina system.

Several other policy changes transpired in CMS during the period of our study. The district ceased busing students to desegregate schools in 2002, and implemented a public school choice plan, incorporating a lottery system for oversubscribed schools the same year (Hastings, Kane, and Staiger 2005, 2006a, 2006b; Hastings et al. 2007; Deming et al. 2011; Vigdor 2011). These changes may have led to systematic declines in instructional quality for African-American and other disadvantaged students (Jackson 2009) that may have confounded the effects of accelerating algebra in CMS. To our knowledge, there were no similar potentially confounding policy shifts in Guilford County at this time. In our analysis below, we present specifications that alternately exclude CMS and Guilford from the sample to investigate this concern.

In the analysis that follows, we make use of data on students enrolled in the ten largest North Carolina school districts, provided by the North Carolina Education Research Data Center. In our most basic specifications, CMS and Guilford are considered to have adopted a “treatment” of accelerating algebra instruction, while the other eight are considered “controls” used to identify counterfactual trends in relevant outcome measures. The eight “control” districts themselves exhibit some degree of variation in algebra-taking patterns over time, and in

[16] The superintendent was Terry Grier. His stated motivation for increasing the number of 8th graders taking Algebra I was his desire to increase the number of students, particularly minority students, in AP course in high school, symbolized by the term used among district administrators at the time, “AP is not for the elite but for the prepared” (personal correspondence and conversation with the administrator in charge of curriculum and instruction at the time, now associate superintendent, Terrence Young, August 29, 2013, and former school board member Dot Kearns, August 5, 2013).

additional specifications we use this variation as well to identify effects. The presence of policy variation in the "control" districts does not bias our main effect estimates so long as it is not perfectly correlated with changes in the "treatment" districts.[17]

Figures 1 and 2 summarize information on algebra-taking patterns in Charlotte-Mecklenburg and Guilford for the age cohorts included in this study.[18] They are based on longitudinal samples of students described in more detail below.[19] For each student, we record the year in which he or she first takes North Carolina's end-of-course test in Algebra I.[20] Students are stratified by cohort – defined as the year they first enrolled in 7th grade – and by performance on the 6th and 7th grade end-of-grade standardized math tests. Each bar in the graph represents a rate of taking Algebra I by 8th grade for a cohort/quintile cell.[21] Cohorts are denoted by the year their members became 7th graders for the first time. Thus it was students in the 2001/02 cohort who first experienced the accelerated timetable for Algebra I introduced by CMS and Guilford in the fall of 2002.

As seen in both figures, baseline rates of algebra-taking by 8th grade were high in both districts relative to the national average for high-performing students, but low for low-performing students. In the first studied cohorts, 97% of CMS students in the top quintile of the statewide 6th grade math score distribution were enrolled in Algebra I by 8th grade, as were 93% of Guilford's high-scoring students, compared to 75% of top quintile 8th graders nationwide, as

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[17] In the event that changes in the "control" districts were perfectly correlated with those in CMS and Guilford, the first stage of our IV procedure would return a coefficient of zero on the instruments.

[18] Note that the cohort entering 7th grade in CMS in 1999/2000 is omitted from this study. Student records for that cohort are unlinkable for the full duration of the panel because of a switch in ID codes affecting that cohort.

[19] Note that all analyses reported in this paper "undo" effects of grade retention by comparing students only to those in their entering cohort. To be precise, therefore, our analyses address not the impact of taking Algebra I by 8th grade, but rather the effect of taking the course within two years of beginning 7th grade.

[20] More precisely, we present the year in which a student first appears as a data point in the Algebra I EOC test file. A small number of students appear in the dataset but do not have a valid test score. These students are excluded from analyses using test scores as a dependent variable below, but are included in analyses of subsequent course-taking.

[21] We stratify students by test score quintiles for purposes of this graph; in regression analyses below we more finely stratify students by decile.

recorded in the 2009 NAEP assessment (Walston and McCarroll 2010). By contrast, only 3% of CMS students, and 1% of Guilford students, in the lowest 6th grade math quintile had enrolled in Algebra I by 8th grade, compared to 13% in the national NAEP data.

Compared to very similar students who passed through the 8th grade in their districts just before the policy change was implemented, the "treated" cohorts in both districts experienced a very different pattern. The rate of early algebra-taking jumped dramatically at lower points in the distribution. For students around the median in CMS, the likelihood of taking Algebra I by 8th grade increased from 51% to 85%. For students in the second-lowest quintile, the rate increased from 18% to 63%. Even in the lowest quintile of the 6th grade math distribution, the rate of Algebra I taking rose to 15%.[22] Guilford's acceleration was even more precipitous than that in CMS. Lowest-quintile students in the 2004 cohort were placed in Algebra I in 8th grade at a rate of 36%, twice the maximum rate observed for that quintile in CMS. Rates of Algebra I placement by 8th grade peaked at 78% in the next-lowest quintile, and in the middle quintile exceeded 90%.

Just two years after the push to accelerate algebra started, however, CMS reversed course. By the time the cohort that entered 7th grade in 2004/05 had reached middle school, assignment patterns in CMS had reverted to levels below those in the 2000/01 cohort, except in the top quintile, where a modest amount of acceleration remained in place. Guilford, by contrast, stuck with its acceleration program through at least the 2004/05 cohort, when our period of observation ends.

[22] Our data are derived from end-of-course test records, which may not accurately measure the number of students assigned to take Algebra I in a given year. Students may withdraw from the course in advance of test administration, for example. There is some evidence that the rate of withdrawal rose in 2002/03 along with the rate of course-taking. In that year, an administrative count of course enrollment in Algebra I for CMS enumerates over 900 students for whom we have no test score record. In most other years, the discrepancy between the two sources of enrollment data is small. We discuss potential implications of this pattern below.

4.2 Data and Sample Selection

Our data are derived from North Carolina Education Research Data Center longitudinal records on students who entered 7th grade between 1999/2000 and 2004/05 and spent the subsequent school year in any one of the state's ten largest districts – CMS, Guilford, and an additional eight districts we consider as "controls" in our basic specifications.[23] We restricted the sample to students with valid scores on the state's standardized 6th and 7th grade mathematics assessment in order to stratify them by prior math performance. We then tracked progress through college-preparatory math courses using the state's end-of-course (EOC) examinations in Algebra I, Geometry, and Algebra II. When using ten districts and all available cohorts, our sample amounts to 194,425 students.[24]

By design, the sample includes some individuals who are never observed enrolling in Algebra I in our dataset. Thus, for some students, our analysis could confound the effect of accelerating algebra with the effect of taking it at all. Excluding non-algebra takers from the analysis, however, might lead us to overstate the negative effects of the acceleration policy, to the extent that acceleration policies expand the overall pool of Algebra I takers. In such a

[23] As noted above, we are unable to include the 1999/2000 cohort for CMS.

[24] Some of the students included in our sample may exit the dataset because they leave North Carolina public schools, to attend a private or out-of-state school. If such students complete Geometry or Algebra II coursework, we will incorrectly code them in our analysis. Due to differences in student ID coding between CMS and other North Carolina districts, we are unable to satisfactorily track students who transfer to a different district or to a charter school, which introduces further possibilities for miscoding. Moreover, given data limitations it is impossible for us to distinguish a student who attrits from one who persists without taking EOC exams. This poses a problem for our analysis only to the extent to which transfer behavior correlates with algebra acceleration, conditional on decile and cohort effects. If parents respond to the decline in mathematics performance associated with algebra acceleration by switching to a different school district, we may in fact overstate our results. Note that we are similarly unable to identify students who drop out of school; since students cannot pass EOC exams after dropping out, however, they are not miscoded.

scenario, marginally-performing students would appear in the sample only in years when acceleration was in effect.[25] We describe our strategy for addressing this potential bias below.

4.3 Main identification strategy: clear policy shifts

Our estimation strategy takes advantage of the large and abrupt policy changes undertaken in CMS and Guilford County beginning in the fall of 2001. We exploit these changes to estimate local average treatment effects for taking Algebra I by the time students reach 8^{th} grade, based on differences between the treated and untreated cohorts. To address concerns that differences across cohorts might reflect changes in the difficulty of standardized tests or other phenomena, we use the state's other eight largest districts as additional "control" observations. Although these districts may have implemented some form of policy change at some point during our sample period, our identifying assumption is that any such policy changes did not affect the exact same deciles and cohorts as were affected by the CMS and Guilford changes.[26] Under this assumption controls for decile-by-cohort fixed effects, included in our main specifications, account for time-varying factors affecting all students at similar performance levels statewide.

Estimated treatment effects are "local" to that set of students subjected to differing treatment status across cohorts in CMS and Guilford, drawn primarily from the mid-to-lower portion of the prior test score distribution. To gain additional insight into the potential impact of algebra timing among higher-performing students, and to serve as an additional specification check, we report the results of additional specifications that make use of apparent policy

[25] Results obtained with a sample restricted to "ever-takers" confirm the existence of this bias (Clotfelter, Ladd, and Vigdor 2012a).

[26] The test of this identifying assumption is the first-stage equation in our two-stage procedure. In all cases, test statistics indicate that our instrumental variables are strong – that the difference in patterns between the accelerated decile/cohorts in CMS and Guilford and patterns affecting the same decile/cohorts in other districts is significant.

variation in the other eight large North Carolina districts. We describe our methods for this analysis more thoroughly below.

Our basic estimation strategy is a version of differences-in-differences: we compare the outcomes of students stratified into deciles of initial ability level, as measured by the mean of 6^{th} and 7^{th} grade math scores, across cohorts. In order to implement this strategy in a manner that produces local average treatment effects, we use instrumental variable estimators capturing the policy-induced variation in individual propensity to take Algebra I by 8^{th} grade.

The second-stage outcome equation is of the form:

(3) $y_{ildc} = \alpha_l + \alpha_{dc} + \beta X_{ildc} + \gamma T_{ildc} + \varepsilon_{ildc}$

where y_{ildc} is the outcome of interest for student i attending district l, belonging to initial achievement decile d in cohort c, α_l is a district fixed effect, and α_{dc} represents a set of cohort-by-decile fixed effects, X_{ildc} represents a vector of student-specific characteristics, T_{ildc} is an indicator for whether the student received the treatment – in this case, taking Algebra I by 8^{th} grade – and ε_{ildc} is an independent and identically distributed error term. Decile-by-cohort fixed effects account for any curricular or test change that would influence all students in a given prior achievement decile in a cohort across the state such as the introduction of a more difficult passing standard that might alter outcomes for moderately-performing students but not those at either end of the distribution. The use of decile effects rather than a linear control for test score also allows us to account for potentially nonlinear effects of initial ability on later outcomes.

Prior work in this literature has often estimated single equations along the lines of (3), arguing that controls for prior achievement adequately correct for unobserved determinants of the outcome that also correlate with the treatment indicator, implying that γ is an unbiased estimate of the true treatment effect. To assess this argument, we present OLS estimates of equation (3) for comparison with our preferred IV results below.

In our preferred specifications, we address the endogeneity of assignment to an accelerated algebra class by estimating the first stage equation:

(4) $T_{ildc} = \phi_l + \phi_{dc} + \rho X_{ildc} + \delta_{ldc} + \eta_{ildc}$

where ϕ_l and ϕ_{dc} are district and cohort-by-decile fixed effects, the δ_{ldc} term represents a set of fixed effects for the district/cohort/decile cells subjected to acceleration, observable in Figures 1 and 2, and η_{ildc} is a second error term.[27] Predicted values of equation (4) are then used in place of actual treatment status in equation (3). The identifying assumption is that students in the accelerated cohort-decile cells in CMS and Guilford differ – relative to their counterparts in the same district, and relative to counterparts in the same decile-by-cohort cell in the other eight districts – only in their likelihood of taking Algebra I in 8th grade. Because the identifying variation in Algebra I timing is at the cohort-by-decile-by-district level, we cluster standard errors at that level.

One possible concern with our identification strategy relates to peer effects given that the the "always-taking" students in a given decile-cohort may be adversely affected by the presence of lower-performing peers in their classroom (Nomi, 2012).[28] To the extent that these peer effects matter, our reduced-form estimates of the impact of acceleration in a cell may combine negative effects on always-takers with more modest, or perhaps even positive, effects on "compliers." In this scenario, we would expect the effects of acceleration to be the least negative in cells with few or no "always-takers." Below we report the opposite pattern.[29]

[27] For purposes of our analysis, we consider the following cohort/decile/district cells as being "treated:" CMS deciles 1-6, 7th grade cohorts of 2001/02 and 2002/03; Guilford County deciles 1-8, 7th grade cohorts of 2001/02 through 2004/05. Note that our formulation of the first stage equation effectively accounts for differential dosage across cells – for example, the fact that not all of the "accelerated" cohorts are equally accelerated. Our choice of cells is based purely on examination of the data; in additional specifications below we use a statistical procedure to identify district/cohort cells apparently subjected to varying policy over time.

[28] Note that adverse impacts on students in higher-performing deciles – the effects studied by Nomi (2010) – would be less of a concern here.

[29] In this scenario, we would also expect less negative effects in cells where the number of compliers is small relative to the number of always-takers. While we do find evidence of this pattern, this evidence is also consistent

In principle, we would like to estimate the effect of early progression to Algebra I on performance in that course and subsequent math courses. Achieving this goal is complicated by the fact that many students in our sample never take Algebra I, let alone any follow-up course. Thus, any effort to estimate the impact of Algebra I timing on performance in that course, in Geometry, or in Algebra II must contend with a serious sample selection problem-- namely, that we can observe performance only for those students who actually take those courses.

Table 2 illustrates the potential severity of this selection problem, by comparing the progress of students in two cohorts in the larger of our two districts, CMS. Consider first the cohort of students who entered 7th grade in 2000/01 and took Algebra I for the first time the following year. Because this cohort arrived before the district's accelerated algebra push, only 32% of them took Algebra I in 8th grade. About 93% of them passed the EOC test in the subject, and 85% of them progressed immediately to Geometry the next year. Most of the non-progressing students retook Algebra I as 9th graders. About 78% of the 8th grade Algebra I takers in this first cohort took the Algebra II EOC two years later, and 90% took the Algebra II EOC by the time they would ordinarily have graduated from high school.

In contrast, among students entering 7th grade in 2002/03, the second cohort subjected to the acceleration initiative, a much higher share, almost half, took Algebra I in 8th grade. The weaker average quality of this group shows up in lower pass rates. Only 70% of them proceeded to Geometry the following year, only 64% completed the three-course sequence by the end of 10th grade, and just 80% finished the sequence by the time they would ordinarily have graduated.[30] This weaker record of course completion for the accelerated cohort, presumably

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with the simpler hypothesis that higher-performing students are better prepared to undertake algebra coursework in 8th grade.

[30] The attrition problem illustrated in Table 2 is even more severe among students who take Algebra I at a later point in time. For students first taking the Algebra I EOC as ninth graders in 2002/03, 66% proceed to take the Geometry EOC the following year, and 54% take the Algebra II EOC the year after that. Interestingly, this progress is excessive in relation to the group's pass rate on the initial Algebra I exam, which is only 48%. These summary

caused by the exit of many lower-performing students, would leave a comparatively strong group of students to take subsequent courses. The likely result, therefore, would be a positive bias on estimates of the effect of acceleration on Geometry or Algebra II test scores.

Table 2 also shows that the overall rate of "ever" taking Algebra I is nearly identical in the two cohorts of CMS students. While this equivalence might suggest that the acceleration policy had little effect on the marginal probability of taking Algebra I and only affected timing, recall that members of the 2000/01 cohort would have been exposed to the acceleration initiative as 9th graders. Additional analysis of later cohorts who progressed through high school after the acceleration had ended reveals significantly lower rates of ever-taking.[31]

Although our primary concern is that non-accelerated students would never take Algebra I and thus not enter our sample, we must also be concerned that families may have responded to the acceleration initiative by exiting the affected school systems or otherwise altering their school choices. To assess the potential severity of this problem, we examined attrition rates for two cohorts of CMS 7th graders: the 1999/2000 and 2002/03 cohorts. One-year and two-year attrition rates for the two cohorts are nearly identical, suggesting that any impact of the policy – or other contemporaneous factors – on attrition is likely to be small.[32]

Rather than attempt to estimate a selection-correction model, which would require the use of either functional form assumptions or a second set of instruments that predict course taking but otherwise do not influence outcomes, we adopt two alternative strategies.[33] Our first strategy

statistics clearly associate the acceleration policy with lower course passage and progression rates. Such a pattern could conceivably be explained entirely by selection patterns, however. Our IV procedure promises to directly compare the performance of marginal students assigned to different courses.

[31] Specifically, in the 2004/05 cohort 83.8% of all students in our sample are counted as "ever-takers." While the proportion has declined only modestly overall, the decline is more pronounced in the lower deciles of the 6th grade math distribution.
[32] Specifically the one- and two-year return rates for the pre-treatment cohort are 92.3% and 84.7% respectively; for the treated cohort they are 94.7% and 84.8%.
[33] One might consider the algebra acceleration initiative itself to satisfy the exclusion restriction in a Heckman-style selection model. This would be appropriate only in the event that the acceleration influenced later course taking but

is to redefine our outcome variables such that all students can be included, whether or not they enroll in a course. Specifically, we analyze whether students attain a passing grade on a mathematics end-of-course test soon enough to keep them on track to complete Algebra II within six years of beginning seventh grade.[34] Students who never take a course are coded as not having passed that course. Were the acceleration to be associated with negative selective attrition – a higher rate of exit for students predisposed to poor outcomes – this coding would address the associated bias.[35] For ease of interpretation, we report the results of two-stage least squares models below.[36]

Our second strategy for addressing selection into the sample of math course takers applies to models estimating the effect of acceleration on Algebra I test scores, which are, by definition, unobserved for students who never enroll in Algebra I. Adopting a strategy used by Neal and Johnson (1996), we assume that students who never enroll in Algebra I would have received a test score that was below the median conditional on observables. Under this assumption, we impute a below-conditional-median test score for these students, and estimate models based on the Chernozhukov and Hansen (2007) Instrumental Variable Quantile

was otherwise unrelated to outcomes. This runs contrary to the basic premise of this article. We are unable to identify any observable factor that influences whether a student takes a course that is otherwise unrelated to the student's performance in that course.

[34] Our definition of a passing grade on the Algebra I and Algebra II EOC 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 EOC 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. In alternative specifications, we also analyzed the propensity to pass mathematics courses within a fixed number of years after first taking Algebra I. Results do not vary substantively across specifications.

[35] We note that positive selective attrition would create a bias not corrected with this procedure; the absence of strong students from treated cohorts will create the illusion of poor performance in those cohorts. We remind the reader that overall attrition rates are effectively identical in treated and untreated cohorts.

[36] Clotfelter, Ladd and Vigdor (2012a; 2012b) show that bivariate probit versions of models with binary outcomes tend to generate qualitatively and quantitatively similar results.

Regression (IVQR) estimator.[37] When estimating quantile regression the exact value of the dependent variable need not be observed, so long as it can be safely assumed to be below the relevant quantile.

4.4 Adapting the method to consider apparent policy shifts

A limitation of the analysis of policy shifts in CMS and Guilford is that our estimates are local to those deciles experiencing significant shifts across cohorts. Thus our approach sheds no light on the effect of acceleration for students at the top of the prior achievement distribution (who typically took Algebra I in 8th grade before the policy shift) and those at the bottom (who rarely took it, before or after). To discover the effects for these students at the top and bottom, we would need to identify policy-driven shifts in algebra timing for students in the highest and lowest deciles. CMS and Guilford offer some opportunity in the lowest deciles, but very little in the highest, as high-performing students in all cohorts take Algebra I in 8th grade at rates approaching 100%.

To overcome this obstacle, we take advantage of the fact that several other districts in the state's top ten appear to have changed their Algebra I timing policies over time. Although we do not have specific documentation of these policy shifts, we can infer their existence on the basis of observed variation in placement patterns across cohorts within a district/decile cell. We refer to policy changes inferred on the basis of observed patterns rather than documented evidence as "apparent" policy variation.

To identify apparent policy variation, we sought to identify district/decile cells with across-cohort variation in algebra-taking-by-8th grade patterns that is too large to be explained by

[37] Specifically, we impute standardized test scores of -4 for non-test takers. This procedure may yield biased results to the extent that some students without test scores have omitted data for reasons other than failure to take the course, e.g. transfer into a private school. We report the results of 2SLS specifications, which avoid imputation problems but introduce sample selection concerns, in footnotes below.

sampling error, and cannot be explained on the basis of variation in observed student characteristics. The procedure, described in more detail in Clotfelter, Ladd and Vigdor (2012b), begins by flagging district-decile cells where, on the basis of a chi-squared test, we can reject the hypothesis that the likelihood of taking Algebra by 8th grade does not vary across cohorts. Then, for each cell in this subset, we regress Algebra I taking by 8th grade on cohort effects and a set of observed student characteristics, flagging those cells where an *F*-test permits us to reject the hypothesis of no residual variation across cohorts.

In the "apparent policy shift" specifications below, we include data for those district/decile cells in which there is a significant, unexplained degree of variation in Algebra I taking patterns across cohorts. Of 100 district-decile cells, 42 are excluded for having insignificant variation across cohorts (Clotfelter, Ladd and Vigdor 2012b). These models use a complete set of district-by-cohort-by-decile indicators as instruments; the coefficients on these indicators equal the rate of taking Algebra I by 8th grade within the cell. Because we use these variables as instruments, we are unable to control for decile-by-cohort indicators in the second stage.

5. Results

For the purpose of comparison, Table 3 presents the simple OLS estimates of equation (3), examining the basic relationship between Algebra I timing and the four outcomes.[38] These estimates should by no means be interpreted as causal effects, even though they include indicators that restrict comparison to students ranked in the same decile by average 6th and 7th grade math score. Even conditional on decile, earlier assignment to algebra in the cross-section is likely to be correlated with unobserved determinants of math achievement. Note also that we

[38] These specifications use the apparent policy variation sample of district-decile cells; OLS results are not very sensitive to sample selection criteria; cf. Clotfelter, Ladd and Vigdor (2012a).

make no effort here to impute test scores for students who never take Algebra I, leading to a second source of bias in the estimates.[39]

Consistent with earlier studies, our OLS specifications associate placement in Algebra I by 8th grade with better outcomes in subsequent years. Although students who complete Algebra I by 8th grade receive lower scores on the standardized test in that subject, they are significantly more likely to attain passing scores on both Algebra I and higher-level math exams on a college-preparatory schedule. The probability of completing the college preparatory sequence, equal to about 50% in our entire sample, is 9 percentage points higher among students who complete Algebra I by 8th grade, conditional on the average 6th/7th grade math test decile. Interpreted naively, the apparent advantage associated with early access to algebra is equivalent to the predicted impact of raising a student's 6th grade math test score by a full decile in the distribution. To reiterate our previous argument, however, these OLS estimates, like some prior estimates in the literature, are very likely to be contaminated by selection bias.

5.1 Main estimates: using the full sample and CMS/Guilford policy variation

Table 4 shows instrumental variable estimates of the impact of taking Algebra I by 8th grade.[40] These estimates include IVQR estimates, with imputed test scores for non-Algebra takers, for models analyzing variation in test scores and two-stage least squares for the three binary outcomes. Each model controls for combined 6th/7th grade test score decile and cohort fixed effects, district fixed effects, and cohort-by-decile fixed effects. The instruments excluded

[39] Students who never take Algebra I would presumably earn lower scores on the test if they did, and would also presumably be less likely to take the course by 8th grade. Note that in addition to students who never take Algebra I, the test score equation excludes approximately 250 students who appear in EOC test records with a missing value for the score. These students are included in specifications which impute scores for non-takers below, and are treated equivalently to non-takers. The EOC data also contain records for students who are coded as exempt from testing. We exclude these students from all specifications.

[40] Technically, the dependent variable measures whether a student has taken the Algebra I EOC exam within two years after beginning 7th grade.

from the second stage separately identify the district and cohort cells in CMS and Guilford that were subjected to significant increases in the likelihood of taking Algebra I in 8th grade, as indicated in Figures 1 and 2. First stage results uniformly indicate a sufficient amount of variation to assuage potential concerns about weak instruments.

The results contrast starkly with the basic patterns revealed in our OLS analysis and previous, correlational analyses. Accelerated students score 37% of a standard deviation lower on their Algebra I end-of-course tests.[41] They are about 4 percentage points more likely to pass the course by the time they complete 10th grade. The apparent contradiction between lower test scores and higher pass rates is explained by course retaking, a point on which we elaborate below. Two-stage least squares estimates indicate that accelerated students are neither more nor less likely to pass Geometry and Algebra II on a college-preparatory schedule.

Table 4 also reports coefficients on the student-level covariates included in the model – with the exception of test score decile indicators, which reveal a predictable pattern that students with higher prior test scores tend to attain better outcomes. Male students tend to attain better outcomes than female students conditional on covariates. Black and Hispanic students, perhaps surprisingly, have better chances of passing Algebra I and Algebra II conditional on covariates including past test scores, though they are slightly less likely to pass Geometry. Students receiving free or reduced price lunch receive lower scores on end-of-course Algebra I tests and are less likely to pass the college preparatory coursework, other things equal.

5.2 Robustness checks

[41] Estimation by 2SLS, without imputing test scores for non-takers, yields a slightly larger coefficient. Given that acceleration coincided with an increase in the overall taking rate, this is the expected pattern if marginal Algebra takers are negatively selected on unobservables. In additional specifications, we examined the effect of algebra acceleration on the 8th grade end-of-grade mathematics test, which is administered to all 8th grade students regardless of course enrollment. We found no significant effects, suggesting that any gain to 8th graders from enrolling in Algebra I are offset by weaker mastery of non-algebraic subjects covered on the EOG test.

Table 5 shows the results of several perturbations to the basic model, alternately deleting or adding districts to the set used to identify the effects of interest. Each entry in the table represents a different instrumental variables specification, with the test score specifications estimated by the IVQR procedure described above.

The first row repeats the basic Table 4 results; the next two are identical to the basic model, but omit CMS and Guilford in sequence. As noted previously, Charlotte-Mecklenburg Schools embarked on a series of policy shifts around the same time it implemented its algebra acceleration initiative. To address concerns that these other shifts may have had effects that confound the effect of interest, the second row of Table 5 uses a nine-district sample that omits CMS entirely, identifying effects solely on the basis of policy variation in Guilford County. The results in the first row are generally similar to those obtained with the full ten-district sample. The estimated impact of acceleration on test scores is substantially more negative in this sample, suggesting that to the extent unrelated CMS initiatives confound our estimates, they bias it towards zero. Across the three regressions examining course passage, the changes brought about by this sample restriction are inconsistent, with coefficients becoming less positive in one case and more positive in two others relative to Table 4. The basic conclusion – that acceleration produced significantly lower test score outcomes and had little or no impact on the likelihood of completing a college-preparatory math sequence – remains intact.

Row 3, in which we return CMS to the sample and remove Guilford County, offers an opportunity to assess whether the negative effects of acceleration can be attributed to transition costs. The transition cost hypothesis generates the prediction that CMS-based estimates would be more negative than Guilford-based ones, because CMS reversed itself on acceleration almost immediately, offering no chance to observe acceleration in “steady-state.”

Results based on the nine-district sample omitting Guilford do, in fact, yield more negative estimates in two specifications – passing Geometry by 11th grade and passing Algebra II by 12th grade – than those in the full sample. The estimated impact on test scores is less negative, however, and the effect on passing Algebra I is essentially unchanged. We also note that neither the Geometry nor the Algebra II specifications featured significant coefficients in the main analysis, and the Algebra II coefficient remains insignificant in the sample omitting Guilford. Ultimately, then, the evidence is weakly consistent with the hypothesis that algebra acceleration imposes short-term transition costs that mask a more neutral or positive effect in steady state. The more consistent story, however, is that the policy yields few if any tangible benefits.

This story is confirmed, and perhaps even strengthened, in the analysis that employs apparent policy variation across all ten large North Carolina districts, as shown in the fourth row of the table. In these specifications, which include only those district/decile cells with significant across-cohort variation, the estimated negative impact of acceleration on Algebra I test scores nearly equals that obtained based on CMS and Guilford variation alone and there continues to be a positive effect on the likelihood of passing Algebra I by 10th grade. Here, though, there are statistically significant negative impacts on the likelihood of passing the follow-up courses on a college-preparatory schedule.

5.3 Effect heterogeneity

The effects estimated in our main analysis are local to the set of moderate-to-low performing students actually subjected to policy variation in CMS and Guilford. It is reasonable to think that the treatment effects of accelerating algebra instruction would be more benign for higher-performing students, and possibly more detrimental for students at the bottom of the achievement distribution. Using the apparent policy variation sample, we are able to test for this form of effect heterogeneity.[42]

Table 6 presents the results of instrumental variables specifications that permit the effect of 8th grade algebra enrollment to differ among students by quintile of the 6th/7th grade math test distribution. Note that the quantile regression strategy is implemented in this case with a reduced-form model (RFQR), rather than a two-stage model, owing to technical difficulties associated with using a large set of instruments (Chernozhukov and Hansen, 2005). 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, in contrast, 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 average effects on Algebra I test scores are negative for students in all quintiles. The RFQR estimates show associate the most negative effects with students in quintiles 2 and 3 and least negative effects with students in the top quintile. As for passing Algebra II, the faintly negative effect observed in the last row of Table 5 shows up in the Table 6 estimates only in the second quintile,

[42] Clotfelter, Ladd and Vigdor (2012b) report additional specifications regarding differential impacts of acceleration by gender, parent education, and free/reduced lunch status. These indicate that the impact of acceleration is more harmful for female students and students with less-educated parents. Effects on free/reduced lunch participants are more harmful in terms of test scores, but less harmful in terms of passing subsequent courses.

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.

5.4 Bounding heterogeneous effects on other outcomes

In the face of the evidence presented above, one might continue to advocate for accelerating algebra coursework on the ground that it creates opportunities for some students to pursue higher-level coursework, such as calculus, in high school. Unfortunately it is difficult to use the CMS and Guilford policy interventions to assess this hypothesis for two reasons. First, as noted above the interventions applied mostly to moderately-performing students, not the high-performers who presumably stand to benefit the most from the opportunity to enroll in calculus. Second, we are not able to track calculus enrollment or performance using our primary database of end-of-course test scores because North Carolina does not administer such tests in calculus. For some cohorts, however, we have access to complete high school transcript data which permits a more limited investigation of calculus taking .

In a simple comparison of calculus-taking rates among students who took Algebra I at varying points, we would expect the same sort of bias established in the analysis of other outcomes above. That is, students who take algebra early are likely to achieve better outcomes largely because they are positively selected. With this expectation in mind, we present basic evidence in Figure 3 to assess the potential positive impact of 8^{th} grade algebra enrollment on calculus taking for students at various points in the achievement distribution, using the full sample of students across ten districts in cohorts observed for a full six years beginning when they first enroll in seventh grade.

Two patterns are readily apparent in Figure 3. First, the plots slope upward: higher-scoring middle school students are more likely to enroll in calculus by the time they complete high school than those with lower test scores. Second, and more importantly for our purposes, the likelihood of taking calculus in high school conditional on middle school math test scores is uniformly higher among those students who took Algebra I no later than 8^{th} grade. Although this pattern may reflect the causal impact of early algebra, it also reflects the likelihood that accelerated students are better prepared than unaccelerated students, even after we control for the average of their 6^{th} and 7^{th} grade math scores. For this reason, the vertical distance between the two lines in the chart can be interpreted as an upper bound on the true causal effect of 8^{th} grade algebra. This upper bound is, for many students, quite low. Among students with the best math test scores in middle school, those who take Algebra I in 8^{th} grade proceed to calculus about 80% of the time; those who wait until high school still manage to proceed to calculus about 60% of the time, presumably because they "double up" on math coursework in one or more high school years. The upper bound on the treatment effect for high-achieving students is thus around 20 percentage points.

For moderately performing students, taking Algebra I in 8^{th} grade improves the chances of taking calculus in high school from the single digits to the teens, suggesting an upper bound for the treatment effect around 10 percentage points. Over 80% of moderately-performing students assigned to algebra courses as 8^{th} graders will fall off track at some point before they reach calculus. Among the lowest performing students – perhaps a third of whom were assigned to 8^{th} grade Algebra in Guilford County in peak years – there is essentially no chance of proceeding all the way to calculus in high school, regardless of when they take Algebra I.

If Algebra acceleration entails introducing subject matter to students who are not prepared to handle it, one predictable consequence would be an increased failure rate, followed by an increased rate of retaking the course. This was our interpretation of the contrasting results in specifications examining Algebra I test scores and pass rates in Table 4. Figure 4 presents a more detailed view, showing evidence on the rate of retaking Algebra I for students who first take the course in 8th grade or earlier, 9th grade, or after 9th grade, as a function of their 6th and 7th grade math test scores. The graph shows that retaking rates tend to be higher among lower performing students. Moreover, although the differences in retaking rates between those who take Algebra I in 9th grade and those who take it in a higher grade are very small, 8th grade Algebra I takers have a far higher risk of repeating the course at virtually all prior achievement levels. For students around the 20th percentile of the initial math achievement distribution, the retaking rate is nearly 50 % for students who attempt the course in middle school, far higher than the 20 percent who take it in high school . We note that this comparison *understates* the true treatment effect to the extent the students selected for early Algebra are unobservably better performers than their counterparts.

In sum, our analysis indicates that enrolling the lowest-performing students in early Algebra introduces significant downside risks with little to no upside potential. For moderately-performing students, there are at best modest potential rewards and significant downside risk. Students in the top 40% of the initial test score distribution appear to suffer few ill effects beyond the first year, and may in fact benefit from the opportunity to access higher-level math coursework in high school.

6. Potential causal mechanisms

One possible explanation for our findings is that the districts undergoing large and rapid policy shifts were able to do so only by reducing the quality of their algebra teachers. In the first year of an acceleration initiative, a district needs to offer algebra instruction to an unusually large group of students – the last un-accelerated cohort and the first accelerated cohort. Between the 2001/02 and 2002/03 7th grade cohorts, for example, the number of CMS students taking the Algebra I EOC exam increased from under 9,000 to over 11,000. It is important to distinguish between this explanation for the poor performance of the of the affected students and the explanation we posited earlier, namely insufficient pre-algebra grounding.[43] The insufficient pre-algebra explanation would imply that a permanent increase in the proportion of 8th graders taking Algebra I would generate the same types of results we observe in Charlotte-Mecklenburg and Guilford. In contrast, if the detrimental effects are merely the result of a temporary fall in instruction quality, the apparent cost of acceleration would be confined to the phase-in period.[44] We have argued above that the contrast in results between specifications focusing on CMS and Guilford is inconsistent with this view, but given the importance of establishing the nature of the negative effects this section closely examines teacher credentials in CMS around the time of the acceleration initiative.

The increased demand for Algebra I instruction could have affected the quality of instruction in numerous ways. Administrators could have responded by boosting class sizes, by assigning less-qualified teachers to the course, or by reallocating highly-qualified instructors

[43] Algebra acceleration might invoke a third causal mechanism when it involves placing moderately-performing students in the same classroom as high-performing students. In such a scenario, the high-performing students might witness a decline in instruction quality because their teachers must modify their curriculum or pedagogy to accommodate lower performers. Such a mechanism would actually lead us to understate the negative impact of accelerating algebra. Students enrolled in early algebra at baseline serve as a control group in our difference-in-difference identification strategy; any negative effect of the treatment on this group would be miscategorized as an exogenous trend in our analysis. As our data do not permit the definitive sorting of Algebra I students into classrooms within schools, we have little opportunity to investigate peer effects.

[44] Although the transition to the accelerated steady-state could have been accomplished in a single year, in practice enrollments persisted at an elevated level for several years. This reflects the increased rate of Algebra I retaking occasioned by the drop in performance documented above. The post-acceleration steady state might therefore result in a permanently higher level of Algebra I enrollment.

away from the subjects they would otherwise teach. Table 7, which tracks the number and qualifications of Algebra I teachers in CMS over time, shows that administrators avoided the first type of response. Between the 2001/02 and 2002/03 school years, the number of Algebra I teachers increased by roughly 25%, and the number of sections taught per teacher increased by 16%, with no increase in class size. In fact, the mean class size for Algebra I was slightly smaller in 2002/03 than it was in 2001/02.[45]

Table 7 also shows a noticeable decline in teacher quality, as proxied by teacher qualifications from 2002 to 2003. The average experience of Algebra I teachers, weighted by enrollment in sections taught, declined from 10.8 years to 8.8 years in 2003. Nearly one-third of Algebra I students were taught by a teacher with less than three years' experience in 2003, up from less than a quarter the year before. Licensure test score information, which is available only for a subsample of teachers, indicates a decline in credentials as well, both on general and subject-specific tests.

Table 8 shows the time allocation of teachers who taught at least one Algebra I section in 2003 and who were also tracked in the state's personnel system in the prior year. In the acceleration year, instructors of Algebra I spent less than half of their time teaching that specific course. The remainder of math teaching time was divided among both less- and more-advanced courses, ranging from pre-algebra to courses beyond Algebra II. A comparison with teaching patterns in the prior year reveals that teachers responsible for increasing the district's Algebra I capacity did so primarily by teaching fewer sections of pre-algebra, as well as teaching fewer other subjects including language arts and science. The proportion of time these teachers devoted to pre-algebra declined dramatically, whereas the proportion of time they devoted to

[45] Of course, the effect of class size on student learning in secondary schools is uncertain. Experimental evidence drawn from the early grades suggests that the beneficial effects of small class sizes dissipate rapidly as students age (Krueger, 1999). On the other hand, survey data indicates that math teachers in secondary schools adopt different practices in smaller classes (Betts and Shkolnik, 1999). There has been at least one experimental study of the impact of class size on performance in high school algebra, but the results were statistically inconclusive (Jensen, 1930).

higher-level subjects held steady or increased. Assuming that administrators tend to assign more qualified math teachers to higher-level courses, this pattern supports the general impression that the acceleration was accomplished by shifting less-qualified teachers into Algebra I.

Could this substitution of less-qualified teachers explain the entire acceleration effect? Students assigned to novice teachers have been repeatedly shown to exhibit poorer test score performance than their peers assigned to veterans (Boyd et al. 2008; Clotfelter, Ladd and Vigdor 2007, 2010; Rivkin, Hanushek, and Kain 2005). Suppose that the novice-veteran differential was 15% of a standard deviation – an estimate at the very high end of the distribution observed in recent studies. Exposing 8.5% of students to novices would then yield a prediction that test scores would decline by just over 1% of a standard deviation – a tiny fraction of the test score effects reported in Tables 4, 5, and 6 above. Additional effects might accrue to the extent that teacher experience levels decline marginally at other points in the distribution. Most estimates in the literature suggest, however, that the returns to experience beyond the first few years are relatively small.

Many studies of the effect of teachers on student test scores conclude that the observed credentials of teachers do not readily translate into measures of teacher effectiveness. These studies typically infer quality on the basis of "value-added" scores, derived from teacher fixed effects in longitudinal models of student achievement growth.[46] Some of these studies report that the difference between a high-performing and low-performing teacher might be as high as a full student-level standard deviation (Rivkin, Hanushek, and Kain, 2005; Rockoff 2004).

[46] We are unable to consistently compute "value-added" scores for the Algebra teachers in our sample for a number of reasons. As indicated above, a substantial number of Algebra teachers have no prior experience. As indicated in Table 8, Algebra teachers spend no more than one-third of their time teaching that course, and their performance in other courses is difficult or impossible to assess with test scores. Assessment of performance as a Geometry instructor is complicated by selection into the course; assessment of performance as a middle school math instructor is rendered impossible by the absence of student-teacher links in the North Carolina administrative data for middle school classrooms.

To assess the hypothesis that the adverse acceleration effects reported in this study primarily represent a decline in teacher "value-added," note that in our test score specifications point estimates indicate effect sizes of up to half a standard deviation. Such an effect could be accomplished only if acceleration were accompanied by a substantial substitution of very low-performing teachers for very high-performing teachers. The data presented in Tables 10 and 11 indicate that 72% of Algebra I sections offered in the acceleration year were taught by a set of individuals who also led 62% of such sections in the prior year. This discussion implies that the reduction in teacher quality required to explain the estimated adverse acceleration effect is too large to be plausible. [47]

As noted above, a comparison of results derived from CMS and Guilford County specifications yields further evidence that transitory mechanisms explain little of what we observe, at least in test score specifications. In CMS, the rapid reversal of the acceleration policy implies that roughly half of accelerated students received their treatment in the phase-in period, when instruction quality may have been compromised. In Guilford, by contrast, the treatment was offered for a longer period of time, implying that a smaller proportion of students received it in a transition year. Were transitory mechanisms largely responsible for the effect, we would expect to see point estimates closer to zero when focusing on across-cohort variation in Guilford County. As noted above, however, we obtain more negative results in test score specifications when we omit CMS and focus on Guilford. Results in higher-level course completion specifications, by contrast, are milder in Guilford County, suggesting that adaptation mechanisms may be important in ameliorating the longer-term effects of skipping a math course.

[47] Suppose that the set of "new" Algebra I teachers were drawn entirely from the bottom tail of the value-added distribution, with scores of -0.5. Suppose further that the teachers who cease teaching Algebra I after 2002 were drawn exclusively from the top tail of the value-added distribution, with scores of 0.5. Assuming the average quality of teachers leading Algebra I sections in both 2002 and 2003 remained the same, the anticipated effect on Algebra I test scores would be -0.23 standard deviations, smaller than any observed test score effect.

Thus, although we find strong evidence that CMS accommodated the surge in Algebra I enrollment associated with the 2003 acceleration by calling upon teachers with weaker credentials, the implied reduction in teacher quality is far too small to explain away the entire negative effect of acceleration on Algebra I test scores. Hence, we interpret our findings in light of the conceptual model presented above, namely that accelerating students into algebra is undesirable for many students because it shortens the time for them to master the skills they need to succeed in algebra and in subsequent math courses.[48]

Finally, we revisit the question of peer effects. As noted above, the standard assumption underlying instrumental variables analysis is that the "treatment" – in this case, acceleration – has an impact only on students who comply with the treatment. Acceleration may have had a negative impact on "always-taking" students who found their classrooms populated with lower-performing students. We note that in a model where these peer effects dominated direct effects on treated students we would expect stronger impacts in decile cells with higher baseline rates of treatment. In fact, we find the opposite: our estimates are most negative among the students with the lowest baseline early Algebra taking rate. We do not wish to dismiss the notion that peer effects matter – our empirical design is not well-suited to address that question – but the evidence is most consistent with direct negative impacts on accelerated students.

7. Conclusion

Algebra is often described as a "gateway" to higher-level mathematics. Because of the largely hierarchical nature of mathematics instruction, however, the gateway label could equally

[48] In unreported specifications, we find that acceleration had no significant impact on 8th grade end-of-grade test scores, which all students are expected to take even if they enroll in Algebra I in 8th grade. This suggests that any negative effects of acceleration on knowledge of non-algebraic concepts are offset by deeper knowledge of algebraic concepts. We also find no effect of acceleration into 7th grade algebra on 8th grade EOG scores, which would rule out a "disillusionment" mechanism whereby a negative experience in early testing leads students to reduce their investment in acquiring new math skills.

well be applied to a range of pre-algebra courses, geometry, or any other math subject in the hierarchy. Moreover, policy makers have often incorrectly interpreted the strong positive correlation between taking algebra early and later success as implying that waiting until high school to take it will limit students' opportunities to enroll in the higher level math courses needed for college. That interpretation is flawed because selection problems make it inappropriate to interpret the observed correlation as a causal relationship. Our empirical evidence, based on a clear policy intervention affecting nearly the entire distribution of students in two large school districts, avoids the selection bias, and shows that early administration of Algebra I – *when not preceded by broader reform of the entire math curriculum* – significantly weakens performance in that course and in Geometry, the typical follow-up course.

Our results imply, for example, that California's abortive initiative to increase the proportion of students taking introductory algebra in 8th grade from 59% to 100%, absent any wholesale reform in pre-algebra math courses, would have yielded adverse effects. Our results also cast doubt on assignment practices in school districts such as the District of Columbia, in which 4th grade math performance is significantly lower than in CMS based on NAEP assessments, yet 8th grade algebra placement is the norm.

We find substantial evidence that introducing algebra in middle school, rather than serving to equalize student outcomes, exacerbates inequality. Students at or above the 60th percentile of the initial achievement distribution appear to suffer few ill effects beyond the first year, and may be as many as 20 percentage points more likely to take a calculus course in high school when they are accelerated. At the lower end of the distribution, since calculus-taking rates are approximately zero regardless of Algebra I timing, the acceleration introduces costs without offering benefits. Patterns in the middle of the distribution are, not surprisingly, somewhere between these extremes.

One interpretation of these findings is that offering algebra for all 8th graders would be a worthy standard if additional reforms raised the performance of all students to the level where the 60th percentile North Carolinian middle school student lies today. More generally, this evaluation illustrates the hazards of basing policy initiatives on simple correlational evidence, without first taking steps to assess the validity of causal interpretation.

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Table 1: North Carolina Standard Course of Study Competency Goals (2003)

Course	Competency Goals
7th Grade Math	Understand and compute with rational numbers. Understand and use measurement involving two- and three-dimensional figures. Understand and use properties and relationships in geometry. Understand and use graphs and data analysis. Demonstrate an understanding of linear relations and fundamental algebraic concepts.
8th Grade Math	Understand and compute with real numbers. Understand and use measurement concepts. Understand and use properties and relationships in geometry. Understand and use graphs and data analysis. Understand and use linear relations and functions.
Introductory Mathematics (High School pre-Algebra)	Understand and compute with real numbers. Use properties and relationships in geometry and measurement concepts to solve problems. Understand and use graphs and data analysis. Understand and use linear relations and functions.
Algebra I	Perform operations with numbers and expressions (exponents, polynomials). Describe geometric figures in the coordinate plane. Collect, organize, and interpret data with matrices and linear models. Use relations and functions to solve problems.

Source: North Carolina, *NC Standard Course of Study*, 2003.
http://www.ncpublicschools.org/curriculum/mathematics/scos/2003/k-8/index, 1/12/12.

Table 2: Progression of math courses for two CMS cohorts

	2000/01 cohort (*n*=7,386)	2002/03 cohort (*n*=8,477)
Proportion of cohort taking Algebra I in 7th grade	10.1%	15.5%
Proportion of cohort taking Algebra I in 8th grade	31.9	46.6
Proportion of cohort ever observed taking Algebra I	87.8	87.4
Conditional on taking Algebra I in 8th grade:		
Proportion passing Algebra I EOC test in 8th grade	92.7	80.6
Proportion enrolled in Geometry in 9th grade	84.9	69.7
Proportion passing Geometry EOC in 9th grade	66.4	46.2
Proportion enrolled in Algebra II in 10th grade	78.2	63.8
Proportion passing Algebra II EOC in 10th grade	67.7	49.5
Proportion enrolled in Algebra II by 12th grade	89.8	79.6

Note: Cohorts are defined by the year in which they first enter 7th grade. For purposes of analysis in this paper, grade-repeating students are re-assigned to their original cohort.

Table 3: 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)
N	113,738	124,505	124,505	124,505
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 and decile fixed effects included but coefficients are not shown in this table. Sample utilized is the "apparent policy variation" sample of district/decile cells.

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

Table 4: Basic instrumental variables results

Independent variable	Algebra I Test Scores (IVQR)	Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Enrolled in Algebra I by 8th Grade	-0.370***	0.043*	-0.023	0.027
	(0.038)	(0.022)	(0.023)	(0.017)
Male	0.101***	0.037***	0.015***	0.053***
	(0.007)	(0.005)	(0.003)	(0.005)
African-American	0.008	0.014***	-0.015***	0.046***
	(0.009)	(0.004)	(0.002)	(0.005)
Hispanic	0.017	0.014***	-0.004	0.026***
	(0.017)	(0.005)	(0.004)	(0.006)
Other Race	0.102***	0.017***	0.017**	0.041***
	(0.016)	(0.005)	(0.005)	(0.009)
Free/Reduced Lunch	-0.182***	-0.060***	-0.076***	-0.099***
	(0.009)	(0.006)	(0.006)	(0.007)
N	194,425	194,425	194,425	194,425
Adjusted R^2	---	0.479	0.533	0.424

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Estimation is by two-stage least squares except as indicated. 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, decile, cohort and decile-by-cohort fixed effects included but coefficients are not shown in this table. Sample consists of all available cohorts, deciles, and districts.
*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 5: Alternate specifications

Modification relative to Table 4:	Algebra I Test Scores (IVQR)	Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Original results (n=194,425)	-0.370*** (0.038)	0.043* (0.022)	-0.023 (0.023)	0.027 (0.017)
Omit Charlotte-Mecklenburg (n=153,944)	-0.526*** (0.040)	0.021 (0.020)	0.011 (0.014)	0.037* (0.017)
Omit Guilford (n=168,939)	-0.312*** (0.069)	0.045 (0.033)	-0.125*** (0.036)	-0.036 (0.021)
Use district/decile cells with apparent policy variation (n=124,505)	-0.374*** (0.006)	0.091*** (0.021)	-0.066*** (0.014)	-0.026* (0.012)

Note: Standard errors, corrected for clustering at the decile-cohort-district level, in parentheses. Estimation is by two-stage least squares except as indicated. 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, decile, and cohort and fixed effects included but coefficients are not shown in this table. Decile-by-cohort effects also included in the first two models; in the final model these effects are used as instruments.
*** denotes a coefficient significant at the 0.1% level, ** the 1% level, * the 5% level.

Table 6: 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*** (0.080)	-0.240*** (0.015)	0.221*** (0.037)	-0.108*** (0.031)	-0.0627 (0.048)
Quintile 2 Student * Enrolled in Algebra I by 8th Grade	-0.456*** (0.034)	-0.397*** (0.006)	0.0921*** (0.022)	-0.081*** (0.012)	-0.0401* (0.016)
Quintile 3 Student * Enrolled in Algebra I by 8th Grade	-0.429*** (0.034)	-0.398*** (0.006)	0.0356* (0.015)	-0.085*** (0.018)	-0.0174 (0.020)
Quintile 4 Student * Enrolled in Algebra I by 8th Grade	-0.324*** (0.048)	-0.260*** (0.006)	0.0462** (0.014)	-0.0129 (0.018)	-0.015 (0.017)
Quintile 5 Student * Enrolled in Algebra I by 8th Grade	-0.306*** (0.092)	-0.140*** (0.010)	0.096*** (0.022)	0.0687** (0.026)	0.011 (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 7: Algebra Teacher Characteristics by School Year, Charlotte-Mecklenburg Schools

	1999/2000	2000/01	2001/02	2002/03	2003/04	2004/05
Number of Unique Teachers	183	222	198	249	228	228
Number of Sections per Teacher	2.038	1.905	2.051	2.378	2.232	2.031
Number of Students per Teacher	43.71	40.68	43.90	49.01	47.84	43.36
Enrollment-weighted mean characteristics						
Years of Experience	11.23	10.56	10.82	8.768	9.895	10.52
2 or Fewer Years' Experience	20.99%	26.85%	23.10%	31.57%	24.91%	27.14%
General Licensure Scores	0.217	0.183	0.138	0.097	0.217	0.100
Number of Teachers with General Scores	*165*	*192*	*171*	*214*	*195*	*203*
Math Licensure Scores	0.639	0.603	0.539	0.453	0.417	0.333
Number of Teachers with Math Scores	*33*	*42*	*35*	*58*	*48*	*42*

Note: Licensure test scores are standardized to have mean zero and standard deviation one for teachers taking the same test in the same year.

Table 8: Teacher Time Allocation in Charlotte-Mecklenburg Schools, 2001/02-2002/03

Subject Areas	2002/03		2001/02	
	Teacher Sections	Percentage	Teacher Sections	Percentage
Mathematics	961	79.1%	838	72.9%
Pre-Algebra & Lower Level	198	16.3%	393	34.2%
Algebra I	428	35.2%	251	21.8%
Geometry	66	5.4%	58	5.0%
Algebra II & Higher Level	79	6.5%	62	5.4%
Other Mathematics	190	15.6%	74	6.4%
Language	163	13.4%	201	17.5%
Science	34	2.8%	48	4.2%
Social Studies	26	2.1%	31	2.7%
Other Subjects	31	2.5%	31	2.7%
Total Observations	1215	100%	1149	100%

Note: Sample consists of teachers assigned to at least one section of Algebra I in 2002/03 who also appear in CMS course assignment records for 2001/02. “Other Mathematics” includes Technical Math I & II, Discrete Math, Integrated Math I & II, and Special Topics in Mathematics. “Other Subjects” includes computer science, health and physical education, vocational education, non-classroom activities (such as SAT preparation) and miscellaneous.

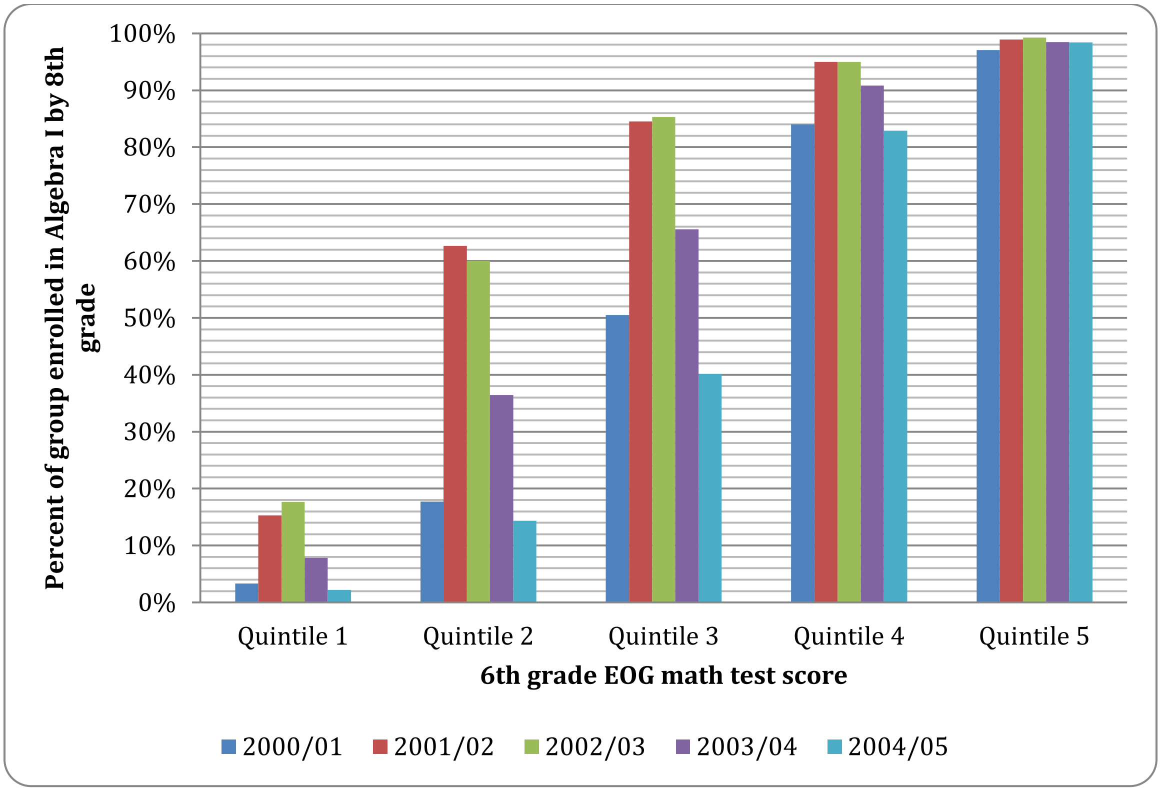

Figure 1: Probability of taking Algebra I by 8th grade, by 6th grade math test score quintile and year entering 7th grade, Charlotte-Mecklenburg Schools.

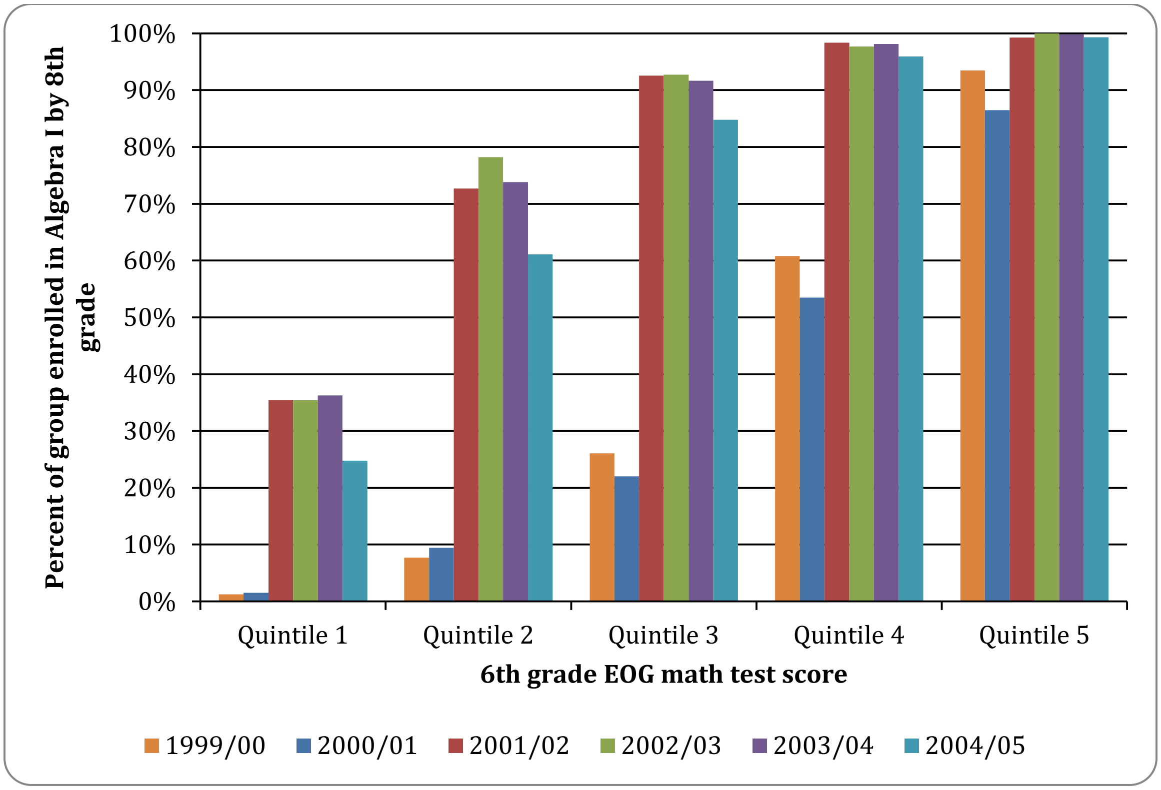

Figure 2: Probability of taking Algebra I by 8th grade, by 6th grade math test score quintile and year entering 7th grade, Guilford County Schools.

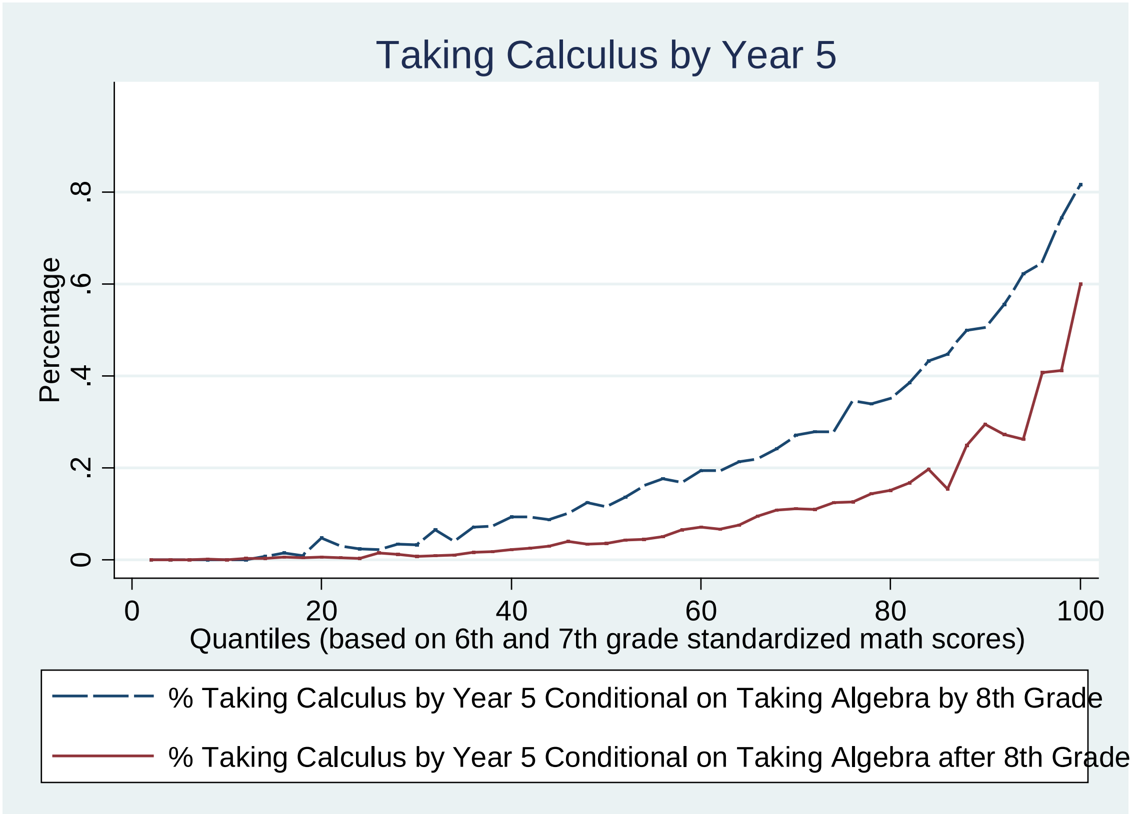

Figure 3: Probability of completing calculus within 5 years of beginning 8th grade, by percentile rank of mean 6th/7th grade math score, ten district sample.

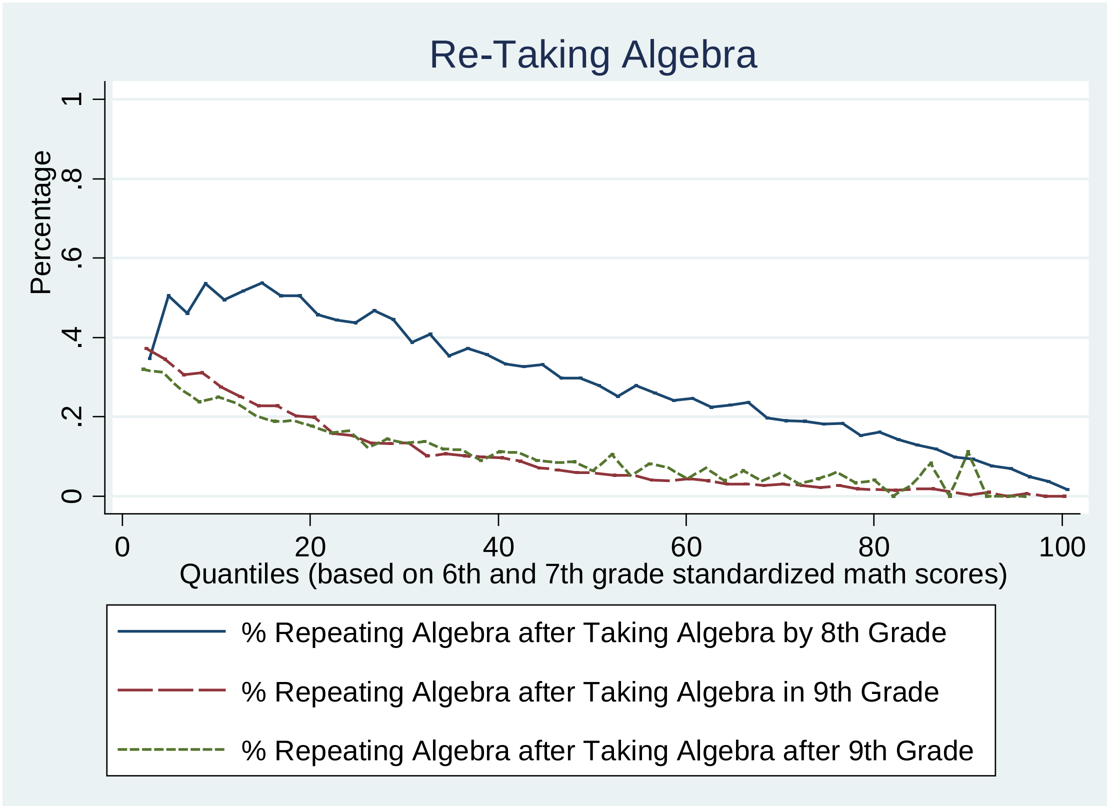

Figure 4: Probability of repeating Algebra I conditional on first taking Algebra I in a specific grade, by percentile rank of mean $6^{th}/7^{th}$ grade math score, ten district sample.

Table A1: Summary Statistics for Dependent Variables

School District	Algebra I test scores	Pass Algebra I by 10^{th} grade	Pass Geometry by 11^{th} grade	Pass Algebra II by 12^{th} grade
CMS	-0.093 (1.053)	72.1%	47.0%	51.9%
Wake County	0.520 (0.987)	81.4%	63.0%	65.3%
Guilford County	-0.226 (1.029)	77.1%	48.1%	54.7%
Forsyth County	-0.046 (1.032)	71.7%	46.2%	49.9%
Cumberland County	-0.143 (0.920)	65.1%	39.4%	43.2%

Note: In each district, sample is restricted to those students observed consistently for a period of 6 years beginning in 7^{th} grade, and who take Algebra I at some point during this period. Mean and standard deviation reported for test scores, sample proportion for all other variables.