Curriculum Reforms and Infant Health*

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Abstract

This paper examines the effects of high school curriculum reforms on infant health by exploiting sharp and staggered changes across states in core course requirements for graduation. Our results suggest that curriculum reforms significantly reduced the incidence of low birth weight and prematurity for black mothers. For white mothers, the estimated effects are small and generally insignificant. Improvements in maternal health behaviors and family income appear to explain a non-negligible fraction of the observed effects. Finally, we calculate a large social gain induced by favorable infant health outcomes. Several robustness checks and different placebo tests support our findings.

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1 Introduction

Infant mortality rates in the U.S. are more than twenty times higher for low birth weight infants than those of normal birth weight, and two-thirds of all infant deaths in 2016 occurred to infants who were born premature (National Center for Health Statistics, 2016).[1] In addition to imminent health risks, a number of studies have shown that poor infant health persists into adulthood and adversely contributes to behavioral and labor market outcomes (see, for example, Almond et al. 2005; Black et al. 2007; Oreopoulos et al. 2008; and Royer 2009). Perhaps not surprisingly, these large private and social costs of health at birth have attracted considerable attention among policymakers and researchers, and maternal education has long been a focal point. From a theoretical perspective, maternal education affects child quality through a variety of channels, ranging from improved financial resources to healthier behavior, from ability to acquire and process information to positive assortative matching (see, for example, Becker 1960; Grossman 1972; and Behrman and Rosenzweig 2002). Despite this belief, the empirical evidence on the intergenerational transmission of education on child health from the U.S. is mixed (see, for example, Currie and Moretti 2003; and McCrary and Royer 2011).[2]

This lack of consensus aside, existing studies generally measure human capital using years of schooling – in part, because educational attainment is relatively easy to measure and there are readily available quasi-experiments generating arguably exogenous variation in mother's years of schooling. Although years spent in school is crucial in understanding human capital dynamics, what a student does in school may equally be important. For example, Goodman (2018) shows that math coursework in high school explains a non-negligible fraction of the Mincerian return to high school education.[3] As also noted in Altonji (1995): "from the point of view of the human capital interpretations, one would hope that a year's worth of high school

[1] Low birth weight is defined as birth weight less than 2,500 grams and prematurity is defined as gestation less than 37 weeks (National Center for Health Statistics, 2016).

[2] Empirical evidence on the intergenerational transmission of education on child health from other countries is equally mixed (see, for example, Breierova and Duflo 2004; Lindeboom et al. 2009; Chou et al. 2010; and Lundborg et al. 2014).

[3] There is a growing body of research investigating the relationship between high school coursework and short- and long-run outcomes (see, for example, Altonji 1995; Levine and Zimmerman 1995; Rose and Betts 2004; Joensen and Nielsen 2009; and Cole et al. 2016).

Relatedly, Cantoni et al. (2017) studied the causal effect of school curriculum on students' political attitudes using a major textbook reform in China.

courses has value regardless of whether one requires an extra year to complete them."

In this paper, we take a novel approach and exploit changes in the U.S. high school curriculum, which shift the allocation of existing time in school, to analyze the relationship between intergenerational transmission of education and infant health (measured by birth weight and gestational age). Our identifying variation comes from a sharp and staggered introduction of the new curriculum requirements across states. To the best of our knowledge, this is the first paper relating mother's coursework in school to child quality.

The curriculum reforms were largely motivated by "*A Nation at Risk: The Imperative for Educational Reform*,"– issued in 1983. This U.S. government report highlighted the existence of a "cafeteria-style curriculum," diluting course material and allowing students to advance through schooling with minimal effort and strongly recommended the adoption of rigorous curriculum standards. Several states reacted immediately by requiring changes in the minimum number of courses for receiving a high school diploma, i.e., students entering high school one year would have minimum course requirements entirely different from those of students who entered high school just the year before. Forty states, in different years, introduced changes to minimum number of courses required for a high school diploma. All but one state first enacted curriculum reforms in math and several states implemented reforms in multiple subjects.

The state-mandated curriculum reforms led to striking changes in the core courses required for a high school diploma. For example, for states adopting changes in English, the average minimum number of required courses (full year of study) increased from 2 to 3.9 post-reforms. Similarly, the required number of math courses more than doubled (from 0.9 to 2.2) in states adopting changes in math. Consistent with these raw trends, Goodman (2018) shows that black female high school graduates subject to math reforms completed an average of around 0.30 more yearlong math courses than they otherwise would have. The impact on white female students' coursework, however, is less pronounced. As such, the curriculum reforms increased the completed math coursework by a statistically insignificant 0.17.

We use confidential Vital Statistics Natality records for high school classes of 1982 to 1994 in a difference-in-differences (DD) framework to tease out the effects of curriculum reforms on birth outcomes. Our most extensive specifications control for state of high school attendance and cohort fixed effects, as well as time-varying state level educational inputs and economic variables (e.g., average pupil-teacher ratio and teacher salary, per-pupil spending, school finance reforms and unemployment rate), mother/child characteristics and census-division specific trends. To the extent that the exact timing of reform adoption is not systematically related to within-state time varying unobserved factors affecting infant health in a given state, our empirical approach yields credible estimates of the effects of curriculum reforms on infant health. We provide several robustness checks (e.g., tests for the existence of pre-treatment trends as well as endogenous births, conditioning on state specific trends and including controls for other major policy changes) and different placebo tests (e.g., permutation tests) supporting our identifying assumption throughout the paper.

As noted above, the empirical evidence regarding the relationship between mother's education and birth outcomes is mixed. Currie and Moretti (2003), using the availability of colleges in the county of residence at age 17 as an instrument, found that higher maternal education improved infant health. McCrary and Royer (2011), on the other hand, compared women born just before and after the school entry date in a regression discontinuity framework and found no differences in birth outcomes (although women born just before the entry cutoff had substantially more schooling). Our attempt to capture the causal relationship in a very different quasi-experimental setting also contributes to this strand of the literature.

We find that curriculum reforms (changes to minimum number of core courses required), on average, reduced the likelihood of low birth weight by 1.5 percent and the likelihood of prematurity by 1.4 percent for black mothers. The results further suggest that these effects are likely to be driven by mothers with only a high school diploma. We also find evidence for dynamic heterogeneity, with the impacts being more pronounced for cohorts graduating high school in later years of post-adoption. In contrast, the estimated effects of curriculum reforms for white mothers are consistently smaller and are generally insignificant. Our examination of the potential channels reveals that improvements in maternal health behaviors (reduced smoking during pregnancy) and family income can explain non-negligible fraction of the observed effects. We also attempt to disentangle subject-specific reform effects and find suggestive evidence that the impacts observed on infant health were largely driven by math reforms. Finally, total social gain induced by favorable infant health outcomes can be (at least) as large as $234 million (in 2017 dollars). The implied gain using the local average treatment effect estimates of math coursework is more than a billion dollars.

The remainder of the paper is organized as follows. Section 2 discusses the background of the curriculum reforms. Section 3 presents the data and Section 4 describes the methodology. Section 5 discusses the results and presents several robustness checks. Our conclusion and policy implications are provided in Section 6.

2 Background-Curriculum Reforms

In 1981, then U.S. Secretary of Education Terrell Bell commissioned a study to investigate the quality of teaching and learning in secondary education. After two years of intensive work, the National Commission on Excellence in Education issued its landmark report "*A Nation at Risk: The Imperative for Educational Reform.*" The report's assessment of the American education system was staggering: problems ranged from high rates of functional illiteracy to dramatic declines observed over the last two decades in average scores on the Scholastic Aptitude Test, from rising demand for remedial education in four-year public colleges to constantly declining performance of U.S. students in international assessments. "*A Nation at Risk*" also highlighted the existence of a "cafeteria-style curriculum," which diluted course material and allowed students to advance through schooling with minimal effort. For example, by the time of the report's release, twenty-five percent of the credits earned by general track high school students were in physical and health education, work experience outside the school, and personal service and development courses. These deficiencies further coincided with an era of excess demand in scientific and technological fields, and according to the commission, the poor state of the American education system could ultimately lead the U.S. to lose its competitive edge in the global economy ("*A Nation at Risk,*" 1983).

The report also made suggestions on how to improve public schools. The bulk of the recommendations were content related and centered on adoption of rigorous curriculum standards. Specifically, the commission suggested: "high school graduation requirements be strengthened and that, at a minimum, 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."

"*A Nation at Risk*" is one of the few reports that galvanized media and public attention and prompted action. Several states reacted immediately by adopting curriculum reforms that required changes in the minimum number of courses for receiving a high school diploma (Cole et al. 2016; and Goodman 2018). Students starting high school one year would have core course requirements that are entirely different than those who entered high school just the year before.

Using the historical information from the Education Commission of the States, as well as information from various annual collections of the U.S Department of Education's Digest of Education Statistics, we construct a panel containing the minimum number of subject-specific course requirements for high school diploma in each state. Table A1 in the Appendix reports the years of new curriculum adoption along with the reform subjects.[4] Forty states, in different years, introduced changes to the minimum number of courses required for high school diploma.[5] The reforms were introduced between 1980 and 1985. Almost all states first implemented curriculum reforms in math. These math curriculum changes were almost always accompanied by changes to the minimum number of required courses in at least one other subject. Note also that several states implemented reforms in at least three subjects (22 states).[6] Finally, although a large number of reforms were prompted by "*A Nation at Risk,*" it may not be possible to attribute all curriculum changes to the recommendations of the National Commission on Excellence in Education. Six states adopted curriculum reforms within a three-year window prior to the 1983 release of the report.

Figure 1 plots the minimum number of courses (full-year of study) required in each subject in the pre-and post-reform periods. For example, for states adopting changes in English, the average minimum number of courses required increased from 2 to 3.9. Similarly, the required number of math courses more than doubled (from 0.9 to 2.2) in states adopting changes in math. As noted, using the High School and Beyond Survey data, Goodman (2018) finds that black female high school graduates, on average, completed around 0.30

[4] Table A1 reports the initial years coupled with the corresponding subjects in which curriculum reforms were adopted. Four states implemented subsequent reforms in other subjects.

[5] The District of Columbia is included in our effective sample.

[6] Nine states adopted reforms in 4 subjects, 13 states in 3 subjects, 10 states in 2 subjects and 8 states in 1 subject.

more math courses post-adoption (12 percent increase relative to pre-reform sample mean). The impact on white female students' coursework, however, is less pronounced and the reforms increased the completed math coursework by a statistically insignificant 0.17 (6 percent increase relative to pre-reform sample mean).

Prior to continuing, it is important to note that other suggestions were proposed in "*A Nation at Risk*," i.e., improvements in teachers' compensation, better time allocation (longer school day and lengthened school year) and leadership and fiscal support. A 2008 report, as opposed to a grade "A" progress made in graduation requirements, found no progress and rated states "F" in all these other domains ("Strong American Schools," 2008).

3 Data

The primary data for this study come from Vital Statistics Natality records. These data are drawn from birth certificates and cover all births in the United States from 1970 onwards. Each birth certificate contains information about health at birth, as well as information on the mother such as race, age and education. By special permission, we also obtained access to confidential geographical information including mother's state of birth. We focus on two birth outcomes: low birth weight, defined as birth weight less than 2,500 grams and prematurity, defined as gestation less than 37 weeks (see, for example, Currie and Moretti 2003; Currie and Walker 2011; and McCrary and Royer 2011).

Our sample consists of mothers from high school cohorts graduating between 1982 and 1994.[7] We choose these particular cohorts primarily because all reforms were enacted between 1980 and 1985 and applied to students entering high school in that particular reform year or later.[8] We impose several restrictions on our research sample. First, we limit our attention to high school graduates only because we do not know the exact grade students were enrolled in for each subject-specific course. This type of restriction may lead to a selected sample and, for that matter, may bias DD estimates if high school graduation status is correlated

[7] Throughout the analysis, we assume mothers attended high school in the state of birth and graduated high school when they turn 18.

[8] Goodman (2018) also shows a strong relationship between curriculum reforms and high school math coursework for these high school classes. This relationship can be considered the first stage in a two-stage least squares model.

with the curriculum reforms. We address this issue in Section 3 and show that changes in minimum course requirements had no impact on the propensity to receive a high school diploma. Second, we exclude 11 states which did not enact state-mandated curriculum reforms. In these states, local school district agencies had the autonomy to determine the minimum course requirements for graduation. That being said, as discussed below, we also experiment with our analysis by including these 11 states in the sample, and doing so does not largely alter our conclusions. Third, we concentrate on only black and white mothers (ages 18 to 49 years old). Goodman (2018) shows mixed evidence on the relationship between high school curriculum reforms and course work for Hispanic students and thus we opt out of including Hispanic mothers in the main analysis (discussed in more details in Section 5.2).[9] Finally, we dropped plural births, as well as births of less than 500 grams and those with less than 26 weeks gestation (Ludwig and Currie 2010; and Currie and Rossin-Slater 2013). Having imposed these restrictions, we end up with a total sample of more than 3.7 and 16.5 million observations for black and white mothers, respectively.

Table 1 presents the descriptive statistics for the selected pre- and post-reform control variables and birth outcomes. In terms of before and after trends, we do not observe any notable differences in Panel A of Table 1.[10] The fraction of births that were low weight births is around 10 percent for black mothers prior to curriculum reforms and it showed a slight decrease for the post-reform cohorts (Panel B of Table 1). We observe a similar trend for the fraction of births that were premature. As for white mothers, however, the fraction of favorable birth outcomes slightly decreased over time. Specifically, the sample mean for low birth weight (premature) was 4.4 (8.4) percent for post-reform cohorts.

4 Empirical Methodology

To evaluate the effects of curriculum reforms (changes to minimum number of core courses required for a high school diploma) on infant health, we rely on the within-state variation in the differential timing of

[9] Ethnicity status is not available for half of the states until the mid-1980s in the Vital Statistics Natality Records. This information became available for almost all states beginning with late 1980s.

[10] We report the fraction of the sample with more than a high school diploma in the table but we do not control for educational attainment in the specifications reported below since it is potentially endogenous to the curriculum reforms. We provide evidence in favor of this hypothesis in Section 5.3.

curriculum reforms and employ a difference-in-differences framework by estimating the following equation

$$Y_{isc} = \gamma_0 + \gamma_1 CR_{sc} + X'_{isc}\gamma_2 + \lambda_s + \tau_c + \epsilon_{isc}, \tag{1}$$

where Y_{isc} is a birth outcome such as low birth weight or prematurity (i denotes the mother, s the state of high school attendance and c the high school entry cohort), CR_{sc} is an indicator that takes the value of one if mother's high school entry cohort was exposed to curriculum reform (mother is 14 or younger by the time reform was adopted in state s), X'_{isc} is a set of observable characteristics (e.g., mother's age categories, indicators for birth order and an indicator for male child), λ_s and τ_c denote state of high school attendance and cohort fixed effects, respectively and finally, ϵ_{isc} is the error term.[11] The coefficient γ_1 represents the reduced form effect of curriculum reforms on infant health.

In this simple setup, we compare how, on average, infant health outcomes changed for mothers exposed to reforms from its average pre-adoption level relative to the average outcome change experienced by mothers whose state of high school attendance had not yet introduced these reforms. The key identifying assumption underlying this framework is that the exact timing of reform implementation is not systematically related to within-state unobserved factors affecting infant health outcomes. Any systematic pre-adoption differences across states affecting the timing of curriculum reforms and health outcomes at the same time may bias the point estimates. To address this potential contamination of the estimated effects of reforms, we also specify a modified version of equation (1) by adding leads and lags as

$$Y_{isc} = \gamma_0 + \sum_{q=0}^{m} \delta_{-q} CR_{s,c-q} + \sum_{q=1}^{n} \delta_{+q} CR_{s,c+q} + X'_{sc}\gamma_2 + \lambda_s + \tau_c + \epsilon_{isc}. \tag{2}$$

Equation (2) allows us to test for parallel trends condition. The existence of any lag effect is likely to

[11] As noted, we assume mothers attended high school in the state of birth and graduated when they turned 18. We opt out of using state of residence to proxy for high school location because it can potentially be endogenous to the curriculum reforms.

Allocating the state of birth as the state of residence during high school years may cause measurement error that will tend to bias our estimates downward. To explore the relevance of measurement error, we experiment with our analysis by excluding mothers who did not live in their birth state. The estimated effects from these models are consistent with those presented throughout the paper and are available upon request.

invalidate our identification strategy and thus forms the basis for one of the many falsification tests. Finally, standard errors clustered at the state level are reported throughout the analysis.

As a preliminary step, we first test for potential contamination due to selection bias by creating an indicator variable that takes the value of one if the mother graduated from high school. We estimate equation (1) using this selection indicator as the outcome of interest. Table A2 in the Appendix reports the point estimates. As can be seen, dropping mothers without a high school diploma does not appear to pose any threat to our estimates.[12]

Next, we investigate whether state-mandated curriculum reforms led to changes in the composition of births and overall fertility. Specifically, we calculate the (i) fraction of births for a given (pre-determined) observable characteristic, and (ii) total number of births at the birth state-cohort-year level. We then replace the dependent variable in equation (1) with measures of composition and fertility and run separate regressions by controlling for state of high school attendance, cohort and survey year fixed effects. We additionally implement a somewhat similar exercise using mother's log of age at birth. Overall, we do not find any credible evidence for endogenous births (Table A3 in the Appendix).

5 Results

5.1 Baseline Results

We report our baseline results for black mothers from equation (1) in Panel A of Table 2. Note that dependent variables have been multiplied by 100 for ease of interpretation. The estimated effects in each column of the first row comes from a separate regression. The first and fourth columns present the results by conditioning on only (birth) state and cohort fixed effects. The point estimates on being subject to curriculum reforms are negative in both columns, but the coefficient is only statistically significant at the 10% level for prematurity. We add mother's characteristics in the second and fifth columns. Doing so does not have any appreciable impact on the coefficient estimates.

[12] The point estimates (standard errors) for first-time mothers are -0.267 (0.291) and -0.068 (0.100) for black and white mothers, respectively (selection indicator is multiplied by 100 to obtain percent values).

The introduction of the reforms may have coincided with other state-level changes affecting infant health. For example, school spending may have been greater for exposed cohorts which may lead us to erroneously attribute observed effects to changes in the minimum number of course requirements. Furthermore, although the bulk of recommendations focused on adoption of rigorous curriculum standards, recall that *"A Nation at Risk"* also suggested improvements in educational inputs such as teachers' compensation and time allocation. To address these concerns, we add several state-level time-varying characteristics (e.g., average pupil-teacher ratio and per-pupil spending, indicator for high school exit exam status, and poverty and unemployment rate) and census division-specific linear trends in Columns 3 and 6 of Table 2.[13] The coefficient estimates from these specifications are very similar in magnitude to those reported from the previous columns, and the impacts are now both statistically significant at the 10% level. These estimates suggest that the curriculum reforms reduced the likelihood of low birth weight and prematurity by 0.16 and 0.23 percentage points, respectively. Taking the fraction of pre-reform low birth weight as our benchmark (10.2%), the estimated impact implies an average decrease of 1.5%. A similar exercise yields a 1.4% reduction in the incidence of prematurity, on average.

Panel A of Table 3 presents the same set of results for white mothers. Note that, relative to black mothers, the point estimates indicate smaller effects on infant health, i.e., 0.7 (0.9) % decrease in the probability of low birth weight (prematurity), on average, when we consider the pre-reform sample means from Table 1 for white mothers. This finding suggests that reforms may have their largest effect on those who ex ante had the least amount of coursework in core subjects.

We next estimate a model where we allow the effects of curriculum reforms to differ depending on the number of years elapsed relative to the reforms. Specifically, we estimate a slightly simpler variant of equation (2) by replacing the single DD indicator for curriculum reforms with indicator variables denoting different post-adoption high school cohorts. Panel B of Table 2 reports these results for black mothers. All cohort-specific point estimates are relative to pre-reform cohorts. The overwhelming majority of the point estimates

[13] We also experimented with our analysis by including additional state-level controls (e.g., average teacher salary and an indicator for whether the teacher was required to be certified in the subject area). The results remain unchanged and are available upon request.

are significant at the 5% level and the influence of curriculum reforms on favorable infant health outcomes appears to be more pronounced for later cohorts. For example, our most extensive specification from Column 3 suggests that the implementation of curriculum reforms reduced the likelihood of low birth weight by 0.27 percentage points for the cohort entering high school two years after the reforms, while the coefficient estimate for the cohort entering high school four years after the reforms is -0.51 percentage points.[14] To put this in perspective, for this particular (fourth) cohort, the estimated effects on the probability of low birth weight and prematurity are both roughly equal to one-fourth of the effect observed from an additional year of maternal education (Currie and Moretti 2003). Turning to white mothers, we continue to observe negative but generally insignificant point estimates (Panel B, Table 3).

The validity of the results presented thus far depend on the exogeneity of the differential timing of curriculum reforms. Pre-adoption differences that affect the timing of reforms and health outcomes at the same time may contaminate the point estimates. For example, suppose a state is more likely to adopt curriculum reforms in a year when the well-being of adolescents (future mothers) is improving for other reasons (e.g., preventive health care spending). To address this concern and to test for the existence of different trends prior to adoption, we estimate equation (2) by omitting the cohort immediately prior to reforms. The results from this event-study exercise are provided in Table 4 and Figure 2 for black and white mothers. Each panel in Figure 2 depicts the cohort-specific point estimates by years elapsed relative to curriculum reforms. The height of the bars extending from each point represents the bounds of the 95% confidence interval. The estimated effects on lagged terms are all small in magnitude, irrespective of mother's race and none of them are statistically different from zero. Overall, we do not observe any evidence for differential trends across states that eventually implemented curriculum reforms.

[14] The muted impact for the first post-adoption cohort may stem from mothers graduating high school when they turned 17. Their assignment to the treatment group can bias the point estimates towards zero.

5.2 Robustness Checks and Additional Estimations

We undertake several sensitivity checks to examine the robustness of our results. For the sake of brevity, we present the estimates from equation (1) in Table 5 and relegate the corresponding event study results from equation (2) to the appendix. We first limit our attention to mothers 24 to 36 years old to address potential decaying effects of curriculum reforms with age (Goodman 2018). The coefficient estimates from this exercise are similar in magnitude to those presented throughout the text (Panel A of Table 5 and Figure A1). Second, we focus only on first-time mothers to minimize concerns arising from differences in average outcomes by birth order. Our findings remain intact (Panel B of Table 5 and Figure A2). Third, recall that we exclude 11 states because local school districts in these states had the autonomy to determine the minimum number of courses required for graduation. Adding these states back to our analysis yields similar results for black mothers, however, several point estimates for white mothers flip signs and become almost indistinguishable from zero in magnitude (Panel C of Table 5 and Figure A3). Fourth, we exclude the most populous states for each demographic subgroup. We continue to observe almost identical point estimates for black mothers. The impacts reported in Panel D of Table 5 for white mothers are now statistically significant (Columns 3 and 4), although these significant results do not generally carry over to event study estimates (Figure A4). We also tried dropping six states which adopted curriculum reforms within a three-year window prior to release of the report in 1983. Our findings remain virtually identical.[15]

Fifth, there may be a concern that the introduction of high school curriculum reforms coincides with other major policy changes. Specifically, in response to large spending gaps across school districts, several states introduced legislative reforms that led to changes in public education funding between 1971 and 2010 (Jackson et al. 2015). These equalization efforts may bias the estimated effects if roll out is correlated with the passage of high school curriculum reforms. A similar concern may arise due to federal and state Earned Income Tax Credit (EITC) policy expansions observed in the 1980s (Bastian and Michelmore 2018; and Lovenheim and Willen 2018). To probe this concern, we control for the (i) total number of years each birth

[15] The point estimates (standard error) on low birth weight and prematurity are -0182 (0.096) and -0.165 (0.115) for black mothers, respectively. The same sample restriction for white mothers yields -0.019 (0.025) and -0.032 (0.046) (infant health outcome measures are multiplied by 100 to obtain percent values).

cohort would have been exposed to legislative or court-ordered school finance reforms, and (ii) federal and state maximum potential EITC exposure a child could have received from birth to age 18.[16] Controlling for alternative policies have almost no effect on our results (Panel E of Table 5 and Figure A5). Our findings are also robust to the inclusion of state-specific (instead of census division) trends where identification is determined by deviations from a pre-existing linear trend and are available upon request. Finally, we cluster the standard errors at the state-by-cohort level, and doing so does not affect the statistical inference (Panel F of Table 5 and Figure A6).

In addition to these robustness checks, we performed two placebo tests. First, we estimate the effect of curriculum reforms on infant health for mothers from the same (birth) cohorts whose educational attainment is less than high school (less than ninth grade). If our results were driven by unobservable differences across states around the time of curriculum adoption that are correlated with infant health, then we would expect to see a significant spurious correlation between curriculum reforms and infant health. As shown in Table 6, the point estimates on being exposed to curriculum reforms for mothers whose education is less than high school either carry opposite (and wrong) signs (Columns 1-3) or are statistically indifferent from zero (Column 4).

Next, we randomly assign reform years to states by drawing dates, without replacement, from the actual pool of curriculum reform years.[17] We do this for 1,000 sets of placebo reforms and estimate equation (1). Figure 3 plots the distribution of point estimates. The vertical red lines in each panel denote the values from Columns 3 and 6 of Tables 2 and 3. We also report the percentage of placebo estimates that are smaller than the baseline effects on the x-axis. Focusing first on black mothers, the location of the true estimates indicates that the likelihood of finding an effect merely by chance is very unlikely (Panels A and C). Specifically, the actual point estimate for low birth weight (prematurity) is larger than only 4 (5) percent of the placebo estimates. Perhaps not surprisingly, consistent with the evidence presented throughout the text, the inference for white mothers is less clear. Specifically, the point estimates for low birth weight and

[16] We use maximum EITC benefits for a two child household. The results are robust to changes in the number of children.

[17] By drawing without replacement, we ensure that the number of states, in any given year, is the same as it would have been had the actual reforms been introduced.

prematurity are larger than 18 and 6 percent of the placebo estimates, respectively (Panels B and D).

We also explore the potential heterogeneity in the estimated effects along the lines of maternal education. Prior to moving forward with this analysis, it is important to note that mother's education is likely to be endogenous (as shown in Section 5.3) to the model, and therefore caution is warranted in interpreting these results. With this proviso in mind, we interact our treatment with a dummy taking the value of one if mother's education is more than high school (e.g., thirteen years or more) and control for mother's education in a continuous manner in the same specifications. The interaction terms from this analysis are all positive but they are not statistically significant at the 5% level (Table A4 in the Appendix).

Recall also that we opt out of including Hispanic mothers in our benchmark analysis because of the inconclusive evidence on the efficacy of high school reforms on course work for Hispanic students. Although the estimated (first-stage) effects are positive and generally significant, Goodman (2018) cautions against the findings of Hispanic students as they were largely concentrated in a number of states by the time of state-mandated changes in the minimum number of core course requirements. Nevertheless, we estimate the impact of curriculum reforms for Hispanic mothers. Figure A7 in the Appendix plots the event study results. We find sizeable and statistically significant effects for low birth weight, while there are no impacts of curriculum reforms on the incidence of prematurity.

5.3 Mechanisms

Results from previous sections suggest that curriculum reforms led to favorable infant health outcomes. In this section, we consider potential explanations for our findings. Increasing minimum number of core courses required for graduation may have affected infant health through a variety of channels. For example, Goodman (2018) showed that reforms in the math curriculum increased cognitive skills and these skills are known to affect several labor market and behavioral outcomes (Heckman et al. 2006 and 2016). To explore the mechanisms, we consider the following maternal domains, which are known to be associated with child quality: (i) health behavior (proxied by smoking during pregnancy); (ii) educational attainment; and (iii) family income (see, for example, Becker 1960; Grossman 1972; Currie and Moretti 2003; Breierova and Duflo

2004; Almond et al. 2005; Chou et al. 2010; Bhalotra and Rawlings 2011; McCrary and Royer 2011; Løken et al. 2012; Aizer and Currie 2014; and Lundborg et al. 2014).

Information on smoking during pregnancy (and educational attainment) is available in the Vital Statistics Natality Records from 1989 onwards.[18] Unfortunately, to the best of our knowledge, there is not one single data set containing measures of infant health and parental income at the same time that also overlaps with our high school entry cohorts of interest. In order to (partially) overcome this challenge, we draw information from two separate sources. Specifically, we rely on the American Community Survey (ACS) data, which are annually administered to a random sample of households from the U.S. population. The ACS includes information on family income as well as state and year of birth. In efforts to improve efficiency, we pool repeated cross section observations between 2000 and 2005 and focus on the same high school cohorts (those graduating high school between 1982 and 1994).

To explore the association between income and infant health, we turn to the National Longitudinal Study of Youth 1979 (NLSY79), a large longitudinal study of young males and females, born between 1957 and 1964, who were first interviewed in 1979. Follow-up surveys were conducted annually from 1979 until 1994, and biennially from 1994 onwards (the most recent survey year is 2014). As part of this panel study, another data set, the Children of the NLSY79, was initiated in 1986 and follows the children of the female respondents of the NLSY79. We match children to their mothers and use birth outcome information along with average family income (in 2017 dollars) in our mechanism analysis.[19]

Columns (1)-(3) of Table 7 report the estimated effects of curriculum reforms on outcomes for black mothers using Vital Statistics Natality Records from 1989 onwards and ACS data. We find a negative and significant effect of increased course requirements on smoking. Specifically, the introduction of reforms

[18] We also consider measures of prenatal care (e.g., total number of visits and the timing of care initiation). The estimated impacts of high school curriculum reforms on measures of prenatal care are small in magnitude and are not statistically different from zero.

[19] Although the birth cohorts from the NLSY79 do not perfectly overlap with those from our study, to the best of our knowledge, it is the only data which include information on family income, birth weight and prematurity at the same time. For example, the Panel Study of Income Dynamics does not include information on gestational length.

We define family income as the average annual family income from all available years for females in the NLSY79 (Løken et al. 2012).

The sample only includes mothers with at least a high diploma, as well as births of more than 500 grams and those with more than 26 weeks gestation.

reduced the likelihood of smoking during pregnancy by 4.3 percent relative to corresponding sample mean (reported in the last row of Table 7). The curriculum reforms also appear to increase educational attainment (Column 2). The estimated effect is 0.6 percentage points and statistically significant. The point estimate for log family income, reported in the third column, is also positive but it falls short of statistical significance.

Panel B of Table 7 presents the associations of these maternal outcomes with infant health measures (Columns 4-9). Note that these specifications control for state of birth and cohort fixed effects and all other covariates (as described in Section 4). As expected, educational attainment and income are negatively correlated with unfavorable health outcomes, while smoking during pregnancy is positively correlated. The estimated associations, with the exception of the last column, are all statistically significant.[20]

To determine the predictive power of each channel, we multiply the point estimates from Columns (1)-(3) with their corresponding counterparts in Columns (4)-(6) for low birth weight and in Columns (7)-(9) for prematurity. This mechanism exercise is akin to mediation analysis (Heckman et al. 2013). As a first step, we reproduce our point estimates from Table 2 for our preferred specification using Vital Statistics Natality Records from 1989 and onwards (information on health behaviors including smoking is not available prior to 1989). The estimated effects are -0.21 and -0.17 percentage points for low birth weight and prematurity, respectively. They are also reported in Table A5 in the Appendix. Multiplying the first and fourth columns of Table 7 yields a value of -0.0004, meaning that smoking during pregnancy can explain around 20 percent of -0.0021. The mediation exercise also suggests that smoking can explain around 14 percent of the estimated effect (-0.0017) of curriculum reforms on prematurity. Educational attainment does not appear to have a large predictive power for black mothers. Applying the same translation to family income (Columns 3, 6 and 9) suggests a predictive power of around 46 and 9 percent for low birth weight and prematurity, respectively.

Table 8 presents a similar exercise for white mothers. Unlike black mothers, we do not find any impact of curriculum reforms on educational attainment and the estimated effect on family income is negative

[20] The estimated associations between family income and the likelihood of low birth weight are consonant with Hoynes et al. (2015) who estimate that a $1,000 increase in income (as a result of an EITC expansion) decreases the probability of low birth weight by 1.9 (0.7) percent for high impact black (white) mothers. Using the NLSY79 and focusing on high impact mothers (single, low education and ages 18-45), we find the aforementioned relationship to be 1.05 and 0.86 percent for black and white mothers, respectively.

and insignificant (Columns 1-3). That being said, we find very similar predictive power of smoking during pregnancy in explaining the effects reported in Columns 3 and 4 of Table A5 in the Appendix (predictive power of around 18 and 8 percent for low birth weight and prematurity, respectively).

In addition to these proposed mechanisms, we examine the impact of curriculum reforms on mother's age at birth. These results are reported in Table A6 in the Appendix. The point estimates are small in magnitude and none of them are statistically significantly different from zero.

Finally, we attempt to disentangle subject-specific reform effects. Recall that all but one state first enacted curriculum reforms in math. These changes in math were almost always accompanied by changes in the minimum number of required courses in at least one other subject. Given this, it may not be misguided to infer that math has been the pivotal subject of curriculum reforms. Although we do not have the power to identify the impact of each course, we extend our preferred specifications to include controls for the total minimum number of courses required in other subjects for graduation. Table 9 and Figure A8 in the Appendix presents the results from this exercise.[21] The insensitivity of the point estimates for black mothers to the inclusion of non-math reforms may lend support to an argument that the effects observed on infant health may have largely been driven by math reforms (Columns 1 and 2 of Table 9 and Panels A and C of Figure A8). As for white mothers, however, the effects of math reforms are all smaller in magnitude when controlling for the total number of courses in other subjects. We also find that the point estimate on non-math reforms for prematurity is statistically significant (Columns 3 and 4 of Table 9 and Panels B and D of Figure A8).[22]

Under the assumption that math reforms are the driving factor, one can take this analysis a step further to get an estimate of the impact of math coursework on infant health. Using the first stage coefficients (regression of math reforms on math course work) for females from Goodman (2018), we find the impact of an additional math course (full year of study) on the likelihood of low birth weight (prematurity) for marginal black mothers whose coursework is affected by the state-mandated math reforms to be -5.6 (-6.3)

[21] New Mexico is the only state where reform in math preceded reform in another subject (science). We dropped New Mexico from our effective sample in Table 9 to minimize confounding effects from pre-math reform treated cohorts.

[22] One can alternatively control for the total number of non-math reforms. Doing so does not alter any of our conclusions.

% on average.[23] The corresponding estimates for white mothers are around 2 percent for both of the infant health outcomes.

6 Conclusion

In 1983, the National Commission on Excellence in Education issued its landmark report "*A Nation at Risk: The Imperative for Educational Reform.*" This report made important suggestions on how to improve public schools; the bulk of the recommendations concerned adoption of rigorous curriculum standards. Several states reacted by enacting curriculum reforms that required changes in the minimum number of core courses for receiving a high school diploma. This paper examines the effects of curriculum reforms on birth outcomes by exploiting the differences in the timing of curriculum adoption in a difference-in-differences framework. Under the assumption that the exact timing of reform adoption is not systematically related to within-state time varying unobserved factors affecting infant health in a given state, our empirical approach yields credible estimates of the effects of curriculum reforms. To the best of our knowledge, this is the first paper relating mother's coursework in school to child quality.

Our results suggest that curriculum reforms, on average, reduced the incidence of low birth weight by 1.5 percent and the incidence of prematurity by 1.4 percent for black mothers. These impacts are presumably driven by mothers with only a high school diploma. We also observe evidence for dynamic heterogeneity with the impacts being more pronounced for cohorts graduating high school in later years of post-adoption. The estimated effects for white mothers, on the other hand, are smaller and they are generally insignificant. This finding is consistent with reforms having their largest effects on those who ex ante had the least amount of coursework in core subjects. Several robustness checks and falsification tests support our findings. We further explore the potential channels leading to favorable infant health outcomes. Improvements in maternal health behaviors (reduced smoking during pregnancy) and family income appear to explain a non-negligible fraction of the observed effects. Finally, we attempt to disentangle subject-specific reform effects and find

[23] We use the first stage estimates for black and white mothers from specifications in which non-math reforms are controlled for. Specifically, the first stage coefficients on math reforms are 0.285 and 0.174 for black and white mothers, respectively. The reported local average treatment effects are relative to pre-reform sample means from Table 1.

suggestive evidence that the effects observed on infant health were largely driven by math reforms.

To put improvements in infant health numbers into a monetary perspective, we provide a simple back-of-the envelope social gain calculation. To be conservative, we only use information from black mothers. This presumably generates a lower bound estimate of the total gain. In addition, given that most premature infants are also low birth weight, we only concentrate on the number of premature births in our calculations. The annual societal cost associated with prematurity in the U.S. was $51,600 per infant in 2005 (Institute of Medicine of the National Academies, 2007).[24] Taking the total number of premature infants from control cohorts as our benchmark, the percent decrease (1.4) implies that 3,619 fewer infants were born premature as a result of curriculum reforms. This decrease corresponds to a total social gain of approximately $234 million (in 2017 dollars). The same gain, using the local average treatment effect estimates of math coursework, is more than a billion dollars.

From a broad perspective, these encouraging results may have important policy implications for designing programs to reduce poverty and inequality. Our findings highlight the importance of intergenerational transmission. Early interventions aiming to enhance skills and abilities for young females from disadvantaged backgrounds may not be only important for them now but also for their offspring in the future. These type of programs can help reduce inequality in health at birth and improve social and economic mobility to the extent that healthier children become more highly educated and productive adults themselves.

[24] The amount that medical care services contributed to the total cost was $33,200 per premature infant. Maternal delivery costs contributed another $3,800. Early intervention and special education services cost an estimated $1,200 and $2,200, respectively. Finally, lost household productivity contributed $11,200.

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Table 1: Summary Statistics

	Blacks		Whites	
	Pre-Reform	Post-Reform	Pre-Reform	Post-Reform
	Mean (Standard Deviation)			
Panel A: Controls				
Mother Age	25.957	25.259	27.977	27.339
	(5.395)	(5.384)	(5.267)	(5.358)
Birth Order	2.615	2.641	2.275	2.245
	(1.603)	(1.647)	(1.401)	(1.415)
Child Male	0.508	0.508	0.513	0.514
	(0.500)	(0.500)	(0.500)	(0.500)
More than High School	0.401	0.436	0.570	0.611
	(0.490)	(0.496)	(0.495)	(0.488)
Pupil/Teacher Ratio	18.307	17.525	18.345	17.848
	(1.636)	(2.135)	(2.266)	(2.623)
State Unemployment Rate (%)	8.52	6.27	8.267	6.208
	(2.35)	(1.35)	(2.346)	(1.429)
Panel B: Birth Outcomes				
Low Birth Weight	0.102	0.099	0.041	0.044
	(0.303)	(0.299)	(0.198)	(0.205)
Premature	0.157	0.151	0.076	0.084
	(0.364)	(0.358)	(0.264)	(0.277)
Sample Size	1,678,794	2,039,850	8,150,397	8,545,102

NOTES: The statistics above reflect our research sample, which consists of mothers who graduated from high school between 1982 and 1994 (birth cohorts from 1964 to 1976). The sample is further restricted to natality records for 1982 to 2015 (mothers 18 to 49 years old) from 40 states.

Table 2: Effects of Curriculum Reforms on Infant Health Outcomes-Black Mothers

	Low Birth Weight			Premature		
	Coefficient(×100) (Standard Error)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
Exposed to Curriculum Reform	-0.137	-0.138	-0.155*	-0.233*	-0.237	-0.226*
	(0.094)	(0.096)	(0.090)	(0.137)	(0.149)	(0.123)
Panel B:						
1st Year Postadoption	-0.107	-0.113	-0.138	-0.234*	-0.242*	-0.241*
	(0.107)	(0.105)	(0.101)	(0.126)	(0.131)	(0.124)
2nd Year Postadoption	-0.213*	-0.215*	-0.269***	-0.341**	-0.344**	-0.351***
	(0.126)	(0.113)	(0.075)	(0.155)	(0.155)	(0.104)
3rd Year Postadoption	-0.341*	-0.354**	-0.421***	-0.508**	-0.527**	-0.544***
	(0.178)	(0.163)	(0.087)	(0.226)	(0.222)	(0.124)
4th Year Postadoption	-0.390*	-0.410**	-0.508***	-0.619**	-0.648**	-0.691***
	(0.216)	(0.196)	(0.110)	(0.299)	(0.299)	(0.139)
5th or more Year Postadoption	-0.301	-0.334	-0.420**	-0.638	-0.669	-0.692***
	(0.290)	(0.285)	(0.167)	(0.472)	(0.505)	(0.204)
Sample Size	3,718,644	3,718,644	3,718,644	3,676,227	3,676,227	3,676,227
Controls:						
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	No	Yes	Yes	No	Yes	Yes
State Characteristics	No	No	Yes	No	No	Yes
Census Division-Specific Trends	No	No	Yes	No	No	Yes

NOTES: Standard errors clustered at the state level are reported. All outcome variables are multiplied by 100 to obtain percent values. Low birth weight is defined as birth weight less than 2,500 grams. Prematurity is defined as gestation less than 37 weeks. Mother/child characteristics include categorical controls for mother's age (less than 20, 20 to 24, 25 to 34 and more than 35), total birth order and an indicator for child's gender. The state of high school attendance controls include an indicator for an exit exam requirement, average pupil-teacher ratio and per-pupil expenditures and unemployment and poverty rate. Census division-specific linear trends are for mother's census of birth.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3: Effects of Curriculum Reforms on Infant Health Outcomes-White Mothers

	Low Birth Weight			Premature		
	Coefficient(×100) (Standard Error)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
Exposed to Curriculum Reform	-0.030	-0.034	-0.031	-0.087**	-0.084*	-0.071*
	(0.026)	(0.027)	(0.025)	(0.040)	(0.043)	(0.040)
Panel B:						
1st Year Postadoption	-0.026	-0.030	-0.025	-0.066*	-0.064*	-0.053
	(0.027)	(0.027)	(0.026)	(0.037)	(0.037)	(0.040)
2nd Year Postadoption	-0.042	-0.050	-0.038	-0.133**	-0.130**	-0.110**
	(0.031)	(0.031)	(0.032)	(0.057)	(0.050)	(0.053)
3rd Year Postadoption	-0.053	-0.063	-0.048	-0.143*	-0.141**	-0.118*
	(0.039)	(0.039)	(0.044)	(0.079)	(0.066)	(0.063)
4th Year Postadoption	-0.031	-0.044	-0.023	-0.091	-0.093	-0.074
	(0.047)	(0.048)	(0.045)	(0.109)	(0.092)	(0.072)
5th or more Year Postadoption	-0.043	-0.059	-0.019	-0.091	-0.093	-0.061
	(0.057)	(0.061)	(0.051)	(0.155)	(0.146)	(0.102)
Sample Size	16,695,499	16,695,499	16,695,499	16,580,032	16,580,032	16,580,032
Controls:						
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Mother/Child Characteristics	No	Yes	Yes	No	Yes	Yes
State Characteristics	No	No	Yes	No	No	Yes
Census Division-Specific Trends	No	No	Yes	No	No	Yes

NOTES: Standard errors clustered at the state level are reported. All outcome variables are multiplied by 100 to obtain percent values. Low birth weight is defined as birth weight less than 2,500 grams. Prematurity is defined as gestation less than 37 weeks. Mother/child characteristics include categorical controls for mother's age (less than 20, 20 to 24, 25 to 34 and more than 35), total birth order and an indicator for child's gender. The state of high school attendance controls include an indicator for an exit exam requirement, average pupil-teacher ratio and per-pupil expenditures and unemployment and poverty rate. Census division-specific linear trends are for mother's census of birth.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4: Effects of Curriculum Reforms on Infant Health Outcomes by Years Elapsed from Adoption-Event Study

	Blacks		Whites	
	Low Birth Weight	**Prematurity**	**Low Birth Weight**	**Prematurity**
	Coefficient(×100) (Standard Error)			
	(1)	(2)	(3)	(4)
1st Year Postadoption	-0.173	-0.251*	-0.019	-0.048
	(0.111)	(0.136)	(0.028)	(0.046)
2nd Year Postadoption	-0.288***	-0.354**	-0.043	-0.110*
	(0.097)	(0.160)	(0.038)	(0.064)
3rd Year Postadoption	-0.429***	-0.539**	-0.060	-0.122
	(0.133)	(0.219)	(0.051)	(0.077)
4th Year Postadoption	-0.503**	-0.680***	-0.045	-0.082
	(0.191)	(0.244)	(0.056)	(0.086)
5th Year and More Postadoption	-0.400	-0.673**	-0.053	-0.074
	(0.271)	(0.296)	(0.065)	(0.118)
1st Year Prior-Omitted	…..	…..	…..	…..
2nd Year Prior	-0.175	-0.059	0.059*	0.047
	(0.131)	(0.176)	(0.032)	(0.046)
3rd Year Prior	-0.067	-0.098	0.040	-0.006
	(0.212)	(0.286)	(0.046)	(0.069)
4th year Prior	-0.120	0.042	0.075	0.025
	(0.266)	(0.337)	(0.053)	(0.098)
5th Year and More Prior	-0.129	-0.097	0.081	0.043
	(0.343)	(0.458)	(0.068)	(0.124)
Sample Size	3,718,644	3,676,227	16,695,499	16,580,032
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes
Census Division-Specific Trends	Yes	Yes	Yes	Yes

NOTES: Standard errors clustered at the state level are reported. Year prior to curriculum reforms is the omitted category. See notes to Tables 2 (or 3) and the text for further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: Robustness Checks-Effects of Curriculum Reforms on Infant Health Outcomes

	Blacks		Whites	
	Low Birth Weight	**Prematurity**	**Low Birth Weight**	**Prematurity**
	Coefficient(×100) (Standard Error)			
	(1)	(2)	(3)	(4)
Panel A: Mothers 24 to 36 Years Old				
Exposed to Curriculum Reform	-0.178*	-0.289**	-0.023	-0.084
	(0.093)	(0.127)	(0.023)	(0.057)
	[1,983,151]	[1,968629]	[11,582,515]	[11,528,449]
Panel B: First-Time Mothers				
Exposed to Curriculum Reform	-0.138	-0.309	-0.054	0.023
	(0.131)	(0.199)	(0.040)	(0.039)
	[1,030,469]	[1,017,144]	[5,906,621]	[5,861,169]
Panel C: Include All States				
Exposed to Curriculum Reform	-0.125	-0.256***	0.013	0.002
	(0.077)	(0.087)	(0.027)	(0.039)
	[4,541,626]	[4,493,352]	[22,660,843]	[22,519,529]
Panel D: Exclude Most Populous States				
Exposed to Curriculum Reform	-0.181*	-0.258*	-0.061*	-0.102**
	(0.101)	(0.146)	(0.031)	(0.044)
	[2,419,472]	[2,385,349]	[10,046,697]	[9,993,878]
Panel E: Control Other Major Policy Changes				
Exposed to Curriculum Reform	-0.150*	-0.188*	-0.022	-0.063
	(0.091)	(0.110)	(0.023)	(0.039)
	[3,718,644]	[3,676,227]	[16,695,499]	[16,580,032]
Panel F: Clustering at the State-by-Birth Year Level				
Exposed to Curriculum Reform	-0.155*	-0.226**	-0.031	-0.071*
	(0.085)	(0.109)	(0.024)	(0.040)
	[3,718,644]	[3,676,227]	[16,695,499]	[16,580,032]
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes
Census Division-Specific Trends	Yes	Yes	Yes	Yes

NOTES: Standard errors are clustered at the state level in Panels A-E, while they are clustered at the state-by-birth year level in Panel F. The sample is restricted to mothers 24 to 36 years old and first-time mothers in Panels A and B, respectively. Panel C includes all 50 states and District of Columbia. Panel D excludes most populous states for blacks and whites in Columns 1/2 and 3/4, respectively. Panel E controls for the total number of years of school finance reforms exposure and federal and state maximum potential EITC exposure from birth to age 18. Sample sizes are reported in square brackets.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: Placebo Effects of Curriculum Reforms on Infant Health Outcomes-Mother's Education Less Than 9th Grade

	Blacks		Whites	
	Low Birth Weight	**Prematurity**	**Low Birth Weight**	**Prematurity**
	Coefficient(×100) (Standard Error)			
	(1)	(2)	(3)	(4)
Exposed to Curriculum Reform	0.710	0.304	0.149	-0.327
	(0.785)	(1.179)	(0.249)	(0.243)
Sample Mean-Pre-Reform (%)	14.78	20.30	7.33	9.57
Sample Size	53,595	52,384	347,878	342,352
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes
Census Division-Specific Trends	Yes	Yes	Yes	Yes

NOTES: Standard errors clustered at the state level are reported. The sample consists of mothers whose education is less than 9th grade. See notes to Table 2 (or 3) and the text for further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7: Mechanisms and Mediation Analysis: Effects of Curriculum Reforms on Infant Health Outcomes-Black Mothers

	Smoked During Pregnancy	More than High School	Log of Family Income	Low Birth Weight			Prematurity		
	Coefficient (Standard Error)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A:									
Exposed to Curriculum Reform	-0.005**	0.006**	0.013	…..	…..	…..	…..	…..	…..
	(0.002)	(0.002)	(0.018)						
Panel B:									
Smoked During Pregnancy	…..	…..	…..	0.080***	…..	…..	0.046***	…..	…..
				(0.002)			(0.003)		
More than High School				…..	-0.016**		…..	-0.020***	
					(0.001)	…..		(0.001)	…..
Log of Family Income				…..	…..	-0.075***	…..	…..	-0.011
						(0.020)			(0.018)
Sample Mean-Pre-Reform (%)	11.56	46.09	10.74						
Sample Size	2,526,034	2,526,034	37,477	2,526,034	2,526,034	1,385	2,526,034	2,526,034	1,282

NOTES: Standard errors clustered at the state level are reported. Information on family income in Column 3 comes from the American Community Survey (2000-2005), while data on birth weight and prematurity and family income in Columns 6 and 9 are obtained from the National Longitudinal Survey of 1979. Family income in Columns 6 and 9 are averaged over all available years (2017 dollars). The NLSY sample further includes mothers with at least a high school degree, as well as births of more than 500 grams and those with more than 26 weeks gestation. Standard errors clustered at the state level are reported. See notes to Tables 2 (or 3) and the text for the set of control variables and further details.

*significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8: Mechanisms and Mediation Analysis: Effects of Curriculum Reforms on Infant Health Outcomes-White Mothers

	Smoked During Pregnancy	More than High School	Log of Family Income	Low Birth Weight			Prematurity		
	Coefficient (Standard Error)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A:									
Exposed to Curriculum Reform	-0.002**	-0.001	-0.008	…..	…..	…..	…..	…..	…..
	(0.001)	(0.002)	(0.005)						
Panel B:									
Smoked During Pregnancy	…..	…..	…..	0.044***	…..	…..	0.024***	…..	…..
				(0.001)			(0.001)		
More than High School				…..	-0.014***	…..	…..	-0.012***	…..
					(0.001)			(0.001)	
Log of Family Income				…..	…..	-0.023**	…..	…..	-0.013
						(0.010)			(0.008)
Sample Mean-Pre-Reform (%)	12.92	62.02	11.25						
Sample Size	11,275,633	11,275,633	264,084	11,275,633	11,275,633	3,127	11,275,633	11,275,633	2,934

NOTES: Standard errors clustered at the state level are reported. Information on family income in Column 3 comes from the American Community Survey (2000-2005), while data on birth weight and prematurity and family income in Columns 6 and 9 are obtained from the National Longitudinal Survey of 1979. Family income in Columns 6 and 9 are averaged over all available years (2017 dollars). The NLSY sample further includes mothers with at least a high school degree, as well as births of more than 500 grams and those with more than 26 weeks gestation. Standard errors clustered at the state level are reported. See notes to Tables 2 (or 3) and the text for the set of control variables and further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9: Effects of Math Curriculum Reforms on Infant Health Outcomes-Controlling for Non-Math Reforms

	Blacks		Whites	
	Low Birth Weight	**Prematurity**	**Low Birth Weight**	**Prematurity**
	Coefficient(×100) (Standard Error)			
	(1)	(2)	(3)	(4)
Exposed to Math Curriculum Reform	-0.162	-0.282*	-0.013	-0.035
	(0.099)	(0.147)	(0.027)	(0.045)
Non-Math Reforms	0.003	0.022	-0.005	-0.014***
	(0.016)	(0.024)	(0.004)	(0.004)
Sample Size	3,713,733	3,671,455	16,611,751	16,497,561
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes
Census Division-Specific Trends	Yes	Yes	Yes	Yes

NOTES: Standard errors clustered at the state level are reported. Non-math reforms are the total number of minimum courses required in other subjects for high school graduation. New Mexico is the only state where reform in math preceded a reform in another subject (science). New Mexico is excluded from the analysis. See notes to Table 2 (or 3) and the text for further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

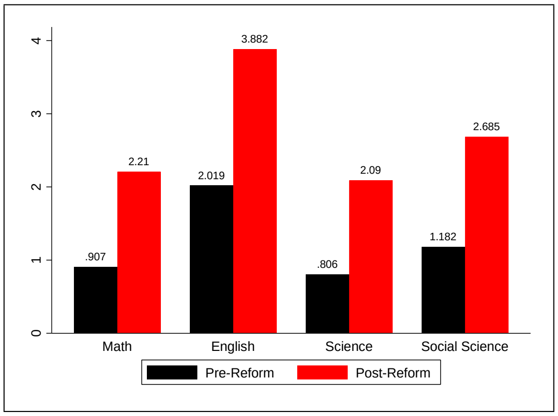


Figure 1: Number of Minimum Courses Required for High School Graduation: Pre-and Post-Reforms

NOTES: The reform years vary across 40 states. The earliest (latest) curriculum reform was enacted in 1980 (1985).

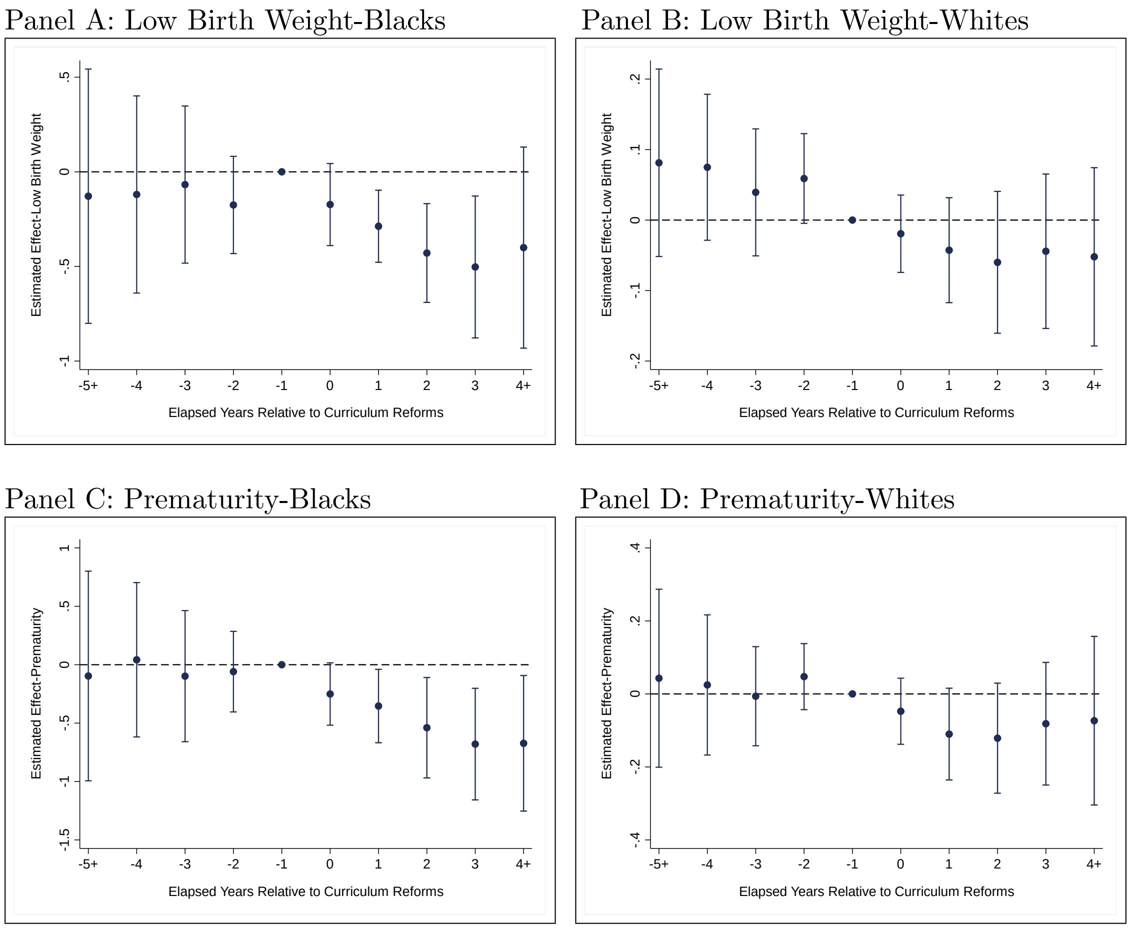
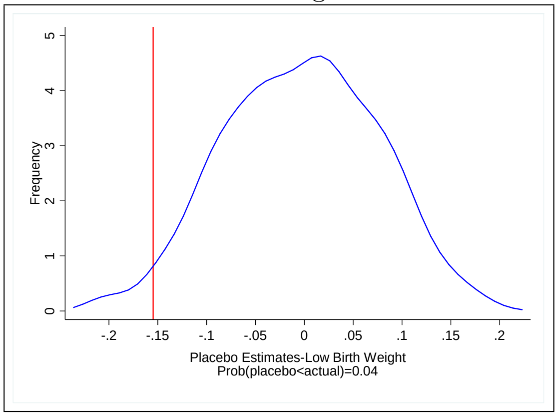


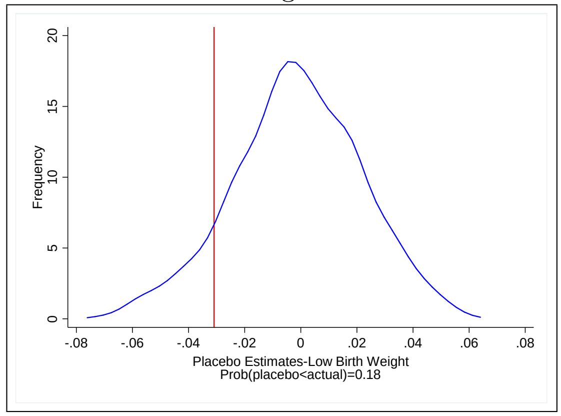
Figure 2: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

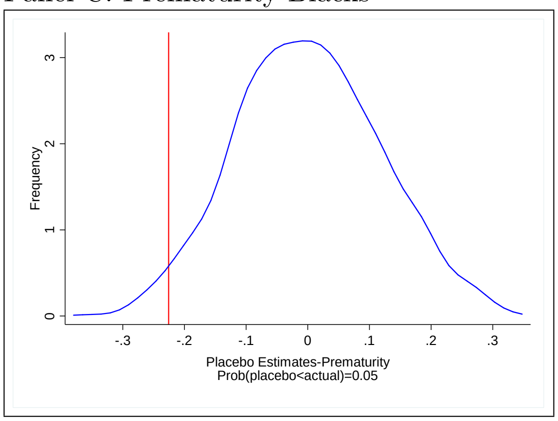
Panel A: Low Birth Weight-Blacks



Panel B: Low Birth Weight-Whites



Panel C: Prematurity-Blacks



Panel D: Prematurity-Whites

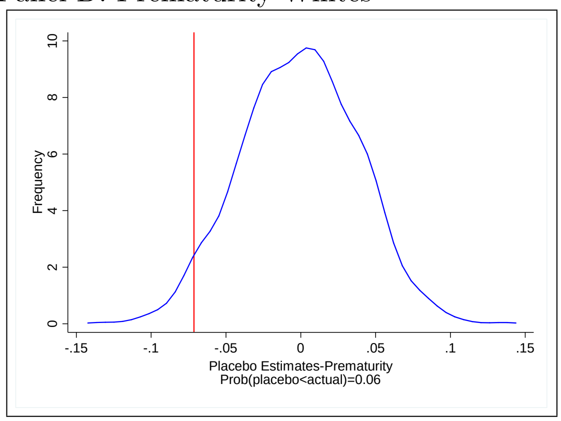


Figure 3: Placebo Estimates of the Effects of Curriculum Reforms on Infant Health Outcomes

NOTES: Distribution of the coefficient estimates resulting from 1,000 sets of random assignments of states to curriculum reforms. The vertical lines denote the actual estimates. The fraction of placebo estimates that are smaller than the baseline estimates are also reported on the x-axis of each panel.

ONLINE APPENDIX-NOT FOR PUBLICATION

Table A1: Curriculum Reforms: States, Subjects and Years of Adoption

States	Reform Subjects	Reform Years
Alabama	Math	1981
Alaska	Math, Science, English	1981
Arizona	Math	1983
Arkansas	Math, Science, Social Studies	1984
California	Math, Science, Social Studies, English	1983
Connecticut	Math, Science, Social Studies, English	1984
Delaware	Math, Science	1983
D.C.	Math, Science, Social Studies	1981
Florida	Math, Science, Social Studies, English	1983
Georgia	Math, Science, Social Studies, English	1984
Idaho	Math, English	1984
Illinois	Math, Science, Social Studies	1984
Indiana	Math, Science, English	1985
Kansas	Math, Science, Social Studies	1985
Kentucky	Math, English	1983
Louisiana	Math, Science, Social Studies, English	1985
Maine	Math, Science, Social Studies	1985
Maryland	Math	1985
Mississippi	Math, Science, English	1985
Missouri	Math, Science, Social Studies, English	1984
Nevada	Math	1982
New Hampshire	Math, Science, Social Studies	1985
New Mexico	Science	1983
North Carolina	Math	1983
North Dakota	Math, English	1980
Ohio	Math	1984
Oklahoma	Math, Science, Social Studies	1983
Oregon	Math, Science	1984
Pennsylvania	Math, Science, Social Studies, English	1985
Rhode Island	Math, Science, Social Studies	1985
South Carolina	Math, Science	1983
South Dakota	Math, Science, Social Studies	1985
Tennessee	Math, Science	1983
Texas	Math, English	1984
Utah	Math, Science, Social Studies	1984
Vermont	Math, Science, Social Studies, English	1985
Virginia	Math, Science	1984
Washington	Math	1985
West Virginia	Math, Science	1981
Wisconsin	Math, Science, Social Studies, English	1985

Table A2: Effects of Curriculum Reforms on High School Graduation

	Blacks	Whites
	Coefficient(×100) (Standard Error)	
	(1)	(3)
Exposed to Curriculum Reform	0.032 (0.282)	-0.026 (0.096)
Sample Size	4,671,804	19,043,700
Controls:		
Cohort Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Mother/Child Characteristics	Yes	Yes
State Characteristics	Yes	Yes
Census Division-Specific Trends	Yes	Yes

NOTES: Standard errors clustered at the state level are reported. See notes to Table 2 (or 3) and the text for further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table A3: Effects of Curriculum Reforms on Endogenous Birth and Fertility

	Birth State-Cohort-Year Cell			
	% of Black Mothers	% of White Mothers	Log of Total Births	Log of Mother's Age
	Coefficient (Standard Error)			
	(1)	(2)	(3)	(5)
Exposed to Curriculum Reform	-0.004	0.004	-0.008	-0.002
	(0.003)	(0.004)	(0.023)	(0.002)
Sample Mean-Pre-Reform (%)	14.15	82.77	5.92	3.45
Sample Size	14,343	14,343	14,343	20,650,596

NOTES: Standard errors clustered at the state level are reported. All specifications control for state of high school attendance, birth cohort and survey year fixed effects.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table A4: Effects of Curriculum Reforms on Infant Health Outcomes-by Mother's Education

	Blacks		Whites	
	Low Birth Weight	**Prematurity**	**Low Birth Weight**	**Prematurity**
	Coefficient(×100) (Standard Error)			
	(1)	(2)	(3)	(4)
Exposed to Curriculum Reform	-0.200*	-0.372***	-0.031	-0.088
	(0.118)	(0.100)	(0.046)	(0.058)
Exposed to Curriculum Reform*More than High School	0.122	0.373*	0.007	0.035
	(0.180)	(0.185)	(0.057)	(0.062)
Sample Size	3,718,644	3,676,227	16,695,499	16,580,032
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes
Census Division-Specific Trends	Yes	Yes	Yes	Yes

NOTES: Standard errors clustered at the state level are reported. See notes to Table 2 (or 3) and the text for further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table A5: Effects of Curriculum Reforms on Infant Health Outcomes-Vital Statistics Natality Records from 1989 onwards

	Blacks		Whites	
	Low Birth Weight	**Prematurity**	**Low Birth Weight**	**Prematurity**
	Coefficient(×100) (Standard Error)			
	(1)	(2)	(3)	(4)
Exposed to Curriculum Reform	-0.212*	-0.168	-0.048	-0.064
	(0.114)	(0.122)	(0.031)	(0.052)
Sample Mean-Pre-Reform (%)	10.01	15.40	3.92	7.43
Sample Size	2,526,034	2,526,034	11,275,633	11,275,633
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes
Census Division-Specific Trends	Yes	Yes	Yes	Yes

NOTES: Standard errors clustered at the state level are reported. See notes to Table 2 (or 3) and the text for further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

Table A6: Effects of Curriculum Reforms on Mother's Age at Birth

	Blacks		**Whites**	
	Log of Mother's Age (Any Birth)	**Log of Mother's Age (First Birth)**	**Log of Mother's Age (Any Birth)**	**Log of Mother's Age (First Birth)**
	Coefficient (Standard Error)			
	(1)	(2)	(3)	(4)
Exposed to Curriculum Reform	-0.003	-0.007	0.000	-0.000
	(0.003)	(0.004)	(0.001)	(0.001)
Sample Size	3,718,644	1,030,469	16,695,499	5,906,621
Controls:				
Cohort Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Mother/Child Characteristics	Yes	Yes	Yes	Yes
State Characteristics	Yes	Yes	Yes	Yes
Census Division-Specific Trends	Yes	Yes	Yes	Yes

NOTES: Standard errors clustered at the state level are reported. See notes to Table 2 (or 3) and the text for further details.
*significant at 10%, ** significant at 5%, *** significant at 1%.

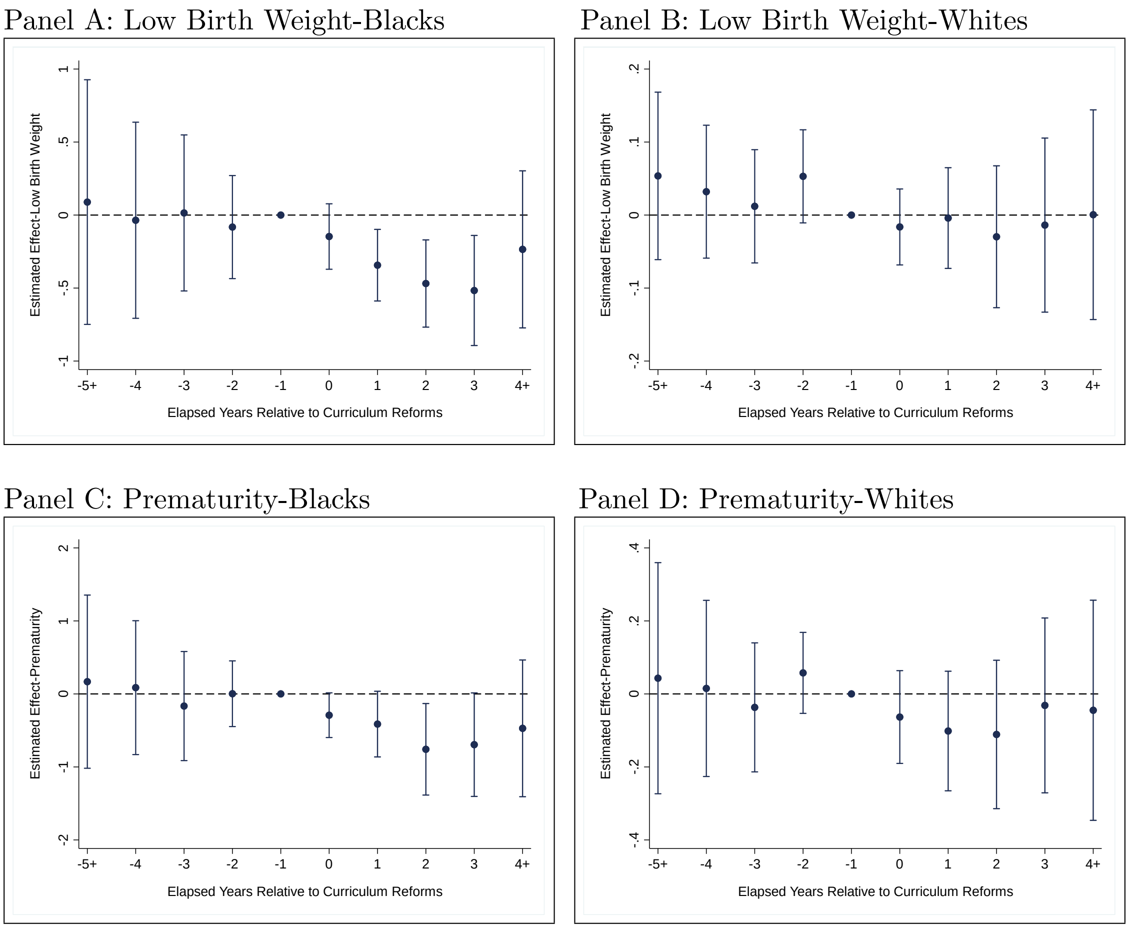


Figure A1: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes-Mothers 24 to 36 Years Old

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

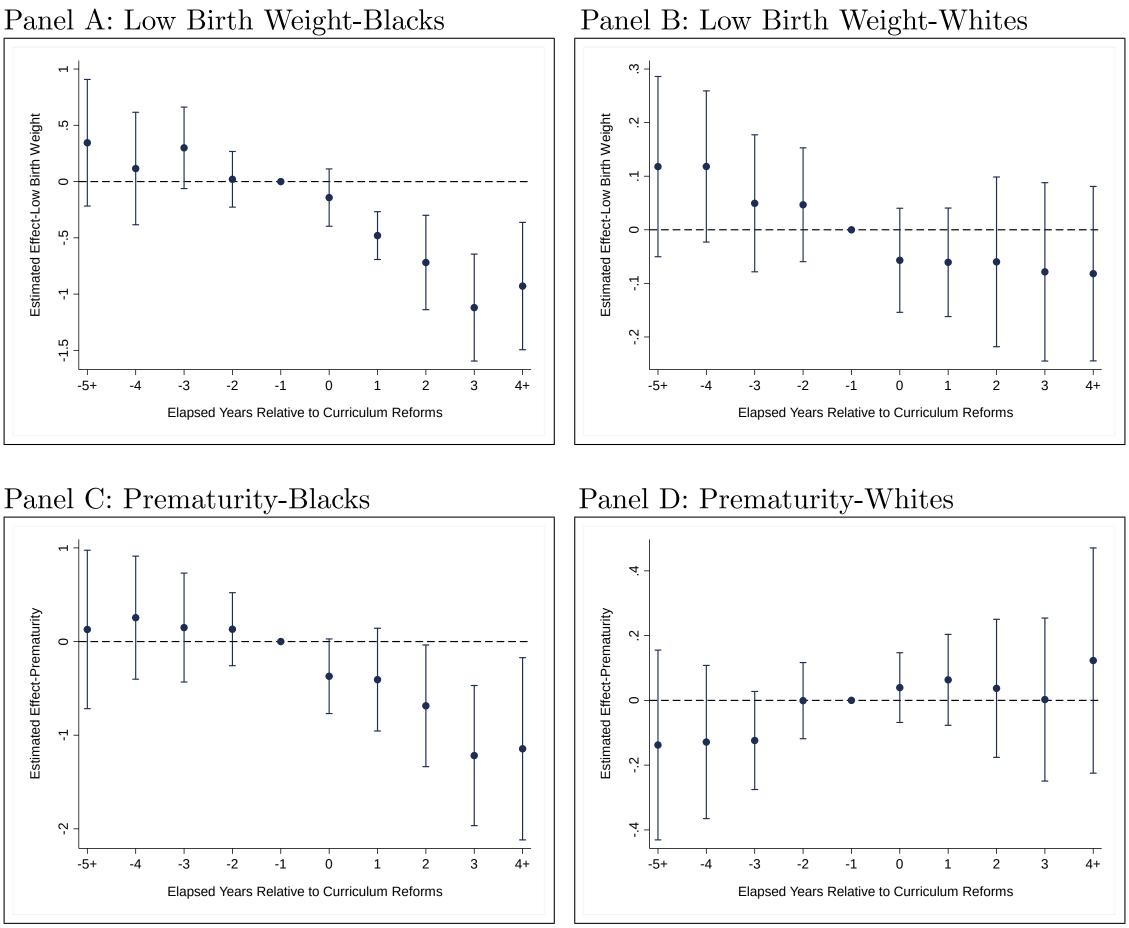


Figure A2: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes-First-Time Mothers

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

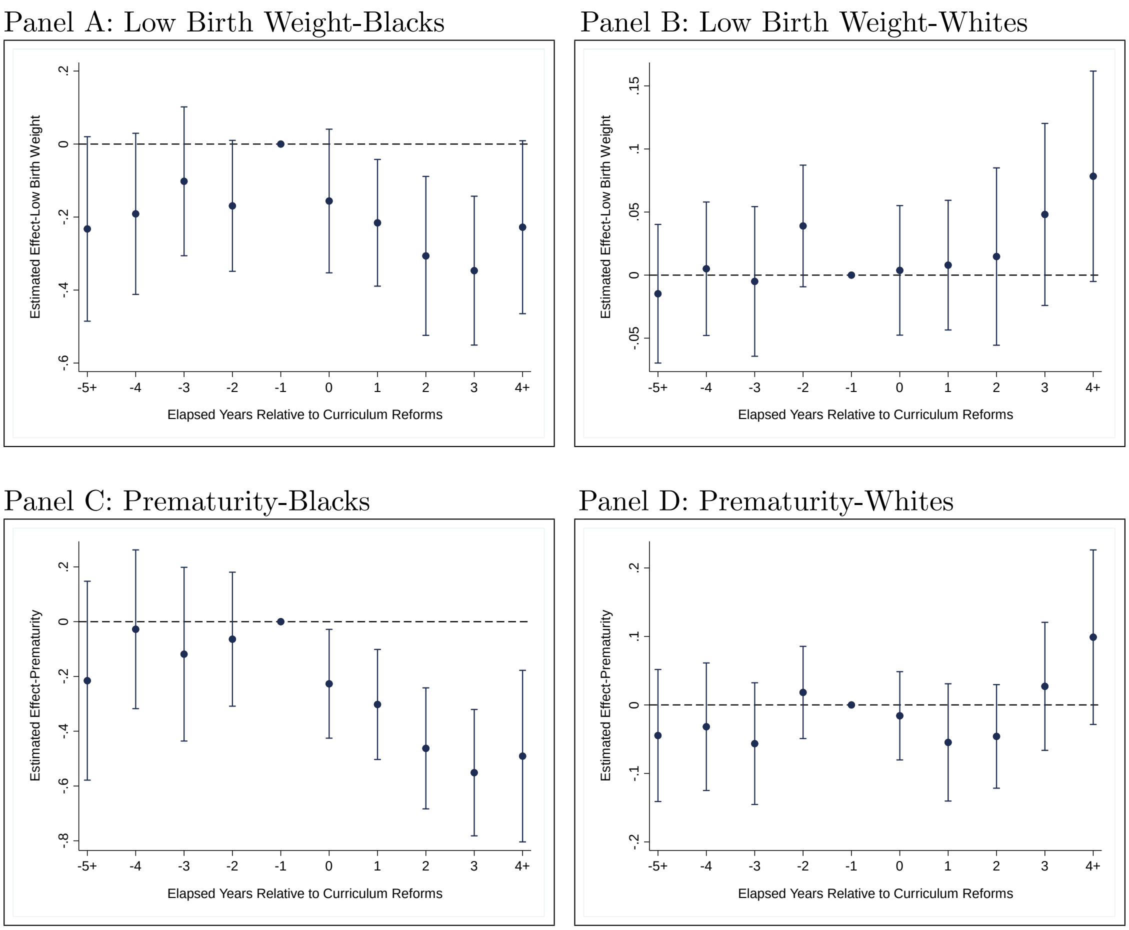
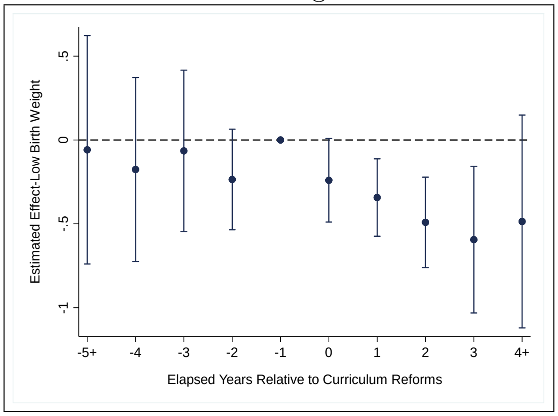


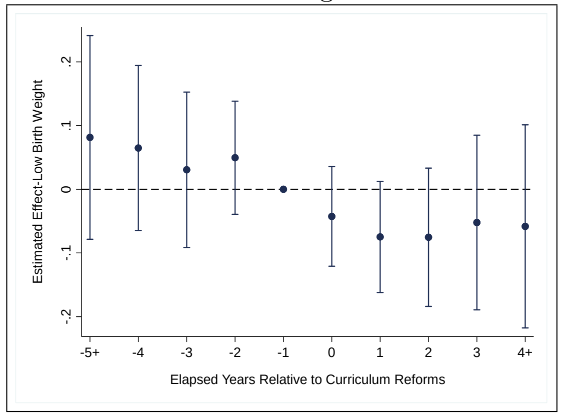
Figure A3: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes-All States (including District of Columbia)

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

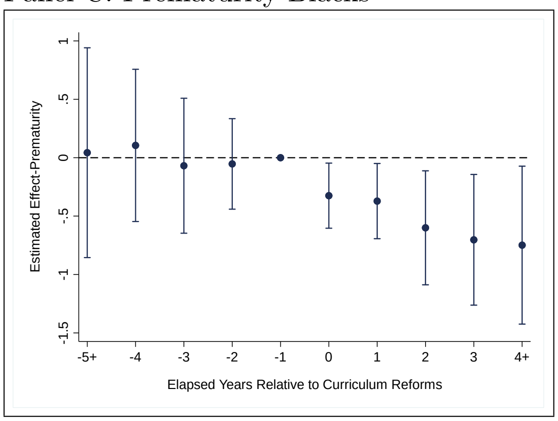
Panel A: Low Birth Weight-Blacks



Panel B: Low Birth Weight-Whites



Panel C: Prematurity-Blacks



Panel D: Prematurity-Whites

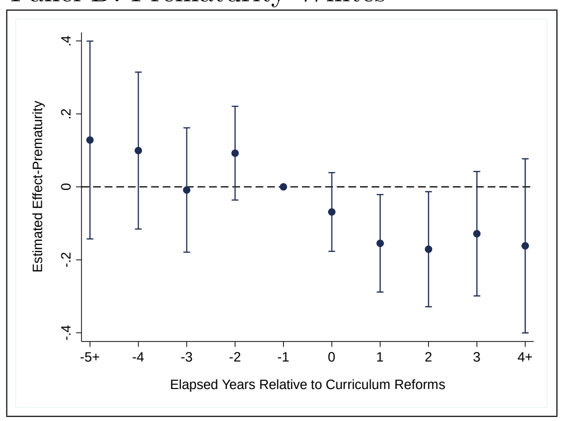


Figure A4: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes-Exclude Most Populous States

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

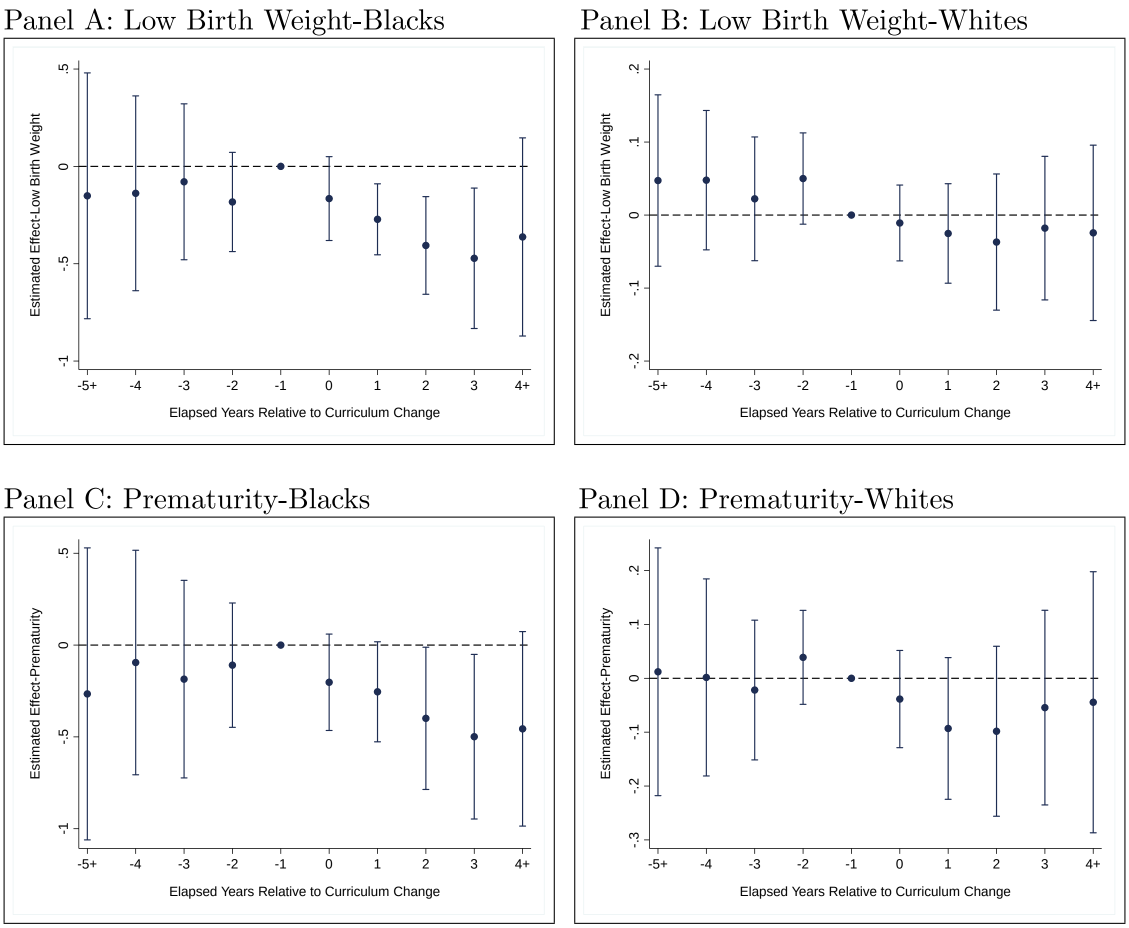
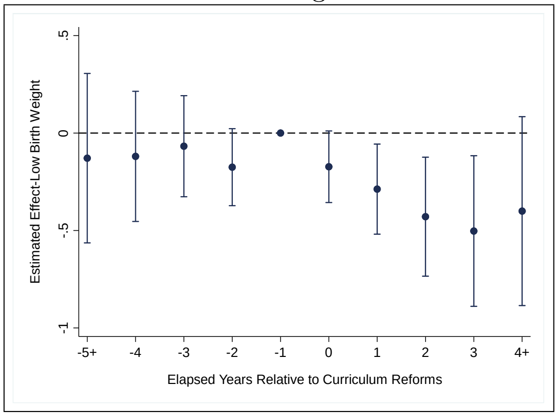


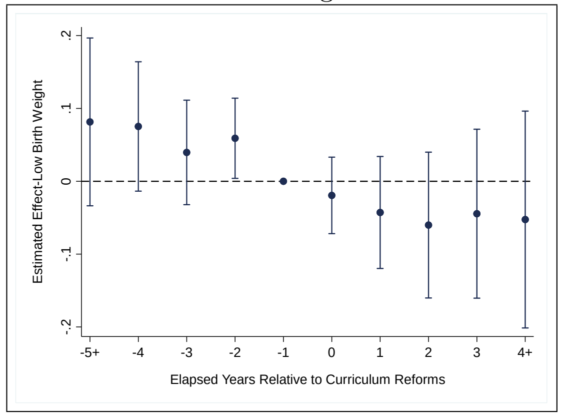
Figure A5: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes-Control Other Major Policy Changes

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

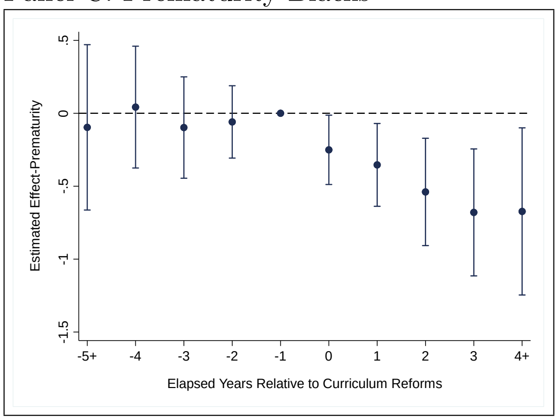
Panel A: Low Birth Weight-Blacks



Panel B: Low Birth Weight-Whites



Panel C: Prematurity-Blacks



Panel D: Prematurity-Whites

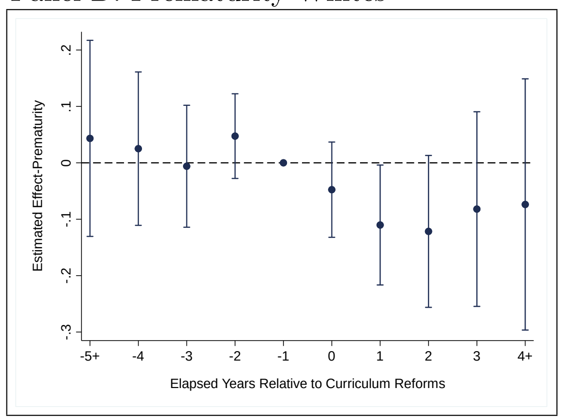


Figure A6: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes-Clustering at the State-by-Birth Year Level

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

Panel A: Low Birth Weight-Hispanics   Panel B: Prematurity-Hispanics

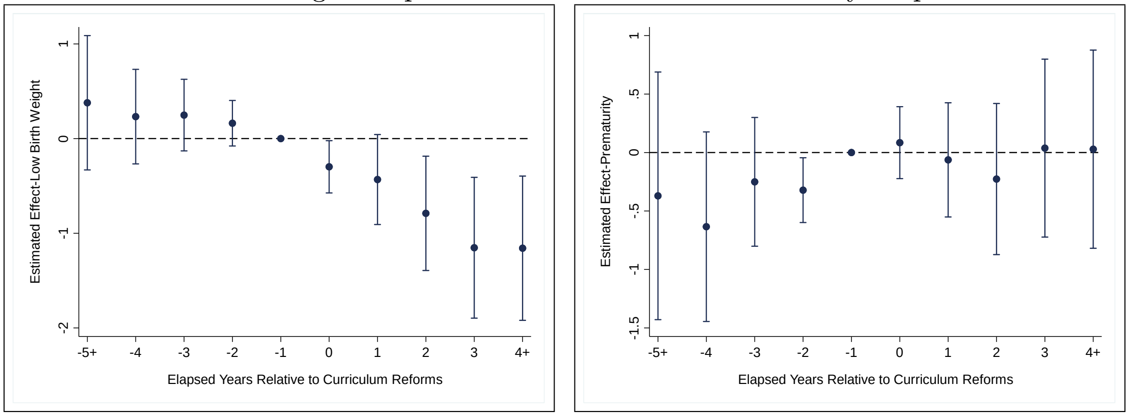


Figure A7: Dynamic Effects of Curriculum Reforms on Infant Health Outcomes-Hispanic Mothers

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.

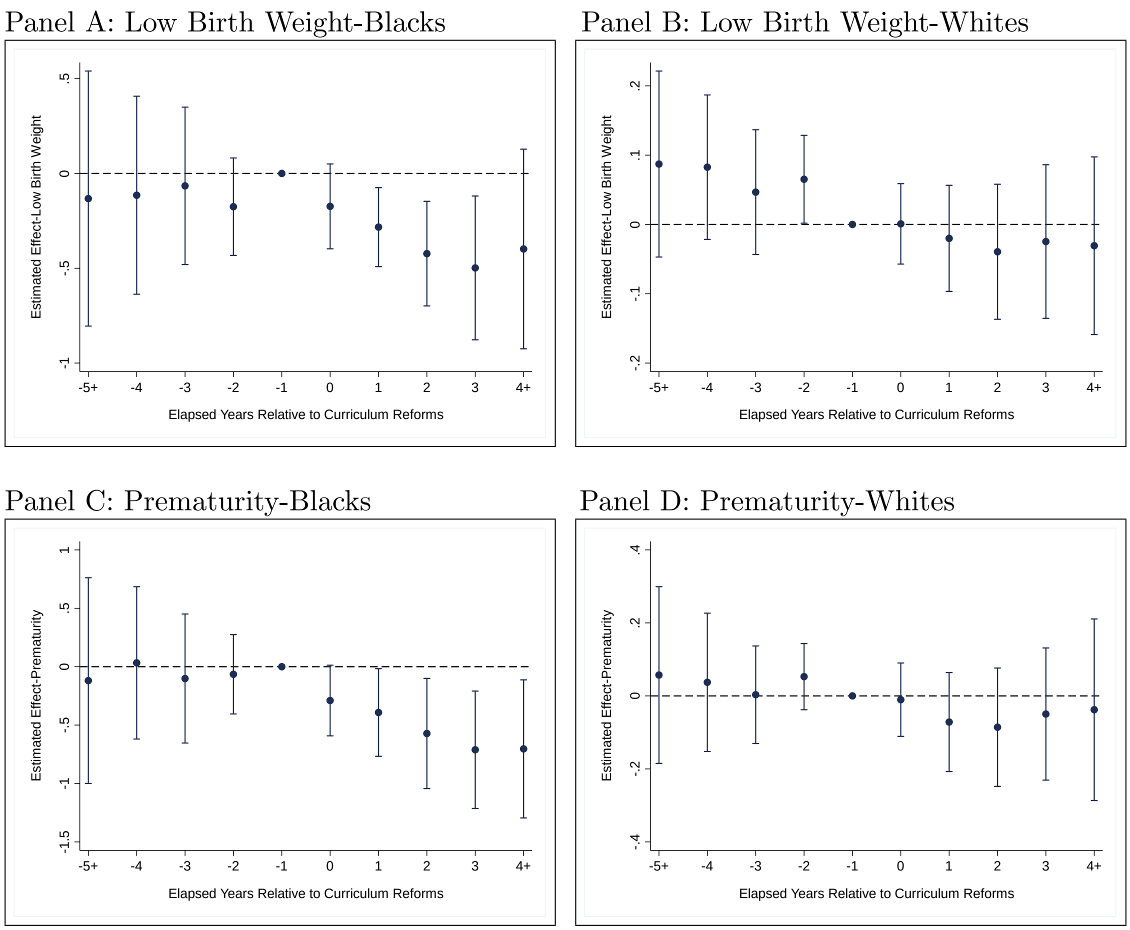


Figure A8: Dynamic Effects of Math Reforms on Infant Health Outcomes-Controlling for the Total Number of Minimum Courses Required in Other Subjects

NOTES: Each panel shows coefficient estimates and 95% confidence intervals based on standard errors clustered at the state level. Year prior to curriculum reforms is the omitted category. All outcome variables are multiplied by 100 to obtain percent values.