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ABSTRACT

Personal Belief Exemptions for School-Entry Vaccinations, Vaccination Rates, and Academic Achievement*

Nonmedical exemptions from school-entry vaccine mandates are receiving increased policy and public health scrutiny. This paper examines how expanding the availability of exemptions influences vaccination rates in early childhood and academic achievement in middle school. We leverage 2003 legislation that granted personal belief exemptions (PBE) in Texas and Arkansas, two states the previously allowed exemptions only for medical or religious regions. We find that PBE decreased vaccination coverage among black and low-income preschoolers by 16.1% and 8.3%, respectively. Furthermore, we find that those cohorts affected by the policy change in early childhood performed less well on standardized tests of academic achievement in middle school. Estimated effects on mathematics and English Language Arts test scores were largest for black students, especially those residing in economically disadvantaged counties.

JEL Classification: H75, I12, I18, I21, I24 I28

Keywords: vaccination, state mandates, nonmedical exemptions, test scores

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1 Introduction

Through the historic success of its immunization program, the United States has achieved dramatic reductions in childhood morbidity and mortality related to vaccine-preventable infections (CDC 1999, 2011). Despite (or, paradoxically, owing to) this remarkable progress, some parents choose to forgo or delay vaccinations for their children (Bauch and Bhattacharyya 2012). The emergence of "hot spots" or clusters of underimmunized children in recent years could pose a serious public health risk. The importance of this issue is underscored by recent outbreaks (Patel 2019) and by the intense debate surrounding the role of state governments in sustaining high vaccination coverage.

State laws that require proof of immunization for childcare or school attendance are an essential tool in ongoing efforts to protect children and communities from vaccine-preventable disease. These school-based mandates function as a "safety net" for children who do not receive recommended vaccines as an infant or toddler, assuring widespread coverage at the time of enrollment regardless of social, economic, or environmental circumstance (Orenstein and Hinman 1999). Prior work has shown mandates to be highly effective at increasing vaccination rates among preschool and school-aged children, and in turn, lowering rates at vaccine-targeted diseases (Lawler 2017; Abrevaya and Mulligan 2011; Carpenter and Lawler 2019).

Vaccine mandates are not without controversy. State legislatures must negotiate an unavoidable and complicated bargain: safeguarding public health (social welfare) while also respecting a parent's individual rights to choose (liberty). Notably, all states permit exemptions from school vaccine requirements based on medical contraindication, and most states allow parents to opt out of required immunizations based on religious belief and/or secular objections.[1] Persistent growth in nonmedical exemption rates, especially in states that grant exemptions based on nonreligious personal belief, threatens to undermine the intent of vaccine mandates.[2] State legislatures have countered by advancing bills that would limit vaccine exemptions.[3]

[1] Almost all states (except California, Mississippi and West Virginia) grant nonmedical exemptions from school immunization requirements for people who have sincerely held beliefs that prohibit immunizations. Currently, fifteen states allow philosophical exemptions for those who object to immunizations because of personal, moral or other beliefs (NCSL, 2020).

[2] Omer et al (2012) compute the annual change in the rates of nonmedical exemptions (NME) from school immunization requirements using data compiled by the Centers for Disease Control and Prevention (CDC). Over the study period, unadjusted rates for NME in states that allowed philosophical exemptions were 2.54 times as high as rates in states that allowed only religious exemptions. High NME rates have been associated with individual and community risk for preventable diseases, including measles and pertussis (whooping cough). Phadke et al (2016) offer a review of the relevant medical and public health literature.

[3] Following a measles outbreak at the Disneyland amusement park, California enacted legislation to repeal NME in 2016. Vermont similarly eliminated philosophical exemptions (but preserved a provision for religious exemptions). Additional outbreaks have triggered many states to consider bills to eliminate NME, including Arizona, Iowa, Maine, Minnesota, New Jersey, New York, Oregon, and Washington (NCSL, 2020).

In this paper, we provide important new evidence on how expanding the availability of exemptions for school-based mandates influences not only vaccination rates in early childhood but academic achievement in middle school. The question of whether mandatory vaccination laws, typically targeting young children prior to school entry, affect outcomes later in life is of considerable policy importance. More broadly, our analyses highlight the role of health and health-related policy in human capital production.

Our study exploits the implementation of a provision allowing philosophical, or personal belief, exemptions (PBE) from school vaccination requirements in Texas and Arkansas in 2003. These states previously allowed exemptions only for medical or religious reasons. In the first stage of our analysis, we use provider-verified data on immunization histories for children aged 19-35 months from the 1999-2006 waves of the National Immunization Survey-Child (NIS-Child). Using a difference-in-differences (DD) framework, we estimate the effects of the policy change on vaccination rates, i.e. the likelihood that a child has received all doses in the early childhood vaccine series, relative to states that do not permit PBE. We use both event-study analyses and permutation inference procedures to bolster our DD approach. In the second stage of our analysis, we use county-level estimates of average test scores for students in grades 5-7 from the 2009-2015 waves of the Stanford Education Data Archive (SEDA). Using similar empirical models and tracking the same birth cohorts from early childhood into adolescence, we estimate the long-term effects of the policy change on children's academic achievement.

We find that state policies allowing PBE for daycare and school-entry vaccination requirements diminished vaccination coverage in two subpopulations of preschool-age children. Following the 2003 policy changes, non-Hispanic black children and children from low-income families were significantly less likely to be up to date on recommended vaccines. Although many parents seek exemptions for sincerely held religious or philosophical objections, some parents opt out of a required vaccine as a matter of convenience (Blank, Caplan and Constable 2013). School personnel may further promote the adoption of exemptions of convenience (rather than risk having a child be excluded from school).[4]

We also find that these new state policies granting PBE had long-lasting effects on students' performance in mathematics and English Language Arts. The evidence is consistent with the

[4]Our first stage results are driven by an increase in undervaccinated children, i.e. children that have received some but not all of their recommended vaccines. it is important to note that this population is socioeconomically and demographically distinct from unvaccinated children, i.e. children who have never been vaccinated. Undervaccinated children are more likely to be black, to have a younger mother who does not have a college degree, and to live in a household near the poverty line (Smith, Chu and Barker 2004). While children tend to be unvaccinated due to medical or philosophical reasons, many children are undervaccinated as a result of financial and logistical barriers to immunization. Fewer than 1% of children in the United States were unimmunized in 2016

first stage of our analysis. Following the 2003 policy changes, black middle school students, especially those residing in economically disadvantaged counties, performed less well on standardized tests of academic achievement. This detriment was measured nearly ten years after the reported decline in vaccination coverage. Overall, our results indicate that state laws that allow personal belief exemptions for school-entry vaccine mandates may inadvertently further disadvantage marginalized communities.

Our research contributes to the growing empirical literature on the determinants of childhood vaccination. Prior work has examined the role of compulsory immunization in sustaining and increasing vaccination coverage rates.[5] Recent studies that leverage the staggered adoption of mandates across states provide credible evidence that state laws requiring certain vaccines as a condition for daycare and/or school attendance not only improve coverage rates in young children (Lawler 2017; Abrevaya and Mulligan 2011) and adolescents (Carpenter and Lawler 2019) but reduce the population-level burden of targeted vaccine-preventable disease. Comparatively little is known about the role of state regulations governing exemptions for mandated vaccines. Richwine, Dor, and Moghtaderi (2019) exploit recent legislation in California to identify the causal effect of eliminating nonmedical exemptions and find that consequent gains in vaccination coverage are offset, in part, by parents' ability to secure medical exemptions in lieu of exemptions based on personal belief. To our knowledge, the effects of state legislation that expanded exemptions for mandatory immunizations, like those enacted in Texas and Arkansas in 2003, have not yet been evaluated within a rigorous causal framework.

Finally, we contribute to the growing literature evaluating the long-term effects of health care and health-related public interventions during early childhood. Prior work has examined the role of newborn and infant health care (Bharadwaj, Løken and Neilson 2013; Chay, Guryan and Mazumder 2009), public health insurance (Miller and Wherry 2019; Cohodes et al. 2016; Brown, Kowalski and Lurie 2019) the Food Stamps Program (Hoynes, Schanzenbach and Almond 2016), and the 1970 Clean Air Act Amendment (Isen, Rossin-Slater and Walker 2017) in promoting better educational or economic outcomes later in life, typically showing that better health outcomes in childhood lead to improved human capital formation, increased labor force participation, higher earnings, and lower welfare dependency. To our knowledge, only one other study has considered the long-term effects of school-entry vaccine mandates. Leveraging the staggered introduction of mandates for the measles vaccine in the 1960s and 1970s, Luca

[5] Additional research has empirically evaluated the role of (mis)information (Anderberg, Chevalier and Wadsworth 2011; Chang 2018), outbreaks of vaccine-preventable disease (Oster 2018; Schaller, Schulkind and Shapiro 2017), and coverage mandates for private insurers (Chang 2016).

(2016) finds that school-based immunization requirements not only reduced childhood morbidity attributable to communicable diseases but generated significant improvements in long-run educational attainment. In addition, estimated gains in years of completed schooling were twice as large among non-white (relative to white) cohorts. Using legislative changes that occurred in the early 2000s, our project is the first to document the long-term effects of state laws relaxing school vaccination requirements.

The rest of our paper is organized as follows: In Section 2, we describe the data we use in our analysis. In Section 3, we outline our empirical strategy. We detail results from our analysis in Section 4, before presenting our conclusions in Section 5.

2 Data

Our analysis draws upon three main data sources: state laws establishing vaccine requirements and permitted exemptions from the Immunization Action Committee (IAC), provider-verified immunization histories for a nationally-representative sample of children aged 19-35 months from the National Immunization Survey-Child (NIS-Child), and county-level measures of academic achievement for students in public and charter schools from the Stanford Education Data Archive (SEDA).

The IAC compiles state laws and regulations governing immunization. Using the IAC repository, we collected information on vaccination requirements for preschool/kindergarten (Appendix Tables A.1-A.2) and middle school (Appendix Tables A.3-A.4) attendance along with the categories of vaccine exemption (i.e., medical, religious, and/or philosophical) permitted in each state. In cases where an exact date of implementation was not recorded in the IAC database, we examined primary sources, e.g. state Department of Health websites and/or state statutes. State policies were treated as in effect beginning in the implementation year.

While all states permit exemptions for medical reasons, and nearly all states allow exemptions for those with religious objections to immunization, provisions for philosophical, or personal belief, exemptions (PBE) are less common. Our analysis focuses on the introduction of PBE in two states – Texas and Arkansas – that previously allowed exemptions only for medical or religious reasons. Figure 1 highlights those states included in the treatment or the control group. Already-treated units, i.e. states with existing PBE provisions in the first year of our sample period, are excluded from the control group.[6]

[6]Goodman-Bacon (2018) analyzes the two-way fixed effects difference-in-difference estimator with variation in treatment timing and finds that when already-treated units act as controls *and treatment effects vary strongly over time*, DD estimates are typically biased away from the sign of the true treatment effect. Potential bias resulting

To estimate the effect of policies allowing PBE for state vaccination requirements on vaccination coverage, i.e. our first stage effect, we combined information from our state immunization law database with childhood vaccination records from the 1999-2006 waves of the NIS-Child. The NIS-Child is an annual state and nationally representative survey conducted on behalf of the Centers for Disease Control and Prevention (CDC) to monitor vaccination coverage among children aged 19-35 months in the United States. Data collection proceeds in two phases. A random-digit-dialed (landline and wireless) telephone survey identifies households with age-eligible children. Interviewers collect basic sociodemographic data and obtain consent from a parent or guardian to contact the child's vaccination provider(s). A mail survey, sent to each nominated provider, collects information from the child's immunization records. Around 30,000 households complete the NIS-Child survey each year. In a typical year, adequate provider data is available for nearly 70 percent of sample children.

Our sample (spanning the 1999-2006 waves of the NIS-Child) includes over 99,000 children with adequate provider data born between 1997 and 2004. Data include individual-level, provider-verified immunization histories as well as a variety of individual- and family-level characteristics expected to influence vaccine completion: child sex, age, birth order (later- versus first-born child), and race/ethnicity; maternal age, education, marital status; and family income and mobility (an indicator for whether the family had moved since the child's birth). Our primary outcome of interest is a child's completion of the combined 4:3:1:3:3:1 six-vaccine series that includes at least 4 doses of diphtheria, tetanus, and pertussis (DTaP), 3 doses of inactivated poliovirus (IPV), 1 dose of measles, mumps, rubella (MMR), 3 doses of Haemophilus influenzae type b (Hib), 3 doses of hepatitis b (HepB), and 1 dose of varicella (VAR).[7] We also consider immunization status for each of the six component vaccines individually. The childhood immunization schedule recommended by the ACIP during our sample period is summarized in Figure 2. Descriptive statistics for the NIS-Child sample are presented in Appendix Table A.5.

The proportion of children aged 19-35 months who were fully immunized increased between 1999 and 2006 (Figure 3). While coverage improved for all groups, disparities in vaccination rates based on income and race persist. Poor and minority children are less likely to complete

from the inclusion of an early treatment group, i.e. the inclusion of 16 states that permitted PBE prior to 1999, is of particular concern in Stage 2 of our analysis as the post-treatment event study coefficients (Figure 7) indicate a negative and growing effect of PBE on minority children's test scores.

[7] Each vaccine included in the 4:3:1:3:3:1 series was universally recommended for children by the CDC's Advisory Committee on Immunization Practices over the duration of our sample period (1999-2006). Several vaccines have been added to the recommended childhood immunization schedule since 1999: the pneumococcal conjugate vaccine (PCV) series in 2001, the annual influenza vaccine in 2005, and the hepatitis A (HepA) vaccine in 2006. Mandates for the PCV and HepA vaccines were uncommon prior to 2006. No state mandated the influenza vaccine.

the immunization schedule recommended by the CDC (Appendix Figure A.1). In contrast, unvaccinated children, i.e. children who have yet to receive any vaccines, are more likely to be white and to live in families with higher incomes (Appendix Figure A.2). Despite recent trends, fewer than 1% of children in the United States were unimmunized in 2016.

To estimate the effect of policies allowing PBE for school-entry vaccine mandates on children's academic achievement, we combined information from our state immunization law database with a nationwide archive of standardized test scores for SY 2008/09 through SY 2014/15 available through the SEDA. The SEDA data comprise repeated cross-sections of academic achievement by grade, school year, subject, race/ethnicity and gender. Estimates of the average math and English Language Arts (ELA) test scores for school districts and counties across the United States. are based on the results of approximately 300 million standardized tests of academic achievement taken by approximately 45 million students in grades 3-8 attending public and charter schools between 2009-2015 (Sean F. Reardon, Andrew D. Ho., Benjamin R. Shear, Erin M. Fahle, Demetra Kalogrides, & Richard DiSalvo. 2017). State-specific mean test scores have been placed on a common scale – SEDA analysts have linked state standardized achievement tests to states' National Assessment of Educational Progress (NAEP) results – so achievement measures can be meaningfully compared across states, counties, school districts, grades, and/or years.

Our sample (spanning the 2009-2015 waves of the SEDA) comprises county-level estimates of average test scores for students in grades 5-7. We consider achievement on the math and ELA subject tests separately. The SEDA includes several measures of academic achievement for counties. The cohort standardized (CS) scale utilized in our analyses compares county-level average achievement in a given grade and year to the national average in that grade in the year when a specific cohort was in that grade (Reardon, Kalogrides and Ho 2017). Estimated effects are interpreted in standard deviation units. In addition to county-level achievement measures disaggregated by grade, school year, subject, and race/ethnicity, we observe the racial and socioeconomic composition of school districts. We use this information, aggregating districts to counties, to construct grade by year measures of the percent of enrolled students across racial and ethnic groups as well as the percent of enrolled students eligible to receive free and reduced-price lunch (FRPL). Descriptive statistics for the SEDA sample are presented in Appendix Table A.6.

Efforts to capture the short- and long-term consequences of PBE rest on our ability to observe a birth cohort during early childhood and again during adolescence. Figure 4 shows the birth cohorts represented in both the NIS-Child and the SEDA analysis samples. If children typically enter kindergarten at age 5, children in the 1997 birth cohort (C0) are captured in the 1999 NIS-Child, enter kindergarten in SY 2002/03, and are first observed in SEDA data as sixth graders in SY 2008/09. Likewise, children in the 2004 birth cohort (C7), are captured in the 2006 NIS-Child, enter kindergarten in SY 2009/10 and are first observed in SEDA data as fifth graders in SY 2013/14. We do not observe achievement measures for all cohorts in all grades. However, within each grade level we observe achievement measures for at least three untreated cohorts and at least two treated cohorts.

3 Empirical Strategy

To estimate the effects of state policies granting PBE on (1) vaccination coverage and (2) academic achievement, we implement a difference-in-differences strategy that exploits state-level policy changes. Texas and Arkansas, two states that previously allowed exemptions only for medical or religious reasons, enacted legislation permitting PBE in 2003. Our approach compares outcomes across groups, i.e. children residing in TX or AR versus children residing in a state that does not allow PBE, and across time, i.e. children born 2001-2004 (cohorts C4-C7 in Figure 4) who would have been subject to the new PBE provision during the preschool years versus children born 1997-2000 (cohorts C0-C3 in Figure 4) who, if vaccinated in accordance with the CDC's official immunization schedule, would have completed the early childhood vaccine series prior to the policy change.

3.1 Stage 1: PBE and Vaccination

To estimate the effect of a PBE provision on vaccination coverage, i.e. our first stage effect, we combine information from our state immunization law database with provider-verified childhood immunization histories from the 1999-2006 waves of the NIS-Child. Using individual-level data and exploiting the introduction of PBE in TX and AR in 2003, we estimate a standard two-way fixed effects difference-in-differences (DD) style model. Our main specification is a linear probability model of the following form:

$$V_{ist} = \beta_0 + \beta_1 PBE_{st} + \beta_2 X_{ist} + \beta_3 Z_{st} + \beta_4 M_{st} + \beta_5 S_s + \beta_6 T_t + \epsilon_{ist} \quad (1)$$

where V_{ist} is a measure of vaccination status (completion of the 4:3:1:3:3:1 combined 6-

vaccine series or completion of all recommended doses for a component vaccine) of individual i in state s and year t. The binary treatment indicator PBE_{st} is equal to one in states and years with a PBE provision. State fixed effects S_s capture any time-invariant unobservable state characteristics that might lead to consistently higher vaccine uptake in one state versus another. Year fixed effects T_t adjust for any evolving trends, e.g. in public perception of immunization and/or parental vaccine behaviors, that are common to all states.

The model includes controls for individual- and family-level characteristics expected to influence vaccine take up and completion, state-level socioeconomic conditions that may influence a child's access to health care, and potentially confounding cross-state differences in vaccine policy that evolve over time. The vector X_{ist} includes child sex, age, birth order (later- versus first-born child), and race/ethnicity; maternal age, education, marital status; and family income, and mobility (an indicator for whether the family had moved since the child's birth). The vector Z_{st} includes measures of the state-level poverty rate, unemployment rate, Medicaid coverage rate, and children's uninsurance rate. The vector M_{st} captures state mandates for the hepatitis A, hepatitis B, varicella, or pneumococcal conjugate (PCV) vaccine. Because errors are unlikely to be independent within states over time, we cluster standard errors at the state level (Bertrand, Duflo and Mullainathan 2004).

In separate specifications, the model in Equation 1 is augmented with state-specific linear cohort trends (where we interact each state fixed effect with a variable *TREND* that equals 1 for cohort C0 children captured in the 1999 NIS-Child, 2 for cohort C1 children captured in 2000 NIS-Child, and so forth) and/or a policy interaction term to test for heterogenous effects by race or economic disadvantage.

The causal interpretation of the two-way fixed effects DD coefficient relies on the assumption that important unmeasured variables are either time-invariant group attributes (captured by the state fixed effects) or time-varying factors that are group invariant (captured by the year fixed effects). In other words, identification relies on the assumption that, in the absence of the policy change in 2003, vaccination outcomes would have evolved similarly in treated and control states. We supplement the two-way fixed effects model in Equation 1 with a regression-based event-study style analysis. For this model, we replace the binary treatment variable with a series of indicator variables, i.e. leads and lags, representing years relative to a treated state's adoption of a PBE provision. All other covariates are consistent with the model presented in Equation 1. This framework provides a visual check for potential policy endogeneity and facilitates examination of the dynamics of estimated treatment effects.

To estimate the effect of policies allowing PBE from mandated immunizations on children's academic achievement, we combine information from our state immunization law database with county-level estimates of average math and ELA test scores from the SEDA. Using the 2008-2015 waves of the SEDA, in Stage 2 we observe the same birth cohorts captured as preschoolers in Stage 1 (i.e. in the 1999-2006 NIS-Child surveys) nearly ten years later as middle schoolers. Again, we exploit the introduction of PBE in TX and AR in 2003 to estimate a standard difference-in-differences model. Our main specification is a linear probability model of the following form:

$$A_{isct} = \beta_0 + \beta_1 PBE_{sc} + \beta_2 X_{ict} + \beta_3 Z_{it} + \beta_4 M_{sc} + \beta_5 C_c + \beta_6 S_s + \beta_7 Tt + \epsilon_{ist} \quad (2)$$

where A_{isct} is the educational outcome (average math or ELA test score) in county i and state s, for cohort c, in year t. Figure 4 shows which birth cohorts and grade levels are included in our SEDA sample. We focus on outcomes measured in grades 5-7. We do not observe achievement measures for all cohorts in all grades. However, *within each grade level* we observe achievement measures for at least two untreated cohorts and at least three treated cohorts. The binary treatment indicator PBE_{sc} is equal to one in cohorts (in counties in TX or AR) who would have been subject to the new PBE provision (cohorts C4-C7 in Figure 4). As in Stage 1, we define cohort exposure relative to the policy environment at age 2, the age at which we expect children to complete their early childhood immunizations.

The model includes a set of cohort fixed effects (C_c), state fixed effects (S_s), and year fixed effects (T_t). The vector X_{ict} includes county-by-cohort controls for the percent of enrolled students across race and ethnicity groups (non-Hispanic black, Hispanic, Asian, American Indian) and the percent of enrolled students eligible for free/reduced price lunch (FRPL). The vector Zit includes measures of the county-level educational attainment (the percentage of residents that have a bachelor's degrees or higher), poverty rate (the percentage of households receiving SNAP benefits), unemployment rate, and children's uninsurance rate. Finally, the model includes controls for coincident (and potentially confounding) changes in state-level vaccine and Medicaid policy. The vector M_{sc} captures state mandates enforced at daycare and/or kindergarten entry for the hepatitis A, hepatitis B, varicella, and PCV vaccines; state mandates enforced at middle school entry for the tetanus, diphtheria, and pertussis (Tdap) booster, meningococcal (MCV), hepatitis B, and human papillomavirus (HPV) vaccines; and state decisions to expand Medicaid eligibility.[8] Standard errors are clustered at the state level.

Using the model specified in Equation 2, we first estimate the effect of state policies permitting PBE on aggregate educational outcomes (county-level average math and average ELA test scores of enrolled students across all racial and ethnic groups). We next estimate the effect of these provisions on the educational outcomes of non-Hispanic black students, specifically. In separate specifications, the model in Equation 2 is augmented with state-specific linear time trends and/or a policy interaction term to test for heterogeneous effects of PBE by economic disadvantage. For the latter, high-poverty counties are defined as counties where more than 50 percent of students are eligible for free or reduced-price lunch.[9]

We supplement the standard DD model in Equation 2 with a regression-based event-study style analysis. This framework provides both a visual check of the parallel trends assumption that underlies identification in the DD framework and an opportunity to examine the dynamics of estimated treatment effects.

4 Results

4.1 PBE and Vaccination

In Table 1, we present DD estimates of the net change in vaccination coverage in states that enacted legislation granting PBE for daycare and school-entry vaccination requirements relative to states that continue to permit exemptions only for medical or religious reasons. The outcome of interest, completion of all doses of six recommended vaccines by age two, represents adherence to the ACIP-recommended vaccine schedule. Each column in the table corresponds to a separate regression. Column 1 presents results from the baseline model specified in Equation 1. In columns 2 and 3, we introduce a policy interaction term in order to test for treatment effect heterogeneity by race and by income, respectively. Finally, in columns 4-6, we present results from specifications mirroring those presented in columns 1-3 that additionally control for state-specific linear time trends.

Across the six specifications presented in Table 1, we find compelling evidence that state policies allowing PBE from mandatory immunizations diminish vaccination coverage – at least among certain subpopulations of preschool-age children. Beginning with the baseline estimate

[8] A mandate is considered in effect for cohort c in state s if there was a binding mandate for preschoolers in the year the cohort is captured in the NIS-Child sample (for the hepA, hepB, VAR, and PCV vaccine) or if there was a binding mandate for middle schoolers in the year the cohort entered sixth grade (for the Tdap, meningococcal, hepB, and HPV vaccines).

[9] FRPL eligibility is a commonly applied proxy for economic disadvantage.

in column 1, we find that the introduction of PBE in TX and AR reduced the likelihood that resident children aged 19-35 months completed the combined six-vaccine series by 3.2 percentage points. Remarkably, we find that the effects of PBE on vaccination coverage are largest for minority and economically disadvantaged populations. The results in column 2 indicate that the policy change led to a 10.2 percentage point reduction in the likelihood that non-Hispanic black children were up to date on recommended vaccines. The results in column 3 indicate that a PBE provision reduced the likelihood that children from low-income families, i.e. households with annual income below $25,000, were fully immunized by 5.2 percentage points. Relative to the mean coverage rates shown in Appendix Table A.5, these estimates translate to a 16.1 percent decrease in the proportion of black preschoolers and an 8.3 percent decrease the proportion of low-income preschoolers protected against vaccine-preventable disease. The introduction of state-specific trends (columns 4-6) diminishes the main effect. However, the policy interactions (by race and by income) remain large and statistically significant.

In Figure 5, we present estimates from a regression-based event study analysis. Each panel corresponds to a different sample: we present results for the full sample in Panel A, the subsample of non-Hispanic black children in Panel B, and the subsample of children from low-income families in Panel C. Each model includes a set of policy leads, i.e. event times that coincide with two, three, or at least four years before treatment, allowing one to empirically probe the credibility of the common trends assumption. Each model also includes a treatment indicator coinciding with first year TX and AR granted PBE from mandatory immunizations (event time 0) and a set of policy lags, i.e. event times that coincide with one, two, or at least years following treatment, allowing one to trace out the immediate and medium-term effects of a PBE provision. The year preceding policy implementation (event time -1) is the omitted category.[10]

Beginning with the results for the full sample in Panel A, we find little indication that legislative provisions for PBE diminish vaccination coverage among young children aged 19-35 months. This population-based estimate of the net change in vaccination coverage following the introduction of PBE masks substantial heterogeneity of treatment effects across subgroups of children. Again, we find that the effects of PBE on vaccination coverage are largest for minority and economically disadvantaged populations. The policy change in TX and AR led to an immediate reduction in vaccination coverage among non-Hispanic black children (Panel B),

[10]Our primary outcome of interest is a child's completion of the combined 4:3:1:3:3:1 six-vaccine series, i.e. whether (or not) a child is fully immunized. Parents may seek exemptions for some vaccine mandates but not others. In Appendix Table A.7, we consider a child's immunization status for each of the six component vaccines, individually. Our results suggest that the effects of PBE on vaccination coverage are likely driven by reductions in the likelihood that children receive the varicella (chickenpox) vaccine. However, among non-Hispanic black children, we find that state policies permitting PBE from mandatory immunizations reduce rates of completion across all childhood vaccines.

and this effect persisted for at least three years after the policy was introduced. Children from low-income families were also less likely to be up to date on recommended vaccines following the introduction of PBE (Panel C) though, in this case, the effects of the policy appear to be relatively short-lived. The combination of a flat pre-treatment period paired with clear post-treatment changes in Figure 5 lends credence to the causal interpretation of DD estimates in Table 1.

To address potential concerns about inference in DD with few treated groups (Wooldridge 2006; Donald and Lang 2007), i.e. our reliance on a 2003 policy change in just two states, we compare our estimates (Table 1) to additional DD estimations where placebo treatment status is assigned to 2 of 32 states that do not permit exemptions for philosophical reasons. This falsification test is akin to the classic framework for permutation inference (Fisher 1935): we repeat the DD estimation, assigning treatment status to each potential pair of control states in our sample, and treat the 496 placebo estimates as the sampling distribution for our parameter of interest. Under the null hypothesis that legislative provisions for PBE have no effect on vaccination coverage, we do not expect estimates of the treatment effect for TX and AR to be unusually large relative to the distribution of the placebo effects. In Figure 6, the estimated treatment effect for TX and AR (solid line) lies near or beyond the boundary of the placebo distribution (dashed lines correspond to the 5th and 95th percentiles). Using our placebo criterion, we can reject the null hypothesis that the effect of PBE among non-Hispanic black children is zero.

The extent to which the legislative changes in Texas and Arkansas should influence vaccination decisions is unclear ex ante. Prior to 2003, those parents most strongly opposed to school-based immunization requirements could have claimed insincere medical or religious exemptions (Richwine, Dor and Moghtaderi 2019; Blank, Caplan and Constable 2013) or chosen to homeschool rather than vaccinate their child(ren). Following the policy change, particularly if the costs of satisfying state vaccine requirements are expected to exceed the costs of obtaining a PBE, parents who ceteris paribus would have complied with school-entry mandates may elect to opt out of one or more required vaccines. This may be especially true within uninsured and vulnerable populations who face both personal and structural barriers to accessing primary healthcare. Our findings are consistent with the possibility that some parents seek exemptions as a matter of convenience or because of poor access to immunization services.[11]

[11] State regulations governing nonmedical exemptions from school immunizations have been studied extensively in the public health literature. In most states, the process for obtaining an exemption requires considerably less effort than satisfying vaccine requirements (Rota et al. 2001). Further, exemption rates are directly correlated with the simplicity of exemption procedures, suggesting that convenience may play an important role in parental decision-making (Blank, Caplan and Constable 2013).

4.2 PBE and Achievement

In Table A.7, we present DD estimates of the net change in academic achievement in TX and AR relative to states that continue to permit exemptions for medical or religious reasons only. The outcome of interest is a county-level average test score in English Language Arts (Panel A) or mathematics (Panel B).[12] Each column in the table corresponds to a separate regression. Column 1 presents results from the baseline model specified in Equation 2. In column 2, we introduce a policy interaction term to test for treatment effect heterogeneity by socioeconomic status, i.e. differential effects of PBE within high-poverty (versus low-poverty) counties.[13] Finally, in columns 3 and 4, we present estimates from analyses mirroring those underlying columns 1 and 2 where we confine our attention to the average achievement of non-Hispanic black students.[14]

The results in Table A.7 reveal lasting effects of PBE on students' academic achievement. The 1997-2004 birth cohorts, observed as preschoolers in Stage 1, are captured nearly ten years later as middle schoolers. Beginning with the baseline estimates in column 1, we find that overall achievement in a county decreased on average by 0.03 standard deviations following adoption of a statewide PBE provision. We show similar changes in students' performance in mathematics and English Language Arts, though estimates for the mathematics subject test are less precise. Subgroup analyses (column 3) show that changes in average achievement for non-Hispanic black students were larger: average scores in English Language Arts and mathematics declined by 0.09 standard deviations and 0.08 standard deviations, respectively. Across column 2 (all enrolled students) and column 4 (non-Hispanic black students) we find consistent evidence that the negative effects of PBE for student learning are concentrated among children residing in low-income communities.

In Figure 7, we present estimates from a regression-based event study analysis. In our models, we assign event time 0 to the first treated cohort, i.e. the first birth cohort that would have been subject to the new PBE provision during the preschool years (cohort C4 in Figure 4). The outcome of interest is a county-level average test score in English Language Arts (Panel A) or mathematics (Panel B). While we observe a modest drop in overall achievement following the policy change, point estimates (excluding one coefficient) are statistically indistinguishable

[12] The SEDA includes several measures of academic achievement for counties. The cohort standardized (CS) scale utilized in our analyses compares county-level average achievement in a given grade and year to the national average in that grade in the year when a specific cohort was in that grade. Estimated effects are interpreted in standard deviation units.

[13] High-poverty counties are defined as counties where more than 50 percent of students are eligible for free or reduced-price lunch.

[14] Our conclusions are robust to adding state-specific trends (Appendix Table A.7).

from zero. Again, the population-based estimate masks substantial heterogeneity of treatment effects across subgroups of children. We find that the 2003 policy change in Texas and Arkansas led to significant drop in non-Hispanic black students' performance in mathematics and English Language Arts. The decrease in average test scores is evident in the first treated cohort and, unfortunately, appears to amplify over time. We find little evidence of pre-event trends in standardized test scores. Our results support the causal interpretation of the DD estimates in Table A.7.

We propose several potential mechanisms for the effects reported in Table A.7. First, the availability of PBE diminished vaccination coverage, placing more children at risk for disease. Vaccine-preventable infections, including measles, pertussis (whooping cough) and varicella (chicken pox), are easily spread within the classroom and cause children to miss school.[15] Second, it is possible that lower vaccination rates correlate with decreased attendance to periodicity schedule for preventive pediatric health care. Routine well-child visits present an opportunity for early detection and intervention in common developmental and behavioral problems. During these visits, pediatricians may provide important information on healthy child development and positive parenting practices as well as physical health. Finally, consequent disruptions in instructional time (e.g., absenteeism, behavioral challenges) may inhibit the learning of student peers.

5 Conclusion

Following recent outbreaks, nonmedical exemptions from school-entry vaccine mandates have come under increased scrutiny. In this paper, we provide important new evidence on how expanding the availability of exemptions for school-based mandates influences both childhood health and human capital formation. Relative to 1- children who would have completed the early childhood vaccine series prior to the policy change and 2- children residing in states that do not allow PBE, children subject to the new PBE procedures in TX and AR were less likely to be fully immunized. Tracking the same birth cohorts from early childhood into adolescence, we find that those cohorts affected by the policy change performed less well on standardized tests in ELA and mathematics. To the best of our knowledge, our paper is the first to document the long-term effects of state laws relaxing school vaccination requirements.

Remarkably, we find that the effects of PBE on vaccination coverage are largest for minority

[15] The societal costs of vaccine-preventable infections, including student absenteeism, have been examined in the epidemiology literature (see, for example, Lee et al. (2004*a*,*b*)).

and economically disadvantaged populations. Our estimates translate to a 16.1 percent decrease in the proportion of black preschoolers and an 8.3 percent decrease the proportion of low-income preschoolers protected against vaccine-preventable disease. Despite media focus on unvaccinated children, vaccine hesitant parents far outnumber vaccine refusers. Our findings are consistent with research in the public health literature that suggests that convenience may play an important role in parental decisions about childhood vaccines (Rota et al. 2001; Blank, Caplan and Constable 2013). Parents who *ceteris paribus* would have complied with school-entry mandates may elect to opt out of one or more required vaccines if the costs of satisfying state vaccine requirements are expected to exceed the costs of obtaining a PBE. This may be especially true within uninsured and vulnerable populations who face both personal and structural barriers to accessing primary healthcare.

The subsequent evaluation of middle school students' educational outcomes recalls first stage results: we find that the consequences of PBE for student learning are largest for black students and students residing in economically disadvantaged communities. Estimated decreases in the average ELA (0.09 standard deviations) and math (0.08 standard deviations) scores of non-Hispanic black students are nearly three times larger than estimated changes in overall achievement (0.03 standard deviations). Additional tests show that our results are largely driven by high-poverty counties where more than 50 percent of students are eligible for free or reduced-price lunch.

Our results indicate that provisions designed to accommodate parents opposed to school vaccination requirements may have unintended and lasting consequences. School-entry vaccine mandates provide an incentive to parents who otherwise might not make the effort to follow recommendations for routine immunizations and, essentially, function as a "safety net" for children, schools, and communities. We found that the availability of PBE diminished vaccination coverage, placing more children at risk for disease. Though we are unable to directly test this hypothesis, it is plausible that lower vaccination rates correlate with decreased adherence to the recommended schedule for preventive pediatric health care (e.g., well-child visits) and, ultimately, missed opportunities to identify developmental delays and behavioral problems. Beyond the affected child, disruptions in instructional time (e.g., absenteeism, behavioral challenges) may inhibit the learning of student peers. If effects are largest for black children and children from low-income families, as our results suggest, state laws granting PBE may further disadvantage marginalized communities. Though we were only able to follow birth cohorts over a period of ten years, documented detriments may become even more pronounced with time, as early achievement gaps have been shown to persist into adulthood (Duncan and Murnane 2011).

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6 Tables and Figures

Figure 1: State Laws Permitting PBE for School-Entry Vaccination Requirements, 1999-2015

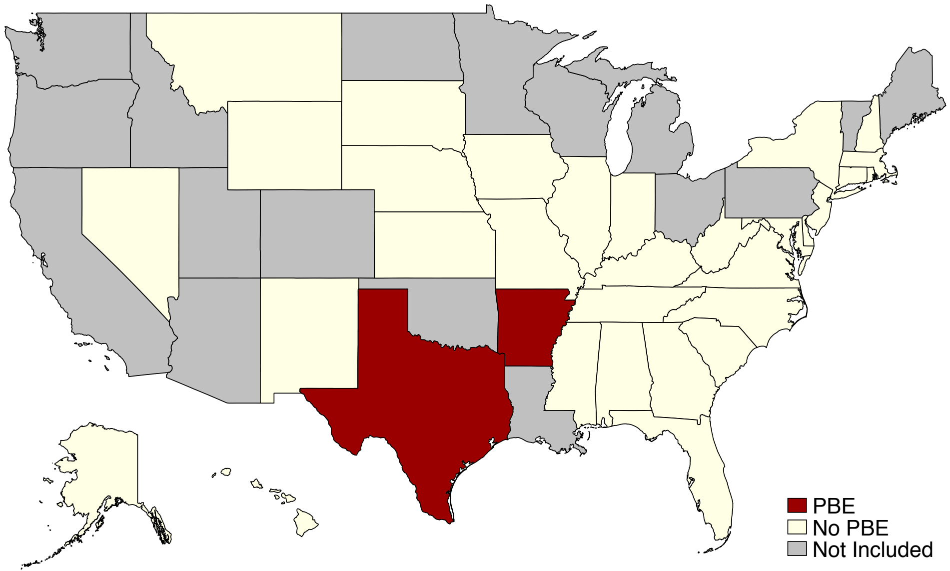


Figure 1 highlights those states included in the treatment or the control group. Our analysis focuses on the introduction of PBE in two states – Texas and Arkansas – that previously allowed exemptions only for medical or religious reasons. Already-treated units, i.e. states with existing PBE provisions in the first year of our sample period, are excluded from the control group.

Figure 2: Recommended Childhood Immunization Schedule, Birth to 3 Years

	Birth	1 mo	2 mos	4 mos	6 mos	9 mos	12 mos	15 mos	18 mos	19-23 mos	2-3 yrs
Diphtheria, tetanus, & acellular pertussis (DTaP: <7 yrs)			1st dose	2nd dose	3rd dose			←4th dose→			
Inactivated poliovirus (IPV:<18 yrs)			1st dose	2nd dose	←3rd dose→						
Measles, mumps, rubella (MMR)					*See notes*		←1st dose→				
***Haemophilus influenzae* type b** (Hib)			1st dose	2nd dose	*See notes*		←3rd or 4th dose→ *See notes*				
Hepatitis B (HepB)	1st dose	2nd dose			←3rd dose→						
Varicella (VAR)							←1st dose→				

Range of recommended ages for all children

Range of recommended ages for catch-up immunization

Range of recommended ages for certain high-risk groups

No recommendation

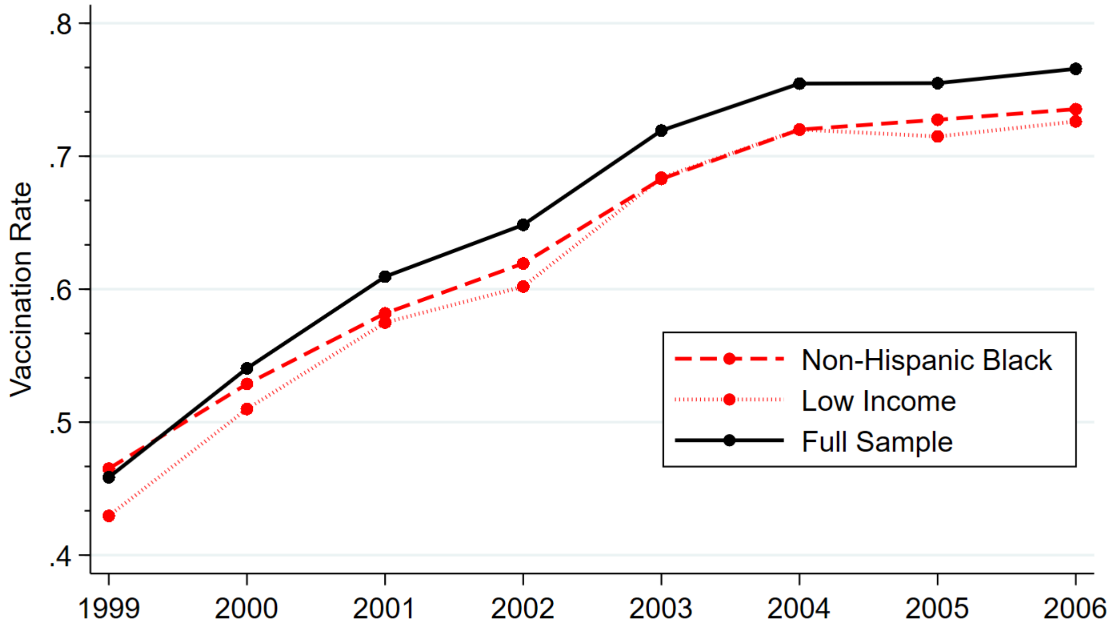
Figure 2 is adapted from the "Recommended Childhood Immunization Schedule – United States, 1999" published by the CDC's Advisory Committee on Immunization Practices.

The guidelines for *Haemophilus influenzae* type b (Hib) depend on the vaccine administered. If PRP-OMP (e.g. PedvaxHIB) is administered at 2 and 4 months of age, a dose at 6 months in not required. The final dose of the series should be administered at age $\geq$ 12 months.

A 4-dose pneumococcal conjugate vaccine (PCV) series was added to the recommended immunization schedule in 2001. Universal annual influenza vaccination for young children was recommended in 2005. The hepatitis A (HepA) vaccine has been recommended for all children since 2006.

Our primary outcome of interest is a child's completion of the combined 4:3:1:3:3:1 vaccine series that includes $\geq$ 4 doses of diphtheria, tetanus, and pertussis (DTaP), $\geq$ 3 doses of inactivated poliovirus (IPV), $\geq$ 1 dose of measles, mumps, rubella (MMR), $\geq$ 3 doses of *Haemophilus influenzae* type b (Hib), $\geq$ 3 doses of hepatitis b (HepB), and $\geq$ 1 dose of varicella (VAR). We also consider immunization status for each of the six component vaccines individually.

Figure 3: Combined 6-Vaccine Series Coverage Among Children Aged 19-35 Months in the United States by Race and Income, 1999-2006



In Figure 3, we present trends in vaccination coverage among children aged 19 to 35 months. While the proportion of children who were fully immunized increased significantly between 1999 and 2006, racial and economic disparities in vaccination rates persisted over the period.

The combined 6-vaccine series includes $\geq$ 4 doses of diphtheria, tetanus, and pertussis (DTaP), $\geq$ 3 doses of inactivated poliovirus (IPV), $\geq$ 1 dose of measles, mumps, rubella (MMR), $\geq$ 3 doses of *Haemophilus influenzae* type b (Hib), $\geq$ 3 doses of hepatitis b (HepB), and $\geq$ 1 dose of varicella (VAR).

Low income assigned based on annual household income below $25,000.

Vaccination rates for each of the six component vaccines are presented in Figure A.1.

Data are from the 1999-2006 waves of the NIS-Child .

Figure 4: Birth cohorts represented in the NIS-Child and the SEDA Samples

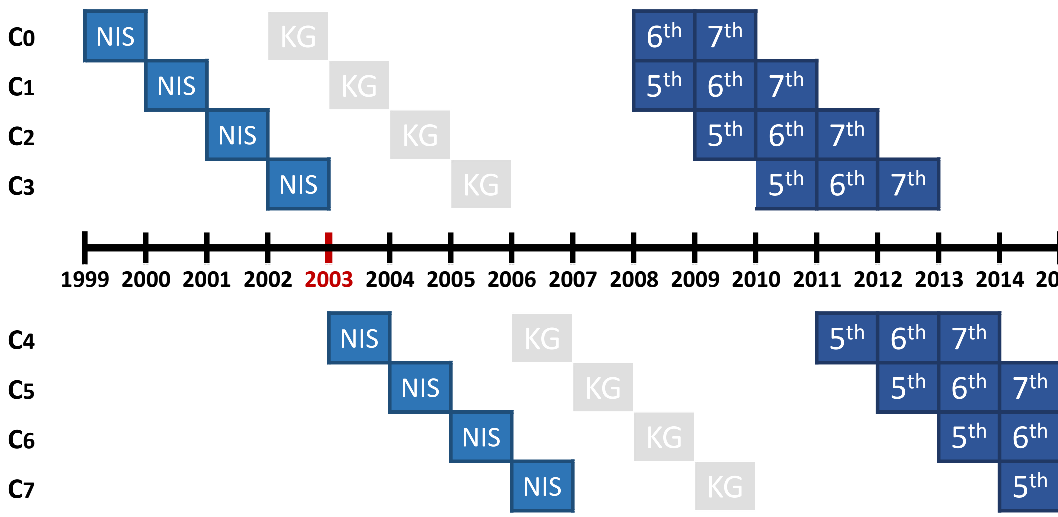


Figure 4 shows the birth cohorts represented in both the NIS-Child and the SEDA analysis samples.

If children typically enter kindergarten at age 5, children in the 1997 birth cohort (C0) are captured in the 1999 NIS-Child, enter kindergarten in SY 2002/03, and are first observed in SEDA data as sixth graders in SY 2008/09. Likewise, children in the 2004 birth cohort (C7), are captured in the 2006 NIS-Child, enter kindergarten in SY 2009/10 and are first observed in SEDA data as fifth graders in SY 2013/14.

We do not observe achievement measures for all cohorts in all grades. However, *within each grade level* we observe achievement measures for at least three untreated cohorts and at least two treated cohorts.

Table 1: Difference-in-Differences Estimates of the Effects of PBE on Early Childhood Vaccination Rates by Race and Income, NIS-Child (1999-2006)

	Child Received All Doses of the Combined 4:3:1:3:3:1 Vaccine Series					
	(1)	(2)	(3)	(4)	(5)	(6)
PBE	-0.032**	-0.025*	-0.017	-0.026	-0.018	-0.010
	(0.013)	(0.013)	(0.012)	(0.025)	(0.025)	(0.021)
PBE x Black		-0.077***			-0.076***	
		(0.011)			(0.010)	
PBE x Low Income			-0.035***			-0.035***
			(0.011)			(0.010)
State FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Time-varying controls	YES	YES	YES	YES	YES	YES
State-specific trends	NO	NO	NO	YES	YES	YES
Observations	99,464	99,464	99,464	99,464	99,464	99,464

In Table 1, we present estimates from the standard DD model in Equation 1.

The combined 6-vaccine series includes $\geq$ 4 doses of diphtheria, tetanus, and pertussis (DTaP), $\geq$ 3 doses of inactivated poliovirus (IPV), $\geq$ 1 dose of measles, mumps, rubella (MMR), $\geq$ 3 doses of *Haemophilus influenzae* type b (Hib), $\geq$ 3 doses of hepatitis b (HepB), and $\geq$ 1 dose of varicella (VAR).

Figure 1 highlights those states included in the treatment or the control group.
Low income assigned based on annual household income below $25,000.

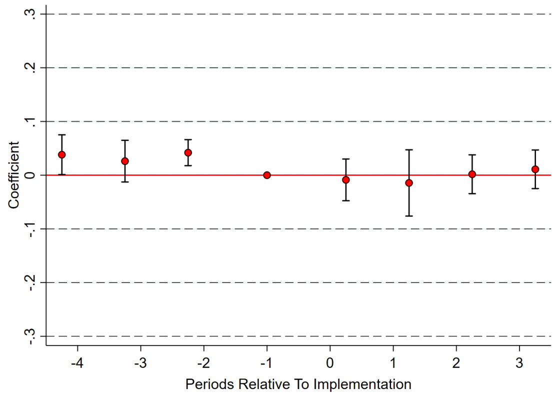
All models include controls for potentially confounding changes in state vaccine policy, i.e. state implementation of a new school-entry mandate for HepB, VAR, PCV, and/or HepA.

Robust standard errors (reported in parentheses) are clustered at the state level.

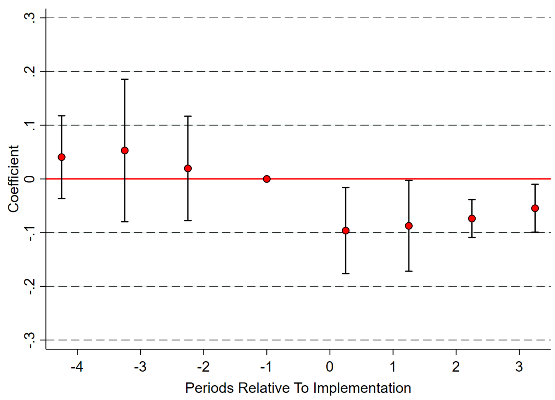
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 5: Event Study Estimates of the Effects of PBE on Early Childhood Vaccination Rates by Race and Income, NIS-Child (1999-2006)

A. Population



B. Non-Hispanic Black



C. Low Income

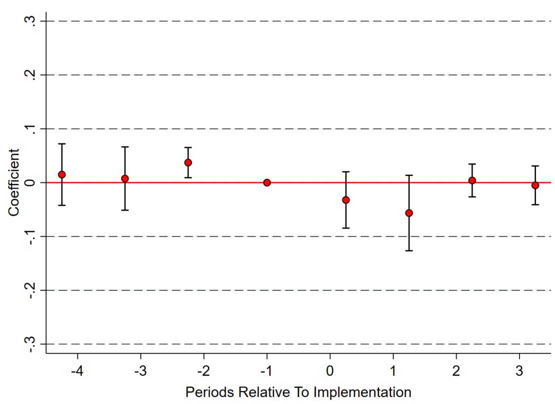
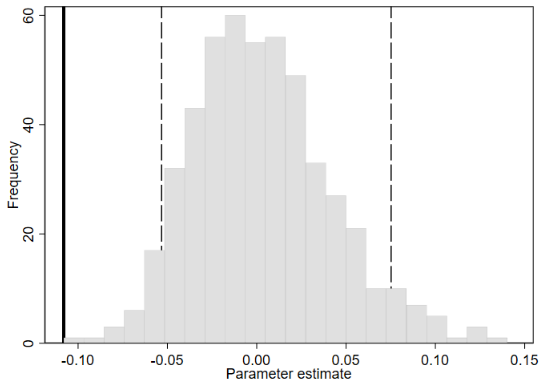


Figure 5 presents estimates from a regression-based event study analysis. Vertical bars represent the 95 percent confidence interval around each point estimate. The year preceding policy implementation (event time -1) is the omitted category. The outcome of interest is whether a child received all doses of the combined 4:3:1:3:3:1 vaccine series. Low income assigned based on annual household income below $25,000. All models include controls for potentially confounding changes in state vaccine policy, i.e. state implementation of a new school-entry mandate for HepB, VAR, PCV, and/or HepA.

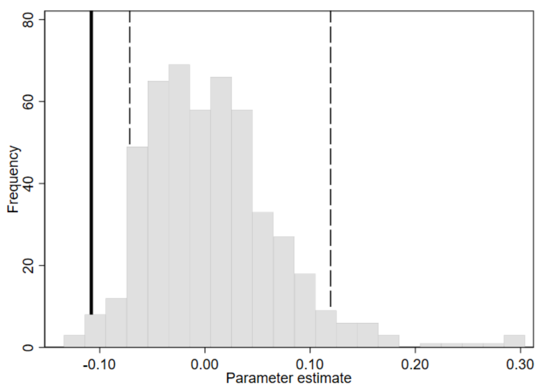
In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Figure **??**).

Figure 6: Difference-in-Differences Estimates of the Effects of PBE on Early Childhood Vaccination Rates by Race and Income (Placebo Tests, TX and AR vs All Control State Pairs)

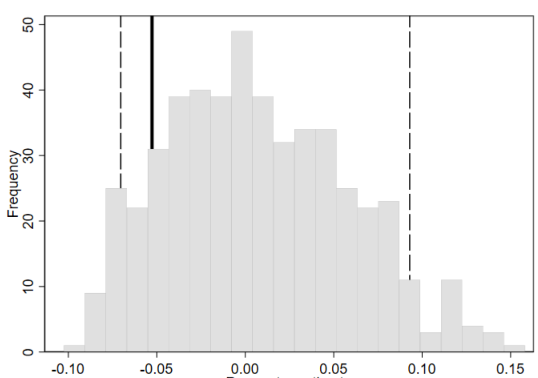
A. Population



B. Non-Hispanic Black



C. Low Income



The histograms in Figure 6 summarize the results of our falsification tests. We compare the estimated treatment effect for TX and AR (solid line, Table 1) to 496 additional DD estimations where placebo treatment status is assigned to 2 of 32 states that do not permit exemptions for philosophical reasons. The 5th and 95th percentile critical values are marked with dashed lines. The outcome of interest is whether a child received all doses of the combined 4:3:1:3:3:1 vaccine series. Low income assigned based on annual household income below $25,000. All models include controls for potentially confounding changes in state vaccine policy, i.e. state implementation of a new school-entry mandate for HepB, VAR, PCV, and/or HepA.

Table A.7: Difference-in-Differences Estimates of the Effects of PBE on English Language Arts and Mathematics Test Scores by Race and Economic Disadvantage, SEDA (2008-2015)

Panel A: **ELA**	All Enrolled Students		Non-Hispanic Black Students	
	(1)	(2)	(3)	(4)
PBE	-0.025***	0.015*	-0.090***	-0.036**
	(0.009)	(0.008)	(0.012)	(0.013)
PBE x High Poverty		-0.048***		-0.067***
		(0.002)		(0.004)
Observations	11,044	11,044	11,044	11,044

Panel A: **Math**	All Enrolled Students		Non-Hispanic Black Students	
	(1)	(2)	(3)	(4)
PBE	-0.029	0.002	-0.075***	-0.030
	(0.024)	(0.024)	(0.027)	(0.023)
PBE x High Poverty		-0.038***		-0.055***
		(0.003)		(0.008)
Observations	10,171	10,171	10,171	10,171

IIn Table 3, we present estimates from the standard DD model in Equation 2.

The SEDA includes several measures of academic achievement for counties. The cohort standardized (CS) scale utilized in our analyses compares county-level average achievement in a given grade and year to the national average in that grade in the year when a specific cohort was in that grade. Estimated effects are interpreted in standard deviation units.

Figure 1 highlights those states included in the treatment or the control group.

Counties are classified as having a high poverty rate if more than 50% of students are eligible for free or reduced-price lunch.

All models include controls for potentially confounding changes in state policy, i.e. school-entry mandates for the HepA, HepB, VAR and PCV vaccines; middle school mandates for the Tdap booster, MCV, HepB and HPV vaccines; and state decisions to expand Medicaid eligibility.

In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Table A.7).

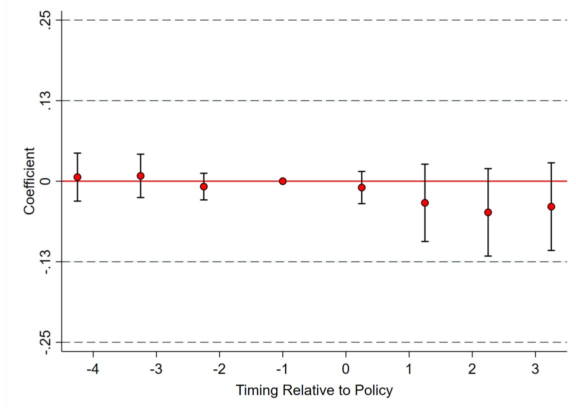
Robust standard errors (reported in parentheses) are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

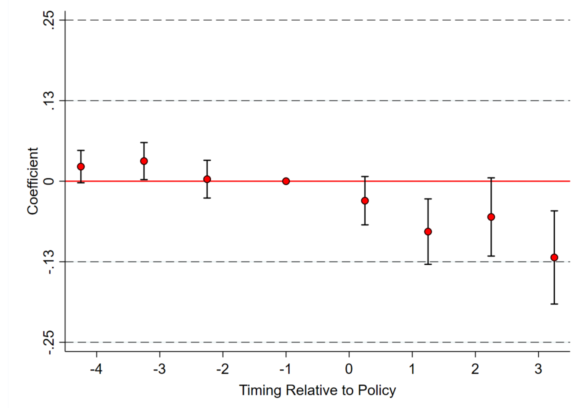
Figure 7: Event Study Estimates of the Effects of PBE on English Language Arts and Mathematics Test Scores by Race and Economic Disadvantage, SEDA (2008-2015)

Panel A: **English Language Arts**

All Enrolled Students

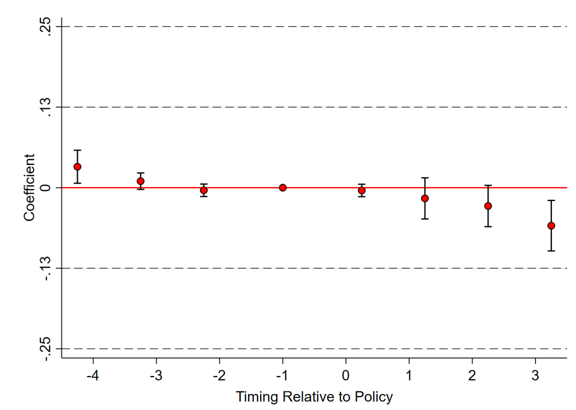


Non-Hispanic Black Students

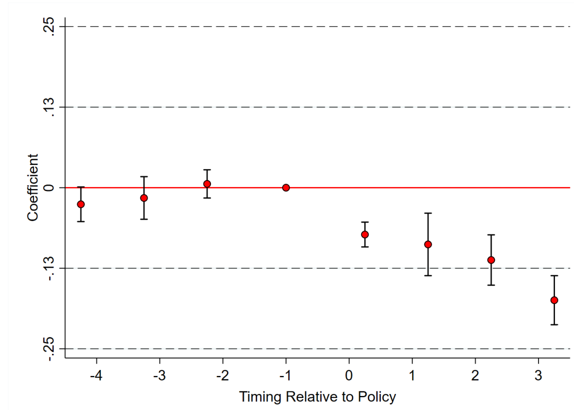


Panel A: **Mathematics**

All Enrolled Students



Non-Hispanic Black Students



Notes: Estimates from an event study specification comparable to Equation 2, where the excluded group is the period before the policy takes effect. 95% confidence intervals reported, where robust standard errors are clustered at the state level. Data from SEDA 2009-2015.

7 Appendix

Table A.1: Laws Establishing Childhood Vaccine Requirements and Permitted Exemptions, 1999-Present

34 States Included in Analysis

	Requirements for Daycare and Pre-Kindergarten Programs								Permitted Exemptions		
State	DTaP	IPV	MMR	Hib	HepB	VAR	HepA	PCV	Medical	Religious	Personal
AL	Y	Y	Y	Y		2000		2009	Y	Y	
AK	Y	Y	Y	2001	2001	2001	2001		Y	Y	
AR	Y	Y	Y	Y	2000	2000	2014	2008	Y	Y	2003
CT	Y	Y	Y	Y	Y	2000	2010	2007	Y	Y	
DE	Y	Y	Y	Y	Y	2002			Y	Y	
DC	Y	Y	Y	Y	Y	Y	2008	2008	Y	Y	
FL	Y	Y	Y	Y	Y	2001		2008	Y	Y	
GA	Y	Y	Y	Y	Y	2000	2007	2007	Y	Y	
HI	Y	Y	Y	Y	Y	2002			Y	Y	
IL	Y	Y	Y	Y	Y	2002		2007	Y	Y	
IN	Y	Y	Y	Y	Y	2003		2003	Y	Y	
IA	Y	Y	Y	Y	Y	2004		2009	Y	Y	
KS	Y	Y	Y	Y	2009	2009	2009	2009	Y	Y	
KY	Y	Y	Y	Y	Y	2001		2011	Y	Y	
MD	Y	Y	Y	Y	Y	2000		2005	Y	Y	
MA	Y	Y	Y	Y	Y	Y			Y	Y	
MS	Y	Y	Y	Y	Y	2002		2007	Y		
MO	Y	Y	Y	Y	Y	2001		2010	Y	Y	
MT	Y	Y	Y	Y	2018	2015		2018	Y	Y	
NE	Y	Y	Y	Y	Y	2004		2008	Y	Y	
NV	Y	Y	Y	Y	2007	2007	2002	2007	Y	Y	
NH	Y	Y	Y	Y	Y	2003			Y	Y	
NJ	Y	Y	Y	Y	2001	2004		2008	Y	Y	
NM	Y	Y	Y	Y	2000	2000	2008		Y	Y	
NY	Y	Y	Y	Y	Y	Y		2008	Y	(2019)	
NC	Y	Y	Y	Y	Y	2002		2015	Y	Y	
RI	Y	Y	Y	Y	Y	Y	2015	2005	Y	Y	
SC	Y	Y	Y	Y	Y	2000		2007	Y	Y	
SD	Y	Y	Y	Y					Y	Y	
TN	Y	Y	Y	Y	Y	Y	2010	2010	Y	Y	
TX	Y	Y	Y	Y	Y	2000	Y	2005	Y	Y	2003
VA	Y	Y	Y	Y	Y	Y		2006	Y	Y	
WV	Y	Y	Y	Y	2000	2000	2006	2001	Y		
WY	Y	Y	Y	Y	Y	2010		2018	Y	Y	

Table A1 summarizes childhood vaccine requirements in the 34 states included in the treatment (red text) or the control group. Year of implementation was obtained from the Immunization Action Coalition (IAC) and, when necessary, review of primary sources. Y indicates a longstanding state law or mandate with an effective date in 1999 or earlier.

Recently enacted legislation removes the religious exemption for public school immunization requirements in New York (2019).

Table A.2: Laws Establishing Childhood Vaccine Requirements and Permitted Exemptions, 1999-Present

17 States Excluded from Analysis

	Requirements for Daycare and Pre-Kindergarten Programs								Permitted Exemptions		
State	DTaP	IPV	MMR	Hib	HepB	VAR	HepA	PCV	Medical	Religious	Personal
AZ	Y	Y	Y	Y	Y	2005	Y		Y	Y	Y
CA	Y	Y	Y	Y	Y	2001			Y	(2016)	(2016)
CO	Y	Y	Y	Y	Y	2000		2007	Y	Y	Y
ID	Y	Y	Y	Y	Y	2011	2011	2011	Y	Y	Y
LA	Y	Y	Y	Y	Y	2003		2006	Y	Y	Y
ME	Y	Y	Y	Y	Y	2002		2002	Y	Y	Y
MI	Y	Y	Y	Y	Y	2000		2007	Y	Y	Y
MN	Y	Y	Y	Y	2000	2004	2014	2004	Y	Y	Y
ND	Y	Y	Y	Y		2004	2008	2008	Y	Y	Y
OH	Y	Y	Y	Y	Y	2015	2015	2015	Y	Y	Y
OK	Y	Y	Y	Y	Y	Y	Y	2007	Y	Y	Y
OR	Y	Y	Y	Y	Y	2000	2008		Y	Y	Y
PA	Y	Y	Y	Y	Y	Y	2006	2001	Y	Y	Y
UT	Y	Y	Y	Y	Y	2002	2008	2008	Y	Y	Y
VT	Y	Y	Y	Y	2011	2011		2011	Y	Y	(2016)
WA	Y	Y	Y	Y	Y	2006		2008	Y	Y	(2019)
WI	Y	Y	Y	Y	Y	2001		2008	Y	Y	Y

Table A2 summarizes childhood vaccine requirements in the 17 states excluded from our analyses. Year of implementation was obtained from the Immunization Action Coalition (IAC) or, when necessary, review of primary sources. Y indicates a longstanding state law or mandate with an effective date in 1999 or earlier.

Several states have recently enacted legislation to remove longstanding provisions for religious and/or personal belief exemptions: California and Vermont (effective 2016), Washington (effective 2019), and Maine (effective 2021).

Table A.3: Laws Establishing Vaccine Requirements for Secondary Schools, 1999-Present

34 States Included in Analysis

State	HepB	Tdap		MCV		HPV	
AL		2010	$Gr6^b$				
AK	2001	2009	Gr 6-9				
AR	2000	2014	$\geq 11yrs^a$	2014	Gr 7		
CO	Y	2007	Gr 6-12				
CT	2000	2011	Gr 7	2011	Gr 7		
DE	Y	2016	Gr 9	2016	Gr 9		
DC	Y	2008	Gr 6-12	2009	Gr 6-12	2009	$Gr6^d$
FL	Y	2009	$Gr7^b$				
GA		2014	$Gr7^c$	2014	$Gr7^c$		
HI	2002						
IL	Y	2012	Gr 6, 9	2015	Gr 6		
IN	2005	2010	Gr 6-12		Gr 6-11		
IA	Y	2013	Gr 7-12	2017	Gr 7-12		
KS	2009	2009	$\geq 12yrs^a$	2019	$Gr7^b$		
KY	2001	2011	Gr 6	2018	$11-15yrs^a$		
MD	2007	2014	$Gr7^b$	2014	$Gr7^b$b		
MA	Y	2011	$Gr7^b$				
MS	Y	2012	Gr 7				
MT		2015	Gr 7-12				
NE	2000	2010	Gr 7				
NV		2008	Gr 7	2017	Gr 7		
NH	Y	2009	$11yrs^a$				
NJ	2004	2008	Gr 6	2008	Gr 6		
NM	Y	2007	$Gr7^b$	2019	Gr 7		
NY	2000	2007	Gr 6-12	2016	$Gr7^b$		
NC	2005	2015	Gr 7	2015	Gr 7		
RI	2000	2009	Gr 7	2009	Gr 7	2015	$Gr7^b$
SC	Y	2013	Gr 7				
SD		2016	Gr 6	2016	$Gr7^c$		
TN	2002	2010	Gr 7				
TX	2000	2009	Gr 7	2009	$Gr7^b$		
VA	2001	2006	Gr 7			2008	$Gr6^d$
WV		2012	Gr 7, 12	2012	Gr 7		
WY	Y	2010	Gr 7				

Table A3 summarizes vaccine requirements for secondary schools in the 34 states included in the treatment (red text) or the control group. Year of implementation was obtained from the Immunization Action Coalition (IAC) or, when necessary, review of primary sources. Y indicates a longstanding state law or mandate with an effective date in 1999 or earlier.

[a] If statutory language establishes mandate by student age (rather than grade level), we assume 11 yrs = Gr 6, 12 yrs = Gr 7, etc. [b] Mandate implemented progressively in successive grades, e.g. Gr 7 in 2010, Gr 7-8 in 2011, and Gr 7-12 in 2016. [c] Mandate also applies to new entrants and students changing school districts. [d] Mandate applies to female students only.

Table A.4: Laws Establishing Vaccine Requirements for Secondary Schools, 1999-Present

17 States Excluded from Analysis

State	HepB	Tdap		MCV		HPV
AZ	2000	2008	$Gr6^b$	2008	$Gr6^b$	
CA	Y	2011	Gr 7			
ID	Y	2011	$Gr7^b$	2011	$Gr7^b$	
LA	2009	2009	$Gr6^b$	2009	Gr 6	
ME		2017	Gr 7	2018	Gr 7	
MI	2002	2010	$Gr7^c$	2010	Gr 7	
MN	2001	2014	$Gr7^b$	2014	$Gr7^b$	
MO	Y	2010	$Gr8^b$	2015	Gr 8	
ND	Y	2014	Gr 7	2018	Gr 7-10	
OH	2006	2012	Gr 7-9	2016	Gr 7	
OK	Y	2011	$Gr7^b$			
OR	2000	2008	$Gr7^b$			
PA	2002	2011	Gr 7	2011	Gr 7	
UT	Y	2007	Gr 7	2015	Gr 7	
VT	Y	2008	Gr 7			
WA	2008	2007	$Gr6^b$			
WI	Y	2008	$Gr6^b$			

Table A4 summarizes vaccine requirements for secondary schools in the 17 states excluded from our analyses. Year of implementation was obtained from the Immunization Action Coalition (IAC) or, when necessary, review of primary sources. Y indicates a longstanding state law or mandate with an effective date in 1999 or earlier.

[a] If statutory language establishes mandate by student age (rather than grade level), we assume 11 yrs = Gr 6, 12 yrs = Gr 7, etc. [b] Mandate implemented progressively in successive grades, e.g. Gr 7 in 2010, Gr 7-8 in 2011, and Gr 7-12 in 2016. [c] Mandate also applies to new entrants and students changing school districts. [d] Mandate applies to female students only.

Table A.5: Descriptive Statistics, National Immunization Survey-Child (NIS-Child) Sample

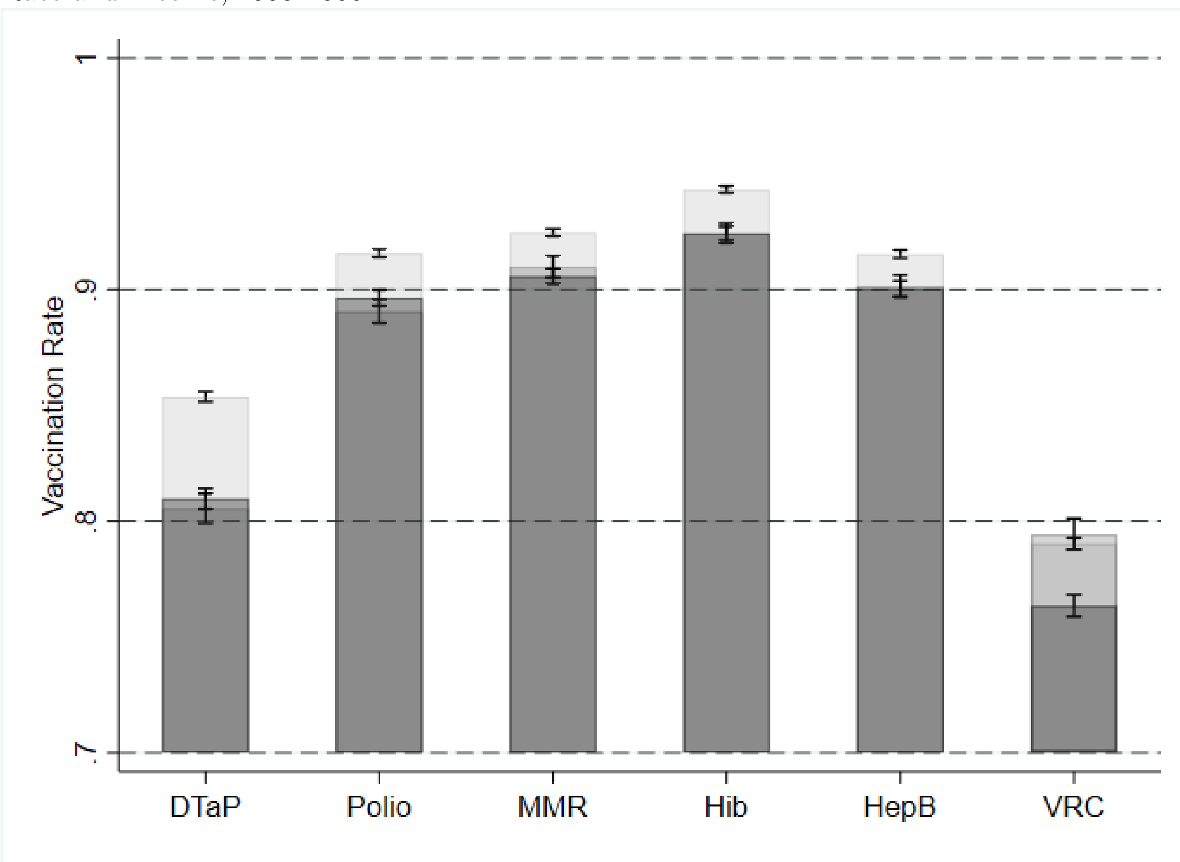
17 States Excluded from Analysis

	Full sample (mean)	Non-Hispanic White (mean)	Non-Hispanic Black (mean)	Hispanic (mean)	Low Income (mean)
Child's Characteristics					
White	0.556	1	0	0	0.331
Black	0.17	0	1	0	0.267
Hispanic	0.208	0	0	1	0.342
Other ethnicity	0.066	0	0	0	0.06
Female	0.488	0.485	0.493	0.487	0.494
Male	0.512	0.515	0.507	0.513	0.506
Age: 19 to 23 months	0.299	0.294	0.303	0.308	0.305
Age: 24 to 29 months	0.344	0.345	0.33	0.347	0.34
Age: 30 to 35 months	0.357	0.361	0.367	0.345	0.355
First-born child	0.411	0.422	0.386	0.388	0.407
Moved from different state	0.096	0.094	0.084	0.102	0.105
Mother's Characteristics					
Age: 30 years or older	0.527	0.59	0.405	0.437	0.326
Less than high school	0.161	0.088	0.155	0.368	0.329
High school	0.345	0.324	0.437	0.342	0.451
Some college	0.176	0.189	0.201	0.132	0.137
College graduate	0.318	0.399	0.206	0.158	0.082
Married	0.703	0.826	0.381	0.624	0.438
Income: $< \$10K$	0.125	0.057	0.26	0.205	0.349
Income: $10-20K	0.163	0.1	0.222	0.287	0.455
Income: $20-25K	0.07	0.056	0.081	0.097	0.196
Income: $25-30K	0.075	0.069	0.082	0.087	0
Income: $30-35K	0.052	0.055	0.046	0.05	0
Income: $35-40K	0.061	0.068	0.053	0.052	0
Income: $40-50K	0.09	0.111	0.063	0.055	0
Income: $50K+	0.364	0.484	0.194	0.167	0
Vaccination Coverage					
Combined 4:3:1:3:3:1 series	0.669	0.676	0.634	0.666	0.629
Dtap: $\geq$ 4 doses	0.843	0.865	0.801	0.818	0.801
IPV: $\geq$ 3 doses	0.91	0.92	0.887	0.902	0.891
MMR: $\geq$ 1 dose	0.921	0.928	0.907	0.913	0.904
Hib: $\geq$ 3 doses	0.939	0.949	0.923	0.929	0.917
HepB: $\geq$ 3 doses	0.914	0.922	0.903	0.905	0.899
VAR: $\geq$ 1 dose	0.794	0.784	0.791	0.809	0.773

Estimates in Table A5 are based on authors' calculations using the 1999-2006 waves of the NIS-Child and the NIS provider-sample weights. Estimates exclude 17 states with existing PBE provisions in the first year of our sample period (Figure 1). Sample includes over 98,000 children with adequate provider data.

Low income assigned based on annual household income below $25,000.

Figure A.1: Vaccination Coverage Among Children Aged 19-35 Months in the United States by Race and Income, 1999-2006



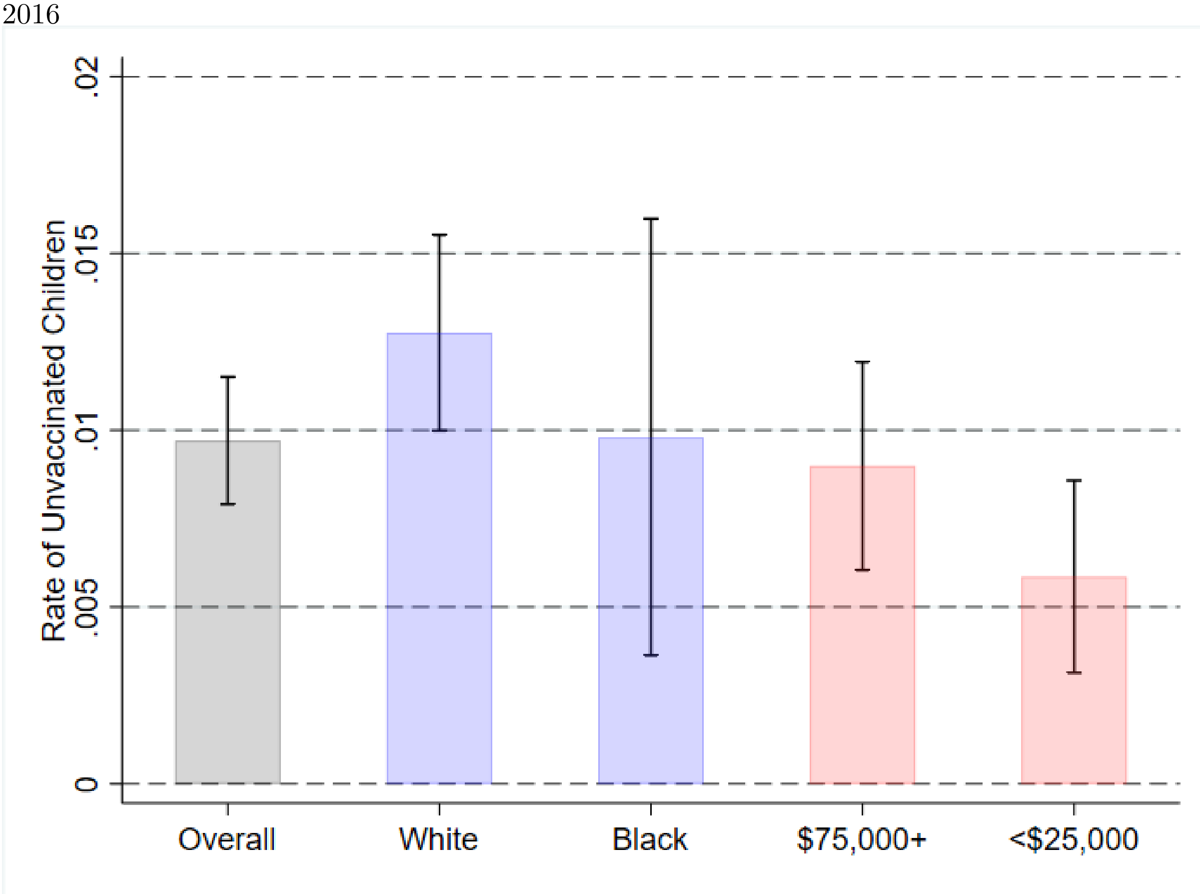
In Figure A1, we present vaccination coverage estimates among children aged 19 to 35 months. The lightest gray shade corresponds to estimates for the full population, the middle gray shade corresponds to estimates for non-Hispanic black children, and the darkest gray shade corresponds to estimates for children from low income households.

The combined 6-vaccine series includes $\geq$ 4 doses of diphtheria, tetanus, and pertussis (DTaP), $\geq$ 3 doses of inactivated poliovirus (IPV), $\geq$ 1 dose of measles, mumps, rubella (MMR), $\geq$ 3 doses of Haemophilus influenzae type b (Hib), $\geq$ 3 doses of hepatitis b (HepB), and $\geq$ 1 dose of varicella (VAR).

Low income assigned based on annual household income below $25,000.

Data are from the 1999-2006 waves of the NIS-Child .

Figure A.2: Unvaccinated Children Aged 19-35 Months in the United States by Race and Income, 2016



In Figure A2, we compare estimated rates of unvaccinated children (children who have yet to receive any vaccines) in various populations.

Data are from the 2016 wave of the NIS-Child.

Table A.6: Descriptive Statistics, Stanford Education Data Archive (SEDA) Sample

	Control	Treatment	All
% Black in grade	0.152	0.107	0.145
	(0.227)	(0.153)	(0.218)
% Hispanic in grade	0.0842	0.344	0.126
	(0.127)	(0.273)	(0.186)
% Asian in grade	0.0123	0.00890	0.0117
	(0.0269)	(0.0161)	(0.0255)
% American Indian in grade	0.0229	0.00490	0.0200
	(0.100)	(0.00715)	(0.0921)
% FRPL	0.554	0.602	0.561
	(0.176)	(0.139)	(0.172)
% Bachelors +	0.199	0.172	0.195
	(0.0929)	(0.0706)	(0.0902)
% SNAP	0.191	0.202	0.193
	(0.102)	(0.0959)	(0.101)
Unemployment Rate	0.0462	0.0382	0.0449
	(0.0194)	(0.0169)	(0.0192)
% < 19 Uninsured	7.656	13.62	8.614
	(3.493)	(5.410)	(4.443)

Notes: Means reported with standard deviations in parentheses. All time-varying countyl-level controls from the SEDA sample included in this table. The first four variables vary at the county-level but look only at kids in the same grades as our sample (5, 6, and 7). Treatment states include Arkansas and Texas. Control states depicted in the map in Figure 1.

Table A.7: Difference-in-Differences Estimates of the Effects of PBE on Early Childhood Vaccination Rates by Vaccine, Race, and Income, NIS-Child (1999-2006)

	(1) $\geq$ 4 doses Dtap	(2) $\geq$ 3 doses IPV	(3) $\geq$1 dose MMR	(4) $\geq$ 3 doses Hib	(5) $\geq$ 3 doses HepB	(6) $\geq$ 1 dose VAR
A. Population						
PBE	0.021***	0.019***	0.010***	0.016***	0.022***	-0.044***
	(0.006)	(0.004)	(0.003)	(0.003)	(0.007)	(0.013)
B. Non-Hispanic Black						
PBE	-0.060***	-0.021***	-0.024***	-0.024***	-0.013	-0.085***
	(0.018)	(0.007)	(0.008)	(0.006)	(0.018)	(0.018)
C. Low Income						
PBE	-0.000	0.011***	-0.006	0.010**	0.009	-0.053***
	(0.008)	(0.003)	(0.005)	(0.004)	(0.010)	(0.013)

In Table A7, we present estimates from the standard DD model in Equation 1. Each row corresponds to a separate regression model. In separate specifications, the baseline model (Panel A) is augmented a policy interaction term to test for heterogenous effects by race (Panel B) or economic disadvantage (Panel C). Marginal effects presented in Panels B and C account for the interaction term.

The combined 6-vaccine series includes $\geq$ 4 doses of diphtheria, tetanus, and pertussis (DTaP), $\geq$ 3 doses of inactivated poliovirus (IPV), $\geq$ 1 dose of measles, mumps, rubella (MMR), $\geq$ 3 doses of *Haemophilus influenzae* type b (Hib), $\geq$ 3 doses of hepatitis b (HepB), and $\geq$ 1 dose of varicella (VAR).

Figure 1 highlights those states included in the treatment or the control group.

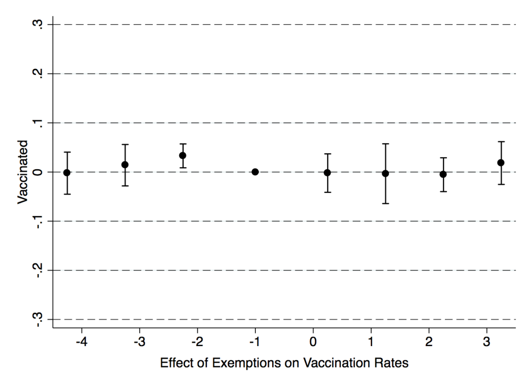
Low income assigned based on annual household income below $25,000.

All models include controls for potentially confounding changes in state vaccine policy, i.e. state implementation of a new school-entry mandate for HepB, VAR, PCV, and/or HepA. Robust standard errors (reported in parentheses) are clustered at the state level.

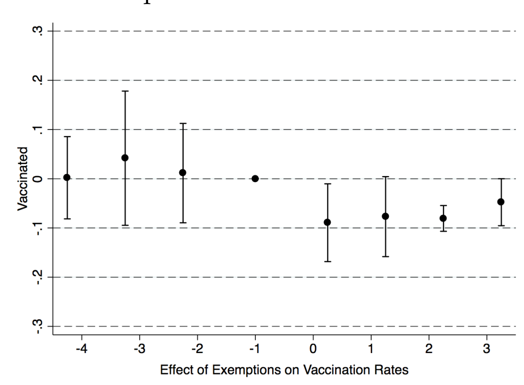
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.3: Event Study Estimates of the Effects of PBE on Early Childhood Vaccination Rates by Race and Income, NIS-Child (1999-2006)

A. Population



B. Non-Hispanic Black



C. Low Income

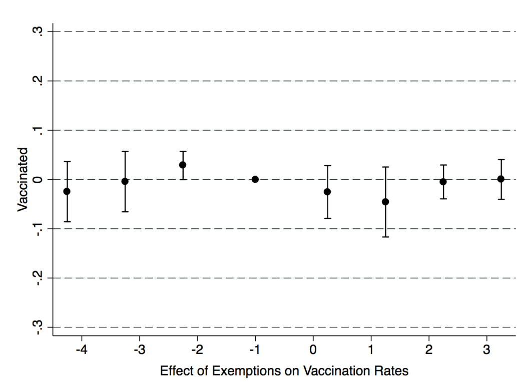


Figure A3 presents estimates from a regression-based event study analysis. Vertical bars represent the 95 percent confidence interval around each point estimate. The year preceding policy implementation (event time -1) is the omitted category. The outcome of interest is whether a child received all doses of the combined 4:3:1:3:3:1 vaccine series. Low income assigned based on annual household income below $25,000. All models include controls for potentially confounding changes in state vaccine policy, i.e. state implementation of a new school-entry mandate for HepB, VAR, PCV, and/or HepA.

Models also include state-specific linear cohort trends (where we interact each state fixed effect with a variable TREND that equals 1 for cohort C0 children captured in the 1999 NIS-Child, 2 for cohort C1 children captured in 2000 NIS-Child, and so forth).

Table A.7: Difference-in-Differences Estimates of the Effects of PBE on English Language Arts and Mathematics Test Scores by Race and Economic Disadvantage, SEDA (2008-2015)

Panel A: **ELA**	All Enrolled Students		Non-Hispanic Black Students	
	(1)	(2)	(3)	(4)
PBE	-0.014	0.024**	-0.108***	-0.057***
	(0.011)	(0.010)	(0.012)	(0.011)
PBE x High Poverty		-0.048***		-0.064***
		(0.002)		(0.003)
Observations	11,044	11,044	11,044	11,044

Panel A: **Math**	All Enrolled Students		Non-Hispanic Black Students	
	(1)	(2)	(3)	(4)
PBE	-0.050**	-0.018	-0.100***	-0.055***
	(0.022)	(0.022)	(0.017)	(0.016)
PBE x High Poverty		-0.039***		-0.055***
		(0.003)		(0.010)
Observations	10,171	10,171	10,171	10,171

In Table A8, we present estimates from the standard DD model in Equation 2 that also include state-specific linear cohort trends (where we interact each state fixed effect with a variable TREND that equals 1 for cohort C0 children captured in the 1999 NIS-Child, 2 for cohort C1 children captured in 2000 NIS-Child, and so forth).

The SEDA includes several measures of academic achievement for counties. The cohort standardized (CS) scale utilized in our analyses compares county-level average achievement in a given grade and year to the national average in that grade in the year when a specific cohort was in that grade. Estimated effects are interpreted in standard deviation units.

Figure 1 highlights those states included in the treatment or the control group.

Counties are classified as having a high poverty rate if more than 50% of students are eligible for free or reduced-price lunch.

All models include controls for potentially confounding changes in state policy, i.e. school-entry mandates for the HepA, HepB, VAR and PCV vaccines; middle school mandates for the Tdap booster, MCV, HepB and HPV vaccines; and state decisions to expand Medicaid eligibility.

Robust standard errors (reported in parentheses) are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$