is beneficial, for example, one effect might be to increase the proportion of marginal students who take follow-up courses, possibly leading to a decline in average performance in those courses.

Table 2 illustrates the potential severity of the selection problem, by tracing the progress of students in two cohorts. Consider first the cohort of CMS students who entered 7th grade in 1999/2000 and took Algebra I for the first time the following year. Because this cohort arrived before the district's accelerated algebra push, only 28.1% of them took Algebra I in 8th grade. About 88% of them passed the EOC test in the subject, and over four-fifths of them progressed immediately to Geometry the next year. Most of the non-progressing students retook Algebra I as 9th graders. About three-quarters of the 8th grade Algebra I takers in the first cohort took the Algebra II EOC two years later, and 82% 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 68.7% of them proceeded to Geometry the following year, only 61.4% completed the three-course sequence by the end of 10th grade, and just 73.6% finished the sequence by the time they would ordinarily have graduated.[22] 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

[22] 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.

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. .

Rather than attempt to estimate a selection correction model, or use any other procedure to account for the selection of marginal students out of the sample of subsequent course-takers, we redefine our outcome variables such that they are observed for all students, whether they enroll in a follow-up 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.[23] 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. In the case of passing Algebra I, the fact that we have restricted our sample to students who took Algebra I at some point in their career permits us to use the actual test score as a dependent variable.

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

[23] Our definition of a passing grade on the EOC test 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. In 2007, the state adopted stricter grading standards on the EOC, 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 test scores over time. 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.

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 outcomes above. 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, assignment to earlier algebra in the cross-section is likely to be correlated with unobserved determinants of math achievement.

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 two-tenths of a standard deviation 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 15 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 between one and two deciles in the distribution. To reiterate our previous discussion, however, these OLS estimates, like many prior estimates in the literature, are potentially 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.[24] These estimates include two-stage least squares results for all four outcome variables as well as bivariate probit results for the three binary outcomes.[25] 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. In the first stage of the Algebra I test score regression, for example, the F-statistic on excluded instruments is 46.0 with a p-value less than 0.0001.

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 36% of a standard deviation lower on their Algebra I end-of-course tests and are significantly less likely to pass courses in Geometry and Algebra II on a college-preparatory schedule.[26] Two-stage least squares estimates indicate that accelerated students are 18 percentage points less likely to pass the Geometry EOC test within five years of beginning 7th grade, and 11 percentage points less likely to pass Algebra II within six years. 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.[27]

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

[25] Note that the sample size for the passing equations is slightly larger than that for the Algebra I test score equations. The analysis includes roughly 250 students – out of a base of over 32,000 – who appear in the EOC test roster for Algebra I but have a missing test score. We include this small set of students in the course passing specifications, coding them as not having passed Algebra I. Results are unaffected by the exclusion of these students. In addition to these 250 students with missing scores, an additional set of students are coded as exempt from testing, and these are systematically excluded from all of our analyses.

[26] 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.

[27] 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.

The two-stage least squares estimate of the impact of acceleration on passing Algebra I is negative, but small in magnitude and statistically insignificant. Given that the acceleration apparently reduces students' Algebra I test scores, the lack of an effect on passing the course (within four years of beginning 7th grade) suggests 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 did worse in subsequent math courses. This finding is consistent with the hypothesis that, by taking Algebra I earlier, they ended up having insufficient grounding in pre-algebraic math.

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.[28]

Accelerated placement into Algebra I in 7th grade was applied primarily to students in the top two quintiles of the 6th grade math 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. Point estimates in course completion specifications are uniformly negative, but at most are one-third the magnitude of their counterparts in Table 4 and statistically insignificant. The

[28] 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.

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. While we are unable to observe tangible benefits of acceleration here, bear in mind that the rate of Algebra II completion among high-performing students is very high at baseline. Moreover, acceleration into 7th grade may increase the likelihood of completing higher-level coursework beyond Algebra II, which we cannot directly observe in our data.

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 full standard deviation decline in Algebra I EOC scores and significant reductions in the likelihood of passing Algebra I or any subsequent course. Two-stage least squares results indicate that accelerated students experience an astounding 46 percentage point drop in the likelihood of passing Algebra I by 10th grade. The bivariate probit coefficient is somewhat more modest in size, indicating that a student with a 50% chance of passing Algebra I at baseline has only a 29% chance upon acceleration. A student with a 50% chance of passing Geometry by 11th grade at baseline is estimated to have a 26% chance if accelerated. The likelihood of passing Algebra II by 12th grade is similarly diminished.

Virtually all CMS students affected by the acceleration initiative exhibited poorer math performance in the year of acceleration. 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 have been harmed more severely. 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 a two-stage estimator identical to that employed in the analysis of patterns in CMS.

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.[29] 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

[29] 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).

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.

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 8^{th} 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 8^{th} grade at a rate above 40%, nearly twice the maximum rate observed for that quintile in CMS. Rates of Algebra I placement by 8^{th} grade peaked at 80% 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 7^{th} grade, Guilford's acceleration is still quite apparent in this last cohort.

Table 7 shows the results of two-stage least-squares and bivariate probit estimates of the effect of 8^{th} grade Algebra I acceleration in Guilford County. Results here are similar to those in CMS in most cases. The estimated impact of acceleration on Algebra I EOC test scores is statistically significant, negative, and nearly identical to the point estimate obtained in CMS. Two-stage least-squares estimates suggest that acceleration raised the likelihood of passing Algebra I by 10^{th} grade, though the bivariate probit coefficient is insignificant and smaller than that obtained in the CMS case. Effects on passing Geometry and Algebra II are uniformly negative and significant, with point estimates comparable to those in CMS.

Generally speaking, then, the Guilford results lend support to the conclusion that accelerating moderately-performing students into Algebra I in 8th leads to 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.

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 6th 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 at least one 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 8th grade, and $\hat{T}_{dc}^{CMS}$ is the treatment rate for students in the same cohort and decile in CMS.[30] 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.

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[30] Standard errors in these equations are estimated using the Huber-White method for clustering at the cohort/decile level.

Table 8 presents estimates of β using three alternate school districts. In one case, Winston-Salem/Forsyth (henceforth WSF), the estimated coefficient is statistically significant at the 5% level. In Wake County, which is now the state's largest district, the coefficient is negative with a *t*-statistic of -1.7, 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, but owing to the district's smaller size it is not statistically significant.

As a result of this evidence, we are not fully confident that any of these alternate counties serve as valid falsification tests.[31] 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. The significant negative effects recorded in CMS and Guilford County are not present here. The Wake County coefficient is in fact statistically significant and opposite in sign to the CMS result – consistent with the observation above that Wake restricted access to Algebra I in the cohort-decile cells where CMS expanded access. The WSF and Cumberland point estimates are both less than one-fourth the magnitude of the CMS coefficient. These findings support our conclusion that accelerated students score 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. As we found no significant effect of acceleration on the likelihood of passing Algebra I by 10th grade in CMS, it is perhaps unsurprising that we find no such effect in the falsification districts either. In the Geometry and Algebra II specifications, we fail to replicate the pattern of significant negative effects observed in CMS and Guilford County, with one exception. In Wake

[31] 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.

County, students in cohort/decile cells subjected to acceleration in CMS exhibit significantly lower rates of passing Geometry by 11th grade. In further investigation, we attribute this result to inexplicably poor performance among Wake Geometry students in a single cohort. As the Forsyth and Cumberland coefficients are both less than half the CMS point estimate, we do not consider the Wake result a particular cause for concern.

7. Investigating an alternative mechanism: instruction quality

As noted above, one explanation for our finding that early exposure to Algebra I was detrimental is that the acceleration caused students to miss important pre-algebra course material. 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 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 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. The course timing 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 in instruction quality, the apparent cost of acceleration would be confined to the phase-in period.[32]

The increased demand for Algebra I instruction could have affected the quality of instruction in many respects. Administrators could respond by boosting class sizes, by assigning

[32] 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.

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%. There is no increase in class size, however. In fact, the mean class size for Algebra I was slightly smaller in 2003 than it was in 2002.[33]

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

[33] 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).

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? Recall that 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 that the returns to experience beyond the first few years are relatively small, however.

Many studies of the effect of teachers on student test scores conclude that teacher quality is not adequately reflected in any observed credential. These studies typically infer quality on the basis of "value-added" scores, derived from teacher fixed effects in longitudinal models of student achievement growth.[34] 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).

[34] 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.

To assess the hypothesis that the adverse acceleration effects represent a decline in teacher "value-added," note that our point estimates indicate effect sizes of up to one standard deviation in some cases. Such an effect could be accomplished only if teachers of non-accelerated students were drawn exclusively from the top tail of the distribution, and teachers of accelerated students from the bottom tail. The data presented in Tables 10 and 11 indicate that 72% of Algebra I sections offered in the acceleration year were taught by a set of individuals who also led 62% of such sections in the prior year. This discussion implies that the reduction in teacher quality required to explain the estimated adverse acceleration effect is far too large to be plausible. [35]

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 well be applied to a range of pre-algebra courses, geometry, or any other math subject in the

[35] 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 the observed test score effect in 8^{th} grade and less than one-fourth the estimated effect in 7^{th} and 9^{th} grades.

hierarchy. Moreover, the strong positive correlation between the timing of Algebra and later outcomes has been incorrectly interpreted 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 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 a longer-run strategy to accelerate the math curriculum* – is actually harmful for success in math.

Our results imply, for example, that California's proposal to increase the proportion of students taking introductory algebra in 8^{th} 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 4^{th} grade math performance is significantly worse than in CMS, according to NAEP assessments, yet 8^{th} grade algebra placement is the norm.

We must be a bit more cautious, however, in evaluating the impact of the past expansion of 8^{th} grade algebra enrollment 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 8^{th} grade would require observing policy variation within that group.

The optimal rate of 8^{th} grade algebra-taking is undoubtedly greater than zero. Indeed, our results indicate that the increase in Algebra I taking among 7^{th} graders in CMS has had no