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ABSTRACT

In 2002/03, the Charlotte-Mecklenburg Schools in North Carolina initiated a broad program of accelerating entry into algebra coursework. The proportion of moderately-performing students taking algebra in 8th grade increased from half to 85%, then reverted to baseline levels, in the span of just five years. We use this policy-induced variation to infer the impact of accelerated entry into algebra on student performance in math courses as students progress through high school. Students affected by the acceleration initiative scored significantly lower on end-of-course tests in Algebra I, and were either significantly less likely or no more likely to pass standard follow-up courses, Geometry and Algebra II, on a college-preparatory timetable. Although we also find that the district assigned teachers with weaker qualifications to Algebra I classes in the first year of the acceleration, this reduction in teacher quality accounts for only a small portion of the overall effect.

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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 eighth-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 a policy initiative introduced in one of the nation's best-performing large school districts, Charlotte-Mecklenburg Schools (CMS) in North Carolina, in 2002/03.[4] This initiative led students at many points in the initial math achievement distribution to take Algebra I earlier than they would have before the initiative. After maintaining the acceleration

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[1] This vote came at the urging of then-governor Arnold Schwarzenegger, who 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; Eddy Ramirez, "8th-Grade Algebra Requirement in California Gets Sidelined," *U.S. News and World Report*, December 29, 2008. Even before this vote, 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).

[4] Charlotte-Mecklenburg ranked first among the set of large American school districts with district-specific reports from the National Assessment of Educational Progress (NAEP) in terms of 4th grade mathematics scores for all test administrations between 2003 and 2009.

policy for two years, the district reversed course, reverting almost entirely to its previous placement pattern.[5] 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 age cohort.

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 seventh grade. Students who do so also meet the North Carolina State Board of Education's minimum standards for a college-preparatory course of study.[6] 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.[7]

Our results indicate that Charlotte-Mecklenburg's acceleration initiative worsened the Algebra I test scores of affected students and reduced their likelihood of progressing through a college-preparatory curriculum. 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, were 10 percentage points less likely to pass Geometry by the end of 11th grade, and were neither more nor less likely to pass Algebra II by the end of 12th grade, compared to otherwise similar students in seventh-grade cohorts that were not subjected to the policy. Similar patterns

[5] As discussed below, the clear negative effects of acceleration may explain why the district reversed course.
[6] 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.
[7] 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.

are observed among higher-performing students accelerated into Algebra I in 7th grade, and among lower-performing students accelerated into Algebra I in 9th grade.

These negative effects could be attributable to transition costs – including the need to staff an unusually large number of Algebra I courses during the year of acceleration – which might be expected to dissipate over time. We consider this possibility by examining the evolution of Algebra I teacher qualifications during the acceleration period, and by considering the experience of a second North Carolina district, Guilford County, which enacted a similar acceleration policy and did not reverse it. We conclude that transition costs can explain at most a small proportion of the overall effect; the main mechanism appears to reflect the opportunity cost of skipping a year's worth of pre-algebra mathematics.

These results 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, 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 caution that our results apply to the impact of varying 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]

[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.

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 rose 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 has been bolstered in part by unwarranted causal interpretation of 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). 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 a backlash against accelerating algebra into middle school. Opponents of accelerated algebra argue that too many students enter the course unprepared for advanced work

[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.

and may in fact fall behind their peers who had originally enrolled in less rigorous coursework. Loveless (2008) documents the poor math performance of some students enrolled in the course by 8th grade, and he notes the inattention to the problem of selection 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 poorly 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 scenario: otherwise identical students who take algebra on a traditional schedule.

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. 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

[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.

[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).

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. While 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 entry and the end of human capital investment. In this formulation, the opportunity cost of accelerating introduction to algebra is clear. Indeed, the question of algebra timing reduces to a matter of how much time to allocate to lower-order subjects. 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 are relatively unimportant in the production of algebra and higher-order skills, and if lower-order skills are similarly unimportant determinants of productivity.

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 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.

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

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

Algebra I acceleration is not the only curricular reform designed to improve mathematics achievement in states around the country. 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 coursetaking (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 the High School and Beyond dataset, using straightforward methods to address concerns about self-selection into higher-order courses. This 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

Our analysis makes use of data on students enrolled in the Charlotte-Mecklenburg Schools (CMS), provided by the North Carolina Education Research Data Center. During the period covered by our analysis, CMS was the largest school district in North Carolina, and one of the 25 largest in the United States, serving over 100,000 students. The district is racially diverse; in 2002/03, the first year of implementation for the Algebra acceleration program we study, 44%

[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.

of all students were black, 8% were Hispanic, and 4% Asian. About 40% of the district's students participated in the federal free and reduced price lunch program, slightly above the state average.

Charlotte-Mecklenburg has a strong reputation for mathematics performance. The district's fourth grade students ranked first among 18 major school districts in the 2009 National Assessment of Educational Progress (NAEP) math assessment. It was the only district in this group with 4th grade math scores significantly higher than the national average. To put this high performance in context, however, we note that CMS has a larger share of middle class students than do most large school districts because it is a large county-wide district that includes both suburban and urban neighborhoods.[15]

Beginning around 2002/03, CMS adopted an unusually aggressive policy to accelerate placement of middle and high school students in Algebra I.[16] The district not only broke from its past patterns of course-taking but also diverged dramatically from policies followed by most other districts in North Carolina. By all appearances, there were two precipitating factors that accounted for Charlotte-Mecklenburg's aggressive approach. First, 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. Second, the district's then superintendent 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

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[15] Ranked by performance of students eligible for federal school lunch subsidies, CMS placed 4th among the 18 districts. Nonetheless, CMS presents a case of algebra acceleration in a large urban district with relatively strong math performance.

[16] Although we have found no written statement of Charlotte-Mecklenburg's policy, its existence and influence have been substantiated by contemporaneous reporting and the recollection of administrators who worked in the system at the time of implementation.

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.[17]

Several other policy changes transpired in CMS during the period of our study. The districted 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. As we detail below, however, we obtain very similar results from an analysis of a similarly-timed algebra initiative in North Carolina's third-largest school district which did not simultaneously change its busing policy.

Figure 1 summarizes information on algebra-taking patterns in CMS for five age cohorts included in this study. It is based on a longitudinal sample of students described in more detail below.[18] For each student, we record the year in which he or she first takes North Carolina's end-of-course test in Algebra I.[19]

The initial cohort enrolled in 7th grade for the first time in 2000/01, two years prior to the initiative to accelerate algebra. For this cohort, rates of algebra-taking by 8th grade were high

[17] *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." [17] http://www.pbs.org/makingschoolswork/dwr/nc/smith.html, 4/5/11.

[18] 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 study not the impact of taking Algebra I by 8th grade, but the effect of taking the course within two years of beginning seventh grade.

[19] 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.

relative to the national average for high-performing students, but low for low-performing students. Ninety-seven percent of CMS students in the top quintile of the statewide 6th grade math score distribution were enrolled in Algebra I by 8th grade, compared to 75% of top quintile 8th graders nationwide, as recorded in the 2009 NAEP assessment (Walston and McCarroll 2010). By contrast, only 3% of CMS students in the lowest 6th grade math quintile had enrolled in Algebra I by 8th grade, compared to 13% in the national NAEP data.

The next entering cohort experienced a very different pattern. The rate of early algebra-taking shifted dramatically at lower points in the distribution. For students around the median, the likelihood of taking algebra 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%.[20]

Just two years after the push to accelerate algebra started, however, the district reversed course. By the time the cohort that entered 7th grade in 2004/05 had reached middle school, assignment patterns 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. This rapid reversal of the acceleration policy provides us with the first means of distinguishing acceleration effects from the effects of resegregation and school choice.

Figure 2 shows that the acceleration policy involved more than pushing students into Algebra I in 8th grade. For certain students, the likelihood of taking Algebra I by 7th grade also increased substantially over time. In the 2000/01 cohort, half of top quintile students, 9% of second quintile students, and 2% of middle quintile students took Algebra I as 7th graders. In the

[20] 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.

top quintile, the rate of 7th grade Algebra I enrollment rose monotonically, reaching 76% by 2005. In the next highest quintile, the 7th grade Algebra I-taking rate rose to nearly 40% in 2004 before retreating somewhat.

For students in the lowest two quintiles of 6th grade math test scores, the acceleration policy had its biggest effect in an increased propensity to take Algebra I by 9th grade. Figure 3 shows a peak among students entering 7th grade in 2000/01, who would have entered 9th grade in 2002/03 – the first year of the acceleration initiative – under normal rates of academic progress. Over 70% of lowest-quintile students in this cohort had taken Algebra I by 9th grade. But by the time the 2004/05 cohort came through, only just over a third of students in the bottom quintile were getting this treatment. Similar fluctuations occurred in the fourth and middle quintiles.

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 2000/01 and 2004/05 and were "at risk" for exposure to Charlotte-Mecklenburg Schools' algebra policy by virtue of being enrolled in that system in the year under study. For example, when evaluating the impact of exposure to Algebra I in 8th grade, we focus on those students enrolled in CMS the year after they begin 7th grade. We restricted the sample to students with valid scores on the state's standardized 6th 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. The size of our sample varies across specifications; in the analysis of acceleration into 8th grade algebra it includes a total of 35,339 students across five age cohorts.[21]

By design, the sample includes some individuals who are never observed as enrolling in Algebra I in our dataset. Thus our analysis may, for some students, confound the effect of accelerating algebra with the effect of taking it at all. But excluding non-algebra takers from the analysis might lead us to overstate the negative effects of the acceleration policy, to the extent the CMS policy change expanded the overall pool of Algebra I takers. In such a scenario, marginally-performing students would be included in the sample only in the acceleration period. Results obtained with a sample restricted to “ever-takers” confirms the existence of this bias.[22]

4.3 Identification Strategy

Our estimation strategy takes advantage of the significant policy changes that took place in Charlotte-Mecklenburg over just a few years. We exploit these changes to estimate local average treatment effects for taking Algebra I by the time students reach a certain grade. We begin by examining the effect of taking the course by 8th grade, and we then turn to the effects of accelerating Algebra I into 7th grade or 9th for students at different points in the initial achievement distribution. The estimated treatment effects are “local” to that set of students subjected to differing treatment status across cohorts within our six-cohort sample. For example,

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[21] Some of the students included in our sample may exit the dataset because they leave the CMS system, to attend a different public district, a private or charter 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. 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.

[22] For example, two-stage least squares analysis of the impact of enrollment in Algebra I by 9th grade using only a sample of ever-takers produces a coefficient of -1.36, more than twice the value of the estimate provided in the first column of Table 6 below.

our estimate of the effect of taking Algebra I by 8th grade applies primarily to students in the middle of the initial test score distribution given that students at the top of the distribution virtually always take Algebra I by 8th grade, while those at the bottom rarely do.[23]

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 6th 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. The outcome equation is of the form:

(4) $y_{idc} = \alpha_c + \alpha_d + \beta T_{idc} + \varepsilon_{idc}$

where y_{idc} is the outcome of interest for student i belonging to initial achievement decile d in cohort c, α_c and α_d are cohort and decile fixed effects, T_{idc} is an indicator for whether the student received the treatment – in this case, taking Algebra I by a certain point in their career – and ε_{idc} is an independent and identically distributed error term. Cohort fixed effects account for policy changes or other contemporaneous effects that apply to all students in a cohort, while decile fixed effects account for broad differences in outcome trajectories for students with differing initial ability. The use of decile effects rather than a linear control for test score 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 (4), arguing that controls for prior achievement adequately correct for unobserved determinants of the outcome that also correlate with the treatment indicator, implying that β is an unbiased

[23] Our results may have some bearing on the most prominent algebra policy debate, regarding California's initiative to require 8th grade algebra. Note, however, that this initiative is most relevant for the bottom 40% of the math ability distribution in that state, since the rate of 8th grade algebra-taking is already close to 60%. Our estimate of the effect of 8th grade algebra acceleration pertains more directly to students towards the middle of the ability distribution. Our results on accelerating low-performing students into 9th grade algebra may provide some additional insight as to the effects of acceleration in that subset of the population.

estimate of the true treatment effect. To assess this argument, we present OLS estimates of equation (4) 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:

$$(5)\ T_{idc} = \gamma_c + \gamma_d + \sum_{d=1}^{10}\sum_{c=1}^{5} \delta_{dc} + \eta_{idc}$$

where γ_c and γ_d are cohort and decile fixed effects, the δ_{dc} terms are cohort-by-decile fixed effects, and η_{idc} is a second error term. Predicted values of equation (5) are then used in place of actual treatment status in equation (4). Effectively, the estimation strategy associates across-cohort-and-decile variation in the propensity to take Algebra I by a certain grade level with across-cohort-and-decile variation in the outcome of interest. We attribute a positive (negative) effect to acceleration if students subjected to a higher probability of earlier algebra than others in the same initial ability decile in another cohort exhibit superior (inferior) performance in Algebra I and subsequent math courses. Because the identifying variation in algebra timing is at the cohort-by-decile level, we cluster standard errors at that level.

In principle, we would like to estimate the impact of early progression to Algebra I on performance in that course and subsequent math topics. 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 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 the selection problem, by comparing the progress of students in two cohorts. Consider first the cohort of CMS 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 first year of 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.[24] This worsening record of course completion for the accelerated cohort, presumably 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. While this 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

[24] 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 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.

analysis of later cohorts who progressed through high school after the acceleration had ended reveals significantly lower rates of ever-taking.[25]

Rather than attempt to estimate a selection-correction model, or use any other procedure to account explicitly for the selection of marginal students out of the sample of course-takers, we adopt two strategies. Our first strategy is to redefine our outcome variables such that they are theoretically observed for all students, whether they enroll in a course or not. 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.[26] Students who never take a course are coded as not having passed that course.

Because both the outcome and treatment variables are binary, the most appropriate means of simultaneously estimating equations (4) and (5) is by bivariate probit. In such a case, both y_{idc} and T_{idc} can be thought of as latent variables, with the observed binary outcome dependent on whether the latent variable exceeds a particular value. For ease of interpretation, we also present two-stage least-squares results.

Our second strategy for addressing selection into the sample of math course takers applies to specifications estimating the effect of acceleration on Algebra I test scores, which are, by definition, unobserved for students who never enroll in Algebra I. We adopt a strategy derived from Neal and Johnson (1996). We assume that students who never enroll in Algebra I would have received a test score that was below-average conditional on observables. Under this

[25] 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.

[26] 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.

assumption, we impute a very low test score for these students, and estimate models based on the Chernozhukov and Hansen (2007) Instrumental Variable Quantile Regression (IVQR) estimator.[27] Following Neal and Johnson's logic, 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.

5. Results

We present the results of three related interventions – accelerating certain students into Algebra I in 7th, 8th, or 9th grade – on Algebra I test scores and indicators for whether students pass Algebra I, Geometry, or Algebra II on a timetable that will permit them to complete the sequence by the time they would ordinarily graduate from high school. To set the stage, Table 3 presents the results of simple OLS specifications examining the basic relationship between Algebra I timing and the four outcomes. These estimates should not be interpreted as causal effects, even though they include indicators that restrict comparison to students in the same decile of the 6th grade math test distribution. 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 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.[28]

Consistent with earlier studies, our OLS specifications associate earlier placement in Algebra I with better outcomes. Students who complete Algebra I by 8th grade score 0.18

[27] 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.

[28] 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.

standard deviations better on the end-of-course test and are significantly more likely to attain passing scores on 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 21 percentage points higher among students who complete Algebra I by 8th grade, conditional on 6th 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 more than two deciles in the distribution. To reiterate our previous argument, however, these OLS estimates, like many prior estimates in the literature, are very likely to be contaminated by selection bias.

5.1 The impact of 8th grade algebra on moderately-performing students

We focus primarily on the effects of offering Algebra I in 8th grade to moderately-performing students, after which we discuss the estimated effects of the other acceleration patterns in CMS. Table 4 shows instrumental variable estimates of the impact of taking Algebra I by 8th grade.[29] 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 as well as bivariate probit results for the three binary outcomes. Each model controls for 6th grade test score decile and cohort fixed effects, and uses a set of cohort-by-decile fixed effects as instruments. The instruments rely on across-cohort variation in the probability of students in a given decile taking Algebra I by a specific point in time. First stage results uniformly indicate a sufficient amount of variation to assuage potential concerns about weak instruments.[30]

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

[30] When estimated by 2SLS without imputation, the first stage of the Algebra I test score regression yields an *F*-statistic on excluded instruments of 46.0 with a *p*-value less than 0.0001. The first-stage regressions across all IV specifications are similar to one another, but the *F*-statistics vary slightly with changes in sample.

In three cases out of four, the results contrast starkly with the basic patterns revealed in our OLS analysis and previous research. Accelerated students score 32% of a standard deviation *lower* on their Algebra I end-of-course tests and are significantly *less* likely to pass courses in Geometry on a college-preparatory schedule.[31] Two-stage least squares estimates indicate that accelerated students are 10 percentage points less likely to pass the Geometry EOC test within five years of beginning 7th grade, and are neither more nor less likely to pass Algebra II within six years.[32] Bivariate probit results indicate similarly large effects: a student with a 50% chance of passing Geometry by the time he or she completes high school at baseline is estimated to have only a 39% chance of completion if accelerated into Algebra I in 8th grade.[33]

The two-stage least squares estimate of the impact of acceleration on passing Algebra I is positive and statistically insignificant; the bivariate probit coefficient is likewise positive but not significant. Given that the acceleration apparently reduces students' Algebra I test scores, the positive point estimates on passing the course (within four years of beginning 7th grade) suggest that students retake the test and pass it the second time around. This pattern is consistent with the basic information in Table 2, which indicates that retaking Algebra I became more commonplace in the wake of the acceleration initiative. In sum, these results show that cohorts exposed to Algebra I on the accelerated schedule implemented by Charlotte-Mecklenburg fared no better, and sometimes significantly worse, in subsequent math courses. This finding is

[31] 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.

[32] The lack of significant impacts in Algebra II may seem incongruous given the Geometry results, given that the former is usually considered a prerequisite for the latter. Data confirm that CMS often promoted students to Algebra II when they had failed to receive a passing score on the Geometry exam.

[33] Probit regressions utilize the standard normal distribution to derive predicted probabilities. Individual coefficients indicate the change in Z-score associated with a one-unit change in the corresponding variable. This change in Z-score can be converted into a change in predicted probability by first specifying a baseline probability. In general, a given probit coefficient yields the largest predicted change in probability for individuals with predicted probabilities near 50% at baseline. Note that the overall probability of passing Geometry for CMS students in our sample is 48.5%. Summary statistics for all dependent variables appears in Appendix Table A1.

consistent with the hypothesis that, by taking Algebra I earlier, these students ended up having insufficient grounding in pre-algebraic math.[34]

5.2 The impact of accelerating Algebra I for high and low achievers

Mirroring Table 4, Tables 5 and 6 examine the impact of accelerating high-performing students into Algebra I in 7th grade and low-performing students into Algebra I in 9th grade, respectively. Table 5 omits results for specifications examining whether students pass Algebra I by 10th grade; the success rate among students subjected to acceleration into Algebra I in 7th grade is sufficiently high that there is almost no variation in the outcome available to analyze.[35]

Accelerated placement into Algebra I in 7th grade was applied primarily to students in the top two quintiles of the 6th grade math score distribution. Results in Table 5 indicate that the students receiving this accelerated treatment experienced a decline in Algebra I EOC test scores comparable to the declines experienced by their counterparts accelerated into Algebra I in 8th grade.[36] Point estimates in course completion specifications differ across the two-stage least squares and bivariate probit specifications. Bivariate probit results suggest no significant impact of acceleration on Geometry passage, and a positive significant effect on Algebra II pass rates. The case for offering Algebra I to high-achieving students in 7th grade thus appears to be stronger than the case for offering the course to moderate-performers in 8th grade. When interpreting the bivariate probit results, one must bear in mind that the baseline success rate

[34] In additional specifications not reported here, we find that the negative effects of acceleration on the likelihood of passing Algebra II are twice as large for boys as for girls. We find a smaller differential effect on Algebra I test scores and no significant gender differential on other outcomes.

[35] These specifications, available on request, indicate a significant negative effect of acceleration on the propensity to pass Algebra I by 10th grade. We infer that this effect reflects particularly negative effects on middle-decile students placed in 7th grade Algebra.

[36] In this case, estimation of the test score regressions by 2SLS rather than IVQR yield a negative coefficient of nearly four times greater magnitude. This suggests a significant problem of attrition of lower-performing students from the sample in years where acceleration was not applied to higher-performing students.

among high-performing students is very high, implying that large probit coefficients translate into relatively small actual effects on predicted probabilities of success.

At the other end of the spectrum, students accelerated into Algebra I in 9th grade, drawn primarily from the lowest two deciles of the 6th grade math test distribution, show strong signs of negative impact. In this group, acceleration is associated with a half-standard deviation decline in Algebra I EOC scores, significant reductions in the likelihood of passing Geometry, and only weak evidence of any positive impact. A student with a 50% chance of passing Geometry by 11th grade at baseline is estimated to have a 40% chance if accelerated. The likelihood of passing Algebra II by 12th grade is higher in the bivariate probit specification, with marginal significance that is not replicated in the two-stage least squares specification.

Virtually all CMS students affected by the acceleration initiative exhibited poorer Algebra I performance as a result. Students with high initial achievement appear to have suffered only modest subsequent adverse effects, if any. Lower-performing students, even those placed in Algebra I in the 9th grade rather than the 10th, appear to suffer a significant reduction in the likelihood of passing Geometry on a college-preparatory timetable. The time path of the policy, showing the district reversed acceleration for low- and moderately-performing students but maintained it at the high end, suggests that the district may well have correctly perceived the pattern of effects.

6. Robustness checks

These results are derived from a fairly strong identification strategy. We exploit variation in cohort exposure to acceleration across deciles, as well as the differential actions taken by CMS after 2004 – maintaining acceleration for some types of students but reversing course for others. As noted above, however, Charlotte-Mecklenburg undertook other significant policy changes at the same time as the algebra initiative. Replacing its former practice of busing for racial balance with a school choice plan could potentially have affected the math performance of some students.

To assess this threat to validity, we perform both verification and falsification tests. For verification, we evaluate an Algebra I placement policy change undertaken by another major North Carolina school district during the time period covered by our analysis. In these verification tests, we use two-stage estimators identical to that employed in the analysis of patterns in CMS.[37]

As a falsification test, we examine test score patterns in districts that did *not* appear to adopt any significant policy change over this time period, to see if the patterns observed in Charlotte-Mecklenburg show up, which should not be the case if our estimates for CMS are indeed the result of the district's algebra policy. Here we use a two-sample instrumental variable estimator.[38] The first stage is estimated using data from CMS. In the second stage of this procedure, using data from one of three other districts, we replace information regarding a student's Algebra I placement with a variable measuring the likelihood that a student in the same state test score decile and cohort would be placed in Algebra I by a certain point in time *had that student been enrolled in CMS*. This application of two-sample instrumental variables analysis is nonstandard, in the sense that we do not expect the procedure to produce significant results. In theory, the instrumental variable is irrelevant in the second sample, and therefore predicted values based on the instruments should not be correlated with outcomes. When analyzing end-

[37] In this alternative district, we incorporate one additional cohorts' worth of data, for students entering 7th grade in 1999/2000. This cohort is not available for analysis for CMS because of coding anomalies in that district in the 1999/2000 school year.

[38] Since the bivariate probit model requires observations to be identical in both equations, our falsification tests exclusively use two-sample-two-stage least squares estimators (Inoue and Solon, 2010). Standard errors are computed using the method of Murphy and Topel (1985).

of-course test scores, we use a reduced-form version of the IVQR procedure applied to the two-sample setting.

6.1 Verification test: Guilford County

The Guilford County school system is the state's third largest, serving the cities of Greensboro and High Point as well as surrounding areas. Figure 4 shows that Guilford pursued a policy of acceleration on a similar timetable to CMS. A student's likelihood of completing Algebra I by 8th grade increased substantially between the 2001 and 2002 cohorts. Guilford's acceleration was actually more dramatic 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%. In contrast with CMS, which had reverted to baseline by the time the 2005 cohort entered 7th grade, Guilford's acceleration is still quite apparent in this last cohort.

Table 7 shows the results of IVQR, two-stage least-squares, and bivariate probit estimates of the effect of 8th grade Algebra I acceleration in Guilford County. Results here are similar to those in CMS in many respects. The estimated impact of acceleration on Algebra I EOC test scores is statistically significant, negative, and slightly larger than the point estimate obtained in CMS. Two-stage least-squares estimates suggest that acceleration raised the likelihood of passing Algebra I by 10th grade, though the bivariate probit coefficient is insignificant. Effects on passing Geometry and Algebra II are uniformly negative and significant. The negative effect on Geometry passage resembles CMS results, but the Algebra II results present a contrast. This may reflect the tendency for CMS to promote students to Algebra II even in cases where they did not first pass Geometry.

Generally speaking, then, the Guilford results lend support to the conclusion that accelerating low-to-moderately-performing students into Algebra I in 8^{th} leads to at best negligible and at worst persistent negative effects on mathematics performance. The Guilford results particularly assuage the concern that the CMS patterns might reflect the impact of the nearly-simultaneous cessation of racial busing and move toward school choice. The similarity of coefficients in CMS and Guilford also factor into our discussion of transitory and permanent causal mechanisms below.

6.2 Falsification tests

A proper falsification tests looks for (spurious) evidence of treatment effects in a sample that was not exposed to the treatment. In this context, we examine the relative performance of students in the 6^{th} grade test score deciles and cohorts that would have been subjected to acceleration had they enrolled in CMS, but who attended different districts.

To ensure this is a valid test, we must first verify that students in other districts were not in fact exposed to the acceleration policy, or to any other simultaneous initiative affecting the same deciles in the same cohorts. Table 8 shows that this is in fact a debatable point in two of the three cases considered here. The results depicted here are derived from individual-level probit equations of the form:

(6) $T_{idc} = \gamma_d + \gamma_c + \beta \hat{T}_{dc}^{CMS} + \eta_{idc}$

where T_{idc} is an indicator for whether student i in cohort c and decile d completed Algebra I by the end of 8^{th} grade, and $\hat{T}_{dc}^{CMS}$ is the treatment rate for students in the same cohort and decile in

CMS.[39] If there were no relationship between placement patterns in student *i*'s district and those in CMS, we would expect the coefficient β to be indistinguishable from zero.

Table 8 presents estimates of β using three alternate school districts. In only one of three cases – Winston-Salem/Forsyth (WSF) – do we fail to reject the hypothesis that the estimate equals zero. In Wake County, which is now the state's largest district, the coefficient is negative and significant, indicating that the cohort/decile cells subjected to acceleration in CMS were subjected to *deceleration* in Wake. In Cumberland County – the state's fifth-largest district, serving the Fayetteville area – the estimate of β is in fact larger in absolute value than in Wake and significant at a similar level.

As a result of this evidence, we are not fully confident that Wake or Cumberland serve as valid falsification tests.[40] Nonetheless, in Table 9 we report the results of the proposed two-sample procedure for all three counties.

The first column of results examines Algebra I test score patterns in the three alternate districts, using a reduced-form two-sample version of quantile regression with instrumental variables. Standard errors in these models are underestimated, but based on our experience with Murphy-Topel (1985) adjustment in later models, we anticipate that adjustment would not alter the conclusions. The significant negative effects recorded in CMS and Guilford County are not present here. In fact, the Wake and Cumberland coefficients are statistically significant and opposite in sign to the CMS result – consistent with the observations in Table 8 above. The Winston-Salem/Forsyth point estimate is roughly one-tenth the magnitude of the CMS coefficient and statistically insignificant. These findings support our conclusion from the CMS

[39] Standard errors in these equations are estimated using the Huber-White method for clustering at the cohort/decile level.
[40] Estimating equation (6) for Guilford County, which by comparison of Figures 1 and 4 appear to have pursued similar acceleration policies, yields a positive coefficient.

patterns that accelerated students scored significantly worse on the Algebra I EOC exam than observationally similar counterparts who were not accelerated.

The remaining columns check the course passage outcomes in the falsification districts. The general pattern in CMS – weak positive effects for Algebra I, stronger negative effects for Geometry, and no effects for Algebra II – are not present in any of the comparison districts. In Winston-Salem/Forsyth, the district that comes closest to being a "true" falsification test, point estimates are uniformly insignificant. In Cumberland County, which showed the strongest evidence of subjecting cohorts to the opposite of their counterparts in CMS, point estimates are universally positive and significant – another indication that deceleration, rather than acceleration, is most conducive to improving longer-run math performance. Wake County exhibits a negative significant coefficient only in the Algebra I specification, which does not match up with CMS results.

7. Potential causal mechanisms

As noted above, one explanation for our finding that early exposure to Algebra I was detrimental to students in CMS is that the acceleration caused students to miss important pre-algebra course material.[41] Another explanation is that the district's need for additional capacity in Algebra I caused it to sacrifice instruction quality. In the first year of the acceleration initiative, the district needed to offer algebra instruction to an unusually large group of students – the last un-accelerated cohort and the first accelerated cohort. Between 2001/02 and 2002/03, the number of CMS students taking the Algebra I EOC exam increased from under 9,000 to over

[41] 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.

11,000. It is important to distinguish between the two alternative explanations for the exposed cohorts' observed poor performance – insufficient pre-algebra grounding or decline in instruction quality.[42] The insufficient pre-algebra explanation would imply that a permanent shift to accelerated algebra would generate the same types of results we observe in Charlotte-Mecklenburg's brief policy experiment. In contrast, if the negative impacts are the result of a temporary fall in instruction quality, the apparent cost of acceleration would be confined to the phase-in period.[43]

The increased demand for Algebra I instruction could have affected the quality of instruction in many respects. Administrators could have responded by boosting class sizes, by assigning less-qualified teachers to the course, or by reallocating highly-qualified instructors away from the subjects they would otherwise teach. Table 10, which tracks the number and qualifications of Algebra I teachers in CMS over time, shows that administrators avoided the first type of response. Between 2002 and 2003, 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 2003 than it was in 2002.[44]

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[42] 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.

[43] 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.

[44] 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).

Table 10 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 under 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 11 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 higher-level subjects held steady or increased. Presuming 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.[45] 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).

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

[45] 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 11, 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.

reduction in teacher quality required to explain the estimated adverse acceleration effect is too large to be plausible. [46]

A comparison of results derived from CMS and Guilford County specifications yields further evidence that transitory mechanisms explain little of what we observe. 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 in Guilford County. As noted above, however, the point estimates are, if anything, more negative in Guilford County than in CMS.

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.

8. 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

[46] 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.

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 the timing of Algebra and later outcomes as implying that failure of students to take the course before high school adversely affects their subsequent ability to enroll in the higher level math courses needed for college. That interpretation is incorrect because selection problems make it inappropriate to conclude that the observed correlation reflects a causal relationship. Our empirical evidence, based on a clear policy intervention affecting nearly the entire distribution of students in one of the nation's largest 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 worsens performance in that course and in Geometry, the typical follow-up course.

Our results imply, for example, that California's proposed 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 worsen rather than improve the college-readiness of affected students. 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 worse than in CMS, according to NAEP assessments, yet 8th grade algebra placement is the norm.

We must be more cautious, however, in evaluating the impact of the past expansion of enrollment in 8th grade algebra from one-sixth to one-third of the nation's students over the past few decades. Presumably, the students affected by this expansion were drawn largely from the top two quintiles of the math achievement distribution. As Figure 1 shows, our identifying variation comes almost entirely from students at lower points in the achievement distribution.

Assessing the impact of placing higher-achieving students in algebra in 8th grade would require observing policy variation within that group.

The optimal rate for taking algebra by 8th grade is undoubtedly greater than zero. Indeed, our results indicate that the increase in Algebra I taking among 7th graders in CMS has had no significant adverse long-term effects. Our evidence also suggests that the optimal rate of 8th grade algebra-taking, in a population equivalent to that in CMS, is at or below the observed baseline rate around 50%.

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:	92.7	80.6
Proportion passing Algebra I EOC test in 8th grade		
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	0.176*** (0.037)	0.239*** (0.011)	0.141*** (0.013)	0.210*** (0.010)
Year entered 7th grade (2000 omitted)				
2001				
2002	-0.071* (0.036)	-0.033*** (0.009)	-0.059*** (0.014)	-0.057*** (0.011)
2003	-0.108*** (0.035)	-0.028*** (0.009)	-0.053*** (0.014)	-0.074*** (0.011)
2004	-0.086** (0.035)	-0.016** (0.008)	-0.046*** (0.013)	-0.055*** (0.009)
2005	0.062 (0.048)	0.013 (0.011)	-0.020 (0.013)	-0.026** (0.010)
6th grade math test score decile (lowest omitted)				
Second lowest	0.235*** (0.050)	0.169*** (0.018)	0.036*** (0.012)	0.067*** (0.011)
Third lowest	0.423*** (0.056)	0.286*** (0.019)	0.087*** (0.016)	0.134*** (0.015)
Fourth lowest	0.587*** (0.047)	0.400*** (0.018)	0.145*** (0.019)	0.213*** (0.014)
Fifth lowest	0.801*** (0.045)	0.473*** (0.023)	0.269*** (0.017)	0.290*** (0.015)
Sixth lowest	0.982*** (0.043)	0.509*** (0.021)	0.388*** (0.018)	0.385*** (0.015)
Seventh lowest	1.219*** (0.050)	0.547*** (0.019)	0.532*** (0.018)	0.460*** (0.014)
Eighth lowest	1.523*** (0.053)	0.566*** (0.020)	0.666*** (0.019)	0.564*** (0.015)
Ninth lowest	1.839*** (0.055)	0.576*** (0.021)	0.758*** (0.017)	0.617*** (0.015)
Highest	2.463*** (0.057)	0.573*** (0.021)	0.816*** (0.018)	0.663*** (0.016)
N	32,020	35,339	35,339	35,339
Adjusted R^2	0.606	0.423	0.499	0.392

Note: Standard errors, corrected for clustering at the decile-cohort level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution for Algebra I and Algebra II, and as obtaining the state's official passing score in Geometry. Grade-retained students are kept with their original cohort.
*** denotes a coefficient significant at the 1% level, ** the 5% level, * the 10% level.

Table 4: Instrumental Variable Estimates of the Impact of Acceleration into Algebra I in 8th Grade

Independent variable	Algebra I Test Score	Pass Algebra I by 10th grade		Pass Geometry by 11th grade		Pass Algebra II by 12th grade	
	IVQR w/imputation	2SLS	BP	2SLS	BP	2SLS	BP
Enrolled in Algebra I by 8th Grade	-0.324***	0.069**	0.046	-0.095***	-0.295***	-0.002	0.057
	(0.027)	(0.030)	(0.098)	(0.025)	(0.091)	(0.023)	(0.090)
N	35,339	35,339	35,339	35,339	35,339	35,339	35,339
Adjusted R^2		0.407		0.474		0.372	

Note: Standard errors, corrected for clustering at the decile-cohort level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution for Algebra I and Algebra II, and as obtaining the state's official passing score in Geometry. Grade-retained students are kept with their original cohort. All models control for 6th grade math test score decile and cohort fixed effects, and instrument for Algebra I enrollment by 8th grade using a set of decile-by-cohort indicators. Columns headed "2SLS" are estimated by two-stage least squares; columns headed "BP" are estimated by bivariate probit. Column headed "IVQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 3,319 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method. *** denotes a coefficient significant at the 1% level, ** the 5% level, * the 10% level.

Table 5: Instrumental Variable Estimates of the Impact of Acceleration into Algebra I in 7th Grade

Independent variable	Algebra I Test Score	Pass Geometry by 11th grade		Pass Algebra II by 12th grade	
	IVQR w/imputation	2SLS	BP	2SLS	BP
Enrolled in Algebra I by 7th Grade	-0.274***	-0.067*	0.066	0.055	0.228**
	(0.028)	(0.038)	(0.119)	(0.034)	(0.112)
N	37,180	37,180	37,180	37,180	37,180
Adjusted R^2		0.459		0.353	

Note: Standard errors, corrected for clustering at the decile-cohort level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution for Algebra I and Algebra II, and as obtaining the state's official passing score in Geometry. Grade-retained students are kept with their original cohort. All models control for 6th grade math test score decile and cohort fixed effects, and instrument for Algebra I enrollment by 8th grade using a set of decile-by-cohort indicators. Columns headed "2SLS" are estimated by two-stage least squares; columns headed "BP" are estimated by bivariate probit. Column headed "IVQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 4,612 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method. *** denotes a coefficient significant at the 1% level, ** the 5% level, * the 10% level.

Table 6: Instrumental Variable Estimates of the Impact of Acceleration into Algebra I in 9th Grade

Independent variable	Algebra I Test Score	Pass Algebra I by 10th grade		Pass Geometry by 11th grade		Pass Algebra II by 12th grade	
	IVQR w/imputation	2SLS	BP	2SLS	BP	2SLS	BP
Enrolled in Algebra I by 9th Grade	-0.553***	-0.017	-0.044	-0.191***	-0.262*	-0.016	0.280*
	(0.035)	(0.076)	(0.101)	(0.040)	(0.155)	(0.029)	(0.161)
N	33,553	33,553	33,553	33,553	33,553	33,553	33,553
Adjusted R^2		0.380		0.480		0.401	

Note: Standard errors, corrected for clustering at the decile-cohort level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution for Algebra I and Algebra II, and as obtaining the state's official passing score in Geometry. Grade-retained students are kept with their original cohort. All models control for 6th grade math test score decile and cohort fixed effects, and instrument for Algebra I enrollment by 8th grade using a set of decile-by-cohort indicators. Columns headed "2SLS" are estimated by two-stage least squares; columns headed "BP" are estimated by bivariate probit. Column headed "IVQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 2,316 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method. *** denotes a coefficient significant at the 1% level, ** the 5% level, * the 10% level.

Table 7: Verification Test using District with Similar Acceleration Policy (Guilford Co.)

	Algebra I Test Score	Pass Algebra I by 10th grade		Pass Geometry by 11th grade		Pass Algebra II by 12th grade	
Independent variable	2SLS	2SLS	BP	2SLS	BP	2SLS	BP
Enrolled in Algebra I by 8th Grade	-0.415***	0.109***	0.138	-0.067**	-0.359***	-0.077***	-0.305***
	(0.011)	(0.027)	(0.145)	(0.029)	(0.137)	(0.024)	(0.109)
N	25,831	25,831	25,831	25,831	25,831	25,831	25,831
Adjusted R^2	0.597	0.348		0.462		0.322	

Note: Standard errors, corrected for clustering at the decile-cohort level, in parentheses. Algebra I test score is taken from the student's first test administration. Course passage is defined as obtaining a standardized test score at or above the 20th percentile of the statewide distribution for Algebra I and Algebra II, and as obtaining the state's official passing score in Geometry. Grade-retained students are kept with their original cohort. All models control for 6th grade math test score decile and cohort fixed effects, and instrument for Algebra I enrollment by 8th grade using a set of decile-by-cohort indicators. Columns headed "2SLS" are estimated by two-stage least squares; columns headed "BP" are estimated by bivariate probit. Column headed "IVQR w/imputation" applies the Neal and Johnson (1996) method of imputing poor performance for 1,520 non-Algebra I-takers and estimating using the Chernozhukov and Hansen (2005) method. *** denotes a coefficient significant at the 1% level, ** the 5% level, * the 10% level.

Table 8: Assessing the Validity of Falsification Tests

	Dependent variable: Enrollment in Algebra I by 8^{th} grade		
Independent variable	Wake County (Raleigh)	Forsyth County (Winston-Salem)	Cumberland County (Fayetteville)
Proportion of CMS students in same cohort/decile who take Algebra I by 8^{th} grade	-0.437** (0.201)	0.365 (0.353)	-0.700** (0.341)
N	35,076	15,192	16,074

Note: Equations are estimated by probit and include cohort and decile fixed effects. Standard errors, corrected for clustering at the cohort/decile level, in parentheses.
** denotes a coefficient significant at the 5% level.

Table 9: Falsification Tests using Three Alternate Districts

Coefficient on 8th grade Algebra I-taking rate in same decile/cohort, CMS in:	Dependent Variable			
	Algebra I test score	Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th grade
Wake County	0.213***	-0.049**	-0.034	-0.006
N=35,076	(0.006)	(0.022)	(0.025)	(0.027)
Forsyth County	0.084	-0.044	-0.028	0.009
N=15,192	(0.031)	(0.037)	(0.038)	(0.041)
Cumberland County	-0.036	0.104**	0.093**	0.126***
N=16,074	(0.054)	(0.041)	(0.040)	(0.044)

Note: Standard errors in parentheses. Test score equation estimated using a reduced-form instrumental variable quantile procedure; standard errors unadjusted. Remaining equations estimates by TS2SLS, with standard errors computed using the Murhpy-Topel (1985) method, as applied to two-sample two-stage least squares by Inoue and Solon (2010).
*** denotes a coefficient significant at the 1% level, ** the 5% level, * the 10% level.

Table 10: 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 11: 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.

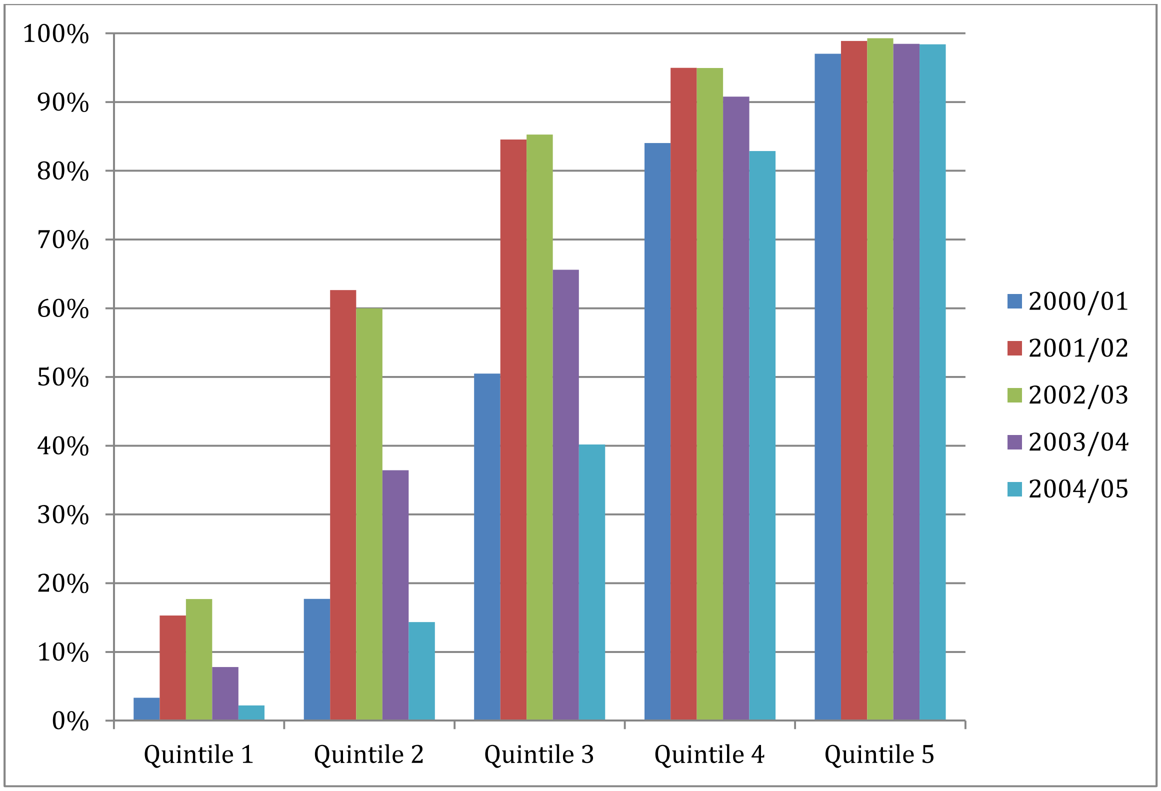

Figure 1: Probability of taking Algebra I by 8th grade, by 6th grade math test score quintile and year entering 7th grade, Charlotte-Mecklenburg Schools.

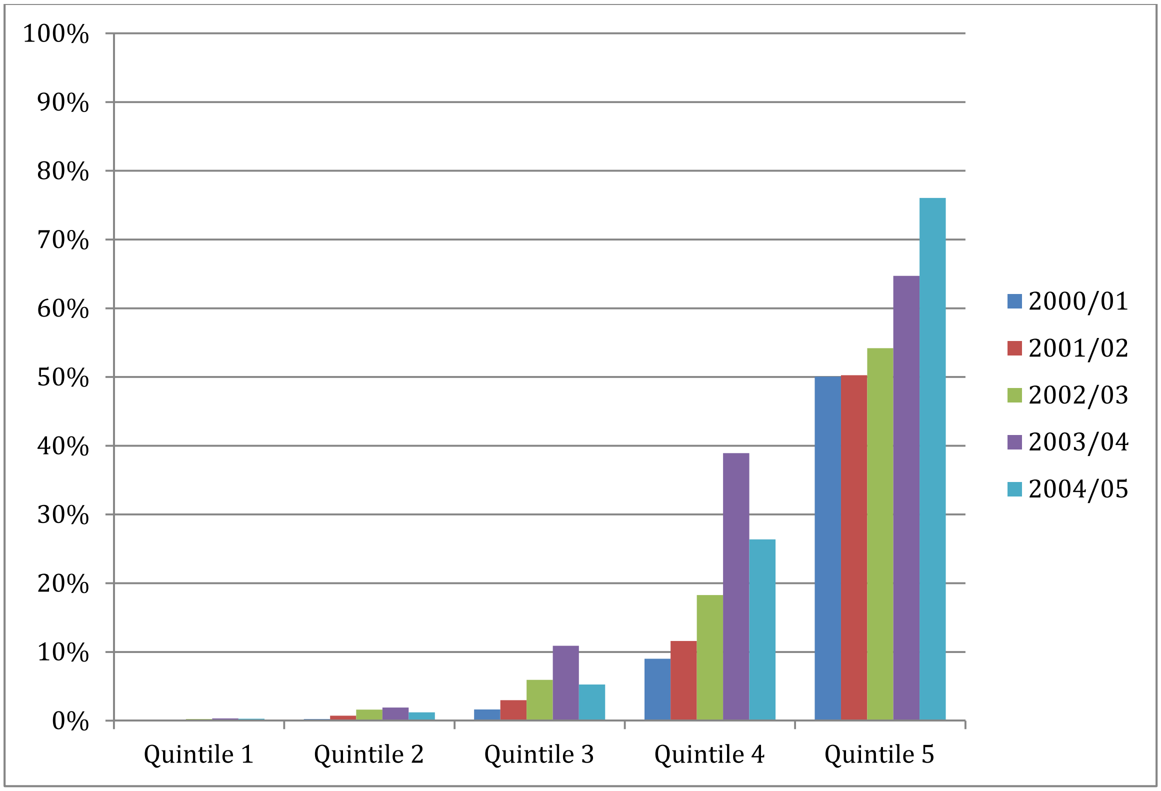

Figure 2: Probability of taking Algebra I by 7th grade, by 6th grade math test score quintile and year entering 7th grade, Charlotte-Mecklenburg Schools.

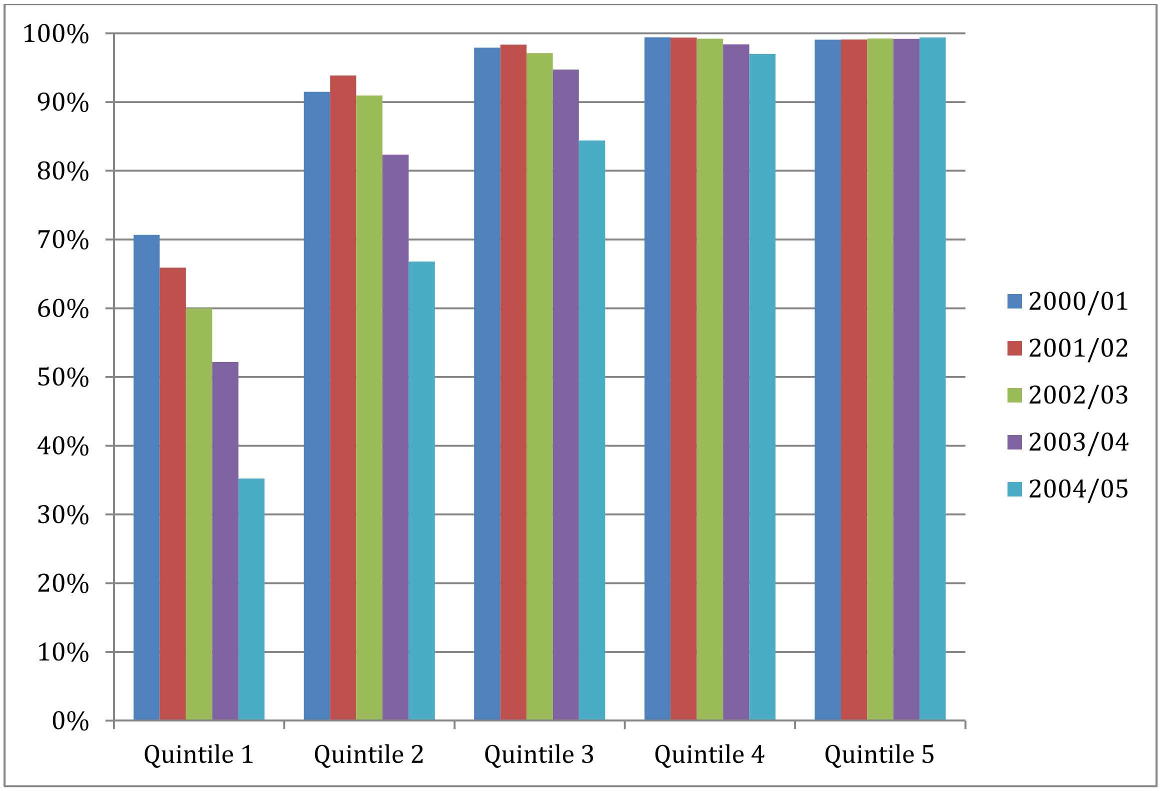

Figure 3: Probability of taking Algebra I by 9th grade, by 6th grade math test score quintile and year entering 7th grade, Charlotte-Mecklenburg Schools.

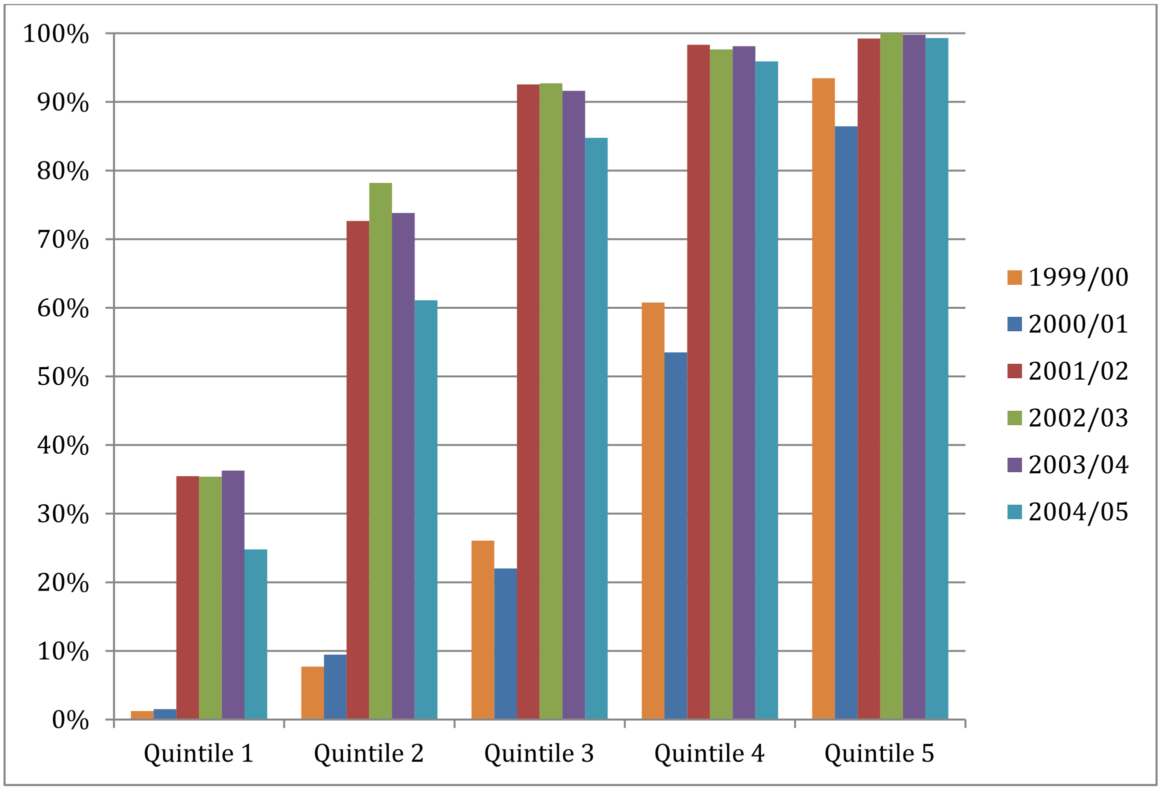

Figure 4: Probability of taking Algebra I by 8th grade, by 6th grade math test score quintile and year entering 7th grade, Guilford County Schools.

Table A1: Summary Statistics for Dependent Variables

School District	Algebra I test scores	Pass Algebra I by 10th grade	Pass Geometry by 11th grade	Pass Algebra II by 12th 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 7th 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.