Return to Algebra II: The Effect of Mandatory Math Coursework on Postsecondary Attainment[1]

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Abstract

Using detailed transcript data from multiple cohorts in Michigan, this paper studies the long-run impacts of math course-taking on student outcomes and the impacts of mandatory Algebra II-taking on college outcomes. First, I find that Algebra II-taking increases the probability of enrollment in a 4-year college and of on-time completion of a bachelor's degree. Second, I use the fraction of Algebra II-takers in a school in 2003 (before the policy was implemented) as an instrument to identify policy-induced variations in the propensity to take Algebra II. Using this instrumental variable strategy, I find significant and quantitatively large positive impacts of mandated Algebra II-taking on the probability of enrollment in both 2-year and 4-year colleges for minority and disadvantaged students and on college degree completion rates for advantaged compliers. I also show a sizable impact of the mandatory course-taking policy on the likelihood of majoring in a STEM field among students from advantaged backgrounds.

JEL Classifications: I21, I24, I28

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1. Introduction

In high school, research indicates that completion of advanced math courses improves the likelihood that students will attend and complete postsecondary education, particularly at 4-year

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institutions (Adelman, 1999; Kim et al., 2018; Riegle-Crumb, 2006; Schneider, Swanson, & Riegle-Crumb, 1998), as well as their long-term labor market outcomes (Altonji, 1995; Goodman, 2019; Levine & Zimmerman, 1995; Rose & Betts, 2004) and health behaviors (Carroll et al., 2017; Hao & Cowan, forthcoming). Acknowledging the benefit of completing advanced math courses in high school and the general federal policy emphasis on college readiness, many states have increased the minimum number of math credits required to graduate from high school, and some states have specified a set of math courses that students are required to complete. For example, between 1993 and 2013, the number of states requiring at least three credits in math grew from 15 to 42 (Digest of Education Statistics 2017).[3]

To date, the effects of requiring high school students to take more rigorous math courses on postsecondary education outcomes are mixed.[4] For example, in Chicago, mandating Algebra I for 9th graders had no effect on test scores or college attendance (Allenworth, Lee, Montgomery & Nomi, 2009; Mazzeo, 2010), whereas low-skilled 9th graders with double instructional time experienced improved test scores and college enrollment rates (Cortes & Goodman, 2014; Cortes, Goodman & Nomi, 2015; Nomi & Allenworth, 2013). Clotfelter, Hemelt, and Ladd (forthcoming), examining the effects of a statewide policy change in North Carolina that increased the number of high school math courses required for admission to any of North Carolina's 15 public 4-year institutions, found positive effects on college enrollment.

In this paper, I study the causal effects of Algebra II-taking and contribute to the literature on long-run impacts of math course-taking on student outcomes, and on impacts of

[3] Despite states' efforts to increase rigorous math course-taking, substantial racial and ethnic gaps in achievement and enrollment in advanced math courses stubbornly persist (Domina and Saldana, 2012; Kelly, 2009; Kim, 2018; Riegle-Crumb & Grodsky, 2010).

[4] In the middle school context, "Algebra for All" in California, which requires all 8th graders to pass algebra, corresponded with declines in test scores (Domina, McEachin, Penner, & Penner, 2015). In Wake County, North Carolina, on the other hand, middle school students were assigned to accelerated math based on prior achievement scores. The policy increased college readiness and intentions to pursue a bachelor's degree (Dougherty et al., 2015; Dougherty et al., 2017).

statewide curricular interventions that increase the minimum number of math credits required to graduate from high school. I accomplish this by using transcript data containing seven cohorts of high school students from Michigan and statewide policy variations in Michigan's high school graduation requirements. In contrast to the national representative datasets used in other studies (e.g., Teitelbaum, 2003), these transcript data provide more detailed information on course-taking for all students from a representative sample of Michigan high schools. In addition, the transcript data provide a unique opportunity to evaluate whether certain subgroups of the student population, such as low-income and minority students, were differentially affected by the policy. The data also include demographics, 8^{th} grade standardized test scores, and college enrollment and completion information. I am thus able to link high school students' math course-taking to their college enrollment, major choice, and degree completion, and use across-cohort-and-school variation in propensity to take Algebra II as an instrument to estimate causal effects of Algebra II-taking on postsecondary attainment.

This study adds to the literature on long-run impacts of math course-taking on postsecondary outcomes. Early research found positive effects of high school math course-taking on labor market outcomes (Altonji, 1995; Joensen & Nielsen, 2009; Goodman, 2019; Levine & Zimmerman, 1995; Rose & Betts, 2004), and recent studies have presented evidence on how high school math course-taking affects postsecondary attainment. Long, Conger, and Iatarola (2012), for example, used transcript data from one cohort of 8^{th} graders in Florida to estimate the effects of advanced high school course-taking. The authors used propensity score matching to minimize selection effects and showed that switching from taking 0 to 1 rigorous course increases the likelihood of graduating high school and enrolling in a 4-year college. Kim et al. (2015) examined the effects of completing Algebra II on attending and graduating from college in Florida and found that completing Algebra II increases the probability of attending only for 2-year colleges but not for 4-year attendance and degree completion. The fact that Kim et al.’s (2015) study reaches a different conclusion may be partly attributed to the behaviors of the compliance group captured in their IV estimates. To deal with selection-into-course bias, the authors used the unemployment rate in 9th grade as an instrument, which focuses only on students for whom the local labor market conditions in 9th grade affected their decision to take Algebra II. This paper provides better identified links between Algebra II-taking in high school and college enrollment, major choice, and degree completion.

I also contribute to the literature evaluating the impacts of statewide graduation requirements in high school. To date, there is little evidence about whether a statewide math education intervention can change students’ skills and education outcomes. Clotfelter, Hemelt, and Ladd (forthcoming) examined the effects of a statewide policy change that increased the number of high school math courses required for admission to any of North Carolina’s 15 public 4-year institutions. As transcript data were only available for recent years, the authors used an end-of-course test in Algebra II by 11th grade as a proxy for whether a student completed four math courses and found positive effects on college enrollment. Recently, two studies have examined the impact of the Michigan Merit Curriculum (MMC)—a statewide policy requiring all high school students to pass a set of 18 courses—on achievement and postsecondary outcomes. Jacob et al. (2015), using administrative data, found that the MMC had little impact on achievement—no improvement in ACT math score but a 0.04 standard deviation improvement in science score, with the largest improvement occurring among the students that entered high school with the weakest academic preparation. Another study, by Kim et al. (2018), used a representative sample of 129 high schools and showed that post-policy cohorts took more courses and completed higher-level courses, with the largest increase occurring among the least-prepared students; however, increased college enrollment rates for post-MMC cohorts were largely driven by relatively well-prepared students and students from advantaged schools.

I find that Algebra II-takers are more likely to enroll in 4-year colleges and that taking Algebra II increases their predicted probability of completing a bachelor's degree and majoring in STEM fields. I use the share of Algebra II-takers in a school that a student attends as an instrument to identify the propensity to take Algebra II, and results suggest that the selection bias does not significantly change the estimates. With respect to the effects of mandatory Algebra II-taking, I use an IV approach and a difference-in-differences design. The model identification stems from both cross-time and cross-school variation in the share of students taking Algebra II. I find that the mandatory course-taking increases the probability of both 2- and 4-year college enrollment for minority and disadvantaged students and college degree completion for advantaged compliers. I also find evidence that the mandatory Algebra II-taking increased the likelihood that students from advantaged backgrounds will major in a STEM field.

Using detailed transcript data from multiple cohorts, this study identifies every math course a student took to determine whether a student took policy-mandated courses or not and successfully captures the policy impacts on college outcomes from all types of colleges by obtaining postsecondary enrollment degree completion from the National Student Clearinghouse (NSC), which covers more than 90% of total student enrollment. By providing the empirical evidence of the long-run impacts of math course-taking and mandatory math course-taking, this study contributes to enriching our understanding of how, why, and if mandatory course-taking impacts postsecondary attainment.

2. Data, Policy Context, and Sample

A. Data

In this study, I seek to identify causal estimates of the impacts of math course-taking and mandatory Algebra II-taking on postsecondary attainment. I do so by using a unique student-level high school transcript dataset combined with state administrative data constructed by the Michigan Consortium of Educational Research (MCER), a partnership between the Michigan Department of Education (MDE), the Center for Educational Performance and Information (CEPI), Michigan State University (MSU), and the University of Michigan. I use transcript data that tracks complete course-taking histories on a class-by-class basis for seven cohorts of representative Michigan high school students (2002–03 through 2008–09),[5] which include detailed information on the course title, course content, and number of earned credits for the universe of courses that are crucial to the analysis.[6] These data enable me to pinpoint every math course a student took and determine whether the student completed the set of policy-mandated math courses. I also use school-specific catalogs to identify the course contents. By comparing pre- and post-policy cohorts, I can observe any variation in course-taking patterns resulting from the introduction of the policy.[7] For each cohort, there are roughly 34,000 students from 101 public schools. Unlike other nationally representative datasets, such as the High School Longitudinal Study of 2009 or the Education Longitudinal Study of 2002, that include a stratified

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[5] For simplicity, I refer to school year using the year in which the spring term occurred (e.g., the 2007–08 school year is 2008).

[6] This study uses the School Courses for the Exchange of Data (SCED) from the National Center for Education Statistics (NCES) and assigns a course intensity level to each course, similar to the categories employed in Riegle-Crumb (2006) and Kim et al. (2018). The categories are: 1—Basic or General Math, Pre-Algebra, Algebra I; 2—Geometry; 3—Algebra II; 4—Advanced Math (Algebra III, Trigonometry, Statistics, and Probability); 5—Pre-Calculus and Introductory Analysis; 6—Calculus; and 7—Advanced Placement (AP) courses. A year-long course is assigned 1 credit and a semester-long course is assigned 0.5 credits.

[7] Another advantage of official transcripts is that the estimates are free from the student- or school-specific measurement errors in course-taking records. For example, Altonji (1995) stated that the systematic error at the school-level is likely to bias the IV estimates downward, relative to other procedures.

group of students from each school, these transcript data provide more accurate measures of the course-taking characteristics of all students from each school.

State administrative data contain information on students' time-invariant demographics (date of birth, gender, race, and ethnicity), time-varying characteristics (enrollment, economic disadvantage status measured by free- and reduced-price meals eligibility (FRL), limited-English-proficiency (LEP) status, special education status, and migrant status), and standardized state test scores (Michigan Educational Assessment Program (MEAP)) in 8^{th} grade to capture math skills of students before taking high school courses. The MEAP scores are standardized across all cohorts relative to the 2005 cohort. Following Michelmore and Dynarski (2017), I create longitudinal measures of disadvantaged status that measure whether a student was always, ever, or never eligible for subsidized school meals over the high school years. School-level characteristics (expenditure per student, and number of teachers and students) are obtained from the Common Core of Data (CCD) and merged with the transcript data by matching the high school where students are enrolled.[8]

Postsecondary enrollment (e.g., dates and types of enrollment) and degree completion (including degree receipt date, major, type, and institution selectivity) information through December of 2016 are obtained by matching students to the NSC. The NSC covers more than 90% of total student enrollment, but its coverage is poor at some types of institutions, such as for-profit private colleges.[9] Considering the sharp increase in the college enrollment coverage of the NSC between the fall of 2008 and the fall of 2009 (Dynarski, Hemelt, & Hyman, 2015), the

[8] I match the school information to schools where the transcript is collected. Students could have taken math courses from other schools, and students or parents might have responded to the MMC by transferring schools or changing schools for specific math courses.

[9] Cellini and Turner (2016) stated that the vast majority of for-profit students work before attending and showed that the average age when students completed a for-profit degree program is 28 for an associate degree and 30 for a bachelor's degree. As our sample measures the college attendance status within six years of starting the 9^{th} grade and completion status within four to six years from the 12^{th} grade, the low coverage for for-profit institutions is not likely to significantly bias the estimates.

sample employs the postsecondary enrollment information from colleges that joined the NSC prior to June 2008.[10] The main enrollment outcome variable is a dummy variable representing whether a student has ever enrolled in a 4-year college or a 2-year college. In particular, enrollment status is measured within four to six years from the 9th grade. Degree completion is measured by type (2-year and 4-year), major (STEM or non-STEM), and institution selectivity (Barron's Profile of American Colleges). The focus on individuals attending high school up to the 2009 cohort means that the data include all course-taking information and postsecondary enrollment as well as on-time degree completion at 4-year institutions.

B. High School Graduation Requirement Policy in Michigan (MMC)

In the spring of 2006, Michigan announced a mandated rigorous college-preparatory curriculum for its high school students in the form of the Michigan Merit Curriculum (MMC), a set of statewide high school graduation requirements. The first cohort of students affected by the MMC entered 9th grade in the 2008 school year and were expected to graduate high school in the 2011 school year. With the intent to promote college readiness and workplace success, the MMC requires that students successfully complete four credits each of mathematics and language arts; three credits each of science and social studies; and one credit each of physical education, art, and online learning.[11] The law also specifies that students must master content that is generally covered in Algebra I, Algebra II, and geometry, as well as Biology I, and Physics or Chemistry. For each required course, the state provides a model unit framework, including instructional guidelines, and the MDE has outlined—for each district—the new content standards and

[10] Dynarski, Hemelt, and Hyman (2015) showed that the NSC covers 86% of college enrollment for 2005–2006 and over 90% for 2009–2010 and 2010–2011. The coverage rates of enrollment for postsecondary institutions in Michigan is 83% for 2005–2006 and over 94% for 2008–2009 and afterwards. If the misclassification of postsecondary enrollment status and poorer coverage among for-profit institutions is uncorrelated with the variable of interest—for example, the MMC—our estimates will be attenuated toward zero (Bound, Brown, & Mathiowetz, 2001).

[11] A credit is equivalent to a course. Beginning with the class of 2016, students are also required to take two world language credits.

expectations for the MMC-mandated subject areas. However, while the state has offered detailed curricular and assessment guidelines, districts have substantial latitude in local decision-making regarding curricular content, which we suspect may yield wide variation in math content that affects high school graduation, college enrollment, and completion, especially in low-performing schools.[12]

A new statewide high school exit exam was also implemented by the state to ensure a high level of rigor in the required classes[13]—in 11th grade, all students are required to take the Michigan Merit Exam (MME). Initially, the MME consisted of the ACT college entrance examination and Michigan-developed assessments in mathematics, science, and social studies. Unlike many other states, however, high school graduation in Michigan is not contingent on passing the MME.

C. Sample Restriction

I make a series of restrictions to obtain the analytic sample, focusing on students who entered high school between 2003 and 2009. First, I restrict the sample to students that are linked either to student demographics and school datasets, which drops two percent of the sample. Second, I exclude observations with unusual academic progress: when a student jumps more than two grades (9th to 11th grade) or goes back a grade (3.4%), or has a gender conflict (0.8%) or a birthdate conflict (1.9%). The final student-year dataset contains information from more than 240,000 students.[14]

[12] See Jacob et. al (2017) and Kim et al. (2015, 2018) for policy details.
[13] Adapted from the Michigan Legislature: Act 451, "Requirements for high school diploma," 2009.
[14] Because of the nature of administrative data, this study includes students who may have left Michigan or transferred to a private school or who transferred from a private or out-of-state high school. As some of the transcript data do not have their full course-taking records, this introduces the possibility that I may miscode their course-taking patterns. For example, if a student completes Algebra I and Geometry, and transfers to a private school and completes Algebra II, I may understate the effects of completing Algebra II.

As the MMC was announced in the spring of 2006, the study divides seven cohorts into three groups. The timing of the MMC announcement and implementation implies that the 2003–2005 cohorts are purely a pre-MMC sample ("pre-MMC" cohorts) and the 2006–2007 cohorts are transition cohorts ("transition" cohorts) who entered high school between the announcement and the implementation of the MMC. The second group will capture school-specific adjustment to the MMC. Jacob et al. (2017) stated that there were some increases in course-taking after the passage of the legislation, but this was prior to the actual mandate taking effect. Minor et al. (2017) also presented evidence that teacher turnover rates increased in 2006. Thus, the second phase contains both the effect of the increase in course-taking and differential responses in schools' preparation to accommodate the policy requirements. Finally, the "post-MMC" students who entered high school in the 2008 and 2009 cohorts had the full policy impacts.

D. Descriptive Statistics

Table 1 shows summary statistics before and after implementation of the MMC. Column 1, which contains all students in the 2003–09 cohorts, shows that 68% of students are White and that 57% of students were economically disadvantaged at least once during high school. Columns 2, 3, and 4 divide the sample by MMC implementation status. Pre-policy (column 2) and post-policy (column 4) cohorts have similar demographic backgrounds, except for the share of disadvantaged students. The fraction of students who were always economically disadvantaged in high school rose from 16% to 24%. The course-taking patterns and college outcomes were slightly improved between the pre-MMC (column 2) and transition (column 3) periods. The percentage of students completing courses up to Algebra II rose from 21% to 25% and college enrollment rates within 4 years of 9^{th} grade increased from 78% to 81%.

After the MMC was implemented, the fraction of students whose highest level of math was Algebra II increased to 35%. Compared to pre-MMC cohorts, these students were more likely to receive any postsecondary degree within 4 years of graduating from high school. The percentage of students who completed either a bachelor's or 2-year degree (associate or certificate) rose from 18% to 21%.

3. Estimating the Long-Run Impacts of Math Course-Taking

In this section, the goal is to identify the long-run impacts of math course-taking on college enrollment, major choice, and degree completion. I do so by presenting two distinct sets of regression estimates using cohorts from 2003–05 when course-taking decisions were not affected by the MMC curricular requirements. I begin with benchmark estimates, regressing outcomes of interest on demographic and school characteristics, prior math score, cohort trend, and school fixed effects, which are close to the correlational models found in prior studies. The second set consists of instrumental variable regressions, where I use a school's cohort-specific share of students who took Algebra II as an instrument.

A. Benchmark Estimates

To begin, I consider the challenges in estimating the returns to math course-taking. For expositional purposes, consider the following linear model of postsecondary outcomes for student *i* in school *s* in cohort c[15]:

$$Y_{isc} = \beta_0 + \beta_1 \boldsymbol{Math}_{isc} + \beta_2 \boldsymbol{X}_{isc} + \mu_c + \mu_s + \epsilon_{isc} \quad (1)$$

where $Math_{isc}$ represents a series of dummy variables indicating the highest math course taken in high school. As I code the highest level of math course completed in seven groups and omit

[15] I also estimate with the logit regression and results in both instances are not statistically different from each other.

the dummy for the lowest level (see footnote 6), each element in the vector of coefficients describes the effect of taking the corresponding math course on postsecondary outcomes, compared to those students who completed only the lowest level, Basic Math or Pre-Algebra. $\boldsymbol{X}_{isc}$ is a vector of individual- and school-level covariates.[16] The coefficients μ_c and μ_s capture cohort and school fixed effects, respectively. Standard errors are clustered at the school level to allow for the correlation of the error terms within schools.

Table 2, Panel A presents the estimated effects of math course-taking on college outcomes from the model in equation (1). The coefficients of each math course indicate the percentage change in the probability of college enrollment within 5 years of 9th grade (columns 1–2) and degree completion within 4 years of high school graduation (columns 3–7). These estimates do not represent any causal effects but instead provide correlational effects of math course-taking on postsecondary outcomes and provide context for subsequent analyses. Consistent with earlier findings, the OLS estimates indicate that students who completed advanced-level math courses are significantly more likely to enroll in 4-year colleges and obtain bachelor's degrees. Completing Algebra II is predicted to increase 4-year college enrollment by 8 percentage points, but Pre-Calculus is associated with a 32 percentage point increase. Finding that the students who take the most advanced courses have the largest increase in predicted probability of enrolling in 4-year colleges suggests that the returns to math coursework depend on the type of courses taken (Rose & Betts, 2004).

[16] $\boldsymbol{X}$ includes student demographics (race dummies with White as the reference group, gender, age and age squared, a dummy for migrant status, longitudinal FRL status with always receiving FRL in high school as a reference group, and indicators for ever in special education or LEP), school-level covariates (average of log of real school expenditures, log of number of teachers, log of number of students, and fractions of Black, Hispanic, Asian, American Indian, White, female, migrant, special education, LEP, ever received FRL, never received FRL, and always received FRL), unemployment rate in 9th, 10th, and 11th grade, and a function of standardized math test score in 8th grade.

The estimated effects of math course-taking have a nonlinear association with the probability of 2-year college enrollment. The predicted probability monotonically increases up to 9 percentage points for completing Algebra II but is lower for Algebra III courses. The coefficients on Pre-Calculus or more advanced courses are negative, which indicates that completing higher levels of math courses than the omitted level, Pre-Algebra, is associated with a lower probability of 2-year college enrollment. Estimates in columns 1 and 2 suggest that the completion of low-level courses leads to an increase in the probability of enrollment in 2-year colleges, whereas advanced math courses improve 4-year college enrollment rates.

The predicted probability of receiving a bachelor's degree is positively associated with Algebra III or more advanced courses.[17] Students who completed Calculus are 2 percentage points more likely to have a degree in STEM majors, but AP courses are associated with a gain of 12 percentage points. Completing advanced courses such as Algebra III or Pre-Calculus modestly increases the probability of obtaining an associate degree.

In sum, the analysis indicates that the likelihood of enrolling in and receiving a degree from postsecondary institutions depends on the level of math courses completed. For example, completing Algebra II helps with 4-year college enrollment, whereas Geometry helps with 2-year college enrollment. With respect to degree completion, Algebra III increases the probability of receiving a bachelor's degree and Pre-Calculus further increases the probability of having a degree in STEM majors, whereas Geometry and Algebra II improve the chance of obtaining an associate degree.

Results have implications for ongoing debates and mixed evidence on the effects of completing Algebra II. Algebra II is considered a "gatekeeper" course in terms of college access and success (e.g., Adelman, 1999; Rose & Betts, 2004), and in 19 states students must complete

[17] Results on degree completion also hold in other specifications that are conditional on college attendance.

Algebra II to graduate from high school (Kim et al., 2015). In fact, Algebra II is the most challenging math course required by multiple states. Therefore, I focus on the effect of taking Algebra II by estimating the following linear model using ordinary least squares:

$$Y_{isc} = \beta_0 + \beta_1 Alg2_{isc} + \beta_2 X_{isc} + \mu_s + \mu_s + \epsilon_{isc} \quad (2)$$

where $Alg2_{isc}$ is an indicator for whether student *i* in school *s* in cohort *c* took Algebra II or more advanced courses. Results in Panel B show the estimated coefficients of Algebra II, which suggest that students who took Algebra II or advanced courses have a large advantage on 4-year college enrollment (around 15 percentage points) and on college completion (around 2–5 percentage points).[18]

However, as pre-MMC students chose their highest-level of math course, the indicator variables, $Math_{isc}$ and $Alg2_{isc}$, are likely to suffer from omitted variable bias, where unobserved characteristics are correlated both with students' decisions to take Algebra II and their college outcomes. For example, some omitted unobserved variables—such as ability and motivation—are positively related both to students' decisions to take Algebra II and to their level of postsecondary education. Thus, the estimated coefficients on $Math_{iscj}$ and $Alg2_{isc}$ in Panels A and B are biased upward to the extent that these omitted characteristics are positively correlated with the college outcomes. Previous studies have tried various techniques to deal with this selection bias, including instrumenting for coursework with average coursework completed at the students' high school, high school fixed effects to control for variation in ability across schools, and controlling for students' measured ability (Altonji, 1995; Kim et al., 2015; Levine & Zimmerman, 1995; Rose & Betts, 2004).

[18] By focusing on math courses and not including other courses such as science or English, the estimated coefficients on math courses suffer from omitted variables bias. However, the direction of the bias is uncertain.

B. Instrumental Variable Estimates

To address these concerns in estimating the effect of taking Algebra II, the second set of estimates use the share among all students in each student's cohort and school group who took Algebra II (or higher) as an instrument, which uses across-cohort-and-school variation in Algebra II-taking shares to predict a student's likelihood of taking Algebra II.[19] I estimate a two-stage least squares (2SLS) where the first stage is:

$$Alg2_{isc} = \rho_0 + \rho_1 ShareAlg2_{sc} + \rho_2 X_{isc} + \mu_c + \omega_{isc} \quad (3)$$

where $ShareAlg2_{sc}$ is the instrument, which records the share of students in school *s* and cohort *c* who took Algebra II. I use the predicted value from equation (3) to estimate the following second-stage equation:

$$Y_{isc} = \beta_0 + \beta_1 \widehat{Alg2}_{isc} + \beta_2 X_{isc} + \mu_c + \epsilon_{isc} \quad (4)$$

I use the variation across a school's share of Algebra II-takers to predict an individual's actual Algebra II-taking and assume that any deviations from the predicted value are caused by selection. The identifying assumption is that students who attended a high school where a large share of peers completed Algebra II differ relative to their counterparts who attended a school with a small share of Algebra II-takers, only in terms of their likelihood of taking Algebra II. The coefficient β_1 estimates the impact of taking Algebra II on postsecondary outcomes for compliers: students whose decisions to take the course are shifted by the share of peers that

[19] This approach includes students who only completed Algebra II and who finished Algebra II and/or more advanced courses, which is likely to bias the estimates upward.

completed Algebra II who entered the same high school during the same school year.[20] These estimates represent local average treatment effects (LATEs).

I present 2SLS estimates in Panel C of Table 2. In most cases, the IV point estimates in Panel C are larger in magnitude than the corresponding OLS estimates in Panel B.[21] The IV estimates suggest that induced Algebra II-taking has positive impacts on 4-year college enrollment. Compared to the OLS estimates in Panel B, the coefficient increases to 0.192 (0.016) but induced Algebra II-takers are less likely to enroll in 2-year colleges. That is, 19% of compliers would not have enrolled in a 4-year institution if they attended a school with a low share of Algebra II-takers and instead would have enrolled in 2-year colleges. The IV estimates also show a larger positive effect of Algebra II on 4-year college degree completion. Students who are induced to take Algebra II are eight percentage points more likely to obtain a bachelor's degree and two percentage points more likely to earn the degree in a STEM major.

With respect to the impacts of Algebra II-taking on associate degree (column 6) or certificate completion (column 7), the IV estimates provide different results. The positive effects of Algebra II-taking found in Panel B are negative when the non-random course selection is accounted for. These students are three percentage points less likely to earn an associate degree. In addition, the estimated coefficients for certificates are close to zero and are not statistically

[20] Altonji (1995) showed that there is substantial variation across schools in courses and that the variation in the number of math courses is positively correlated with the quality of the course and with school and personal characteristics that are favorable to wages and college attendance (p. 414). As Altonji (1992, 1995) argued, the IV estimator is likely to overstate the effects of Algebra II but the upward bias is likely to be less severe than OLS and OLS with fixed effects.

[21] The first-stage regression estimate on the instrumental variable (the fraction of students taking Algebra II) is 0.405 (0.006) and the t-statistics associated with the coefficient on the instrumental variable is 62.5 (Table A.1 column 1). As the estimated coefficient on the instrumental variable is highly significant, a weak instrument is not a particular concern (see Bound, Jaeger and Baker (1995) for issues with a weak IV).

different from zero, which suggests that some marginal students would still have completed their associate degree or certificate if they had not been induced to take Algebra II.[22]

Table A.2 presents the estimated effects of induced Algebra II-taking by changing the sample. Given the fact that the NSC coverage for college enrollment and completion sharply increases in 2009 (Dynarski, Hemelt, & Hyman, 2015), Panel B uses only the 2005 cohort that was not affected by the policy announcement. I then include the transition cohorts (the 2006 and 2007 cohorts) in Panel C. Estimates are qualitatively comparable, except that the negative effects on 2-year college enrollment become weaker as more recent cohorts are included. In Table A.3, I examine the Algebra II effects on additional postsecondary enrollment and completion outcomes. Across different sample restrictions, results indicate that the positive effects of Algebra II-taking on on-time college enrollment and bachelor's degree completion persist. Together with the negative effects on associate degree completion, these results suggest that these compliers completed bachelor's degrees as a result of attending a school with a large share of peers taking Algebra II.

4. Estimating the Impacts of Mandatory Algebra II-Taking

Finally, I estimate the effects of mandatory Algebra II-taking on college enrollment and degree completion. In particular, I provide two sets of causal estimates of the impact of mandatory math course-taking. I use an instrumental variable strategy that exploits the policy-induced variation across schools to identify the "treatment on the treated" and a difference-in-

[22] Schools with the same course title may differ greatly in quality or content. Kim et al. (2015) presented evidence that course titles can be misleading in terms of the actual content being taught. In addition, the quality of courses is positively correlated with favorable personal and school characteristics (Altonji, 1995). Thus, the extent to which the discrepancy between course title and content is related to individual and school characteristics is another source of bias for math course estimates.

differences design to estimate the aggregate effect of the mandatory Algebra II-taking, which has an "intent to treat" interpretation.

A. Changes in Algebra II-Taking

Before turning into the empirical results, I first present an overview as to whether the policy provided increased opportunities for minority and disadvantaged students to take more advanced math courses. Figure 1 shows the fraction of students who took Algebra II by cohort. From Panel A, we can see that the fraction of Algebra II-takers stayed constant at around 65% in pre-policy periods (i.e., 2003–2007 cohorts) and then the share jumped significantly to 80% in the 2008 cohort when the policy was implemented. Panel B displays the share of Algebra II-takers by math test score in 8th grade. Across all quintiles, except for the most prepared students in 5th quintile, the rates of taking Algebra II increased after the policy, with the largest increase occurring among the least prepared students (1st and 2nd quintiles). Thus Figure 1 seems to suggest that the policy provided more equitable access to Algebra II. However, as the figures only present unconditional differences, I now examine whether the access to Algebra II became more equitable in terms of student characteristics and present results in Table 3.

Table 3 presents estimates from an OLS regression of an indicator as to whether an individual took Algebra II on a set of covariates, including the school fixed effects used in Panel B in Table 2. As the policy requires all students to take Algebra II, the estimated coefficients of math ability measured by 8th grade math score are smaller in post-MMC periods. The math-ability gap in Algebra II-taking probability drops substantially, such that the coefficient in the 2009 cohort is one-third of that in the 2003 cohort. Results suggest that more less-prepared students are taking Algebra II as a result of the policy and that the classroom peer ability levels are more likely to be more mixed than in pre-policy periods. The findings are consistent with the graphical presentation in Panel B in Figure 2, where the least-prepared students have the largest increase in the likelihood to take Algebra II.

The policy also improved access to Algebra II courses for disadvantaged students. When compared to students who were never disadvantaged in high school, students who were ever disadvantaged were around 8 percentage points less likely to enroll in Algebra II. After the mandatory course-taking policy was implemented, this gap decreased to 6 percentage points in the 2008 cohort and then to 4 percentage points in the 2009 cohort. Students with limited English proficiency or in special education also experienced improved access to Algebra II courses. Furthermore, gender gaps in the probability of Algebra II-taking diminished over time, after the policy was announced. As the policy successfully increased both equalized access to and enrollment in Algebra II, in the next section I seek to provide evidence on whether the improvement in course-taking resulted in improvement in college access and success.

As the access to Algebra II courses improved for disadvantaged and less-prepared students, one can easily conjecture that the beneficial impact of taking Algebra II on postsecondary outcomes may have been changed. Thus I estimate and report reduced form estimates of equation (2) with different time periods in Table A.4. The reduced form estimates represent the average effects of taking Algebra II, and comparing the effects across different time periods provide the projection about the changes in Algebra II-taking effects. I present the results in three groups: students in pre-policy periods (2003–2005 cohorts) in Panel A, in pre-policy and transition cohorts (2003-2007 cohorts) in Panel B, and in post-policy periods (2008–2009 cohorts) in Panel C. Comparison of estimates between Panel A and Panel B indicate that the effects of Algebra II-taking are positive for 4-year college enrollment and degree completion, although the point estimates are statistically smaller when the transition cohorts are included.

Estimates in Panel C indicate that taking Algebra II is associated with higher chances of enrolling in 4-year colleges (column 1) and completing a bachelor's degree (column 4), with the coefficients becoming smaller than those in Panel B by one-half or four-fifths. The changes in returns to Algebra II-taking are most striking for 2-year college enrollment: post-MMC students who took Algebra II are more likely to enroll in 2-year colleges, whereas students from previous cohorts experienced a negative association between Algebra II-taking and 2-year college enrollment. Although the estimates are likely to be biased, as described in Section 3.A, estimates in Panel C suggest that mandatory Algebra II-taking may have changed the benefits of taking Algebra II, which I examine in detail in next section.

B. Instrumental Variable Estimates

Due to the implementation of the MMC, students who would have not taken Algebra II are required to take the course. I therefore use the implementation of the MMC as a natural experiment to identify post-policy students who took Algebra II but would have not taken it in the absence of the policy. The fact that the proportion of students who took Algebra II increased from 65% to 80% offers a rare opportunity to measure the average return to Algebra II compared to previous research using an instrumental variable estimation.[23] Similar to the logic in equation (3), I argue that the likelihood of taking Algebra II is affected by the course-taking patterns within the school.

Figure 2 shows the changes in the share of students who took Algebra II between the 2003 and 2009 cohorts. Each dot represents a rate of Algebra II-taking for a school within a given cohort. The figure shows that the share in the 2003 cohort varies significantly across schools, from 21% to 86%, and that the majority of schools experienced an increase in the share

[23] Oreopoulos (2006) presented simulation results that the LATE is closer to the ATE when the fraction affected by the treatment is large (for example, 0.5).

by 2009, which is located above the diagonal line. This increase in the share, which varies up to 65%, is larger for schools that had a lower share in 2003. Given the large variation in Algebra II-taking shares in response to the mandatory course-taking, it is unlikely that the policy-induced Algebra II-takers are uniformly distributed across schools. Rather, one could reasonably conjecture that a school with a smaller share of students taking Algebra II prior to the policy is likely to have a larger increase in the share of Algebra II-takers, and thus more policy-induced Algebra II-takers. On the other hand, schools that already had a large share of students taking Algebra II are likely to have a smaller number of policy-induced Algebra II-takers.

Therefore, I use these policy-induced variations across schools in the share of students taking Algebra II to identify the causal estimate of mandatory course-taking on the full set of marginal students whose decisions to take Algebra II were affected by the policy. In particular, I calculate the fraction of students who took Algebra II in each student's school in 2003, prior to the MMC announcement so their decision was not affected by the MMC. Then, to use cross-time variation, I interact the share with the MMC indicator and use the interaction as an instrumental variable to predict the likelihood of taking Algebra II as a result of the mandated course-taking policy. The result represents the local average treatment effect for policy compliers (those who were induced to take Algebra II by the policy). I estimate the following first-stage using OLS:

$$Alg2_{isc} = \rho_0 + \rho_1 MMC \times Share03_s + \rho_2 MMC + \rho_3 Share03_s + \rho_4 X_{isc} + \rho_5 Cohort_c + \omega_{isc} \quad (5)$$

where $Alg2_{isc}$ is an indicator for whether student *i* in school *s* in cohort *c* took Algebra II and $Share03_s$ is the share of students in school *s* in the 2003 cohort who took Algebra II. First-stage estimates are reported in Table 4. The first-stage estimates indicate that the instrument is highly correlated with the treatment variable with the expected sign and is statistically significant at the

1% level (Table A.1 column 2). Estimates imply that schools with a smaller number of students taking Algebra II before the policy disproportionally have more Algebra II-takers after the policy. The magnitude of the F-statistic in the first-stage is 3,477—large enough for the instrument to be a strong predictor of the endogenous variable.

Next, the predicted value derived from equation (5), $\widehat{Alg2_{isc}}$, is used in place of actual course-taking status in the second-stage equation:

$$Y_{isc} = \beta_0 + \beta_1 \widehat{Alg2_{isc}} + \beta_2 MMC + \beta_3 Share03_s + \beta_4 X_{isc} + \beta_5 Cohort_c + \epsilon_{isc} \quad (6)$$

The identifying assumption is that students differ only in terms of their induced propensity to take Algebra II as a consequence of attending a particular school within a certain cohort. For example, students who attended a school in 2008 or 2009 that had a low school share of Algebra II-takers in 2003 are more likely to take mandated courses than peers who attended a school where a higher share of students took these courses in 2003. Given the identifying assumption, β_1 in equation (6) gives the effect of taking Algebra II on the likelihood of outcome Y_{isc}, compared to those who did not take the course.

Table 5 presents instrumental variable estimates of the effect of the mandatory math course-taking on the type of college enrollment and type and quality of degree completed. College enrollment is measured within five years after the 9th grade and degree completion is recorded within four years of high school graduation. These estimates represent local average treatment effects for compliers, who were induced to take Algebra II based on the school share of Algebra II-takers in the 2003 cohort.

The estimates in columns 1–3 suggest that mandatory Algebra II-taking has no impact on 4-year college enrollment,[24] while policy-induced Algebra II-takers are about five percentage points less likely to enroll in 2-year colleges. However, 2SLS estimates indicate that induced Algebra II-takers are significantly more likely (by six percentage points) to enroll in both 2- and 4-year colleges within five years of 9th grade. Again, these estimates represent local average treatment effects for compliers—those students induced to enroll in college because of the mandated Algebra II policy. Estimates in columns 1–3 suggest that these compliers, who would have enrolled in only 2-year colleges if not for the mandatory Algebra II-taking, are more likely to enroll in 2-year colleges and then transfer to 4-year colleges.

The mandatory Algebra II-taking also raised degree completion rates by 12 percentage points. The pre-MMC means imply that mandatory math course-taking doubles bachelor's degree completion rates. At the same time, the induced Algebra II-takers are three percentage points more likely to graduate with a major in a STEM field. Many researchers have conjectured that algebra courses prepare students by providing them with stronger math skills and are thus a gateway to STEM courses (e.g., Clotfelter, Ladd, and Vigdor, 2015; Kim et al., 2015), and results indicate that the effect is also positive for those who are policy-induced takers. Given the fact that the lifetime wage premium is different across degrees and majors and that STEM graduates receive on average $1.5 million more than high school graduates over their lifetime (Webber, 2014; Webber 2016) and that the wage premium is especially for higher for young STEM workers (Deming & Noray, 2018), mandatory course-taking could affect later labor market earnings by channeling students into more productive majors (Rose & Betts, 2004).

[24] The estimation includes transition cohorts, which may contain the effect of the school-specific adjustment to increase course-taking in response to the MMC. I examined the sensitivity to dropping transition cohorts and found that it yields qualitatively similar coefficients.

It is worth noting that the increase in math course-taking in high school is associated with an increase in the probability of completing a bachelor's degree from selective institutions that are classified by the 2009 Barron's Profile of American Colleges as "very competitive" or higher. As the mandatory Algebra II-taking lowered the probability of enrolling only in 2-year colleges (column 2), the mandating policy has no effects on associate degree or certificate completion rates, which implies that few marginal students would have earned degrees in 2-year colleges if not for the mandated course-taking. Given that the current NSC data span through 2016, it is too early to observe whether mandatory Algebra II-taking has effects on the completion of a bachelor's degree within five or six years of high school graduation.

C. Heterogeneous Effects

Given the remarkably large response of Algebra II-takers to the mandatory course-taking, one could reasonably conjecture that the mandatory Algebra II-taking effects are heterogeneous. It is important to understand the extent to which the heterogeneity in changes in propensity to take Algebra II improved postsecondary attainment for subgroups of interest. If disadvantaged students are more induced to take Algebra II (Kim et al., 2018) and the propensity to enroll in college is increased, the policy can be considered successful in terms of closing the gap in college enrollment by providing learning opportunities to these students and improving their human capital.

In Table 5, I report the estimated impacts of mandatory Algebra II-taking on college enrollment and degree completion, estimated separately by student gender, race, disadvantage status, and school quality. School quality is measured by the fraction of disadvantaged students, with the fraction divided into quintiles from low to high. Results indicate that mandatory course-taking raised the predicted enrollment rates in both 2- and 4-year colleges for minority and disadvantaged students who were induced to take Algebra II: by 6 percentage points for Black/Hispanic compliers (column 4) and 12 percentage points for those who were always disadvantaged in the high school years.

It is worth noting that advantaged compliers have also benefitted from the mandatory course-taking, especially in terms of degree completion. For example, column 3 indicates that White/Asian compliers are 18 percentage points more likely to complete a bachelor's degree from very competitive institutions and there is a 12 percentage point gain for those who were never disadvantaged in high school (column 5).

Splitting the sample by school quality also reveals substantial heterogeneity. Column 10 shows that compliers from the most disadvantaged schools, measured by the proportion of disadvantaged students, are more likely to enroll in both 2- and 4-year colleges (nine percentage points). The estimated effects on college enrollment are the largest among students in the middle quintiles, who were on the margin between not taking Algebra II and taking it. These students also see large increases in college enrollment (18 percentage points), bachelor's completion (18 percentage points), and bachelor's degree attainment in selective institutions (6 percentage points; column 9). The point estimates for college enrollment for compliers from the most advantaged schools in column 8 are small in magnitude and statistically not different from zero, however their predicted probability in bachelor's degree completion was significantly improved.

Estimates for the subgroups divided by math ability, measured by standardized math score in the 8th grade quintiles, tell a similar story (results not presented). One-third of compliers from the lowest part of the math ability distribution (1st quintile) would not have enrolled both in 2- and 4-year colleges if not for the policy, while around two-thirds of these students would have enrolled only in 2-year colleges. Thus, the mandatory math course-taking increased the probability of enrolling both in 2- and 4-year colleges for students who would otherwise have enrolled only in 2-year colleges. At the same time, for relatively well-prepared students (4th quintile), the probability of completing a bachelor's degree in four years doubled and the probability of majoring in STEM tripled. In sum, disadvantaged and minority students experienced an improvement in college enrollment, whereas college degree completion effects are the largest among the advantaged compliers.

D. Difference-in-Difference Estimates

The IV estimates do not provide the aggregate effect of mandatory Algebra II-taking. The impact of the requirement to take Algebra II likely has very different effects on students in different parts of the achievement distribution. To estimate the aggregate causal impact of mandatory Algebra II-taking, I exploit the variation in changes in the share of Algebra II-takers and use a difference-in-differences design. Specifically, I compare changes in postsecondary outcomes between pre- and post-policy periods in schools that have a low school share of Algebra II-takers to those that had a large share. I estimate the following equation using OLS:

$$Y_{isc} = \beta_0 + \beta_1 MMC + \beta_2 Low_{isc} + \beta_3 MMC * Low_{isc} + \beta_4 X_{isc} + \beta_5 Cohort_c + \epsilon_{isc} \quad (7)$$

where Low_{isc} is a dummy for attending a school that had a low share of Algebra II-takers in 2003. Specifically, I divided schools into quintiles from low to high and assigned the two lowest quintiles to the group "low" and the two highest to the group "high." I also included a dummy for the transition cohort and an interaction of Low_{isc} and the transition dummy.[25] The coefficient of interest is β_3, which is a difference-in-differences estimator of the impact of mandatory math course-taking on postsecondary outcomes. Similar to the rationale behind the IV estimates above,

[25] In the analysis, I dropped the third quintile schools to construct control and treatment groups. In different specifications where I used only 1st and 5th quintile groups or dropped transition cohorts, estimates are qualitatively similar. In addition, the interaction of the Low and transition dummy is not statistically different from zero.

the rationale here is that schools with a low share of Algebra II-takers in 2003 will have a larger increase in Algebra II-taking after the policy than schools with a high share. The key identifying assumption is that any changes in postsecondary outcomes after the policy between students in these two groups are due to the policy, and that any mandatory course-taking benefits for students who attended a school with a high share of Algebra II-takers in 2003 are smaller than those who attended schools with a small share. This assumption is reasonable since schools experienced a large increase in the share in response to the MMC.[26]

In Table 6, I present difference-in-differences estimates, which can be interpreted as the "intent-to-treat" effects of mandatory math course-taking in each school in each time period. The estimated coefficients are roughly one-twelfth to one-half the magnitude of the corresponding coefficients in Table 4, consistent with the findings that mandatory Algebra II-taking affected less than 55% of all students. The coefficients on the *MMC* dummy indicate that the probability of enrolling only in 4-year colleges post-policy among students at schools with a high proportion of Algebra II-takers in the 2003 cohort is increased by about three percentage points. On the one hand, the estimates on the interaction term indicate that students in schools with a low fraction of Algebra II-takers post-policy did not experience significantly different college enrollment outcomes. On the other hand, these students have improved probability regarding completing a bachelor's degree in four years (two percentage points) with STEM majors (0.6 percentage points). The mandatory course-taking policy also marginally raised associate degree completion rates by 0.4 percentage points.[27]

[26] The assumptions required to identify the mandatory course-taking effects would be violated if the MMC caused students or parents with systematically different unobserved characteristics to move to *Low* schools.

[27] As Clotfelter et al. (2015) described, there is a concern that this reduced-form strategy relates to peer effects. Students who always take Algebra II are assumed to have the same postsecondary outcomes regardless of their treatment status, either in the "low" or the "high" group. As the increase in the fraction of Algebra II-takers is greater at "low" schools, the academic composition of Algebra II-takers is more likely to be changed at "low" schools. In this case, always-takers in "low" schools are more adversely affected by taking a course with less-

E. Potential Mechanism: Pre-Calculus and Calculus

Results indicate that the mandatory course-taking improves college access for disadvantaged and minority students and college success and completion for the advantaged compliers. Although it is too early to determine using the current data whether the policy helped disadvantaged and minority students graduate within five years, one can easily speculate that the advantaged and prepared students are more likely to take more advanced courses after the policy was implemented. I present evidence consistent with this hypothesis. In Table 7, I explore whether the mandatory Algebra II-taking policy helped students take the next-level course, Algebra III. I then further investigate whether the policy improved the likelihood of taking advanced math courses such as Algebra III, Pre-Calculus, Calculus, and AP courses. To answer the above hypothesis, I examine whether the impacts differ by disadvantaged status (Panel B) and by math ability (Panel C). I follow the same specifications and sample used in Table 4.

Results in Table 7 reveal three interesting points: first, as the MMC required students to complete four credits of math, column 1 shows that most of the compliers took at least one additional math course that was more advanced than Algebra II. Second, columns 2 and 3 show that disadvantaged and least-prepared students were more likely to take the next-level course, Algebra III, whereas more advantaged and prepared students were more likely to take more advanced courses, such as Pre-Calculus. It is important to note that the disadvantaged and least-prepared students were also more likely to take Pre-Calculus but the improved probability is lower compared to advantaged and prepared students. Third, the policy not only helped students take advanced courses but also changed the type of advanced courses that advantaged students

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prepared students than those in "high" schools (Nomi 2012). The extent to which the policy has changed the classroom academic peer performance composition of math courses and sorting of students into mandated math courses by the students' prior math ability is beyond the research question of this study, but will be examined in a follow-up study.

took. In particular, these students were less likely to take Algebra III or Calculus but more likely to take the next level of courses, such as Pre-Calculus and advanced placement (AP) courses.

F. Robustness

Table 8 tests the robustness of the key results for a variety of alternative specifications. Panel A reports the baseline estimates from Table 4. Panel B replicates the baseline specification dropping transition cohorts (2006–2007), and results indicate that there existed short-term anticipatory responses that are not large enough to invalidate the results. Panel C restricts the sample to students who attended public high schools in Michigan in both 11th and 12th grade. The concern is that students who enter and/or leave public schools in Michigan are unlikely to provide a full history of math course-taking, which is likely to bias the estimates. The magnitude and statistical significance of estimates in Panel C are comparable to those of baseline estimates in Panel A.

As the MMC required a set of courses for students to take, the effects of mandatory Algebra II-taking on postsecondary outcomes may include the effects of taking additional science or English courses, in which case the Algebra II-taking effects are likely to be overestimated. Although examining the full effects of the curricular requirement policy is beyond the scope of this study, I examine whether the inclusion of a set of science course-taking measures affect the mandatory Algebra II-taking estimates. I additionally include science course-taking variables, including indicators as to whether an individual took Biology, Chemistry, or Physics, which are further divided by the course level (e.g., regular and advanced). Estimates in Panel D indicate that including science coursework information causes the effects of mandatory Algebra II-taking on 4-year college enrollment to increase—although it is marginally significant, at 10%—and 2-year college enrollment and degree completion rates to decrease by 10% from the baseline estimates. None of these estimates are statistically different than the main estimates in Panel A, suggesting that mandated Algebra II-taking has effects on postsecondary outcomes.

Finally, in Panel E, I use the course description from course catalogs to infer what students learned in Algebra II. Course catalogs are often more useful to assess the material and content that students are taught because course descriptions are often based on the content of textbooks and because course titles can be misleading in terms of the actual content being taught (Cogan, Schmidt, & Wiley, 2001). For example, in Michigan, 30% of high school math courses have a course description that differs from what would be inferred by the course title (Kim et al., 2015, 2018). Using course descriptions increases the magnitude of college degree completion rate estimates, suggesting that the course title analysis led us to underestimate the mandated Algebra II-taking effects.[28]

5. Discussion

Michigan implemented a statewide college-preparatory policy that applies to all high school students from the graduating class of 2011 onward, imposing similar graduation requirements as those put in place in several other states. The policy appears to have resulted in no increase in ACT math scores (Jacob et al., 2017) but a measurable improvement in course-taking—measured by the number of math courses taken and the highest level of math completed—and an increase in college enrollment rates for post-policy students (Kim et al., 2018).

This paper extends the findings from these two papers and examines the mechanism through which the policy may impact student academic outcomes through math course-taking. In

[28] There are considerable variations in how course catalog data are maintained: some schools store their data electronically and others in paper format, and the availability of school catalogs differs by school. As such, I lose 20% of the sample in the analysis. Baseline estimates do not change when the sample is limited to schools that have catalogs.

particular, using actual high school transcript data, this paper seeks to answer whether the completion of specific types of advanced math courses, such as Algebra II, translates into improved access and successful completion of postsecondary education; and whether the increase in students' propensity to take Algebra II as a result of a mandatory course-taking policy affected postsecondary outcomes.

To answer the first question, I focused on pre-policy cohorts when math course-taking was unaffected by the policy and use the share among all students in each student's cohort and school group who took Algebra II as an instrument to predict the propensity to take Algebra II. Results suggest that students who are induced to take Algebra II are less likely to enroll in 2-year colleges and more likely to obtain a bachelor's degree.

The relationship between Algebra II-taking and college outcomes, however, could be different for students who chose to take the course versus those who took it because they were required to do so. Specifically, prior to the policy, students who chose to take Algebra II did so because they expected to benefit from it—forcing others to take the course could have a less positive impact on outcomes. However, if students from a school with a large proportion of low-SES peers did not take Algebra II because they had less accurate information about its benefits, they are likely to benefit from the mandatory course-taking.

Prior to the policy, the share of students taking Algebra II varied significantly across schools. As each school's share increased in response to the policy requirements, I was able to exploit the policy-induced variations across schools in the share of students taking Algebra II; this enabled me to identify the causal estimate of mandatory course-taking on the marginal students whose decision to take Algebra II was affected by the policy. Results indicate that the mandatory Algebra II-taking increases the probability of 4-year college enrollment for disadvantaged and minority students and college degree completion for advantaged compliers, suggesting that mandating Algebra II helps prepare students to enroll in college and improves their chances of receiving a college degree. I also find evidence that the mandatory course-taking policy has an effect on students' choice of major. Together, results suggest that a statewide policy that requires students to take more rigorous courses was effective in improving postsecondary attainment. The returns to a postsecondary education continue to rise and the effects of advanced math course-taking manifest as changes in postsecondary attainment (Joensen & Nielsen, 2009). The findings that the policy improved access to advanced math courses as well as access to college for disadvantaged students could contribute to closing racial or gender earnings gaps (Rose & Betts, 2004), although due to data availability constraints it is somewhat early to draw a concrete conclusion.

As more states are including Algebra II in their graduation requirements policy, this study has broader implications than just for the Michigan context, especially for state officials who plan similar policies in other states: results provide a better understanding of how and why the policy may or may not impact student academic outcomes, particularly the extent to which the statewide mandatory course-taking policy will affect subgroup students—including disadvantaged students from schools that serve low-income and minority students. In addition, based on the findings, teachers and schools can target more pro-academic course-taking patterns for specific student populations, especially among minority and low-income students.

First implication for policymakers is that implementing Algebra II requirements does not guarantee that all states or schools will cover the same concepts in their Algebra II classes. If concepts covered in Algebra II are different across schools or districts, we may observe differential impacts of taking Algebra II on student outcomes. In this case, policymakers may need to consider standardizing the course content or specifying the minimum level of content to be covered.

Second, given the finding that there are differential effects on college outcomes by math ability and socioeconomic status—even though all students are required to take Algebra II in high school—the statewide policy may have been more effective if policymakers had examined the differences in course preparedness in early grades and provided additional support to students in need to prepare them for mandated courses in high school. As about one-third of graduates who take Algebra and Geometry are ill-prepared for college-level course work (Loveless, 2013), policies mandating higher standards may be more effective in improving student achievement among underprepared students if additional pedagogical and instructional support is provided.[29]

Third, when a state requires increased course-taking, some schools may have limited capacity to comply with the policy and some students may not be ready to take the required courses—these are circumstances in which students will not experience the full intended policy benefits. Thus policymakers should allocate resources and materials to districts and schools prior to the policy's implementation so that they can be ready to implement the policy.

Fourth, policymakers and researchers may want to use other college outcomes to evaluate the policy impacts on college readiness. In addition to college enrollment and completion outcomes, the predicted probability of attending remedial courses may be another measure of college readiness and policy effectiveness. About half of all first-year students and 70% of community college students take at least one remedial course during their college careers (Chen, 2016) and the remediation costs nearly USD 7 billion annually in tuition alone, not including indirect costs (e.g., students' opportunity costs, and the impact of remediation on students' future

[29] For example, Cortes and Goodman (2014) examined the effects of the "double-dose" algebra policy in Chicago Public Schools, which required 9th graders with low math scores to take an additional support class, and found that "double-dosed" students benefitted from the additional instructional time and improved pedagogy.

outcomes; Scott-Clayton and Crosta, 2014). If the policy helped improve remediation placement and/or college-level course-taking for less-prepared and disadvantaged students, this may show another way in which the policy can influence student outcomes.

Lastly, policymakers should be aware of the policy mechanisms from other states and try to maximize the policy impacts by designing statewide education policies based on their own contexts. Further work is required to understand the mechanism through which a statewide graduation requirement policy may impact students' postsecondary attainment. There are several key factors that the policy indirectly affects, of which this study provides only one example—i.e. mandatory math course-taking. Much less is known about whether high school classroom academic composition or teacher supply changed as a result of the statewide requirement policy. With respect to peer academic composition prior to the MMC implementation (i.e. when students chose to take Algebra II), compared to post-MMC cohorts, classroom peer ability levels were more likely to be homogeneous and advanced math courses such as Algebra II were likely to be taken by high-skill students. In response to the mandated curriculum, schools may have created more mixed-ability classrooms or kept ability grouping and the peer ability levels comparable to those in the pre-policy period. Regarding the quality of students' math teachers, if schools do not have enough resources, they may hire new math teachers who have completed minimal training or previously had non-math assignments to teach state-mandated classes, potentially limiting the intended policy impacts. I plan to examine how the policy-induced changes among these other factors affected students' postsecondary attainment and labor market outcomes.

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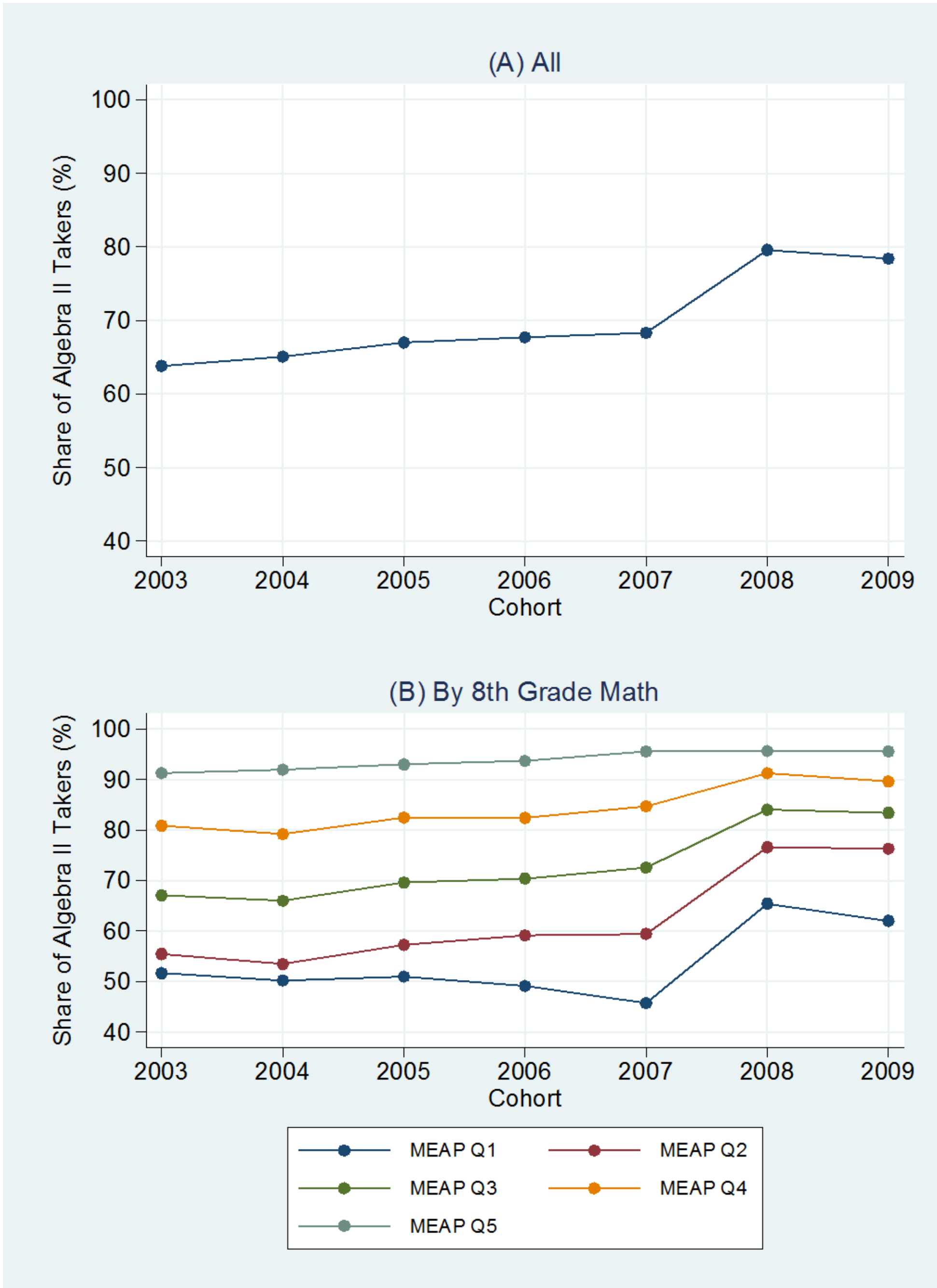
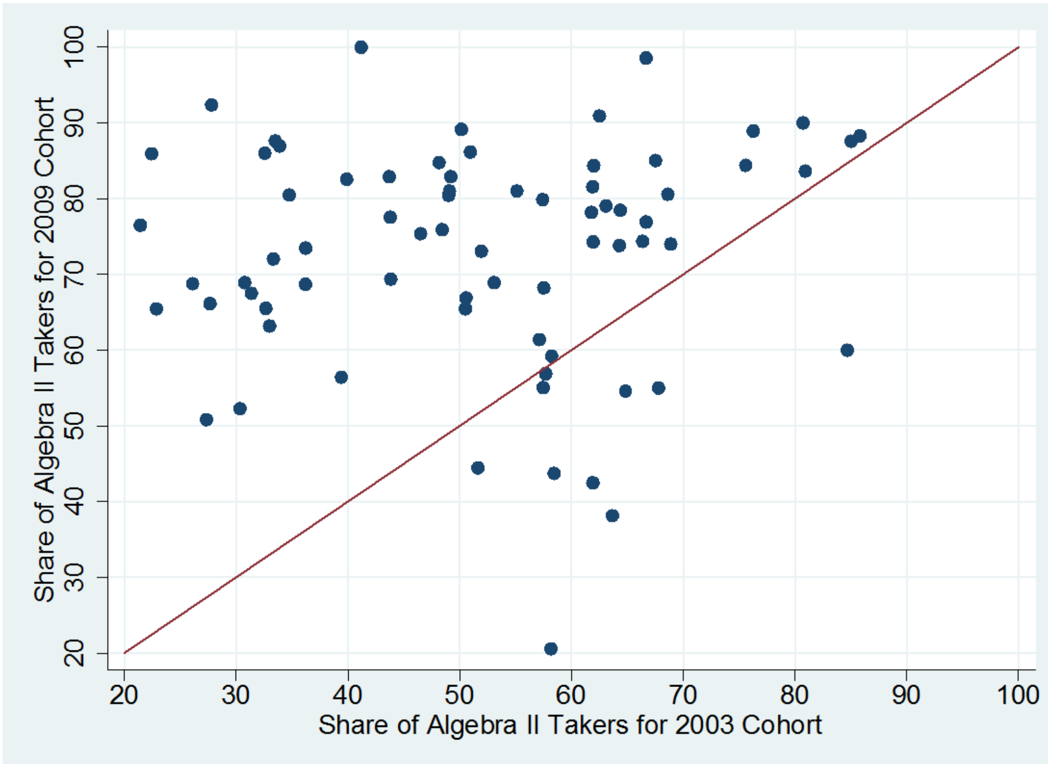
Figure 1. Fraction of Students taking Algebra II



Note: The share of students who took Algebra II is calculated for each cohort in Panel A and by cohort and by 8th grade standardized math test score quintile in Panel B. Cohorts are defined as the year students started 9th grade.

Figure 2. Fraction of Students taking Algebra II: 2003 and 2009 Cohorts



Note: The share of students who took Algebra II is calculated for each school. Cohorts are defined as the year students started 9th grade.

Table 1. Summary Statistics

	All Cohorts (2003-2009) (1)	Pre-MMC (2003-05) (2)	Transition (2006-07) (3)	Post-MMC (2008-09) (4)
Demographics				
Female	0.496	0.495	0.494	0.497
White	0.682	0.672	0.703	0.675
Black	0.234	0.251	0.211	0.232
Hispanic	0.042	0.037	0.043	0.049
Never Disadvantaged	0.429	0.383	0.428	0.494
Ever Disadvantaged	0.571	0.617	0.572	0.506
Always Disadvantaged	0.194	0.157	0.198	0.244
Special Education	0.126	0.126	0.127	0.126
Limited English	0.049	0.047	0.048	0.053
Migrant	0.002	0.002	0.002	0.002
Math Course-Taking (Highest Completed)				
Algebra I or lower	0.106	0.117	0.105	0.090
Geometry	0.182	0.220	0.205	0.103
Algebra II	0.264	0.214	0.249	0.349
Algebra III	0.183	0.210	0.167	0.159
Precalculus	0.138	0.128	0.138	0.152
Calculus	0.020	0.020	0.018	0.023
AP	0.108	0.091	0.118	0.124
College Outcomes				
Enrolled in Any College	0.800	0.780	0.806	0.824
Enrolled in 4-year	0.489	0.476	0.490	0.509
Enrolled in 2-year	0.333	0.327	0.336	0.341
Completed BA	0.152	0.143	0.153	0.167
Completed BA in STEM	0.038	0.035	0.038	0.042
Completed BA Selective	0.060	0.069	0.070	0.036
Completed AA	0.044	0.043	0.044	0.046
Completed Certificate	0.017	0.018	0.015	0.016
Completed Any Degree	0.194	0.184	0.195	0.211
8th Grade Math	0.095	-0.042	0.129	0.243
Total students	241,526	100,998	69,919	70,609

Notes: The sample is students entering 63 high schools between 2003 and 2009, who are enrolled in 11th and 12th grade. The sample has one observation per student. Disadvantaged is an indicator for economically disadvantaged status. English is an indicator of Limited English Proficiency (LEP) status. Measures of college quality are calculated using Barron's Profile of American Colleges. 8th grade math is standardized math test scores in the Michigan Educational Assessment Program (MEAP) and is normalized within the cohort.

Table 2. The Estimated Effects of Math Course-Taking on College Enrollment and Completion

Variable	College Enrollment		College Completion				
	4-year	2-year	BA	BA STEM	BA Elite	AA	Certificate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 2003–2005 Cohorts (Pre-MMC)							
Geometry	0.000	0.075***	-0.013***	-0.004***	-0.010***	0.004*	-0.002
	(0.004)	(0.006)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Algebra II	0.076***	0.085***	-0.000	-0.008***	-0.016***	0.027***	0.004**
	(0.005)	(0.007)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Algebra III	0.115***	0.038***	0.021***	-0.004**	-0.005*	0.019***	0.001
	(0.005)	(0.007)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
Pre-Cal	0.324***	-0.108***	0.106***	0.011***	0.051***	0.012***	-0.002
	(0.006)	(0.007)	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
Calculus	0.388***	-0.175***	0.150***	0.027***	0.081***	-0.000	-0.009***
	(0.012)	(0.011)	(0.010)	(0.005)	(0.008)	(0.005)	(0.003)
AP	0.423***	-0.190***	0.270***	0.119***	0.216***	-0.010***	-0.008***
	(0.007)	(0.008)	(0.006)	(0.004)	(0.005)	(0.003)	(0.002)
Panel B: Return to Algebra II (Fixed Effects)							
Algebra II	0.152***	-0.036***	0.051***	0.007***	0.021***	0.016***	0.002*
	(0.003)	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Panel C: Return to Algebra II (IV)							
Algebra II	0.192***	-0.186***	0.079***	0.018**	0.050***	-0.030***	-0.004
	(0.016)	(0.017)	(0.012)	(0.006)	(0.010)	(0.007)	(0.004)
Mean	0.299	0.277	0.102	0.025	0.063	0.033	0.014
Obs.	85,002	85,002	85,002	85,002	85,002	85,002	85,002

Notes: Each column reports coefficients from an OLS regression at the student level with standard errors in parentheses, clustered by school. The dependent variable in columns 1–2 is enrollment status measured 5 years from the 9th grade. The dependent variable in columns 3–7 is degree completion status measured 4 years from the 12th grade. Panel A reports coefficients on the highest level of math completed from a fixed-effect OLS regression. Panel B estimates the return to Algebra II-taking with school fixed effects. Panel C reports the local average treatment effects (LATEs) of Algebra II-taking, using the likelihood of taking Algebra II as the endogenous variable in the first stage of the 2SLS system, instrumented by the share among all students in each student's school and cohort group who took Algebra II. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of economically disadvantaged students), local unemployment rates, school fixed effects, cohort trend, and standardized math score in grade 8.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3. The Estimated Effects of Mandatory Course-Taking on Algebra II Access

Variable	Cohort 2003 (1)	Cohort 2004 (2)	Cohort 2005 (3)	Cohort 2006 (4)	Cohort 2007 (5)	Cohort 2008 (6)	Cohort 2009 (7)
Female	0.031***	0.032***	0.035***	0.020***	0.015***	0.011***	0.019***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Black/Hispanic	0.052+	0.066*	0.036	0.013	-0.025	-0.005	0.012
	(0.029)	(0.027)	(0.027)	(0.029)	(0.025)	(0.021)	(0.017)
Ever Disadv	-0.061***	-0.084***	-0.075***	-0.095***	-0.084***	-0.060***	-0.041***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
Always Disadv	0.013	0.024**	0.028***	0.021**	0.018*	0.021***	0.015**
	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)
Special Ed	-0.098***	-0.111***	-0.093***	-0.122***	-0.107***	-0.084***	-0.073***
	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)
Limited English	0.037*	0.016	-0.009	0.022+	0.048***	0.020*	0.019*
	(0.015)	(0.013)	(0.013)	(0.012)	(0.012)	(0.010)	(0.009)
8^{th} Math	0.125***	0.118***	0.133***	0.122***	0.115***	0.042***	0.032***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Mean (pre-MMC)	0.381	0.398	0.395	0.379	0.388	0.482	0.564
Obs.	25,023	28718	31,261	32,092	31,393	33,002	30,837

Notes: Each estimate comes from a linear regression of an indicator as to whether an individual took Algebra II on demographic controls (including migrant, age, and age squared), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of disadvantaged students), local unemployment rates, and school fixed effects. White and never disadvantaged are the omitted categories. Cohorts are defined as the year students started 9^{th} grade. Standard errors clustered by school are in parentheses.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4. The Estimated Effects of Mandated Algebra II-Taking

Variable	College Enrollment			College Completion			
	4-year	2-year	Both 4&2 yrs	BA	BA STEM	Selective	AA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Algebra II	-0.009	-0.049^{*}	0.058^{*}	0.124^{***}	0.034^{***}	0.088^{***}	0.004
	(0.023)	(0.024)	(0.026)	(0.016)	(0.008)	(0.013)	(0.010)
Female	0.049^{***}	-0.010^{***}	-0.039^{***}	0.042^{***}	-0.002^{**}	0.020^{***}	0.011^{***}
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Asian	0.080^{***}	-0.077^{***}	-0.002	0.066^{***}	0.051^{***}	0.083^{***}	-0.017^{***}
	(0.006)	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)	(0.002)
Black	0.062^{***}	0.001	-0.063^{***}	-0.007^{**}	0.003^{*}	-0.001	-0.019^{***}
	(0.004)	(0.004)	(0.005)	(0.002)	(0.001)	(0.002)	(0.002)
Hispanic	0.008^{+}	-0.074^{***}	0.066^{***}	-0.002	-0	0.007^{***}	-0.013^{***}
	(0.005)	(0.006)	(0.006)	(0.002)	(0.001)	(0.002)	(0.002)
Ever Disadv	-0.108^{***}	-0.012^{***}	0.120^{***}	-0.036^{***}	-0.003^{***}	-0.019^{***}	-0.009^{***}
	(0.003)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Always Disadv	0.014^{***}	0.038^{***}	-0.052^{***}	-0.001	-0	0.001	0.004^{***}
	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
8th Math	0.151^{***}	-0.073^{***}	-0.078^{***}	0.063^{***}	0.027^{***}	0.050^{***}	-0.003^{**}
	(0.003)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Mean (pre-MMC)	0.298	0.281	0.421	0.105	0.026	0.064	0.033
Obs.	201,685	201,685	201,685	201,685	201,685	201,685	201,685

Notes: Each estimate reports the local average treatment effects (LATEs) of Algebra II-taking, using the likelihood of taking Algebra II as the endogenous variable in the first stage of the 2SLS system, instrumented by the share of students in each student's school who took Algebra II in the 2003 cohort. The dependent variable in columns 1–3 is enrollment status measured 5 years from the 9th grade. The dependent variable in columns 4–8 is degree completion status measured 4 years from the 12th grade. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of disadvantaged students), local unemployment rates, and cohort trend. White and never disadvantaged are the omitted categories. Standard errors clustered by school are in parentheses.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5. The Estimated Heterogeneous Effects of Increased Probability of Taking Algebra II

	Whole Sample	Female	White/ Asian	Black/ Hispanic	Never Disadv	Ever Disadv	Always Disadv	School Disadv Q1	School Disadv Q3	School Disadv Q5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Effects on College Enrollment										
Only in 4-year college within 5yr	-0.009	0.010	0.024	-0.020	0.031	-0.080^{**}	-0.006	0.027	-0.069	-0.033
	(0.023)	(0.031)	(0.035)	(0.028)	(0.039)	(0.025)	(0.031)	(0.048)	(0.078)	(0.022)
Only in 2-year college within 5yr	-0.049^{*}	-0.041	-0.071^{*}	-0.042	-0.040	-0.099^{**}	-0.113^{**}	-0.044	-0.109	-0.053^{+}
	(0.024)	(0.032)	(0.035)	(0.033)	(0.036)	(0.032)	(0.041)	(0.046)	(0.089)	(0.031)
Both 4- and 2-year college within 5yr	0.058^{*}	0.031	0.048	0.062^{+}	0.009	0.178^{***}	0.119^{**}	0.017	0.178^{+}	0.086^{**}
	(0.026)	(0.035)	(0.038)	(0.035)	(0.040)	(0.033)	(0.042)	(0.051)	(0.094)	(0.032)
Panel B: Effects on College Completion										
BA within 4yr	0.124^{***}	0.125^{***}	0.034^{***}	0.035^{***}	0.088^{***}	0.090^{***}	0.246^{***}	0.061^{***}	0.177^{***}	-0.014
	(0.016)	(0.022)	(0.008)	(0.011)	(0.013)	(0.018)	(0.025)	(0.014)	(0.021)	(0.012)
Selective BA within 4yr	0.088^{***}	0.090^{***}	0.177^{***}	-0.020^{*}	0.116^{***}	-0.025^{***}	-0.031^{**}	0.154^{***}	0.063^{+}	-0.067^{***}
	(0.013)	(0.018)	(0.021)	(0.009)	(0.023)	(0.008)	(0.010)	(0.029)	(0.035)	(0.008)
BA in STEM within 4yr	0.034^{***}	0.035^{***}	0.061^{***}	0.006	0.035^{*}	-0.001	-0.001	0.064^{***}	0.015	-0.035^{***}
	(0.008)	(0.011)	(0.014)	(0.005)	(0.015)	(0.005)	(0.006)	(0.019)	(0.022)	(0.006)
Obs.	201,685	101,425	147,779	52,242	118,754	82,931	37,588	45,379	39,030	36,501

Notes: Each estimate reports the local average treatment effects (LATEs) of Algebra II-taking, using the likelihood of taking Algebra II as the endogenous variable in the first stage of the 2SLS system, instrumented by the share of students in each student's school who took Algebra II in the 2003 cohort. School quality is measured by the fraction of disadvantaged students, with the fraction divided into quintiles from low to high. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of economically disadvantaged students), local unemployment rates, cohort trend, and standardized math score in grade 8. Standard errors clustered by school are in parentheses.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6. Difference-in-Differences Estimates of Mandatory Algebra II-Taking

	College Enrollment			College Completion			
Variable	4-year	2-year	Both 4&2 yrs	BA	BA STEM	Selective	AA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Low	-0.035***	0.035***	0.001	-0.020***	-0.003*	-0.014***	0.002
	(0.003)	(0.004)	(0.004)	(0.002)	(0.001)	(0.002)	(0.001)
MMC	0.033***	-0.029***	-0.003	0.036***	0.014***	0.029***	0.004
	(0.008)	(0.008)	(0.009)	(0.006)	(0.003)	(0.005)	(0.003)
MMC*Low	-0.006	0.002	0.004	0.023***	0.006***	0.018***	0.004+
	(0.005)	(0.005)	(0.006)	(0.003)	(0.002)	(0.003)	(0.002)
Obs.	171,097	171,097	171,097	171,097	171,097	171,097	171,097

Notes: Each estimate reports the difference-in-differences effects of mandatory math course-taking. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of economically disadvantaged students), local unemployment rates, cohort trend, standardized math score in grade 8, a dummy for transition cohort, and an interaction of *Low* and the transition dummy. Standard errors clustered by school are in parentheses.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7. The Estimated Effects of Mandated Algebra II-Taking on Advanced Math Coursework

Variable	Take > Algebra II (1)	Take Algebra III (2)	Take Pre-Calculus (3)	Take Calculus (4)	Take AP (5)
Panel A: Overall					
Algebra II	0.072***	-0.141***	0.309***	-0.012	0.114***
	(0.023)	(0.025)	(0.022)	(0.008)	(0.017)
Osb.	201,685	201,685	201,685	201,685	201,685
Panel B: By Disadvantaged Status					
Always Disadvantaged	0.287***	0.718***	0.088***	0.000	0.132***
	(0.032)	(0.035)	(0.030)	(0.013)	(0.024)
Ever Disadvantaged	-0.092	0.219***	0.277***	0.011	0.281***
	(0.089)	(0.082)	(0.076)	(0.028)	(0.061)
Never Disadvantaged	0.125***	-0.201***	0.327***	-0.016*	0.097***
	(0.024)	(0.027)	(0.024)	(0.009)	(0.018)
Panel C: By Math Ability					
8th Math Quintile 1	0.085**	0.738***	0.083**	0.003	0.125***
	(0.039)	(0.042)	(0.036)	(0.016)	(0.027)
8th Math Quintile 2	0.147***	0.426***	0.113***	-0.003	0.131***
	(0.027)	(0.027)	(0.025)	(0.010)	(0.018)
8th Math Quintile 3	0.267***	0.385***	0.179***	0.006	0.140***
	(0.025)	(0.025)	(0.024)	(0.009)	(0.017)
8th Math Quintile 4	0.309***	0.328***	0.213***	-0.004	0.096***
	(0.024)	(0.025)	(0.024)	(0.009)	(0.017)
8th Math Quintile 5	0.312***	0.311***	0.178***	0.001	0.071***
	(0.023)	(0.026)	(0.025)	(0.010)	(0.021)

Notes: Each estimate reports the local average treatment effects (LATEs) of Algebra II-taking, using the likelihood of taking Algebra II as the endogenous variable in the first stage of the 2SLS system, instrumented by the share of students in each student's school who took Algebra II in the 2003 cohort. The dependent variables are whether a student took a course higher than Algebra II (column 1); whether a student took Algebra III (column 2); whether a student took Pre-Calculus (column 3); whether a student took Calculus (column 4); and whether a student took an AP course (column 5). Each row in Panels B and C is estimated separately. Panel A presents estimates for the whole sample. Panel B presents estimates by disadvantaged status. Panel C presents estimates by math ability measured by 8th grade standardized math score quintiles. All columns include for demographic controls (including migrant, age, and age squared), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of disadvantaged students), local unemployment rates, and cohort trend. White and never disadvantaged are the omitted categories. Standard errors clustered by school are in parentheses.

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table 8. The Estimated Effects of Mandated Algebra II-Taking

Variable	College Enrollment			College Completion			
	4-year	2-year	Both 4&2 yrs	BA	BA STEM	Selective	AA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Baseline Estimates							
Algebra II	-0.009	-0.049*	0.058*	0.124***	0.034***	0.088***	0.004
	(0.023)	(0.024)	(0.026)	(0.016)	(0.008)	(0.013)	(0.010)
Obs.	201,685	201,685	201,685	201,685	201,685	201,685	201,685
Panel B: Drop Transition Cohorts							
Algebra II	-0.000	-0.042+	0.043+	0.100***	0.027***	0.076***	0.006
	(0.021)	(0.022)	(0.023)	(0.014)	(0.008)	(0.012)	(0.009)
Obs.	142,596	142,596	142,596	142,596	142,596	142,596	142,596
Panel C: Transcript Restrictions							
Algebra II	-0.027	-0.050+	0.076**	0.134***	0.039***	0.095***	0.007
	(0.026)	(0.027)	(0.029)	(0.018)	(0.010)	(0.015)	(0.011)
Obs.	167,101	167,101	167,101	167,101	167,101	167,101	167,101
Panel D: Add Science Course-taking Measures							
Algebra II	-0.048+	-0.039	0.087**	0.111***	0.026**	0.079***	0.001
	(0.025)	(0.026)	(0.028)	(0.017)	(0.009)	(0.014)	(0.010)
Obs.	201,685	201,685	201,685	201,685	201,685	201,685	201,685
Panel E: Use Course Description							
Algebra II	0.041	0.005	-0.045	0.756***	0.164***	0.461***	0.096**
	(0.082)	(0.086)	(0.092)	(0.071)	(0.033)	(0.053)	(0.037)
Obs.	157,890	157,890	157,890	157,890	157,890	157,890	157,890

Notes: Each estimate reports the local average treatment effects (LATEs) of Algebra II-taking, using the likelihood of taking Algebra II as the endogenous variable in the first stage of the 2SLS system, instrumented by the share of students in each student's school who took Algebra II in the 2003 cohort. The dependent variable in columns 1–3 is enrollment status measured 5 years from the 9th grade. The dependent variable in columns 4–8 is degree completion status measured 4 years from the 12th grade. Panel A presents baseline estimates from Table 4. Panel B drops transition cohorts (2006–2007) and Panel C restricts the sample to students who are enrolled in a Michigan high school in both 11th and 12th grade. Panel D adds science course-taking measures (including take regular biology, take advanced biology, take regular chemistry, take advanced chemistry, take regular physics, and take advanced physics) and Panel E uses course-descriptions from catalogs. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of disadvantaged students), local unemployment rates, and cohort trend. White and never disadvantaged are the omitted categories. Standard errors clustered by school are in parentheses.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.1. First-Stage Estimates of Algebra II-Taking

	Algebra II-taking (1)	Mandatory Algebra II-taking (2)
$Share_c$	0.405***	
	(0.006)	
MMC × $Share03_s$		-0.350***
		(0.007)
Female	0.032***	0.021***
	(0.003)	(0.002)
Asian	0.072***	0.040***
	(0.007)	(0.004)
Black	0.144***	0.120***
	(0.005)	(0.003)
Hispanic	0.031***	0.014**
	(0.009)	(0.005)
Ever Disadvantaged	-0.063***	-0.059***
	(0.004)	(0.003)
Always Disadvantaged	0.025***	0.028***
	(0.005)	(0.003)
Special Education	-0.102***	-0.103***
	(0.005)	(0.003)
Limited English	0.031***	0.068***
	(0.008)	(0.005)
Migrant	0.140***	0.085***
	(0.028)	(0.020)
8^{th} Math	0.122***	0.097***
	(0.001)	(0.001)
MMC		0.228***
		(0.005)
$Share03_s$		0.296***
		(0.005)
Obs.	85,002	201,685

Notes: Dependent variable is equal to 1 if the student enrolled in Algebra II and 0 otherwise. Panel A reports first-stage coefficients in Section 3: Estimating the Long-Run Impacts of Math Course-Taking using 2003–2005 cohorts. Panel B presents first-stage coefficients in Section 4: Estimating the Impacts of Mandatory Algebra II-Taking using 2003–2009 cohorts. All columns include for demographic controls (including age and age squared), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of economically disadvantaged students), local unemployment rates, and cohort trend.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2. The Sensitivity of Estimated Effects of Math Course-Taking on College Enrollment and Completion

Variable	College Enrollment			College Completion				
	4-year	2-year	Both 4&2 yrs	BA	BA STEM	BA Elite	AA	Certificate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2003–2005 Cohorts								
Algebra II	0.192***	-0.186***	-0.006	0.079***	0.018**	0.050***	-0.030***	-0.004
	(0.016)	(0.017)	(0.018)	(0.012)	(0.006)	(0.010)	(0.007)	(0.004)
Mean	0.299	0.277	0.424	0.102	0.025	0.063	0.033	0.014
N	85,002	85,002	85,002	85,002	85,002	85,002	85,002	85,002
Panel B: 2005 Cohort								
Algebra II	0.196***	-0.137***	-0.059+	0.083***	0.025*	0.068***	-0.043***	0.005
	(0.027)	(0.029)	(0.030)	(0.020)	(0.011)	(0.017)	(0.013)	(0.008)
Mean	0.300	0.284	0.416	0.104	0.026	0.063	0.033	0.016
N	31,261	31,261	31,261	31,261	31,261	31,261	31,261	31,261
Panel B: 2005–2007 Cohorts								
Algebra II	0.195***	-0.113***	-0.082***	0.101***	0.021***	0.081***	-0.038***	0.003
	(0.015)	(0.017)	(0.017)	(0.011)	(0.006)	(0.009)	(0.007)	(0.004)
Mean	0.295	0.290	0.416	0.107	0.027	0.063	0.034	0.014
N	94,746	94,746	94,746	94,746	94,746	94,746	94,746	94,746

Notes: Each estimate reports the local average treatment effects (LATEs) of Algebra II-taking, using the likelihood of taking Algebra II as the endogenous variable in the first stage of the 2SLS system, instrumented by the share among all students in each student's school and cohort group who took Algebra II. The dependent variable in columns 1–3 is enrollment status measured 5 years from the 9th grade. The dependent variable in columns 4–8 is degree completion status measured 4 years from the 12th grade. Panel A uses 2003–2005 pre-policy cohorts and Panel B uses only 2005 cohort. Panel C uses 2005–2007 cohorts. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of economically disadvantaged students), local unemployment rates, cohort trend, and standardized math score in grade 8. Standard errors clustered by school are in parentheses.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3. The Sensitivity of Estimated Effects of Math Course-Taking on Additional Postsecondary Outcomes

	Enroll On-time (1)	Any Degree In 5 Yrs (2)	BA In 5 Yrs (3)	BA In 6 Yrs (4)	AA In 2 Yrs (5)	AA In 3 Yrs (6)	AA In 4 Yrs (7)	Certificate In 2 Yrs (8)	Certificate In 3 Yrs (9)	Certificate In 4 Yrs (10)
Panel A: 2003–2005 Cohorts										
Algebra II	0.136***	0.109***	0.141***	0.139***	-0.016***	-0.025***	-0.030***	-0.001	0.001	-0.004
	(0.017)	(0.016)	(0.015)	(0.015)	(0.003)	(0.006)	(0.007)	(0.002)	(0.004)	(0.004)
Mean	0.516	0.231	0.193	0.239	0.006	0.021	0.033	0.004	0.009	0.014
Observations	85,002	85,002	85,002	85,002	85,002	85,002	85,002	85,002	85,002	85,002
Panel B: 2005 Cohort										
Algebra II	0.207***	0.130***	0.168***	0.174***	-0.019***	-0.019*	-0.043***	0.002	0.009	0.005
	(0.029)	(0.026)	(0.025)	(0.026)	(0.006)	(0.010)	(0.013)	(0.005)	(0.006)	(0.008)
Mean	0.529	0.232	0.194	0.241	0.006	0.020	0.033	0.005	0.011	0.016
Observations	31,261	31,261	31,261	31,261	31,261	31,261	31,261	31,261	31,261	31,261
Panel C: 2005–2007 Cohorts										
Algebra II	0.183***	0.136***	0.167***	0.158***	-0.014***	-0.026***	-0.038***	0.001	0.001	0.003
	(0.017)	(0.015)	(0.014)	(0.014)	(0.003)	(0.006)	(0.007)	(0.003)	(0.004)	(0.004)
Mean	0.544	0.241	0.201	0.233	0.006	0.021	0.034	0.005	0.010	0.014
Observations	94,746	94,746	94,746	94,746	94,746	94,746	94,746	94,746	94,746	94,746

Notes: Each column reports estimates of the local average treatment effects (LATEs) of Algebra II-taking, using the likelihood of taking Algebra II as the endogenous variable in the first stage of the 2SLS system, instrumented by the share of students in each student's school who took Algebra II in the 2003 cohort. The dependent variable is on-time enrollment in column 1; receive any postsecondary degree in 5 years in column 2; receive BA degree in columns 3–4; receive AA degree in columns 5–7; and receive certificate in columns 8–10. Panel A uses the 2003–2005 cohorts and Panel B uses only the 2005 cohort. Panel C uses the 2005–2007 cohorts. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of economically disadvantaged students), local unemployment rates, school fixed effects, cohort trend, and standardized math score in grade 8.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4. The Estimated Effects of Algebra II-Taking (OLS)

Variable	College Enrollment			College Completion				
	4-year	2-year	Both 4&2 yrs	BA	BA STEM	BA Elite	AA	Certificate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 2003–2005 Cohorts (Pre-MMC)								
Algebra II	0.152***	-0.036***	-0.116***	0.051***	0.007***	0.021***	0.016***	0.002***
	(0.003)	(0.004)	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Mean	0.299	0.277	0.424	0.102	0.025	0.063	0.033	0.014
N	85,002	85,002	85,002	85,002	85,002	85,002	85,002	85,002
Panel B: 2003–2007 Cohorts (Pre-MMC & Transition)								
Algebra II	0.138***	-0.023***	-0.115***	0.046***	0.005***	0.017***	0.017***	0.002***
	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean	0.296	0.283	0.421	0.105	0.026	0.063	0.034	0.014
N	148,487	148,487	148,487	148,487	148,487	148,487	148,487	148,487
Panel C: 2008–2009 Cohorts (Post-MMC)								
Algebra II	0.062***	0.071***	-0.133***	0.009***	-0.001	0.001	0.010***	0.001
	(0.004)	(0.006)	(0.007)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Mean	0.280	0.277	0.443	0.058	0.014	0.032	0.029	0.011
N	63,839	63,839	63,839	63,839	63,839	63,839	63,839	63,839

Notes: Each column reports coefficients from an OLS regression at the student level with standard errors in parentheses, clustered by school. The dependent variable in columns 1–3 is enrollment status measured 5 years from the 9th grade. The dependent variable in columns 4–7 is degree completion status measured 4 years from the 12th grade. Panel A uses 2003–2005 (pre-policy) cohorts and Panel B adds 2006–2007 (transition) cohorts. Panel C uses 2008–2009 (post-policy) cohorts. All columns include for demographic controls (including gender, race, migrant, age, age squared, and disadvantaged status), school characteristics (including log of the number of teachers, log of real per-pupil expenditures, log of enrollment and enrollment squared, magnet school indicator, and the share of economically disadvantaged students), local unemployment rates, school fixed effects, cohort trend, and standardized math score in grade 8. Standard errors clustered by school are in parentheses.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$