The Labor of Division: Returns to Compulsory Math Coursework

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RETURNS TO COMPULSORY MATH COURSEWORK

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ABSTRACT

Labor economists know that a year of schooling raises earnings but have little evidence on the impact of specific courses completed. I identify the impact of math coursework on earnings using the differential timing of state-level increases in high school graduation requirements as a source of exogenous variation. The increased requirements induced large increases in both the completed math coursework and earnings of blacks, particularly black males. Two-sample instrumental variable estimates suggest that each additional year of math raised blacks' earnings by 5-9%, accounting for a large fraction of the value of a year of schooling. Closer analysis suggests that much of this effect comes from black students who attend non-white schools and who will not attend college. The earnings impact of additional math coursework is robust to changes in empirical specification, is not driven by selection into the labor force, and persists when earnings are conditioned on educational attainment. The reforms close one fifth of the earnings gap between black and white males. Estimates for whites are similar to those of blacks but are much noisier due to the reforms' weaker impact on white students' coursework. These results suggest that math coursework is an important determinant of the labor market return to schooling, that simple minimum requirements largely benefit low-skilled students, and that more demanding requirements might be necessary to improve the outcomes of high-skilled students.

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Introduction and Previous Literature

An extensive literature within labor economics concludes that an additional year of schooling raises individuals' labor market earnings by an average of roughly 10-15% (Card, 1999; Oreopoulos, 2006). Relatively little is known about whether the content of that additional year of schooling affects the returns to that schooling. The problem is twofold. First, most data sets used by labor economists contain only the amount of completed schooling, not the coursework completed during that schooling. Second, even when the coursework completed by students is known, researchers have generally been unable to deal with the bias arising from the non-random selection of students into courses. I overcome the data limitations by compiling a nationally representative time series of high school transcripts that contained detailed information on students' completed coursework. I address selection bias by instrumenting students' coursework with differentially timed state-level reforms of high school graduation requirements. These reforms, combined with unique data described in more detail below, allow for the first clear causal estimates of the impact of coursework on labor market outcomes.

This paper builds on a large literature demonstrating the labor market return to a year of schooling. Simple correlations between schooling and earnings are likely biased by omission of variables such as ability and family background, so researchers have sought instrumental variables that provide exogenous variation in individuals' schooling attainment. Perhaps the most commonly used instruments are compulsory schooling laws that require students to remain in school until a certain age. Changes in such laws, or interactions between birth timing and such laws, have provided a rich set of results, starting with Angrist and Krueger (1991) and continuing more recently with Acemoglu and Angrist (2000), Lleras-Muney (2005) and Oreopoulos, Page and Stevens (2006). These studies conclude that compulsory schooling laws do increase schooling attainment and that this increased attainment in turn improves earnings, mortality rates, and the educational progress of the children of those affected by such laws. The causal benefit of schooling is thus clear. What is less clear is why schooling attainment has such a large, positive impact on earnings (as well as the other outcomes measured). The compulsory schooling laws that serve as instruments in these studies affect only the amount of time spent in school. This paper provides the first evidence that compulsory changes in the amount of time spent in specific courses can be a significant determinant of the labor market return to a year of schooling.

Only a few previous studies have attempted to explore the value of specific coursework and have met with mixed success. Altonji (1995), Levine and Zimmerman (1995), and Rose and Betts (2004) all use longitudinal surveys of high school students to track the relation between coursework and labor market outcomes. These papers use various techniques to deal with selection bias, including high school fixed effects, instrumenting for coursework with average coursework completed at the students' high schools, and controlling flexibly for students' measured abilities. Though the results of these papers vary substantially, the latter two find that math courses are powerful predictors of earnings later in life, at least for some demographic subgroups. The fact that these studies reach differing conclusions about the value of specific coursework may in part be attributed to the imperfections in the techniques for dealing with selection bias. High school fixed effects do not control for differences in individual ability, results from controlling for individual ability directly may depend heavily on functional form assumptions, and average high school courseload is not a valid instrument as it is almost certainly correlated with other factors (such as the quality of the high school's college guidance department) that should directly impact future wages. One of the contributions of this paper is to deal with selection bias in a more convincing way by employing the exogenous shock of state-level policy changes.

Whether specific coursework affects the value of a year of schooling is a critical public policy question because it affects both the allocation of scarce public funds within educational systems and the extent to which policymakers should view school curricula as tools for improving the workforce. This is of particular concern given a recent literature that attributes rising income inequality in the U.S. in part to skill-biased technological change, as summarized in Autor and Katz (1999) and given a longer historical perspective in Goldin and Katz (2007). One particular version of this theory, explored by Autor, Levy and Murnane (2003) and by Goos and Manning (2007) suggests that technology is replacing ``middling'' routinized jobs, thus polarizing the workforce into low- and high-skilled non-routinized jobs. There is a sense that a high quality educational system should impart to its students skills that increase their probability of obtaining high-earning, technologically sophisticated jobs. This was certainly the belief of the authors of the report that spurred the reforms central to this paper. Whether specific courses are particularly effective in providing good labor market opportunities to students is of critical importance given this country's growing income inequality.

To preview the paper's central results, I find that The increased requirements induced large increases in both the completed math coursework and earnings of blacks, particularly black males. Two-sample instrumental variable estimates suggest that each additional year of math raised blacks' earnings by 5-9%, accounting for a large fraction of the value of a year of schooling. Closer analysis suggests that much of this effect comes from black students who attend non-white schools and who will not attend college. The earnings impact of additional math coursework is robust to changes in empirical specification, is not driven by selection into the labor force, and persists when earnings are conditioned on educational attainment. The reforms close one fifth of the earnings gap between black and white males. Estimates for whites are similar to those of blacks but are much noisier due to the reforms' weaker impact on white students' coursework. These results suggest that math coursework is an important determinant of the labor market return to schooling, that simple minimum requirements largely benefit low-skilled students, and that more demanding requirements might be necessary to improve the outcomes of high-skilled students.

Description of Reforms

The increased graduation requirements that serve here as the exogenous source of variation in student coursework were prompted largely by the publication in April 1983 of ``A Nation at Risk'', the final report of President Reagan's National Commission on Excellence in Education. The commission had been convened to address perceived declines in quality of education received by American high school students. The first two sentences of the report read: ``Our Nation is at risk. Our once unchallenged preeminence in commerce, industry, science, and technological innovation is being overtaken by competitors throughout the world.'' The report continued by mentioning Japan, South Korea and Germany as countries making technological advances in industries where America had historically been dominant, concluding that ``Learning is the indispensable investment required for success in the 'information age' we are entering.''

One of the primary causes of the educational decline cited by the commission was that ``Secondary school curricula have been homogenized, diluted, and diffused to the point that they no longer have a central purpose.... This curricular smorgasbord, combined with extensive student choice, explains a great deal about where we find ourselves today.'' Noting that American high school students earned 25% of their credits in ``physical and health education, work experience outside the school, remedial English and mathematics, and personal service and development courses'', the commission proposed that state and local graduation requirements be strengthened dramatically. Specifically, the commission recommended that ``all students seeking a diploma be required to lay the foundations in the Five New Basics by taking the following curriculum during their 4 years of high school: (a) 4 years of English; (b) 3 years of mathematics; (c) 3 years of science; (d) 3 years of social studies; and (e) one-half year of computer science.''

The vast majority of states reacted to the commission's recommendations by increasing (or imposing for the first time) the minimum number of years in various subjects necessary for a students to receive a high school diploma, though not necessarily to the levels that the commission had recommended. Based on documents from the Education Commission for the States, I construct for each state and for each graduating class of 1982 through 1994 the minimum number of math, science, social studies, English and other courses a student would need to complete in order to receive a high school diploma, where ``course'' refers to a full year of study. Though many states enacted multiple increases simultaneously, I focus in this paper on the increases in math requirements because they are the most common type of reform, because math skills are considered a particular weakness of the American educational system, and because prior research suggests that math coursework may be particularly important in determining wages. I will also show that controlling for other curricular reforms has does not substantially change the estimated impacts of the math reforms.

Figure 1 shows the timing of the math reforms enacted by the states. Only a handful of states enacted reforms applying to classes prior to 1987. The bulk of the reforms are roughly evenly split between the classes of 1987, 1988, and 1989, with only two states enacting reforms after that period. This timing is explained by state legislatures responding relatively quickly to ``A Nation at Risk'' by legislating increased graduation requirements in year Y (where Y was generally 1983, 1984, or 1985) to apply to students entering high school that year, and thus graduating with the class of Y+4. Compared to the class of 1982, the class of 1994 faced higher minimum math requirements in 41 of the 51 states in the U.S.

Table 1 categorizes states by their pre- and post-reform numbers of math courses required for graduation, so that each cell represents a particular type of reform. The most common reform is an increase from one to two math courses required, while the second most common is an increase from two to three. States in the rightmost cells of the top row are those that enacted statewide minimum graduation requirements for the first time.   The majority of states that enacted reforms thus set their new minimum at two courses, lower than the commission's recommendation. The ten states along the diagonal enacted no reforms during this time period.

Figure 2 provides a graphical representation of the reforms. Panels (A) and (B) show each state's minimum math requirements at the beginning and end of the time period in question. In 1982, the vast majority of states allowed students to graduate high school with zero or one completed math courses, while only a handful of states required a two course minimum and none required three. By 1994, the vast majority of states required at least two math courses, and a number required three. Only six states set no minimum requirements at that point in time. Panel (C) shows the different timing of the reforms, dividing the states into those that reformed earlier (1984-1987) and those that reformed later (1988-1990). As will be explained below, these two time periods correspond to those available in the transcript data. There is some geographic correlation among the early reforming states, with such states concentrated in the southeastern and western parts of the country. To account for this, some of the empirical specifications used below will include Census division-specific trends that will control for any geographically differing trajectories of different parts of the country.

Though most states' reforms were one course increases, some states enacted apparently stronger reforms by moving from no statewide minima to two- and three-course minima. I will exploit only the differential timing of the reforms and not their differing magnitudes to achieve identification, for two reasons. First, states that had no requirements at the beginning of this time period, such as California, nonetheless had very few students actually graduating with no completed math coursework. This is likely due to local school districts setting higher minima than required by the state. It is thus unclear whether to label California's increase from zero to two courses a stronger reform than Virginia's increase from one to two courses. Second, some states issue multiple types of high school diplomas that distinguish students by the difficulty of their completed coursework. Thus, although I have categorized states by the lowest requirements that allow graduation from high school, reforms to higher types of diplomas sometimes occurred simultaneously, thus clouding the issue of precisely how strong each state's reform was. The timing of the reforms is, however, less ambiguous and, as seen in figure 2, sufficiently varied as to achieve identification.

Data

If a single data set existed containing information on high school coursework and labor market outcomes for individuals from classes in the 1980s and 1990s, I could use the reforms to instrument for coursework and thus derive unbiased estimates of the impact of coursework on earnings. Unfortunately, there does not exist a single data set containing all of this information in a way that allows me to exploit the differential timing of the reforms spurred by ``A Nation at Risk''. The few longitudinal studies that follow students from high school to the labor market (such as the National Longitudinal Survey of Youth, High School and Beyond, and the National Education Longitudinal Study) cover too few graduating classes to be useful, while most traditional data sets used by labor economists (the Current Population Survey, the Panel Survey of Income Dynamics) contain little or no information on high school coursework.

I solve this problem by exploiting two separate data sources, a time series of transcript studies that I construct and Census data containing labor market outcomes. The transcript data allow estimation of a first stage impact of the reforms on coursework, while the Census data allows estimation of the reduced form impact of the reforms on earnings. Combining these estimates through two-sample instrumental variables (TSIV), as will be described in detail below, generates the impact of coursework on earnings. The following two subsections describe the data sets in more detail.

Transcript Data

The primary challenge in determining whether these reforms affected students' coursework is that no data set contains detailed information on students' coursework on a class-by-class and state-by-state basis. The federal government does, however, collect a national sample of high school transcripts every few years through the National Center for Education Statistics (NCES). One of the earliest such collections occurred for the class of 1982, with the High School and Beyond Survey (HSB). Three more waves followed for the classes of 1987, 1990 and 1994, in transcript studies associated with the National Assessment of Educational Progress (NAEP). For each of these waves, NCES collects from high schools around the country both a set of students' transcripts, which list the specific courses students have completed, as well as each high school's handbook of course descriptions. The latter allows NCES to uniformly code courses that might otherwise have different names in different high schools, according to a scheme known as the Secondary School Taxonomy.

I compile these four transcript collections into a single data set of roughly 70,000 students for whom I can identify their state of high school attendance and the class with which they graduated.[1] The timing of the four waves implies that the 1982 wave is a purely pre-reform sample, the 1987 wave is a mixture of states that had and had not yet enacted reforms, and the 1990 and 1994 waves are post-reform samples. Because the 1990 and 1994 waves include only high school graduates, I exclude high school dropouts from the earlier waves in order to make the sample comparable over time. I also exclude Hispanic students because using state of birth as a proxy for state of high school in the Census data will eliminate the large fraction of Hispanic respondents born in foreign countries. The subsequent analysis will thus focus on four separate demographic groups (black males, black females, white males and white females) because the reforms had differential impacts on these groups' coursework and earnings.

[1] To my knowledge, this is the first such use of this data, perhaps because accessing the state identifiers requires obtaining a restricted-use license from NCES. This license in turn requires users to access the data on a pre-approved site that contains a secured computer. I am grateful to Professor Thomas Bailey and Matthew Heidelberg at the Community College Research Center at Teachers College, Columbia University, for allowing me to use the data through their site.

For each high school graduate of the classes of 1982, 1987, 1990 and 1994, the transcript data allow construction of the number of courses for which the student received credit in various subject areas.[2] For simplicity, I divide math courses into two categories, which I label basic and advanced. Basic courses are those with titles such as vocational mathematics, consumer mathematics, basic mathematics, special education mathematics and pre-algebra. Advanced courses include algebra I, geometry, algebra II, pre-calculus, calculus and statistics. I also compute the total number of completed courses in all other subjects.

Panel (A) of table 2 shows the mean completed coursework for each group over this entire time period. The top row reveals that black students completed nearly identical amounts of math coursework as white students, with each group averaging slightly more than three courses. The composition of that coursework differed greatly, however, between black and white students, with black students and particularly black males completing more of their coursework in basic math. This suggests that black students are exposed to less rigorous math curricula than white students, have lower math skills than white students, or some combination of those two factors. Panel (A) also shows that over this time period, only 38% of black students complete at least four years of math, compared to 43% of white students. Black students also complete fewer total credits in other courses. All of these facts suggest substantial differences in the high school curricula to which black and white students are exposed. Completing a high school degree may thus imply substantial skill differences between these two populations, hence the importance of exploring the returns to not only years of schooling but also to specific coursework.

Census Data

[2] NCES provides a standardized unit of credit, called a Carnegie unit, that represents a standard full-year course.

Because the NAEP waves of the transcript data do not follow students beyond high school to observe later outcomes, I turn to the 5% Public Use Microdata Sample (PUMS) of the 2000 Census, which aims to survey 1 out of every 20 Americans.[3] Outcomes of interest contained in PUMS include respondents' educational attainment, labor force participation, earnings in the past year (i.e. 1999), and occupation. Educational attainment is coded into four categories: high school dropouts, high school graduates, those who attended some college but have not earned a degree, and those who have a college degree (from a two- or four-year college). I recode incomes below $5,000 and above $150,000 as missing to prevent outliers from unduly influencing the results below. Respondents also report their occupation, which PUMS codes into nearly 500 categories according to the Standard Occupational Classification system. I merge these occupations with a normalized measure of the mathematical skill required for each occupation, which I derive from characteristics contained in the federal government's Occupational Network Database (O-Net 4.0).[4] For simplicity, I then divide occupations into those in the upper half of the mathematical skill distribution, which I label ``skilled'' occupations, and those in the lower half, which I label ``unskilled''.

Unlike the transcript data, the Census data does not contain information on the state in which respondents attended high school. I therefore assume that respondents attended high school in their reported states of birth and that they graduated in the class of the year they turned 18 (the median and modal graduation age for Americans).

[3] I accessed the data through http://usa.ipums.org/usa/, which provides a user-friendly interface and excellent documentation.

[4] Specifically, O-Net contains for each occupation four measures of the importance of math, rated on a scale of one to five. These measures are the knowledge of mathematics, the skill of mathematics, the ability of mathematical reasoning, and the ability of number facility. The distinction between these categories is unclear and correlation between them is quite high, so I simply take the average of the four scores.

For comparability to the transcript data, I then limit the sample to those students who turned 18 between 1982 and 1994. The sample thus consists of respondents who are between 24 and 36 years of age in 2000. I avoid assigning state of high school attendance by state of current residence due to the potential for selection bias that might arise from high earning individuals migrating to states with improved educational systems. State of birth, though obviously an imperfect measure of state of high school, is at least exogenous to the reforms studied here.[5] As mentioned previously, because an extremely large fraction of Hispanic respondents are born outside of the U.S., this procedure forces me to exclude Hispanic students from the analysis.

Panel (B) of table 2 shows the mean characteristics of the PUMS sample. I limit the sample to those students who are at least high school graduates to make the sample comparable to the transcript sample. This procedure excludes 23% of black males, 17% of black females, and roughly 10% of white respondents. I show later that this does not create selection bias but nonetheless run many of the subsequent regressions both excluding and including high school dropouts. Even with the sample limited to those with at least a high school degree, large educational disparities are immediately obvious between black and white students and even between black males and females. Conditional on having a high school degree, black students (and particularly black males) are roughly half as likely to be college graduates as their white counterparts. Though black and white women have similar labor force participation, only 77% of black males report being in the labor force relative to 92% of white males. Only 79% of black males report any earnings from the previous year, compared to 92% of white

[5] Rough estimates from the 1990 PUMS suggest that over 75% of high school age students reside in their state of birth, so that measurement error is not overwhelmingly large. Assigning high school class based on current age also introduces measurement error, as some students graduate high school at earlier or later ages. These facts should bias the subsequent results toward zero, suggesting that I may be underestimating the impacts of these reforms on earnings.

males. Conditional on reporting earnings from the previous year, blacks earn $6,000 less than whites, with the gap between black and white males closer to $8,000.

Finally, the measure of occupational math skill suggests that blacks and particularly black males are in less skilled occupations relative to their white counterparts. Only 36% of black males in the sample are in skilled occupations relative to 48% of white males, as defined by the measure of mathematical skill mentioned above. Part of this disparity is clearly due to educational differences, but even conditional on educational attainment blacks and particularly black males are significantly less likely to be in skilled occupations than whites. This can been seen in figure 3, in which panel (A) shows the overall fraction of each subgroup in a skilled occupation (the same fraction shown in the final row of table 2). Panels (B), (C) and (D) show that even conditional on education, black males are 5-10% percentage points less likely to be in skilled occupations than white males, and a similar or even larger gap appears between black and white females. Assuming individuals enter occupations in which they have comparative advantages, these figures suggest that blacks are at a serious disadvantage with regard to the skills required by these occupations. Between education, occupation and earnings, the overall picture is thus a consistent one of blacks disadvantaged relative to whites and black males particularly disadvantaged.

Impact of Reforms on Coursework

Identification Strategy

As the previous literature on this topic has shown, simply comparing the earnings of individuals with differing amounts of math coursework is likely to lead to biased estimates of the impact of this coursework, due to omitted variables such as ability. Previous papers have dealt with this by using instrumental variables or fixed effects techniques that suffer from the flaws mentioned previously. I use the timing of the math reforms induced by ``A Nation at Risk'' as a source of exogenous variation in the math coursework students complete, thus avoiding the bias potentially plaguing previous estimates. Identification will be achieved through the within-state changes in math coursework, controlling for year-specific nationwide shocks. This means that identification comes from the comparison of states whose increased requirements applied to the class of 1987 (or earlier) to those states who increased requirements applied to the classes of 1988-1990.

One potential concern with this strategy is that states enacting earlier reforms may be fundamentally different than states enacting later reforms, particularly with regard to their educational systems. If, for example, states are more likely to pass reforms early if their students have low levels of completed math coursework, then the estimates below might represent mean reversion rather than a true causal impact of the reforms. To show that the date of the reforms seems plausibly exogenous to the math coursework in a given state, columns (1) through (3) of Table 3 shows the mean initial completed coursework (in 1982) of states by the date of their reform. The top row of panel (A) reveals that black students completed 2.77 math courses in early-reforming states while black students in later-reforming states completed 2.81 on average, a statistically insignificant difference as the p-value in column (4) demonstrates. Nor was initial coursework in those state that never enacted reforms statistically different from the later reforming states, as shown in column (5). The next two rows show the fraction of students in 1982 who fell below the new, higher minimum requirements that the state would later set were nearly identical in early- and later-reforming states, as were the average number of courses by which such students failed to meet that new minimum. The final row of panel (A) shows that earnings in early and later reforming states were quite similar for black graduates of the class of 1982, though the non-reforming states did have higher mean incomes. As a result, the empirical analysis below will show results both including and excluding the non-reforming states. Panel (B) suggests roughly similar results for white students, though mean coursework in 1982 does differ significantly by reform wave, so that mean reversion may be a concern for white students.

Initial evidence that the reform timing is clearly associated with sharp breaks in both completed coursework and earnings for black students comes from figure 4. Panels (A) and (B) graph mean completed coursework over time, with early and later reforming states shown separately. For simplicity, non-reforming states have been omitted here. Panel (A) shows that, for black students, completed math coursework rose most sharply in the early time period for early reforming states and the later time period for later reforming states. Overall, black students' completed a remarkable 0.4 more math courses in 1994 than they did in 1982, a nearly 15% increase. Panel (B) repeats the exercise for white students, who show even larger increases in math coursework but somewhat less connection to the timing of the reforms.

Panels (C) and (D) show similar results with the logarithm of annual earnings as the outcome. In panel (C), black students in the class of 1982 have nearly identical earnings in early and later reforming states. A gap opens up, however, for classes in the mid-1980s, where respondents from early reforming states in those classes earn more than their classmates in later reforming states. By 1990, this gap has vanished. In panel (D), no such gap is observable for white students. Taken as a whole, these panels strongly suggest that the math reforms had large impacts on both the coursework and earnings of blacks and smaller or no impacts on those of whites.

To rigorously quantify the impacts of the reforms, I run regressions of the form

$$\text{Courses}_{isc} = \alpha + \beta \text{ MathReform}_{sc} + \mu_c + \nu_s + \varepsilon_{isc}$$

where *Courses* represents the number of courses completed by individual *i* in state *s* in class *c* and *MathReform* indicates whether that individual was subject to an increased math requirement. Class and state fixed effects are included so that β identifies the within-state effect of a reform to math requirements, controlling for nationwide class-specific shocks.

To that baseline specification I sequentially add further controls for other state-level education policies and economic conditions. These regressions thus have the form

$$\text{Courses}_{isc} = \alpha + \beta\ \text{MathReform}_{sc} + \phi\ \text{EdPolicy}_{sc} + \mu_c + \nu_s + \varepsilon_{isc}$$

and

$$\text{Courses}_{isc} = \alpha + \beta\ \text{MathReform}_{sc} + \phi\ \text{EdPolicy}_{sc} + \delta\ \text{Economy}_{sc} + \kappa_d c + \mu_c + \nu_s + \varepsilon_{isc}$$

Here, *EdPolicy* includes the total number of other course requirements, an indicator for an exit exam requirement, and per-student expenditures and student-teacher ratios for state *s* and class *c*.[6] *Economy* includes the state-level poverty and unemployment rates and κ is a vector of linear time trends by Census division *d*.[7] Heteroskedasticity robust standard errors are clustered by state to allow for within-state serial correlation in the error terms, a concern raised by the now well-known result of Bertrand, Duflo and Mullainathan (2004).

Empirical Results

Panel (A) of table 4 contains the central results for the impact of the reforms on completed coursework. The reforms had large and highly statistically significant impacts on black students, inducing black males to complete 0.40 more math courses and black females to complete 0.28 more math courses. For black males, this increase was roughly evenly split between basic and advanced courses, while for black women advanced courses accounted for more than half of the increase. The impact of the reforms is less strong on white students, inducing a 0.19 course rise for white males and a statistically insignificant 0.10 course rise for white females (though white females do

[6] Though some states did introduce exit exams during this time period, the timing of such reforms turns out to be largely uncorrelated with the math reforms of interest, so that inclusion of the exit exam requirement has little impact on any of the subsequent estimates.

[7] Ideally, I would control for state-specific trends, but the four transcript waves represent too few data points per state with which to estimate such trends.

show a statistically significant 0.12 increase in basic math). The reforms thus had powerful impacts on black students, particularly males, smaller impacts on white males, and little impact on white females' coursework. Panel (A) will serve as the first-stage estimates of the impact of the reforms on students' coursework. The reforms are thus strong instruments for black students' coursework (with F-statistics greater than the value of 10 recommended by Bound, Jaeger and Baker (1995)), but less so for white students. As such, I will argue that the subsequent estimates of the impact of math courses on earnings for blacks are clear, while those for white students are at best suggestive.

The bottom half of panel (A) shows the impact of the reforms on the distribution of math courses completed by students. Interestingly, much of the increase in coursework is attributable to large (18 percentage point) drops in the number of black students completing two or fewer math courses. Two thirds (12 percentage points) of those students take a full four years of more of math as a result of the reforms, far above the minima set by the state. This suggests that the state reforms induced schools to emphasize or require amounts of math coursework beyond the state minima, perhaps because schools did not want to be seen as simply fulfilling the bare minimum requirements themselves.[8] Finally, the last row of panel (A) shows that the timing of the math reforms is not associated with statistically significant increases in the total amount of non-math coursework, implying that the reforms worked primarily through increased math coursework.

Panel (B) shows how the estimated impacts of the math reforms change if other education policy controls are added, if economic controls and trends are added, or if the sample excludes those states that did not pass reforms. The general results discussed above hold in all four specifications. Black students are strongly affected by the reforms, with black males more strongly affected. White males are more weakly affected and

[8] This is yet another reason not to use the “strength” of the state reforms as the exogenous variable, as many schools seem to have pushed students beyond the specific state requirements.

white females are barely affected if at all. Panel (C) and (D) apply the four specifications to basic and advanced math. Again, the overall picture changes little with each specification. Black males' increased coursework was roughly split between basic and advanced courses, while black females' increased coursework is generally dominated by advanced coursework, particularly in the specification with the maximum number of additional controls.

Table 5 explores the impact of these reforms by school type. In panel (A), I divide schools into white and non-white schools, where the former are defined as those schools in which more than 80% of the transcripts were collected from white students.[9] Though the reforms have some impact on students in white schools, the bulk of the impact for both black and white students come from those students in non-white schools. Assuming that racial composition is a good proxy for socioeconomic status, this unsurprisingly suggests that the reforms had their largest impact on the most disadvantaged schools. Panel (B) runs a similar analysis, dividing schools into public and private, the latter of which constitute roughly 10% of the sample. Again, unsurprisingly, the reforms had much larger impacts on public schools, which were legally bound by the state requirements than on private schools, which were not.

Impact of Reforms on Earnings

Identification Strategy

To quantify the impacts of the reforms on earnings, I run regressions on the Census data identical in form to those used with the transcript data, but with earnings as the dependent variable. The estimating equations thus look like:

$$\text{Ln(earnings)}_{isc} = \alpha + \beta \text{ MathReform}_{sc} + \mu_c + \nu_s + \varepsilon_{isc}$$

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[9] Other measures of school racial and socioeconomic composition are not comparable across the different waves of transcript data.

$$\text{Ln(earnings)}_{isc} = \alpha + \beta \text{ MathReform}_{sc} + \phi \text{ EdPolicy}_{sc} + \mu_c + \nu_s + \varepsilon_{isc}$$

$$\text{Ln(earnings)}_{isc} = \alpha + \beta \text{ MathReform}_{sc} + \phi \text{ EdPolicy}_{sc} + \delta \text{ Economy}_{sc} + \kappa_d c + \mu_c + \nu_s + \varepsilon_{isc}$$

where the dependent variable represents the logarithm of annual earnings of individual *i* whom I have assigned as a high school student in state *s* and class *c*. All independent variables are the same as in the transcript regressions, so that the impact of the reforms is identified by within-state changes in earnings, controlling for class-specific shocks.

Note that these regressions are not typical Mincer earnings regressions because they control neither for educational attainment nor for experience explicitly. I omit educational attainment here in order to match covariates with the transcript data, which do not contain educational attainment of individuals beyond high school. Labor market experience is approximated by the class fixed effects, which are equivalent to controlling for a nationwide age profile in earnings. The TSIV technique used below necessitates that both stages of estimation involve the same covariates, hence the parsimonious form of the above regressions. The previous regressions (of coursework on reforms) will provide the first stage for the TSIV estimates while these regressions (of earnings on reforms) will act as the reduced form.

Empirical Results

Table 6 shows the impact of the reforms on the annual earnings of each demographic group. Here the sample has been limited to high school graduates in order to best match the sample represented in the transcript data. In the baseline specification, the reforms raise black males' earnings by 3.2% and black females earnings by 2.2%, for a combined impact of 2.6%. These results are all highly statistically significant. The impact on white males is a practically large but statistically insignificant 1.6%, while white females experience no earnings impact. What is perhaps most notable about the coefficients from the baseline specification is they look remarkably similar in relative magnitude to the impacts of the reforms on math coursework, with the strongest impact on black males, a strong but smaller impact on black females, a weaker impact on white males, and no impact on white females.

The estimated impact of the reforms on earnings are fairly stable across specifications. Including additional education policy controls reduces the reforms' impacts slightly. Adding the economic controls and trends further reduces the reforms' impacts, particularly for black females and white males. Even with all of these additional controls, the impact of the reforms on black males' earnings is still a high significant 2.1% increase. Removing the non-reforming states also reduces the apparent impact of the reforms, particularly for whites, though even so the coefficient on black earnings is still a significant 1.9%. The fifth row of the table limits the Census sample to only students from the same classes as the transcript data contain (1982, 1987, 1990 and 1994). Doing this increases the estimated impacts on blacks' earnings. The final row of the table adds back to the sample the high school dropouts previously excluded, the result of which is to leave the estimated impact on blacks' earnings largely unchanged. This suggests the impact is not being driven by selection into the high school graduate sample.

Two-Sample Instrumental Variables

If the observed increased earnings can be attributed solely to the additional math coursework completed by students subject to the reforms, then combining the results of tables 4 and 6 allows me to estimate the return to a year of math coursework. To combine the estimates from the first-stage equations in table 4 with the reduced-form equations in table 6, I turn to the two-sample instrumental variable technique first introduced by Angrist and Krueger (1992).[10] TSIV can be used when one data set

[10] Subsequent papers that have used TSIV include Currie and Yelowitz (2000), who study the impact of public housing on children's outcomes, and Dee and Evans (2003), who study the impact of alcohol consumption on educational attainment.

contains the instrument (the reforms) and endogenous regressor (coursework) and the other data set contains the instrument (the reforms) and outcome of interest (earnings). Both data sets must also share any other covariates to be included in the regressions. In this case, a two-stage least squares procedure can be used, just as in usual IV estimation, but with the first-stage estimates coming from a different data set than the second-stage estimates. Practically, this means using the transcript data to estimate the amount of coursework completed by students in each class and state and then imputing those estimates to individuals in the Census data. Standard errors must then be adjusted to account for the error introduced by imputation.

These two-sample instrumental variables estimates are presented in Table 7, which basically shows the ratio of the coefficients from those two previous tables. The baseline specification suggests that each additional year of math coursework raised blacks' earnings by roughly 8%, with statistically indistinguishable impacts on males and females. Across the various specifications, the estimated return to blacks of an additional year of math coursework ranges from 4.5% to 9.3% and is always statistically significant. Separate estimates for black males and females are at least marginally significant in 11 of the 12 regressions and are never statistically distinguishable from each other. Interestingly, the estimated coefficients for white males range from 3.1% to 10.8% but are only marginally significant in two of the six specifications, an unsurprising result given the relatively weak impact of the reforms on white males' coursework. The noisy estimates for white females range from -3.0% to 6.7%, an unsurprising result given that the first stage is quite weak for such students.

The TSIV estimates imply that blacks benefitted substantially from the additional math courses they were induced to take by these reforms. Each class is estimated on average to raise blacks' earnings by around 8%. Given that 15% is usually an upper estimate for the value of a year of schooling, these results imply that math coursework can a substantial portion of that value, at least for blacks. The results for white males are of the same general magnitude but are only suggestive given the weakness of the instrument for them.

Labor Force Attachment and Human Capital

One concern about these results, particularly for blacks, is that the observed earnings increases associated with the reforms could be driven by selection into the labor force. If, for example, these reforms induced low-earning blacks to drop out of high school or out of the labor market, then the earnings of the remaining workers would rise mechanically and not due to any improvement in individual outcomes. To check whether this is the case, table 8 explores the impact of the reforms on various measures of educational attainment and labor force attachment. The columns alternate between samples including all states and samples excluding non-reforming states, to test the robustness of the results.

Panel (A) focuses on educational attainment. The theoretical impact of increased graduation requirements on educational attainment is ambiguous. More stringent requirements may raise dropout rates as higher proportions of students find themselves unable to meet the new requirements. Conversely, better academic preparation in high school may allow students to succeed in pursuing college educations. The former theory is supported by Dee and Jacob (2006), who find that introduction of exit exam requirements raise dropout rates particularly among black students, and by Lillard and DeCicca (2001), who argue that increased total course requirements have a similar impact. The first row of panel (A) suggests that the reforms are not associated in any practically or statistically significant way with the proportion of black respondents dropping out of high school.[11] Nor is there any robust relationship between the reforms and other measures of educational attainment, for both whites and blacks. This suggests that selection into the high school graduate sample can not be driving the earnings

[11] Though not shown here, the coefficient on an exit exam requirement, when included, is strongly predictive of higher dropout rates among blacks, replicating the results of Dee and Jacob (2006).

results and also that increased educational attainment is not likely the primary channel driving those results.

In panel (B), which limits the sample to the high school graduates considered in previous tables, there is no indication that the reforms are associated with changed labor force participation rates, as measured by the probability or reporting earnings or the employment rate. Selection on this margin is thus unlikely to be driving the earnings results for blacks. The only slightly odd result here is that the reforms seem associated with a substantial 1.7 percentage point drop in the number of white females reporting earnings, though the result disappears when non-reforming states are excluded. Though this association is almost certainly spurious, it does suggest that the results for white females' earnings may be biased by selection into the labor force.

Finally, panel (C) explores the impact of the reforms on occupational skill. Though none of the results is statistically significant, it is at least suggestive that the coefficients on black males are substantially larger than those on any other demographic group. Overall, this table suggests both that selection bias is not driving the earnings results and that neither educational attainment nor occupational choice are strongly associated with the reforms.

Table 9 explicitly explores the impact of educational attainment on these results. Panel (A) uses the full Census sample including high school dropouts, whereas panel (B) is limited to those with at least a high school degree. The first row of each panel runs the baseline specification as in table 6. The second row adds the full set of interactions between four education levels (high school dropout, high school graduate, some college and college degree) and class to fully control for both education and experience levels. These are thus closer to a traditional Mincer regression. The bottom portion of each panel retains those education and experience controls, interacting the math reforms with different education levels to study whether the impact of the reforms is heterogeneous.

The top row of panel (A) shows that in most specifications, inclusion of the high school dropouts either slightly decreases the estimated impacts of the reforms or has little effect. In the specification where the reforms are interacted with education level, two patterns emerge. For black males, the reforms have no impact on high school dropouts, the largest impact on high school graduates, and smaller, statistically insignificant impacts on those with further education. For black females, the results consistent across specifications are the reforms' moderate and statistically insignificant impact on high school graduates and a larger and sometimes significant impact on those with college degrees. The fact that high school dropouts seem unaffected by the reforms is reassuring, as there is no particular reason to believe that the coursework of such students should be changed by increased graduation requirements. That the impact on black males is largest for high school graduates is also reassuring, as the students most affected by such increased minimum requirements are presumably those with the lowest probability of continuing on to higher education. That black female college graduates see an earnings increase may be attributable to the increased advanced coursework completed by such students, though it is impossible to test this specific hypothesis.

Panel (B) shows that the above results are basically unchanged when the sample is limited to high school graduates. The second row of each panel should also be noted, as they suggest that curricular reforms have an earnings impact even conditional on educational attainment and experience, further pointing to the potential shortcoming of traditional Mincer regressions that ignore differences in the quality of schooling.

Racial Gaps

The analysis to this point has focused on black and white students separately, so the final table explores whether these math reforms had an impact on the racial gaps in coursework and earnings between men and women. Each panel shows results, separately for males and females, of regressing given outcomes on a reform indicator, a black indicator and the interaction of the two, as well as the usual state and class fixed effects. The coefficients on the reform indicator measure the mean impact of the reforms, while the coefficients on the interaction measure any additional impact of the reforms on blacks. The black indicator thus measures the within-state race gap.

Column (1) of panel (A) shows that the reforms raised average math coursework for males by 0.19 courses, but that the impact was nearly twice as strong for black males (and additional 0.14 courses). This additional impact thus closed roughly two-thirds of the 0.22 course gap between white and black males in the same state. Omitting non-reforming states in column (2) does not substantially change these conclusions. Columns (3) and (4) show that the within-state gap in advanced coursework was a staggering 0.7-0.8 courses and that reforms closed up to a quarter of that gap. Columns (5) and (6) show that, conditional on education and experience, the within-state gap between white and black males was 22% and the reforms closed 4%, or nearly a fifth of that gap. Similarly, the reforms seemed to have closed about a fifth to a third of the within-state occupational skill gap.

In contrast, panel (B) suggests that the reforms closed relatively little if any of the math coursework gap between black and white females. Oddly, the reforms seem to have widened the earnings gap between the two groups, though this result is likely due to selection bias from the spurious correlation between the reforms and white females' labor participation rates, as noted in Table 8. As with males, the reforms do seem to have closed some portion of the occupation skill gap between black and white females.

Conclusion

This paper presents strong evidence that the specific coursework completed during a year of schooling has a significant impact on the labor market return to that year of schooling. For blacks, particularly males, increased graduation requirements induced more completion of math courses, which in turn led to significantly higher earnings. The reforms also closed some of the occupational skill gap between blacks and whites. In this sense, the curricular reforms succeeded at least somewhat in achieving the curricular improvement envisioned by the authors of ``A Nation at Risk'', causing black students to leave high school with better mathematical preparation.

The increased graduation requirements studied here did not, however, have much impact on the majority of students. In this sense they were a failure, given that the reforms had been suggested as a means to increase the nation's educational and technological competitiveness on the world stage. There is little evidence that higher minimum courseloads improved the productivity of most workers or their capacity to enter the math-intensive occupations that preoccupied the authors of ``A Nation at Risk''. These increased requirements did not appear to produce any additional rocket scientists.

There are two likely explanations for this. First, the new minimum requirements set by most states were relatively low, and most students were already completing relatively high numbers of math courses even prior to the reforms. Second, the graduation requirements generally specified only the number of courses necessary and not the minimum set of skills students would need in order to graduate. As such, the reforms focused on the amount of time spent in class rather than the specific content learned. Students could fulfill the requirements by taking a series of low-level math courses. Schools could enroll students in classes with advanced titles without guaranteeing that any actual objective standards were being met in those classes. For low-skilled groups of students, such as black males, additional low-quality courses may have had value, but high-skilled students would have benefited little from these types of courses.

A subsequent generation of reforms, beginning with exit exam requirements and continuing with the No Child Left Behind Act, has moved beyond simple measurement of time spent in class to measurement of students' skills and academic achievement. This renewed focus on student capabilities may come closer to achieving the goals envisioned a quarter of a century ago by the authors of ``A Nation at Risk'', and will provide rich avenues for future research.

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Figure 1: Timing of Math Reforms

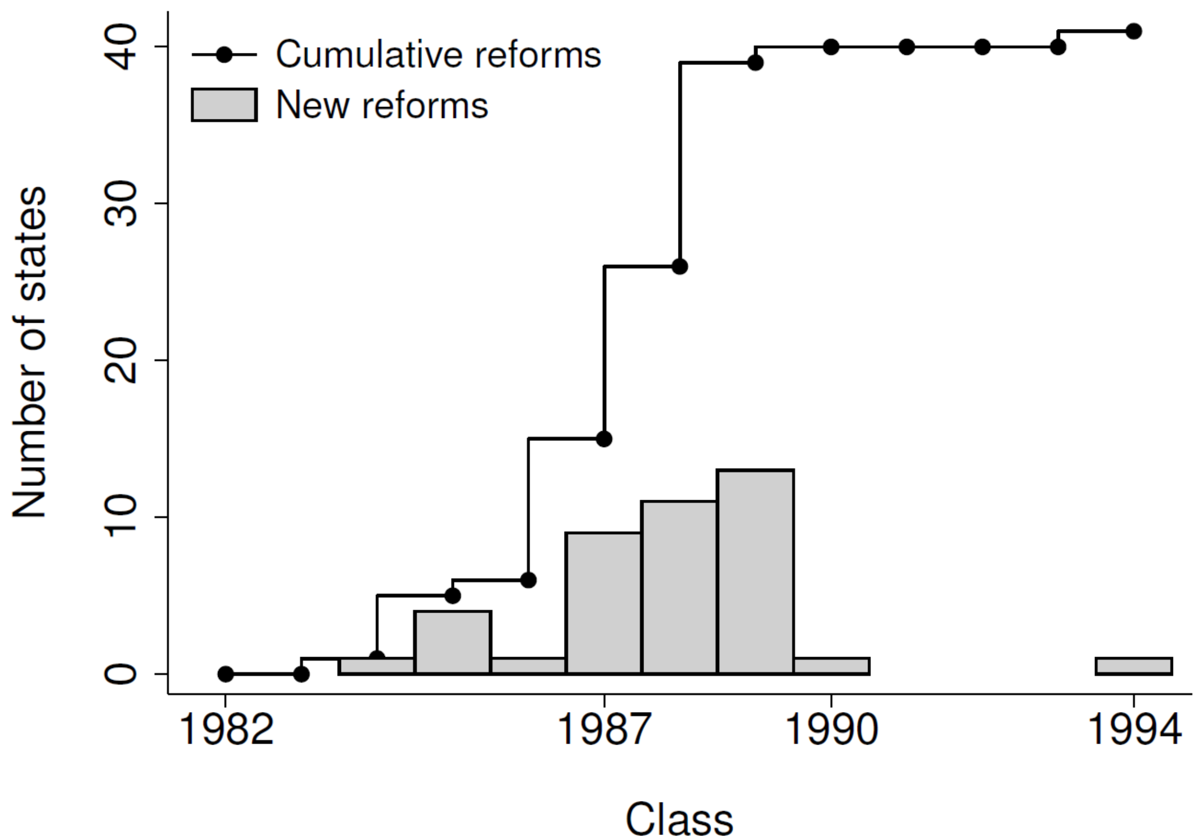

Figure 2: Geographic Variation in Math Reforms

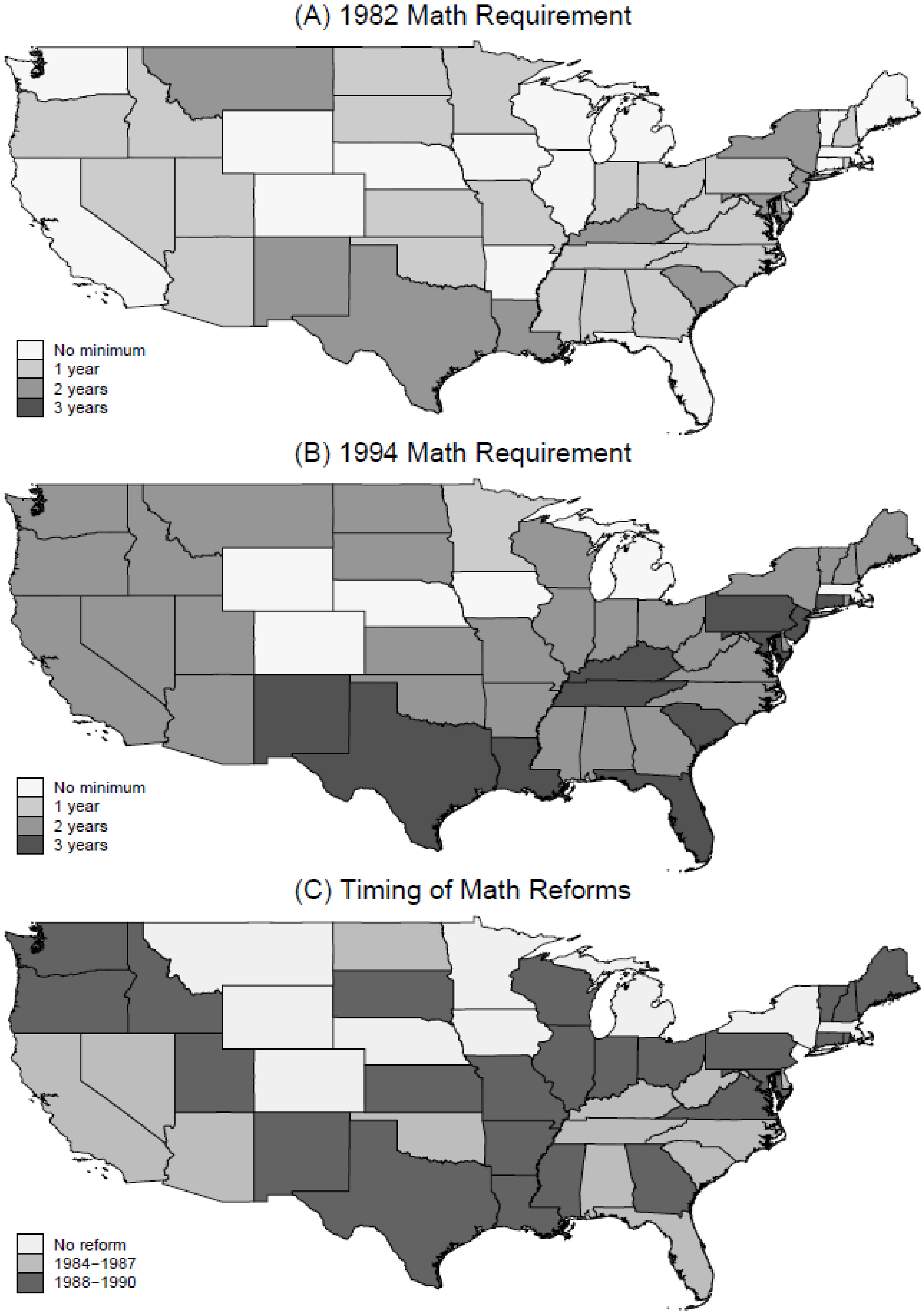

Figure 3: Fraction of Skilled Workers, by Race, Gender and Education

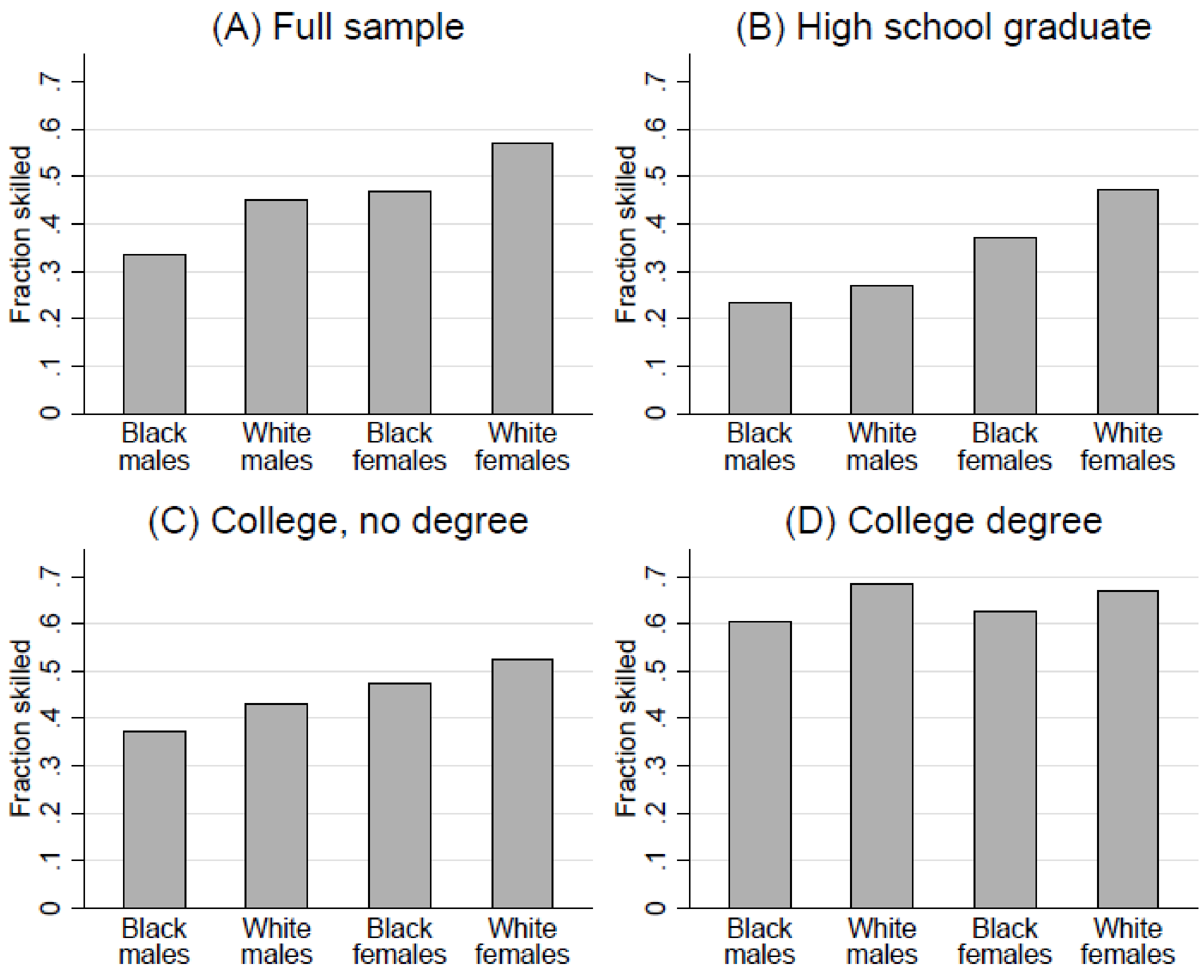

Figure 4: Coursework and Earnings by Reform Wave

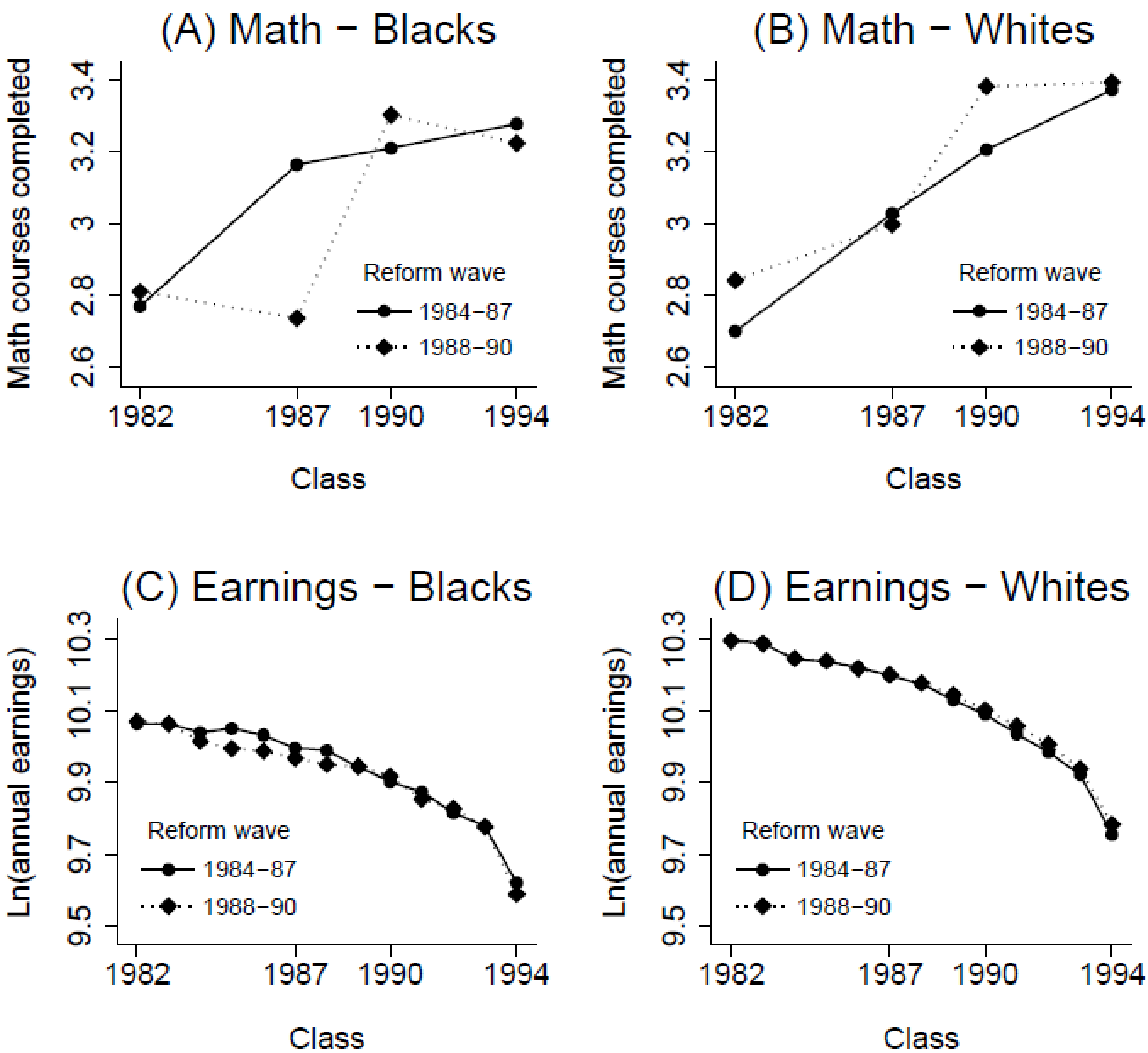

Table 1: State Reforms to Minimum Math Requirements

Pre-Reform Math Courses Required	Post-Reform Math Courses Required: 0	1	2	3
0	CO, IA, MA, MI, NE, WY		1987: CA 1988: AR, IL 1989: ME, VT, WA, WI	1987: FL 1988: CT
1		MN	1984: ND 1985: AL, AK, DC, WV 1986: NV 1987: AZ, DE, NC, OK, TN 1988: GA, ID, MO, OH, OR, UT, VA 1989: IN, KS, MS, NH, RI, SD	1989: PA
2			HI, MT, NY	1987: KY, SC 1988: TX 1989: LA, MD 1990: NM 1994: NJ, TN

Table 2: Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	All blacks	Black males	Black females	All whites	White males	White females
(A) Transcript data						
Math courses	3.09	3.08	3.11	3.18	3.20	3.16
Basic math	1.27	1.37	1.18	0.75	0.82	0.68
Advanced math	1.82	1.70	1.93	2.43	2.38	2.47
Math courses = 2	0.23	0.23	0.23	0.22	0.22	0.22
Math courses = 3	0.40	0.40	0.39	0.35	0.33	0.36
Math courses ≥ 4	0.38	0.37	0.38	0.43	0.45	0.42
Non-math courses	19.61	19.30	19.89	20.34	20.11	20.57
N	11,850	5,470	6,380	54,000	26,640	27,360
(B) Census data						
All high school graduates						
High school graduate	0.42	0.48	0.38	0.31	0.34	0.29
College, no degree	0.34	0.31	0.35	0.27	0.27	0.27
College degree	0.24	0.21	0.27	0.42	0.39	0.44
In labor force	0.78	0.77	0.79	0.85	0.92	0.78
Employed	0.71	0.71	0.72	0.82	0.89	0.76
Positive earnings	0.78	0.79	0.78	0.84	0.92	0.76
With positive earnings						
Annual earnings	25,841	28,478	23,691	32,065	36,791	26,606
Occupational math skill	2.82	2.70	2.92	2.98	2.90	3.07
Skilled occupation	0.43	0.36	0.49	0.53	0.48	0.58
N	209,759	93,439	116,320	1,429,439	699,458	729,981

Notes: In panel (A), sample sizes are rounded to the nearest 10 to comply with the NCES restricted-use data license. Panel (B) limits the Census sample to those who completed at least a high school degree. This excludes 23% of black males, 17% of black females, 11% of white males and 8% of white females.

Table 3: Class of 1982 Outcomes, by Math Reform Wave

	(1)	(2)	(3)	(4)	(5)
	Math reform wave			p-value	
	1984-87	1988-90	Never	(1)=(2)	(2)=(3)
(A) Blacks					
Math courses	2.77	2.81	2.86	0.54	0.28
Fraction below new minimum	0.27	0.26		0.61	
Extent below new minimum	0.93	1.01		0.20	
Ln(annual earnings)	10.06	10.07	10.21	0.64	0.00
(B) Whites					
Math courses	2.70	2.84	2.91	0.00	0.00
Fraction below new minimum	0.25	0.25		0.91	
Extent below new minimum	1.00	0.97		0.34	
Ln(annual earnings)	10.30	10.29	10.38	0.75	0.00

Notes: Columns (1)-(3) show mean values for high school graduates of the class of 1982, by math reform wave. Columns (4) and (5) show p-values resulting from t-tests of the equality of those means.

Table 4: First-Stage Impact of Math Reforms on Coursework

	(1)	(2)	(3)	(4)	(5)	(6)
	All blacks	Black males	Black females	All whites	White males	White females
(A) Baseline specification						
Math courses	0.338***	0.398***	0.281***	0.140**	0.186**	0.100
	(0.093)	(0.116)	(0.085)	(0.065)	(0.078)	(0.065)
Basic math	0.137	0.184*	0.105	0.103**	0.091	0.116**
	(0.094)	(0.097)	(0.109)	(0.045)	(0.055)	(0.047)
Advanced math	0.201*	0.214*	0.177	0.038	0.095	-0.016
	(0.116)	(0.124)	(0.123)	(0.084)	(0.096)	(0.089)
Math courses ≤ 2	-0.177***	-0.196***	-0.160***	-0.083***	-0.098***	-0.071***
	(0.032)	(0.040)	(0.033)	(0.025)	(0.029)	(0.024)
Math courses = 3	0.059**	0.043	0.072**	0.040*	0.032	0.050**
	(0.028)	(0.036)	(0.032)	(0.020)	(0.023)	(0.020)
Math courses ≥ 4	0.118***	0.153***	0.087**	0.043	0.066**	0.021
	(0.038)	(0.046)	(0.036)	(0.030)	(0.033)	(0.031)
Non-math courses	0.348	0.378	0.310	0.065	-0.097	0.243
	(0.397)	(0.330)	(0.508)	(0.280)	(0.317)	(0.279)
(B) Math courses						
Baseline specification	0.338***	0.398***	0.281***	0.140**	0.186**	0.100
+ ed. policy controls	0.324***	0.377***	0.277***	0.175**	0.223**	0.131*
+ econ. controls, trends	0.333***	0.416***	0.271**	0.129*	0.157**	0.101
Baseline - non-reformers	0.420***	0.490***	0.357***	0.119	0.168	0.072
(C) Basic math						
Baseline specification	0.137	0.184*	0.105	0.103**	0.091	0.116**
+ ed. policy controls	0.155*	0.197**	0.129	0.085*	0.073	0.099*
+ econ. controls, trends	0.131	0.250**	0.025	0.088*	0.068	0.109**
Baseline - non-reformers	0.216**	0.216**	0.211*	0.064	0.052	0.078
(D) Advanced math						
Baseline specification	0.201*	0.214*	0.177	0.038	0.095	-0.016
+ ed. policy controls	0.169	0.180	0.147	0.090	0.149	0.032
+ econ. controls, trends	0.202	0.167	0.246*	0.041	0.089	-0.008
Baseline - non-reformers	0.204	0.274*	0.146	0.055	0.115	-0.006
N	11,850	5,470	6,380	54,000	26,640	27,360

Notes: Heteroskedasticity robust standard errors are clustered by state (* p<.10 ** p<.05 *** p<.01). Each coefficient comes from a separate regression of completed coursework on a math reform indicator. All regressions include state and class fixed effects and a gender indicator. The baseline specification, used throughout panel (A), includes no other controls. The second specification adds state-level measures of non-math course requirements and an indicator for an exit exam requirement, as well as statewide expenditures per student and student-teacher ratios. The third specification adds state-level unemployment and poverty rates in the year of high school graduation, as well as Census division-specific linear time trends. The fourth specification is the baseline specification but omits those states that had not enacted reforms by 1990. Sample sizes are rounded to the nearest 10 to comply with the NCES restricted-use data license.

Table 5: Impact on Math Coursework, by School Type

	(1)	(2)	(3)	(4)	(5)	(6)
	All blacks	Black males	Black females	All whites	White males	White females
(A) By school racial composition						
Math reform * white school	0.176	0.247**	0.130	0.111	0.156*	0.072
	(0.107)	(0.120)	(0.128)	(0.069)	(0.081)	(0.069)
Math reform * non-white school	0.353***	0.410***	0.298***	0.187***	0.237***	0.141**
	(0.098)	(0.123)	(0.090)	(0.065)	(0.076)	(0.067)
Non-white school	-0.162*	-0.159*	-0.140	-0.072*	-0.077*	-0.067
	(0.090)	(0.086)	(0.123)	(0.039)	(0.043)	(0.041)
R^2	0.109	0.118	0.109	0.097	0.087	0.113
(B) By school sector						
Math reform * public school	0.338***	0.391***	0.287***	0.163**	0.201**	0.130**
	(0.093)	(0.117)	(0.086)	(0.065)	(0.077)	(0.062)
Math reform * private school	0.127	0.281*	0.045	0.022	0.062	-0.026
	(0.116)	(0.155)	(0.170)	(0.076)	(0.099)	(0.079)
Public school	-0.477***	-0.519***	-0.430***	-0.440***	-0.491***	-0.402***
	(0.083)	(0.107)	(0.106)	(0.047)	(0.052)	(0.052)
R^2	0.118	0.130	0.116	0.114	0.107	0.128
N	11,850	5,470	6,380	54,000	26,640	27,360

Notes: Heteroskedasticity robust standard errors are clustered by state (* p<.10 ** p<.05 *** p<.01). Each coefficient comes from a separate regression of completed coursework on a math reform indicator interacted with school type. In panel (A), white schools are those in which more than 80% of transcripts were collected from white students. All regressions include state and class fixed effects and a gender indicator. Sample sizes are rounded to the nearest 10 to comply with the NCES restricted-use data license.

Table 6: Reduced Form Impact of Math Reforms on Ln(Annual Earnings)

	(1)	(2)	(3)	(4)	(5)	(6)
	All blacks	Black males	Black females	All whites	White males	White females
Baseline specification	0.026***	0.032***	0.022***	0.006	0.016	-0.004
	(0.007)	(0.010)	(0.007)	(0.007)	(0.011)	(0.005)
+ ed. policy controls	0.021***	0.028***	0.017*	0.008	0.015*	0.002
	(0.007)	(0.008)	(0.009)	(0.006)	(0.008)	(0.006)
+ econ. controls, trends	0.015**	0.021***	0.010	0.004	0.008	0.000
	(0.006)	(0.008)	(0.008)	(0.004)	(0.005)	(0.004)
N	163,972	73,629	90,343	1,200,052	642,758	557,294
Baseline - non-reformers	0.019**	0.019*	0.019*	0.004	0.004	0.004
	(0.007)	(0.011)	(0.010)	(0.004)	(0.004)	(0.006)
N	135,849	60,959	74,890	876,461	470,352	406,109
Baseline, transcript classes	0.031***	0.036**	0.029**	0.005	0.011	-0.002
	(0.010)	(0.015)	(0.013)	(0.009)	(0.012)	(0.008)
N	49,361	21,945	27,416	368,252	196,768	171,484
Baseline, with dropouts	0.026***	0.027***	0.026***	0.009	0.019*	-0.002
	(0.008)	(0.009)	(0.008)	(0.007)	(0.011)	(0.005)
N	188,756	87,534	101,222	1,296,571	708,157	588,414

Notes: Heteroskedasticity robust standard errors are clustered by state (* p<.10 ** p<.05 *** p<.01). Each coefficient comes from a separate regression of the logarithm of annual earnings on a math reform indicator. All regressions include state and class fixed effects and a gender indicator. The first four specifications are the same as those in Table 4. The fifth specification includes only the classes of 1982, 1987, 1990 and 1994. The sixth specification includes high school dropouts.

Table 7: Impact of Math Coursework on Ln(Annual Earnings)

	(1)	(2)	(3)	(4)	(5)	(6)
	All blacks	Black males	Black females	All whites	White males	White females
Baseline specification	0.079***	0.083***	0.078***	0.049	0.089	-0.030
	(0.021)	(0.023)	(0.028)	(0.052)	(0.061)	(0.048)
+ ed. policy controls	0.066***	0.075***	0.059*	0.051	0.068*	0.020
	(0.021)	(0.021)	(0.031)	(0.037)	(0.038)	(0.047)
+ econ. controls, trends	0.045**	0.052***	0.035	0.033	0.051	0.005
	(0.017)	(0.018)	(0.030)	(0.030)	(0.034)	(0.041)
Baseline - non-reformers	0.045**	0.041*	0.050*	0.040	0.031	0.067
	(0.018)	(0.023)	(0.029)	(0.032)	(0.027)	(0.082)
Baseline, transcript classes	0.093***	0.091**	0.103**	0.042	0.069	-0.004
	(0.030)	(0.035)	(0.047)	(0.067)	(0.069)	(0.085)
Baseline, with dropouts	0.079***	0.070***	0.091***	0.068	0.108*	-0.011
	(0.022)	(0.023)	(0.030)	(0.053)	(0.062)	(0.048)

Notes: Heteroskedasticity robust standard errors are clustered by state (* p<.10 ** p<.05 *** p<.01). Each coefficient comes from a separate two-sample IV regression of the logarithm of annual earnings on estimated math coursework. The specifications are the same as those used in Table 6.

Table 8: Impact of Reforms on Education, Labor Force Attachment, and Occupational Skill

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Black males		Black females		White males		White females	
(A) Full sample								
High school dropout	-0.004	0.008	-0.005	0.001	-0.005**	-0.001	0.001	0.003
	(0.006)	(0.008)	(0.004)	(0.006)	(0.002)	(0.002)	(0.002)	(0.003)
High school graduate	-0.010	-0.006	-0.006	0.003	0.002	-0.001	0.001	-0.001
	(0.008)	(0.008)	(0.006)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)
College, no degree	0.006	-0.007	0.009	-0.003	0.002	-0.003	0.004	-0.001
	(0.007)	(0.008)	(0.005)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)
College degree	0.008**	0.005	0.002	-0.001	0.001	0.005	-0.007*	-0.001
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)
N	121,203	100,234	140,595	116,864	784,855	580,940	795,065	589,373
(B) High school graduates								
Positive earnings	0.004	-0.008	-0.005	-0.008	-0.001	0.000	-0.017**	0.001
	(0.005)	(0.005)	(0.005)	(0.008)	(0.001)	(0.002)	(0.007)	(0.003)
Employed	0.005	0.005	-0.006	-0.015	0.003	0.001	-0.015**	0.004
	(0.006)	(0.007)	(0.006)	(0.009)	(0.002)	(0.002)	(0.007)	(0.003)
N	93,439	77,291	116,320	96,339	699,458	511,899	729,981	536,065
(C) Positive earnings								
Skilled	0.009	0.007	0.000	-0.001	0.001	0.004	0.003	-0.001
	(0.007)	(0.010)	(0.004)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)
Occupational math skill	0.012	0.007	0.002	0.007	-0.000	0.004	0.003	-0.002
	(0.010)	(0.012)	(0.008)	(0.013)	(0.006)	(0.006)	(0.004)	(0.007)
Observations	72,614	60,073	90,159	74,738	639,688	467,817	557,289	406,057
Exclude non-reformers		X		X		X		X

Notes: Heteroskedasticity robust standard errors are clustered by state (* p<.10 ** p<.05 *** p<.01). Each coefficient comes from a separate regression of the outcome on a math reform indicator. All regressions include state and class fixed effects and a gender indicator. Even-numbered columns omit states that had not enacted reforms by 1990.

Table 9: Impact of Reforms on Ln(Annual Earnings), by Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Black males		Black females		White males		White females	
(A) Full sample								
Math reform (baseline)	0.028***	0.014	0.026***	0.014	0.020*	0.005	-0.001	0.004
	(0.009)	(0.012)	(0.008)	(0.010)	(0.012)	(0.004)	(0.005)	(0.005)
Math reform (+ ed./exp. controls)	0.019**	0.014	0.016**	0.011	0.011	0.004	-0.005	0.004
	(0.008)	(0.010)	(0.008)	(0.010)	(0.010)	(0.003)	(0.004)	(0.006)
Math reform * HS dropout	-0.003	0.003	0.025	-0.008	0.018	-0.018	0.005	0.005
	(0.013)	(0.014)	(0.024)	(0.029)	(0.027)	(0.015)	(0.015)	(0.019)
Math reform * HS graduate	0.036***	0.024	0.013	0.018	0.021	-0.001	0.004	0.000
	(0.012)	(0.024)	(0.009)	(0.014)	(0.017)	(0.005)	(0.012)	(0.011)
Math reform * college, no degree	0.015	0.010	0.005	-0.004	0.015	0.011	-0.004	0.000
	(0.017)	(0.028)	(0.013)	(0.020)	(0.013)	(0.007)	(0.007)	(0.011)
Math reform * college degree	0.013	0.010	0.029**	0.028	-0.002	0.010	-0.011	0.009
	(0.017)	(0.023)	(0.013)	(0.018)	(0.005)	(0.007)	(0.006)	(0.010)
N	87,601	72,587	101,256	84,250	709,708	524,359	588,828	431,618
(B) HS graduates								
Math reform (baseline)	0.033***	0.020*	0.022***	0.018*	0.017	0.005	-0.003	0.005
	(0.009)	(0.011)	(0.008)	(0.010)	(0.011)	(0.005)	(0.005)	(0.006)
Math reform (+ ed./exp. controls)	0.026***	0.018*	0.015**	0.016	0.011	0.004	-0.006	0.004
	(0.008)	(0.010)	(0.008)	(0.011)	(0.010)	(0.004)	(0.004)	(0.006)
Math reform * HS graduate	0.038***	0.025	0.013	0.020	0.021	-0.005	0.003	0.000
	(0.012)	(0.025)	(0.009)	(0.015)	(0.019)	(0.006)	(0.012)	(0.011)
Math reform * college, no degree	0.018	0.012	0.005	-0.002	0.015	0.008	-0.005	0.000
	(0.015)	(0.027)	(0.012)	(0.020)	(0.014)	(0.007)	(0.008)	(0.011)
Math reform * college degree	0.014	0.011	0.029**	0.030	-0.001	0.007	-0.011*	0.009
	(0.015)	(0.021)	(0.012)	(0.018)	(0.005)	(0.007)	(0.006)	(0.010)
N	73,691	61,011	90,375	74,916	644,262	471,343	557,703	406,377
Exclude non-reformers		X		X		X		X

Notes: Heteroskedasticity robust standard errors are clustered by state (* p<.10 ** p<.05 *** p<.01). Panel (A) includes the full sample, while panel (B) excludes high school dropouts. Each column in each panel shows three regressions, all of which include state and class fixed effects and a gender indicator. The first regression shows the impact of the math reform on earnings. The second adds education and experience controls as described in the text. The third includes those controls and interacts the reform with education levels. Even-numbered columns omit states that had not enacted reforms by 1990.

Table 10: Impact of Math Reforms on Racial Gaps

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Math courses		Advanced math		Ln(earnings)		Occ. math skill	
(A) Males								
Math reform	0.187**	0.186*	0.090	0.088	0.009	0.000	-0.001	-0.004
	(0.071)	(0.100)	(0.094)	(0.134)	(0.009)	(0.003)	(0.005)	(0.005)
Math reform * black	0.144**	0.182**	0.118	0.214**	0.038***	0.042***	0.028***	0.045***
	(0.068)	(0.068)	(0.098)	(0.104)	(0.005)	(0.005)	(0.009)	(0.006)
Black	-0.216***	-0.253***	-0.712***	-0.808***	-0.216***	-0.222***	-0.130***	-0.147***
	(0.059)	(0.061)	(0.080)	(0.091)	(0.007)	(0.009)	(0.009)	(0.007)
R^2	0.089	0.094	0.084	0.087	0.163	0.159	0.136	0.136
N	32,113	26,035	32,113	26,035	717,953	532,354	712,302	527,890
(B) Females								
Math reform	0.125**	0.114	0.006	-0.000	-0.001	0.008	0.001	-0.008
	(0.060)	(0.087)	(0.086)	(0.119)	(0.004)	(0.006)	(0.004)	(0.006)
Math reform * black	0.019	0.064	0.079	0.112	-0.021***	-0.015**	0.020**	0.036***
	(0.069)	(0.072)	(0.103)	(0.120)	(0.007)	(0.006)	(0.008)	(0.006)
Black	-0.094	-0.143**	-0.613***	-0.651***	-0.011	-0.018	-0.133***	-0.149***
	(0.061)	(0.064)	(0.082)	(0.106)	(0.010)	(0.012)	(0.010)	(0.011)
R^2	0.107	0.117	0.089	0.090	0.150	0.148	0.031	0.033
N	33,735	27,093	33,735	27,093	648,078	481,293	647,448	480,795
Exclude non-reformers		X		X		X		X

Notes: Heteroskedasticity robust standard errors are clustered by state (* p<.10 ** p<.05 *** p<.01). The entire table includes only high school graduates. Each column in each panel shows a regression of the outcome on the variables listed, as well as state and class fixed effects, and a full set of education and experience controls in columns (5)-(8). Even-numbered columns omit states that had not enacted reforms by 1990.