Journal of Health Economics 78 (2021) 102464

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/econbase

Personal belief exemptions for school-entry vaccinations, vaccination rates, and academic achievement☆

Nicole L. Hair[a,*], Anja Gruber[b], Carly Urban[c,d]

[a] *Department of Health Services Policy and Management, Arnold School of Public Health, University of South Carolina, United States*
[b] *University of Colorado at Boulder, United States*
[c] *Montana State University, United States*
[d] *Institute for Labor Studies (IZA), Germany*

ARTICLE INFO

Article history:
Received 7 February 2020
Received in revised form 7 April 2021
Accepted 9 April 2021
Available online 24 April 2021

JEL classification:
H75
I12
I18
I21
I24
I28

Keywords:
Vaccination
State mandates
Nonmedical exemptions
Test scores

ABSTRACT

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 that 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 test scores were largest for Black students and economically disadvantaged students.

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.

☆ This work has been supported (in part) by Grant #1808-07191 from the Russell Sage Foundation and the William T. Grant Foundation. Any opinions expressed are those of the principal investigator alone and should not be construed as representing the opinions of either foundation. The research reported in this article was made possible (in part) by a grant from the Spencer Foundation (Grant #20190039). The views expressed are those of the authors and do not necessarily reflect the views of the Spencer Foundation. We thank Mark Anderson, Chris Stoddard, Nathan Tefft, and seminar participants at the 2019 Meeting on Improving Education and Reducing Inequality in the United States and the 2019 APPAM Fall Research Conference for helpful comments.

* Corresponding author.
E-mail addresses: hairnl@mailbox.sc.edu (N.L. Hair), carly.urban@montana.edu (C. Urban).

https://doi.org/10.1016/j.jhealeco.2021.102464

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 diseases. 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 of 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]

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 et al., 2013; Rota et al., 2001b).

We also find that these new state policies granting PBE had long-lasting effects on students' performance, particularly in mathematics. The evidence is consistent with the first stage of our analysis. Due to the 2003 policy changes, non-Hispanic Black middle school students and economically disadvantaged students 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. We find suggestive evidence for the ways in which the negative effect of PBEs on test scores may operate. After the PBE, children are less likely to go to well-child visits at young ages. This could leave early health issues undiagnosed for extended periods. In addition, we find evidence for an increase in chronic absenteeism—particularly for low-income children—in early grades. This absenteeism could have disruptive consequences for unvaccinated children, as well as their peers.

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.[4] 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 and adolescents but reduce the population-level burden of targeted vaccine-preventable disease (Lawler, 2017; Abrevaya and Mulligan, 2011;

[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).

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

Carpenter and Lawler, 2019). Comparatively little is known about the role of state regulations governing exemptions for mandated vaccines. Richwine et al. (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 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 et al., 2013; Chay et al., 2009), public health insurance (Miller and Wherry, 2019; Cohodes et al., 2016; Brown et al., 2019) the Food Stamps Program (Hoynes et al., 2016), and the 1970 Clean Air Act Amendment (Isen et al., 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 (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 provide background information on relevant immunization policies and a brief discussion of plausible mechanisms through which state policies granting PBE could affect academic achievement. In Section 3, we describe the data we use in our main analyses. In Section 4, we outline our empirical strategy. In Section 5, we detail our results, and in Section 6 we present our conclusions.

2. Background

2.1. Changes to nonmedical exemption policies in Texas and Arkansas

Our analysis focuses on legislative changes in Texas and Arkansas. Historically, a child could be exempted from school-entry vaccine mandates in these two states provided a parent or guardian established either (1) immunization would be detrimental to the health and well-being of the child or (2) immunization would conflict with "the tenets and practices of a recognized church or religious denomination of which [they are] an adherent or member."[5,6] In Arkansas, the determination of whether to officially recognize a church or religious denomination as a reason for claiming an exemption was based on consideration of several factors, including the permanent address, membership, meeting practices, governing documents, written doctrine, and governmental filing of the applicant's church.[7] Rules in Texas were similarly restrictive: the religious exemption applied only to those children who were members of certain recognized religions, e.g. Christian Science or Jehovah's Witnesses, that opposed vaccination (House Research Organization. Texas House of Representatives, 2003).[8]

State laws that define religious exemptions narrowly, like those that existed in Texas and Arkansas prior to 2003, "run the risk of engaging in a constitutionally impermissible preference toward certain religions" (Salmon et al., 2005). Following two challenges in federal courts, legislators in Texas were forced to rewrite the state's nonmedical exemption policy. The policy in Arkansas was amended preemptively. Both states started granting philosophical exemptions in 2003. A child in Arkansas can be exempted from school-entry vaccine mandates as long as a parent or guardian completes an education component, signs an informed consent form, and provides a notarized statement requesting an exemption (Ark.Code Ann. §6-18-702(d)(2) (2003)). In Texas, a parent or guardian must obtain an affidavit form from the Department of Health (via online form, mail, fax or hand-delivery) before submitting a notarized statement of the reason for exemption (Texas Education Code §38.001 (2003)). Applications are rarely denied.[9]

[5] See Ark.Code Ann. §6-18-702(d)(2) (1999) and Texas Education Code §38.001 (1995).

[6] Relative to other states, the process of securing a nonmedical exemption in Arkansas or Texas during the late 1990s was complex (Rota et al., 2001b). For example, in Arkansas, a parent or guardian seeking a religious exemption was required to submit a form to the Arkansas Department of Health along with "a written statement of the church or denomination specifying that immunization conflicts with religious tenets and practices; and a notarized statement from a church or denomination official reflecting that the applicant is currently a church member in good standing" according to McCarthy v Boozman, 212 F Supp 2d 945 (WD Ark 2002).

[7] See McCarthy v Boozman, 212 F Supp 2d 945 (WD Ark 2002).

[8] It is possible that a parent or guardian strongly opposed to immunization may feel compelled to commit perjury and submit a signed affidavit where they falsely claim adherence to recognized religious beliefs in order to secure an exemption for their child. However, previous research documenting higher rates of nonmedical exemptions in states that grant PBE relative to states that permit religious exemptions only suggests that many parents refrain from claiming a religious exemption when their objections to immunization are not religious (Omer et al., 2012; Blank et al., 2013). In both Texas and Arkansas, exemptions increased steadily following the adoption of a statewide PBE provision (Safi et al., 2012; Olive and Matthews, 2016).

[9] This statement is based on phone conversations with staff in the Arkansas Department of Health and the Texas Department of State Health Services in September 2020.

2.2. From exemptions to academic achievement

In deciding whether or not to vaccinate one's child, parents or caregivers must weigh the costs and benefits. While vaccines decrease the incidence of disease and benefit peers, once herd immunity is reached for a given disease, the likelihood of contracting the disease is low. Thus, parents who are time-constrained or who are uninsured may face barriers to vaccinating their children. PBE policies allow parents to opt-out of vaccines at a relatively low cost. For example, in Arkansas, parents must fill out a form and get that form notarized in order to obtain an personal belief exemption. In Texas, parents have to first request a form via their website and then fill out that form. In both states, applications are rarely denied.[10] On the contrary, staff from Arkansas' Department of Health mentioned that 50% of medical exemptions were rejected.

Do PBEs increase the number of exemptions granted? We obtained data on the number of exemptions granted by school year in Arkansas and Texas directly from the state health departments. However, only Arkansas collected exemption data prior to allowing PBEs. Texas started to collect data on nonmedical exemptions in school year 2003–2004. Consequently, we cannot determine if PBEs increased the number of exemptions granted in Texas.[11] Fig. 3 shows the total number of nonmedical exemptions awarded per 1000 kindergarten children in Arkansas and Florida, based on data reported from the states to the CDC.[12] The graph shows that after PBEs are permitted in Arkansas, additional kindergarteners receive exemptions and this increases through 2007. At the same time, religious exemptions become less common. However, the increase in PBEs outpaces the decrease in religious exemptions. In Florida, religious exemptions remain relatively flat from 2001 to 2007. Our findings are consistent with evidence from Omer et al. (2006) who show that state PBE policies, as opposed to just religious exemptions, increase exemptions.

Since the PBE policy increases exemptions, in what ways may these exemptions affect downstream academic performance? We hypothesize three different mechanisms. First, exempting from vaccines could impair the health of the child and could then reduce instructional time and impair academic success. Second, the PBEs remove a requirement to see a health care provider at critical developmental ages. This could reduce the likelihood of having early diagnoses that are important to healthy development. Third, it could be that the first two individual effects are negative externalities for the remaining cohort. By having peers that are more frequently absent or disruptive in class, it is possible that they deter from the education of the whole class.

3. 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 and A.2) and middle school (Appendix Tables A.3 and 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. Fig. 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.[13]

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

[10] This statement is based on phone conversations with the staff in September 2020.

[11] We obtained annual data on the number of exemptions granted at the state-level from the CDC for school years 1992–1993 to 2018–2019 and additional data directly from states where available. The CDC did not collect data for the 2010–2011 school year. Further, the sample design varies by state: some states report exemptions from a census of all schools and some from a random sample of schools. For some states, counts may include temporary as well as permanent exemptions. Further, counts of different exemptions are not necessarily mutually exclusive. Counts are missing throughout the report for most states, especially for years prior to 2003. Where available, we compared the data obtained from the CDC with the data obtained from states directly and found large discrepancies. Consequently, we are not able to draw any reliable conclusions regarding the changes in state-level exemptions over time.

[12] We reached out to all of our potential control states, but Florida was the only state collecting continuous data over the sample period in a similar format to Arkansas and with data from the CDC that matched the state Department of Health's own data.

[13] 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 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 (Fig. 7) indicate a negative and growing effect of PBE on minority children's test scores.

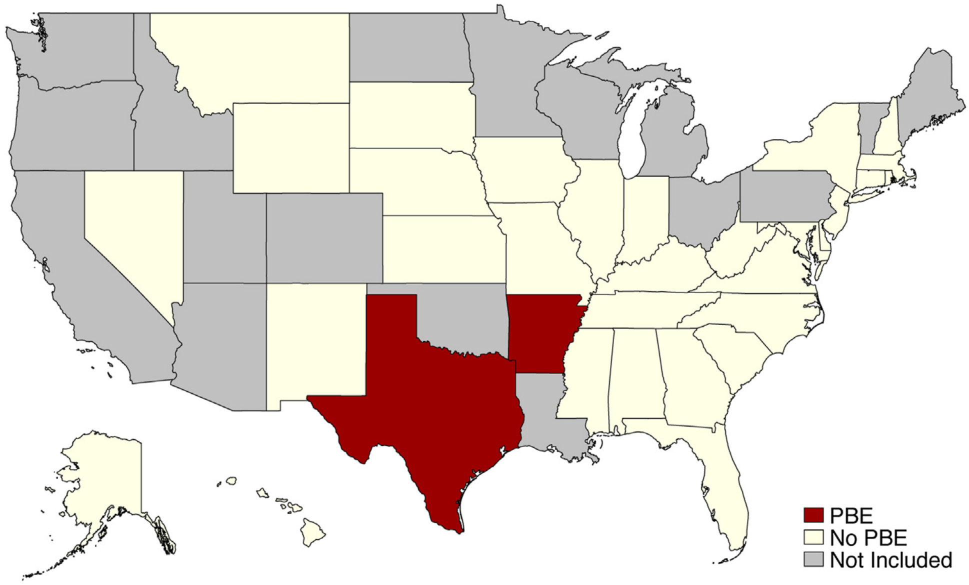


Fig. 1. State laws permitting PBE for school-entry vaccination requirements, 1999–2015. This figure highlights those states included in the treatment or the control group. Our analysis focuses on the introduction of PBE in two states—Texas and Arkansas—that previously allowed exemptions only for medical 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.

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% 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 influenza* type b (Hib), 3 doses of hepatitis B (HepB), and 1 dose of varicella (VAR).[14] 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 Fig. 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 (Fig. 4). 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 the immunization schedule recommended by the CDC (Appendix Fig. 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 Fig. A.2). Fewer than 1% of children in the United States were unimmunized in 2016. Thus, mandates to complete required vaccinations prior to school-entry may serve as a safety net for low-income families and non-Hispanic Black families.

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 2008/2009 through SY 2014/2015 available through the SEDA. Since tests are administered in the Spring term, we will refer to our sample as spanning 2009–2015 for the remainder of the paper. 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

[14] 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 prior to 2006.

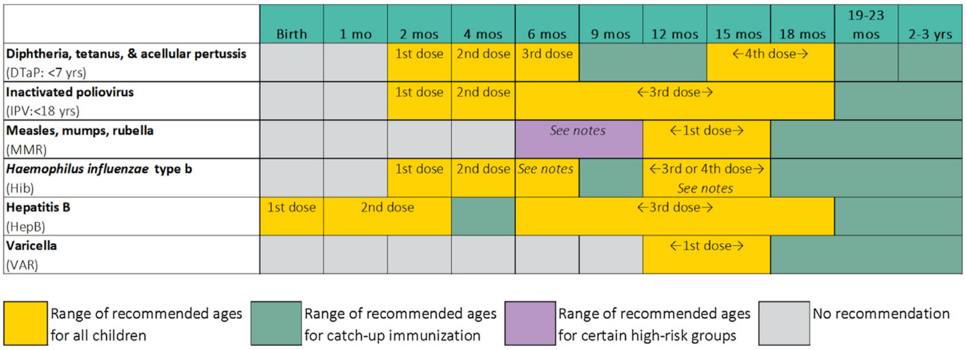


Fig. 2. Recommended childhood immunization schedule, birth to 3 years. This figures is adapted from the Recommended Childhood Immunization Schedule – United States, 1999 published by the CDC's Advisory Committee on Immunization Practices. The guidelines for *Haemophilus influenzae* type b (Hib) depend 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 ≥ 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 ≥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). We also consider immunization status for each of the six component vaccines individually.

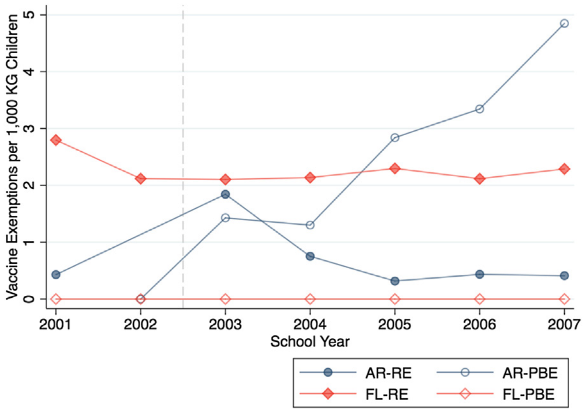


Fig. 3. Nonmedical exemptions by type. This figure shows exemptions over time. Exemption rates were calculated from counts of non-medical exemptions and total populations of kindergarteners by school year in Florida and Arkansas as requested and provided directly from the CDC. The graph compares exemptions in kindergarten in AR and FL, where AR began allowing PBEs in 2003 and FL never allowed them. PBEs represent personal belief exemptions, and REs represent religious exemptions. AR's data are missing in 2002. We assume that PBEs remained at zero in 2002 in AR since the policy was not in effect until 2003.

in grades 3–8 attending public and charter schools between 2009 and 2015 (Reardon et al., 2017a). 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 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 data include 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 et al., 2017b). Estimated effects are interpreted in standard deviation units. In addition to county-level achievement measures disaggregated by grade, school year, subject for all students, the SEDA data include achievement measures by race/ethnicity and for economically disadvantaged students. We will use three different outcome measures in our analysis, all at the county-grade-year-subject level: achievement among all enrolled students, achievement among non-Hispanic Black students, and achievement among economically disadvantaged students, though it should be noted that these achievement measures are not reported in areas with too few observations to generate the scaled standard deviations (Fahle et al., 2019).[15] We further observe the racial and socioeconomic composition of school districts, 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

[15] The definition of economic disadvantage varies by state, but it is highly correlated with free and reduced-price lunch status Fahle et al. (2019).

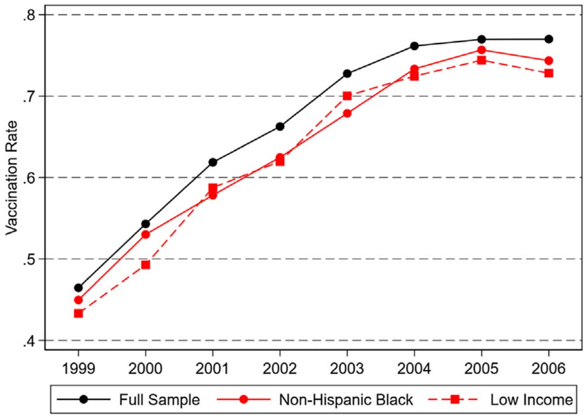


Fig. 4. Combined 6-vaccine series coverage among children aged 19–35 months in the United States by race and income, 1999–2006. In this figure, we present trends in vaccination coverage among children aged 19–35 months. While the proportion of children who were fully immunized increased significantly between 1999 and 2006, racial and economic disparities in vaccination rates persisted over the period. The combined 6-vaccine series includes ≥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). Low income assigned based on annual household income below $25,000. Vaccination rates for each of the six component vaccines are presented in Fig. A.1. Data are from the 1999–2006 waves of the NIS-Child.

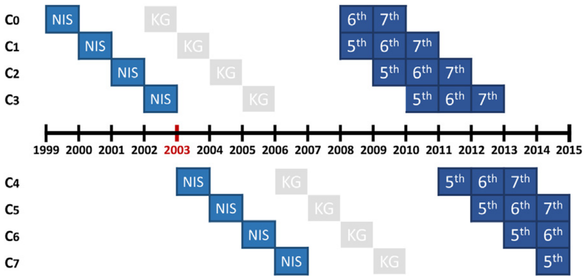


Fig. 5. Birth cohorts represented in the NIS-Child and the SEDA samples. This figure shows the birth cohorts represented in both the NIS-Child and the SEDA analysis samples. If children typically enter kindergarten at age 5, children in the 1997 birth cohort (C0) are captured in the 1999 NIS-Child, enter kindergarten in SY 2002/2003, and are first observed in SEDA data as sixth graders in SY 2008/2009. Likewise, children in the 2004 birth cohort (C7), are captured in the 2006 NIS-Child, enter kindergarten in SY 2009/2010 and are first observed in SEDA data as fifth graders in SY 2013/2014. 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.

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. Fig. 5 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 2002/2003, 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 2009/2010 and are first observed in SEDA data as fifth graders in SY 2013/2014. 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.

In order to better understand the mechanisms that may link exemptions from school-entry vaccine requirements to children's academic achievement, we supplemented our main data sources with data on preventive health care utilization and missed school days from the National Survey of Children's Health (NSCH) and disease incidence from the CDC's National Notifiable Disease Surveillance System (NNDSS).[16]

The NSCH is state and nationally representative survey conducted by the CDC on behalf of the Maternal and Child Health Bureau to track a variety of health indicators for children and youth ages 0–17 years. The telephone survey collects information on a broad range of topics, including child and family demographics, access and use of health care services, early childhood-specific information (0–5 years), and middle childhood and adolescent-specific information (6–17 years). The NSCH was administered three times between 2003 and 2012: 2003, 2007, and 2011/2012. First, we used information on

[16] Additional details regarding our supplemental analyses can be found in Section 5.3.

the number of well-child check-ups in the past 12 months to test whether policies allowing PBE for school-entry vaccine mandates decreased utilization of preventive pediatric health care. For this analysis, we focused on children aged 3–5 years in the 2003 NSCH. Next, we used information on school days missed to check whether PBE were associated with more disruptions in instructional time. We defined chronic absenteeism as missing at least 18 days (10% of school days) in a school year. For this analysis, we focused on children aged 7–10 years in the 2003, 2007, and 2011/2012 NSCH.

The NNDSS gathers information on specified infectious and noninfectious conditions from local, state, and territorial public health departments. The CDC publishes statistical data based on the NNDSS to support efforts to monitor shifts in disease patterns, identify outbreaks, and evaluate disease control activities. We obtained counts of pertussis cases by state, year (2000–2015), and age group (0–4 years and 5–9 years). For this analysis, we focused on morbidity data for pertussis, or "whooping cough." We choose pertussis because (1) the DTaP vaccine helps children develop immunity to this disease, (2) it is highly contagious and spreads easily from person to person, (3) it was defined as a nationally notifiable condition prior to 2003 (varicella, another relevant condition, was not monitored until 2003), and (4) it is a common disease in the United States (other relevant conditions, including polio and measles, had few or no reported cases among young children in some years).

4. 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 Fig. 5) who would have been subject to the new PBE provision during the preschool years versus children born 1997–2000 (cohorts C0–C3 in Fig. 5) 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.

4.1. 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 et al., 2004).

In separate specifications, the model in Eq. (1) is augmented with state-specific linear trends (where we interact each state fixed effect with a variable *TREND* that equals 1 for the first year of the NIS-Child (1999), 2 for second year (2000), 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 Eq. (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 Eq. (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 2009–2015 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:

$$A_{isct} = \beta_0 + \beta_1 PBE_{sc} + \beta_2 X_{ict} + \beta_3 Z_{it} + \beta_4 M_{sc} + \beta_5 C_c + \beta_6 I_i + \beta_7 T_t + \epsilon_{isct} \tag{2}$$

where A_{isct} is the educational outcome (average math or ELA test score for a specified group on a cohort standardized scale) in county i and state s, for cohort c, in year t. Fig. 5 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 Fig. 5). 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), county fixed effects (I_i), 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 Z_{it} 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), and hepatitis B (HepB) vaccines; and state decisions to expand Medicaid eligibility.[17] Standard errors are clustered at the state level.

Using the model specified in Eq. (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 all enrolled students). We next estimate the effect of these provisions on the educational outcomes of non-Hispanic Black students and subsequently on students with economic disadvantage. In separate specifications, the model in Eq. (2) is augmented with state-specific linear time trends.

We supplement the standard DD model in Eq. (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.

5. Results

5.1. 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 Eq. (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 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. The magnitudes of the effects of the PBE on vaccination rates are smaller than recent work documenting the effects of vaccination recommendations and requirements on vaccination rates. For example, Lawler (2017) finds the HepA recommendation increased vaccination rates by 20 percentage points and the HepA requirement increased vaccination rates by an additional 8 percentage points. She also finds that policy effects of recommendations are smallest for white children and those with parents from the middle of the income distribution. Carpenter and Lawler (2019) find that mandating middle school Tdap vaccination increases Tdap immunization rates by 13.4 percentage points.

[17] 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, MCV, and HepB vaccines).

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	−0.011
	(0.013)	(0.013)	(0.012)	(0.024)	(0.024)	(0.021)
PBE × Black		−0.077***			−0.076***	
		(0.011)			(0.010)	
PBE × low income			−0.035***			−0.034***
			(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 this table, we present estimates from the standard DD model in Eq. (1).
The combined 6-vaccine series includes ≥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).
Fig. 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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% decrease in the proportion of Black preschoolers and an 8.3% decrease in 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.[18]

In Fig. 6, 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 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 three 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.

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), 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.[19] The combination of a flat pre-treatment period paired with clear post-treatment changes in Fig. 6 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

[18] In a series of robustness checks, we verify that our main results are unaffected when we include group-specific trends (Appendix Table A.7), exclude Arkansas (Appendix Table A.8), or omit observations from cohorts straddling the treatment date (Appendix Table A.9).
[19] We show that our results are robust to the inclusion of state-specific linear trends in Appendix Fig. A.3.

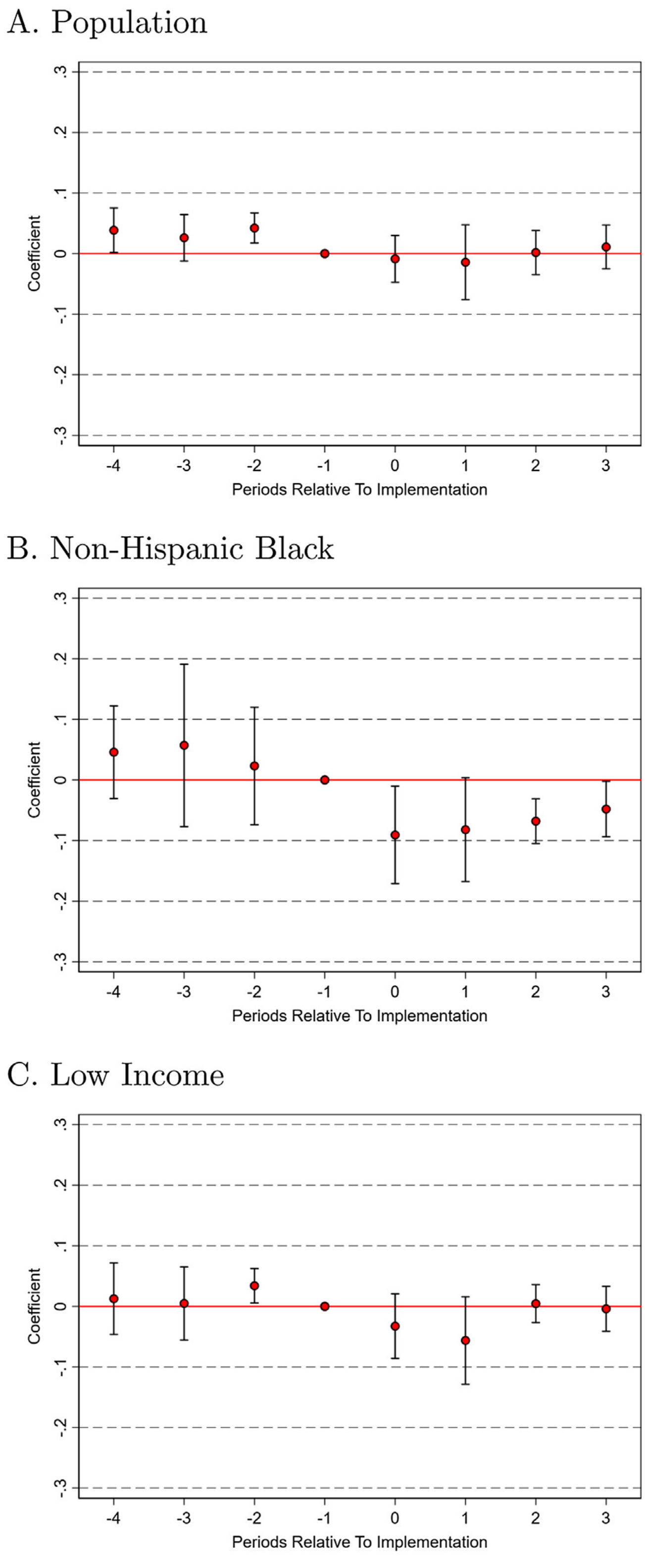


Fig. 6. Event study estimates of the effects of PBE on early childhood vaccination rates by race and income, NIS-Child (1999–2006). This figure presents estimates from a regression-based event study analysis. Vertical bars represent the 95% confidence interval around each point estimate. The year preceding 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 Fig. A.3).

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 Fig. A.4, 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. However, we cannot reject that the effect among low income children is zero.

We next consider parental decisions underlying the diminished likelihood of adherence to the ACIP-recommended vaccine schedule (i.e., completion of the combined six-vaccine series). 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 immunizations. Parents may choose to immunize their child against some diseases but not others. In Appendix Table A.10, we consider a child's immunization status for each of the six component vaccines, individually. The effects of a PBE provision on vaccination coverage are likely driven by reductions in the proportion of children immunized against varicella; however, among non-Hispanic Black children, we do find statistically significant reductions in the likelihood of completion for each of the six component vaccines.[20] We note that the overall effects in Table A.10 are oddly positive for all vaccines except for varicella in our baseline specification, but those results are not robust to the inclusion of state-specific trends. Further, the coefficients on the interaction terms (Black and low income) remain negative across nearly every vaccine regardless of specification. We provide event studies of each vaccine for the overall population, as well as among the non-Hispanic Black population and the low-income sample in Figs. A.5, A.6, and A.7, respectively.

In Appendix Table A.11, we present results from models where the outcome of interest is the number of component vaccines initiated. The introduction of a PBE provision was associated with a small reduction in the number of vaccines initiated. 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.[21] 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.[22]

Since we observe children at age two and not at age six—the age of kindergarteners—it could be that children are not vaccinated by age two but have caught up by age six. While not perfect, we conduct an analysis using the NIS-Teen data to understand if this catch up is happening. The NIS-Teen data have smaller samples for any given age, do not include information on all vaccinations included in the 4:3:1:3:3:1 series, and do not have power to split samples by race or income. However, we find that for all vaccines available in the NIS-Teen (HepB, measles, and varicella), our effects, presented in Table A.12 are the same in magnitude as in Table A.10. It does not seem that catch-up is happening, particularly for varicella.

5.2. PBE and achievement

In Table 2, 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).[23] Each column in the table corresponds to a separate regression. Column 1 presents results from the baseline model specified in Eq. (2). In column 2, we confine our attention to the average achievement of non-Hispanic Black students. Column 3 limits the sample to those students from economically disadvantaged backgrounds.[24]

The results in Table 2 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 following adoption of a statewide PBE provision, though the effects are not statistically different from zero. Column 2 reports a 0.06 standard deviation reduction in mathematics achievement for non-Hispanic Black students and a negative but not statistically significant effect on

[20] Since varicella is highly contagious, this could certainly affect school attendance. In addition, among non-Hispanic Black children, we see a reduction in DTaP vaccination, which protects from pertussis commonly referred to as "whooping cough." Carpenter and Lawler (2019) find that mandating Tdap vaccination—the middle school equivalent of DTaP—increases Tdap vaccination coverage by 29% and reduced whooping cough incidence by 53% among the full population. This suggests our DTaP findings suggesting lower rates of immunization may be harmful to those not vaccinated, as well as the full population.

[21] 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 et al., 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.

[22] 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., 2001b). 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 et al., 2013).

[23] The SEDA data include 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.

[24] Our conclusions are the same sign and similar in magnitude but have larger standard errors when adding state-specific trends (Appendix Table A.13).

Table 2
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, SEDA (2009–2015).

	All enrolled (1)	Non-Hispanic Black (2)	Low income (3)
Panel A: ELA			
PBE	−0.008	−0.038	−0.027**
	(0.012)	(0.024)	(0.012)
Observations	42,518	19,214	39,738
Panel A: Math			
PBE	−0.027	−0.064***	−0.037*
	(0.017)	(0.019)	(0.019)
Observations	38,392	16,968	36,206

In this table, we present estimates from the standard DD model in Eq. (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. The outcomes are the CS scale for all enrolled students in the county (column 1), for only non-Hispanic Black students within the county (column 2), or only economically disadvantaged students within the county (column 3) for a given grade and year. The latter two are only defined when there are enough students in that group to report and calculate a CS scale. Low income is based on state classification of economic disadvantage for students, which is very highly correlated with free and reduced-price lunch status.
Fig. 1 highlights those states included in the treatment or the control group.
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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Table A.13).
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

English/Language Arts. For economically disadvantaged students (column 3), we see a 0.03 standard deviation reduction in English/Language Arts scores and a 0.04 standard deviation reduction in mathematics scores. These findings are consistent with our first stage results: the students most likely to see reductions in vaccinations due to the exemption policy also see reductions in test scores during middle school.

Because there are fewer counties for which SEDA can report achievement measures for non-Hispanic Black and economically disadvantaged children, our samples are smaller in columns 2 and 3 of Table 2. We restrict the sample of the all enrolled students specification in Table A.14 to those counties where we have academic achievement measures for non-Hispanic and economically disadvantaged students, respectively. We replicate the first column, the overall sample, from Table 2 for comparison. Appendix Table A.14 shows that the sample does not change the overall effect of the PBE on the all student measure with one exception: the PBE reduces overall achievement in mathematics by 0.04 standard deviations.[25] We interpret this result as follows: students in schools with relatively more non-Hispanic Black students (enough such that the achievement measures are reportable) have lower math scores due to the PBE policy. This result may suggest that the reduction in math test scores for non-Hispanic Black students may result in students of other races and ethnicities being held back due to their peers' slower progress.

When we restrict the sample to students in Grade 5 (Appendix Table A.15), Grade 6 (Appendix Table A.16), or Grade 7 (Appendix Table A.17), the observed patterns suggest that the effects for math grow by grade, with the largest effects in Grade 7. In Appendix A we show that our results are robust to excluding Arkansas (Appendix Table A.18) and omitting observations from cohorts straddling the treatment date (Appendix Table A.19). We further provide permutation tests, as in the vaccination results, in Appendix Fig. A.8. None of the permutation tests rejects the null hypothesis that the coefficients are zero. However, in all cases where our results are statistically different from zero, the bulk of the distribution of placebo estimates falls to the right of the estimate for AR and TX.

To provide context for our findings, we compare them to another health intervention, where Bharadwaj et al. (2013) show that additional medical care at birth improved test scores by 0.15 standard deviations in eighth grade in Chile and 0.22 standard deviations in tenth grade in Norway. Our reductions are smaller than the medical care at birth intervention, where our test scores are measured slightly earlier in grades 5–7. Even our highest estimates for math in grade 7 are smaller in absolute value: 0.10 standard deviations for non-Hispanic Black students and 0.09 standard deviations for economically disadvantaged students.

In Fig. 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 Fig. 5). The outcome of interest is a county-level average test score in English Language Arts or mathematics. While we observe a modest drop in overall achievement following the policy change, point estimates (excluding

[25] When we probed this further, we saw that this overall reduction does not solely come from non-Hispanic Black students' scores but also from reductions in white students' test scores.

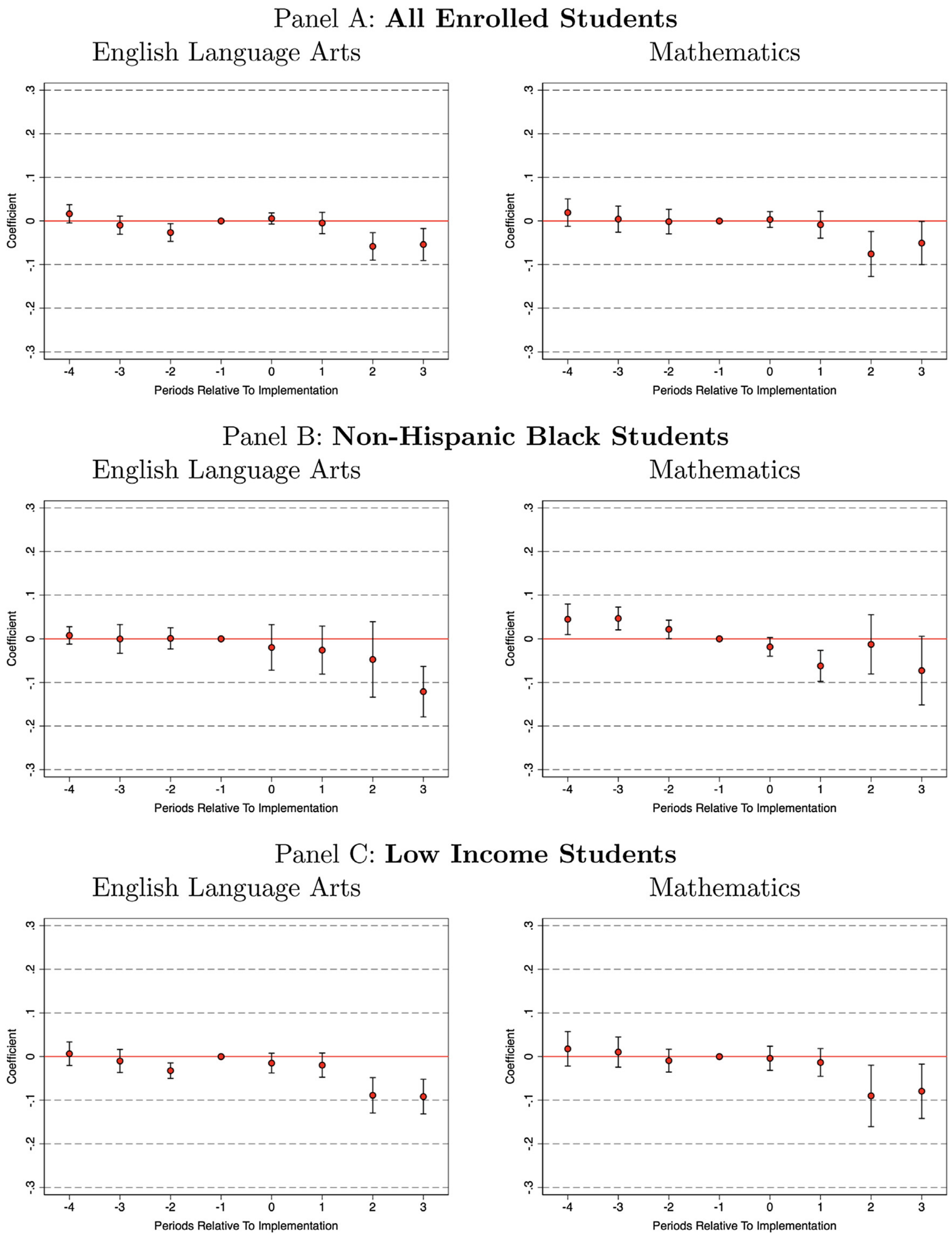


Fig. 7. Event study estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, SEDA (2009–2015). Notes: This figures shows estimates from an event study specification comparable to Eq. (2), where the excluded group is the period before the policy takes effect. 95% confidence intervals reported, where robust standard errors are clustered at the state level. Data from SEDA 2009–2015.

Table 3
Mechanisms: well-child visits.

	Well-child visits			
	(1)	(2)	(3)	(4)
PBE	−0.339***	−0.308***	−0.136	−0.0551
	(0.0517)	(0.0707)	(0.117)	(0.101)
PBE × low income		−0.0178		−0.0154
		(0.0492)		(0.0468)
State FE	YES	YES	YES	YES
Age FE	YES	YES	YES	YES
State linear age trends	NO	NO	YES	YES
Observations	20,613	18,728	20,613	18,728
Mean DV	3.06	3.03	3.06	3.03

In this table, we present estimates from a DD model that controls for an age fixed effects and state fixed effects, using variation across different age children in treatment and control states. Data come from the NSCH 2003. We compare treated children (0, 1, and 2 years old) to control children (3, 4, and 5. years old) across treatment and control states.
Well-child visits are defined as the number of times in the last 12 months the child saw a doctor, nurse, or other health care provider for preventive medical care such as a physical exam or well-child checkup. Chronic absenteeism is whether or not the student missed at least 18 days of school (10% of the total 180 days).
Fig. 1 highlights those states included in the treatment or the control group.
All models include controls for potentially confounding changes in state policy, i.e. school-entry mandates for the HepA, HepB, VAR and PCV vaccines.
Low income assigned based on annual household income below 200% FPL.
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

one coefficient) are statistically indistinguishable 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 a significant drop in non-Hispanic Black and economically disadvantaged 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 2.

5.3. Mechanisms

We propose three potential mechanisms for the long-run effects of PBEs on test scores reported in Table 2 and provide suggestive evidence for the pathway that exists. In examining these mechanisms, we rely upon nationally-representative data from the NSCH. For early-life outcomes, we use data from the 2003 wave and for school-age outcomes, we use data from all three waves (2003, 2007, and 2011) of the NSCH. In all of our exercises to detect mechanisms, we limit our sample to the same states we have used as treatment and control states (Fig. 1) throughout.

First, it is possible that lower vaccination rates correlate with decreased visits for preventive pediatric health care. Routine well-child visits present an opportunity for early detection and intervention in common developmental problems. During these visits, pediatricians may provide important information on healthy child development and positive parenting practices as well as physical health. We test this hypothesis with data from the 2003 NSCH. Children in Arkansas and Texas who were under one, age one, and age two in 2003 (the first year the NSCH data are available) were allowed to exempt from mandatory vaccines. Children in Arkansas and Texas who were three, four, or five years old in 2003 were likely vaccinated prior to the passage of the PBE. We define children as treated if they are in states with PBE and were two years old or younger when the PBE came into effect. This identification strategy compares children who are slightly younger (who were ages 0–2) and slightly older (who were ages 3–5) across states where the PBE did and did not take effect.[26] Our main dependent variable of interest is the number of well-child visits the child attended in the past 12 months. To split our sample, we consider our children to be in low-income households if income is below 200% of the poverty line.

Table 3 column 1 shows that children have 0.34 fewer well-child visits in the last twelve months—or an 11% decrease—due to the PBE. This reduction is slightly larger but not statistically different from the average effect for children from low-income families (column 2). Note that the relative small number of Non-Hispanic Black children in NSCH precludes the estimation of specifications exploring heterogeneity across racial groups. The coefficients remain negative but noisier and smaller in magnitude when we include state-specific linear trends in age (columns 3 and 4). Fig. A.9 shows an event study style graph depicting the statistical difference across the treatment and control states at different ages, where $t-3$ corresponds to 5 year olds, $t-2$ corresponds to 4 year olds, $t-1$ (the excluded group) corresponds to 3 year olds, $t=0$ corresponds to 2 year olds, $t+1$ corresponds to 1 year olds, and $t+2$ corresponds to those under 1 year of age. For both the full sample (panel A) and the effects for low-income children (panel B), there are no statistical differences across the older ages, but a drop

[26] While ideally we would have pre- and post-data within the same age groups over time, the first survey began in 2003, our first year post policy.

Table 4
Mechanisms: chronic absenteeism.

	Chronic absenteeism			
	(1)	(2)	(3)	(4)
PBE	0.00255	−0.000597	0.00290	−0.000268
	(0.00535)	(0.00333)	(0.00497)	(0.00298)
PBE × low income		0.00974*		0.00977*
		(0.00546)		(0.00546)
State FE	YES	YES	YES	YES
Age FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
State linear age trends	NO	NO	YES	YES
Observations	40,019	39,946	40,019	39,946
Mean DV	0.018	0.018	0.018	0.018

In this table, we present estimates from a DD model that controls for an age fixed effects and state fixed effects, using variation across different age children in treatment and control states. Data come from the 2003, 2007, and 2011 NSCH. We compare treated children—those ages 7 through 10 in 2011—to control children—those ages 7 through 10 in 2003 and 2007—across treatment and control states.
Chronic absenteeism is whether or not the student missed at least 18 days of school (10% of the total 180 days).
Fig. 1 highlights those states included in the treatment or the control group.
All models include controls for potentially confounding changes in state policy, i.e. school-entry mandates for the HepA, HepB, VAR and PCV vaccines.
Low income represents whether or not the child receives free or reduced-price lunch.
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in the two treated states compared to the control states for the younger children. Again, this is because the older children were already vaccinated, whereas the younger children were allowed to exempt once they became the appropriate ages for vaccination. These findings provide evidence that young children are going to the doctor less during important times.

Second, it could be that the availability of PBEs 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. The societal costs of vaccine-preventable infections, including student absenteeism, have been examined in the epidemiology literature (see, for example, Lee et al., 2004a,b). Omer et al. (2006) show that non-medical exemptions—and in particular PBE policies—are associated with an increase in pertussis. Further, Carpenter and Lawler (2019) show that requiring middle school students to vaccinate for Tdap substantially reduces the incidence of pertussis among the population. Since we see that non-Hispanic Black children are less likely to vaccinate for DTaP, which also protects from pertussis, we expect an increase in disease incidence to follow.

We obtain disease incidence data by state, year, and age group (0 through 4 year-olds and 5 through 9 year-olds) directly from the CDC. We focus on pertussis—commonly known as whooping cough—as this is a disease the DTaP vaccine protects from, is a communicable disease, and is prevalent among children. Other diseases, such as polio, tetanus, or measles have few or no reported cases in the 2000 and 2001 data for these age groups. Diseases such as varicella are not required to be reported by states to the CDC in all years. We create a state panel, omitting years for which some part of the age group is treated and some is not. Specifically, when using the 0–4 year-old age band, we omit 2003 and 2004 because 3 and 4 year-olds were likely already vaccinated at the time the PBE went into effect. Similarly, when using the 5–9 year old age band, we omit 2006–2009, as it is not until 2010 that all 5–9 year olds were treated by the policy. We estimate a difference-in-difference specification, controlling again for state-level covariates and changes in other state vaccination policies. These DD results are presented in Table A.20. While the effects are positive, none are statistically different from zero. We present event studies in Fig. A.10, where again the results are noisy but potentially point to an increase in disease incidence.

Disease incidence—either through the direct channel of getting sick through communicable diseases one is not vaccinated for or from undiagnosed early conditions that continue to linger—may show through disruptions in instructional time (e.g., absenteeism). We again use data from the NSCH, but this time, since we study those school-aged, we can compare across the three years of NSCH data: 2003, 2007, and 2011. Beginning in 2011, those aged 7, 8, 9, and 10 in Arkansas and Texas can be considered "treated" by the PBE. In 2003 and 2007, this set of ages was not "treated." Thus, we compare within the same age across the treatment year (2011) and the two control years (2003 and 2007), as well as across states that did and did not allow for exemptions. Since there are only two pre-treatment periods, we cannot show a formal event study. In Table 4, we show the overall effect of the PBE on chronic absenteeism, as well as an interaction with whether or not the student receives FRPL to most closely match our SEDA definition of economic disadvantage. In an abundance of caution, we also present a specification with state-specific linear trends in age, though the inclusion of these variables reduce the precision of the model by fitting only four ages. Overall, the effects suggest that the effect is positive and larger for those children receiving free or reduced-price lunch. The effect for low-income children is an increase in chronic absenteeism of roughly 0.9 percentage points. This finding suggests that children from low-income families—those most likely to use the exemption—are also less likely to attend early-life well-child visits and more likely to miss school.

Third, it is possible that disruptions in instructional time (e.g., absenteeism and peers who are behind) may result from a lack of early-life diagnoses coming from well-child visits that may inhibit the learning of students and have consequences on their classmates. The long-run impacts of PBEs can be attributed to both own-effects and potential spillover effects from peers. More disruptive peers, who did not have early health conditions diagnosed early, may have educational consequences for the entire cohort. Peers moving at a slower pace could slow the progress of classes and reduce tests scores for a wider group. Carrell et al. (2018) and Carrell and Hoekstra (2010) find that disruptive peers—measured by one additional child who is the victim of domestic violence in a class of 20–25—reduce test scores by about 0.014 standard deviations. Our overall effect on academic achievement is not statistically different from zero. However, the effect of the PBE on non-Hispanic Black students' math scores is roughly 4.5 times larger. This could be because more than one child in the class has been affected by the PBE, creating further classroom disruption, or due to the focus on specific populations of interest in this study.

6. 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 suggestive evidence 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 and economically disadvantaged populations. Our estimates translate to a 16.1% decrease in the proportion of Black preschoolers and an 8.3% 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., 2001a; Blank et al., 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 are most pronounced in math scores of non-Hispanic Black students (0.06 standard deviations) and economically disadvantaged students (0.04 standard deviations).

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. We find suggestive evidence that lower vaccination rates result in decreased adherence to the recommended schedule for preventive pediatric health care, such as well-child visits. This reduction in well-child visits ultimately results in missed opportunities to identify developmental delays and health problems. We then document disruptions in instructional time through absenteeism that could inhibit the affected child as well as 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).

Authors' contribution

N.L. Hair and C.J. Urban conceptualized and contributed to the design of the study. All authors contributed to the analysis and interpretation of the data. N.L. Hair and C.J. Urban drafted portions of the manuscript. All authors reviewed the manuscript for important intellectual content. All authors approved the final version of the article.

Appendix A

Table A.1
Laws establishing childhood vaccine requirements and permitted exemptions, 1999–present.

34 states included in analysis											
State	Requirements for daycare and pre-kindergarten programs								Permitted exemptions		
	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	

This table summarizes childhood vaccine requirements in the 34 states included in the treatment (bold 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).

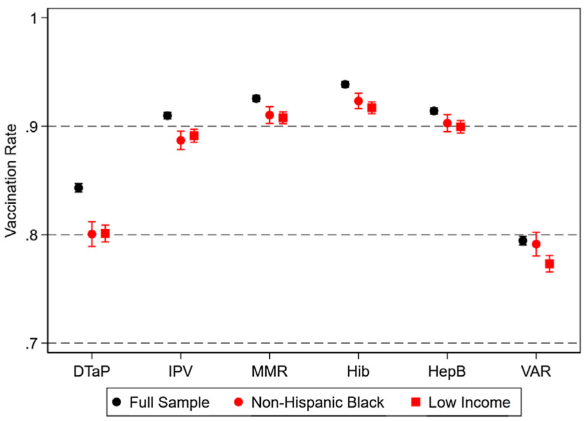


Fig. A.1. Vaccination coverage among children aged 19–35 months in the United States by race and income, 1999–2006. In this figure, we present vaccination coverage estimates, along with 95% confidence intervals among children aged 19–35 months. The black circle corresponds to estimates for the full population, the red circle corresponds to estimates for non-Hispanic Black children, and the red square corresponds to estimates for children from low income households. The combined 6-vaccine series includes ≥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 influenza* type b (Hib), ≥3 doses of hepatitis B (HepB), and ≥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.

Table A.2
Laws establishing childhood vaccine requirements and permitted exemptions, 1999–present.

17 states excluded from analysis											
State	Requirements for daycare and pre-kindergarten programs								Permitted exemptions		
	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

This table 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).

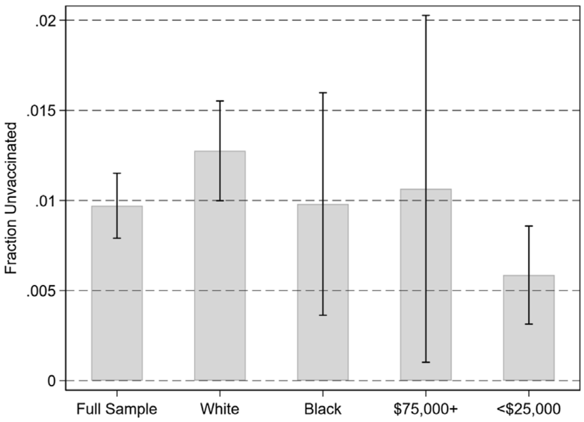


Fig. A.2. Unvaccinated children aged 19–35 months in the United States by race and income, 2016. In this figure, we compare estimated rates of unvaccinated children (children who have yet to receive any vaccines) in various populations. Low income assigned based on annual household income below $25,000. Data are from the 2016 wave of the NIS-Child.

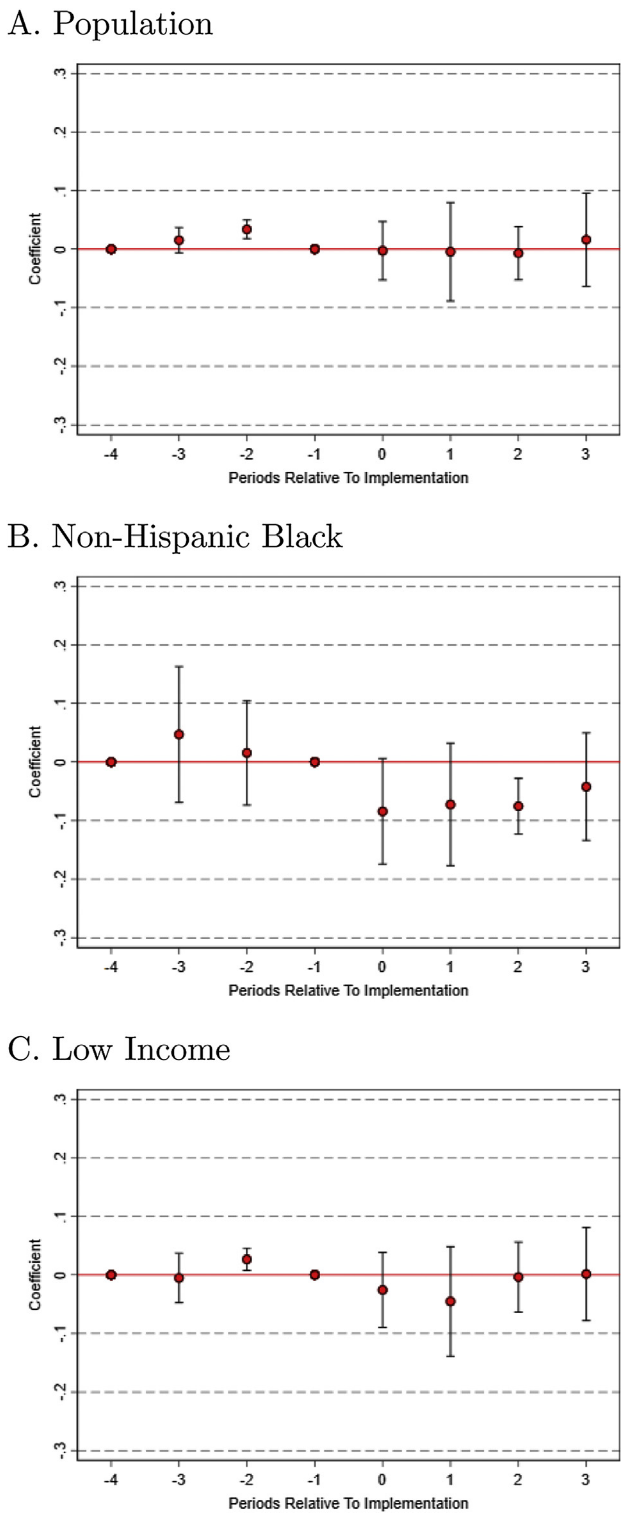
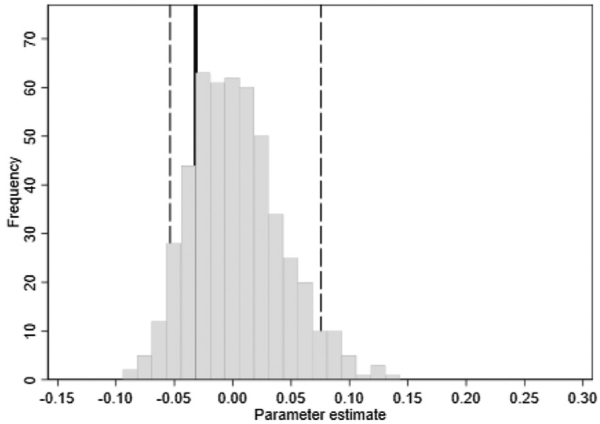
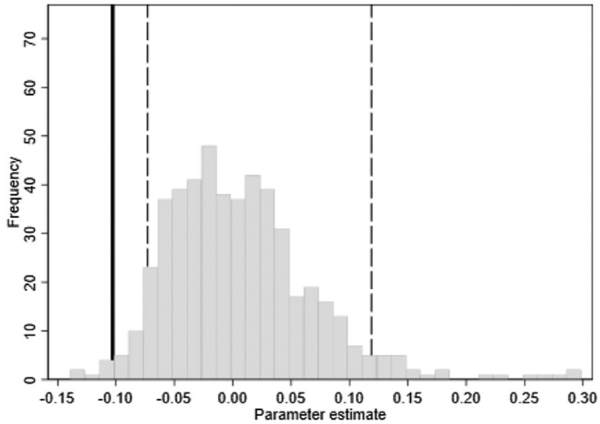


Fig. A.3. Event study estimates of the effects of PBE on early childhood vaccination rates by race and income with state linear trends, NIS-Child (1999–2006). This figures presents estimates from a regression-based event study analysis. Vertical bars represent the 95% confidence interval around each point estimate. The year preceding policy implementation (event time −1) is the omitted category. The inclusion of the state linear trend requires dropping one additional period, so we also omitted event time −4. 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.

A. Population



B. Non-Hispanic Black



C. Low Income

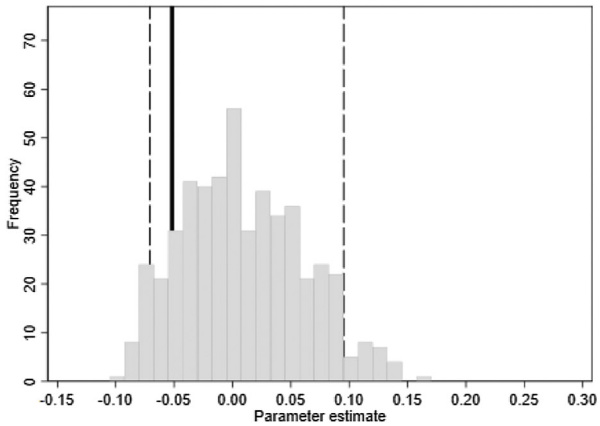


Fig. A.4. Difference-in-differences estimates of the effects of pbe on early childhood vaccination rates by race and income (placebo tests, TX and AR vs all control state pairs). The histograms in this figure summarize the results of our falsification tests. We compare the estimated treatment effect 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.

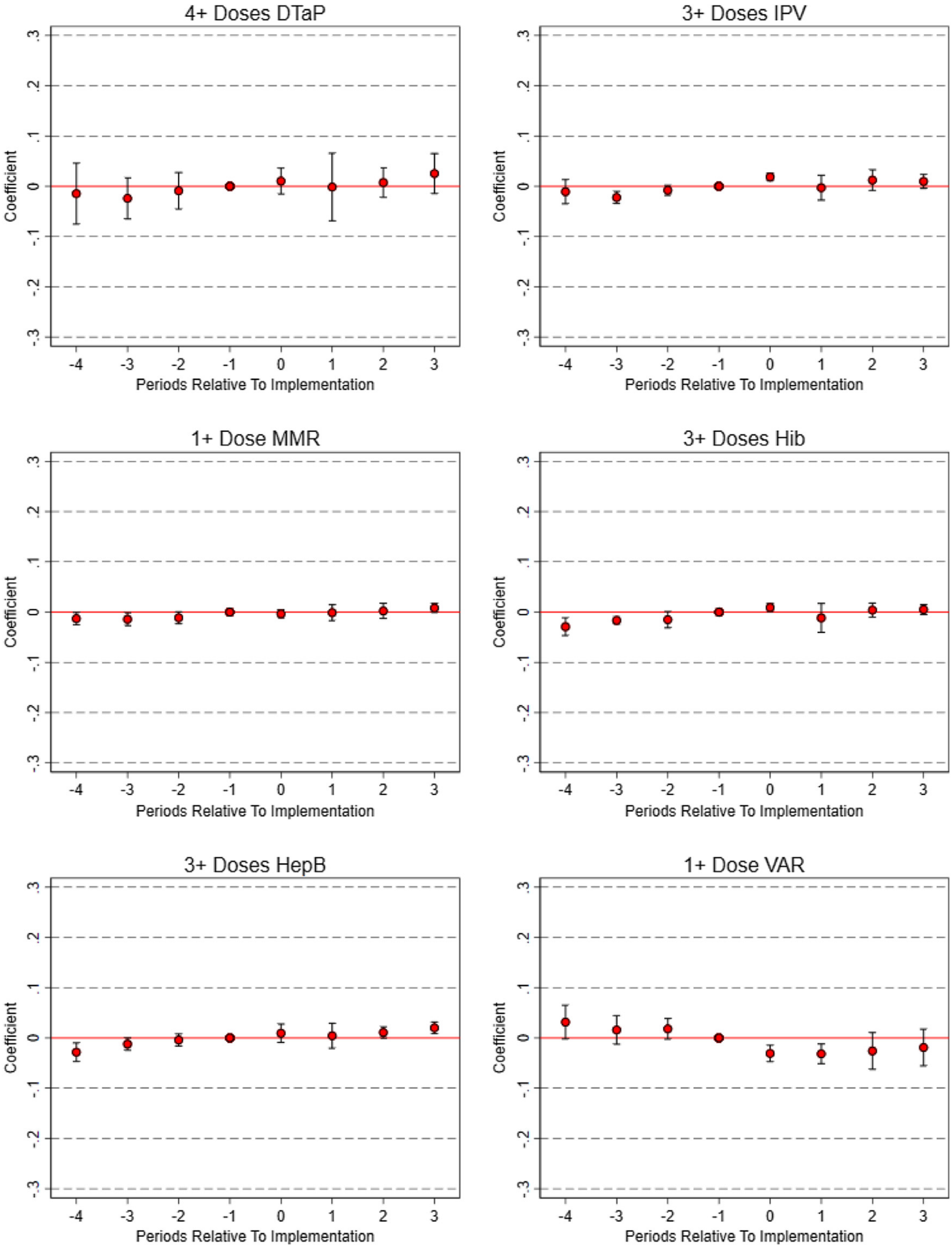


Fig. A.5. Event study estimates of the effects of PBE on early childhood vaccination rates by vaccination for the full sample, NIS-Child (1999–2006). This figure presents estimates from a regression-based event study analysis. Vertical bars represent the 95% confidence interval around each point estimate. The year preceding policy implementation (event time −1) is the omitted category. The dependent variables are each dummy variables equal to one if the child completed each of the following, respectively: ≥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). 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.

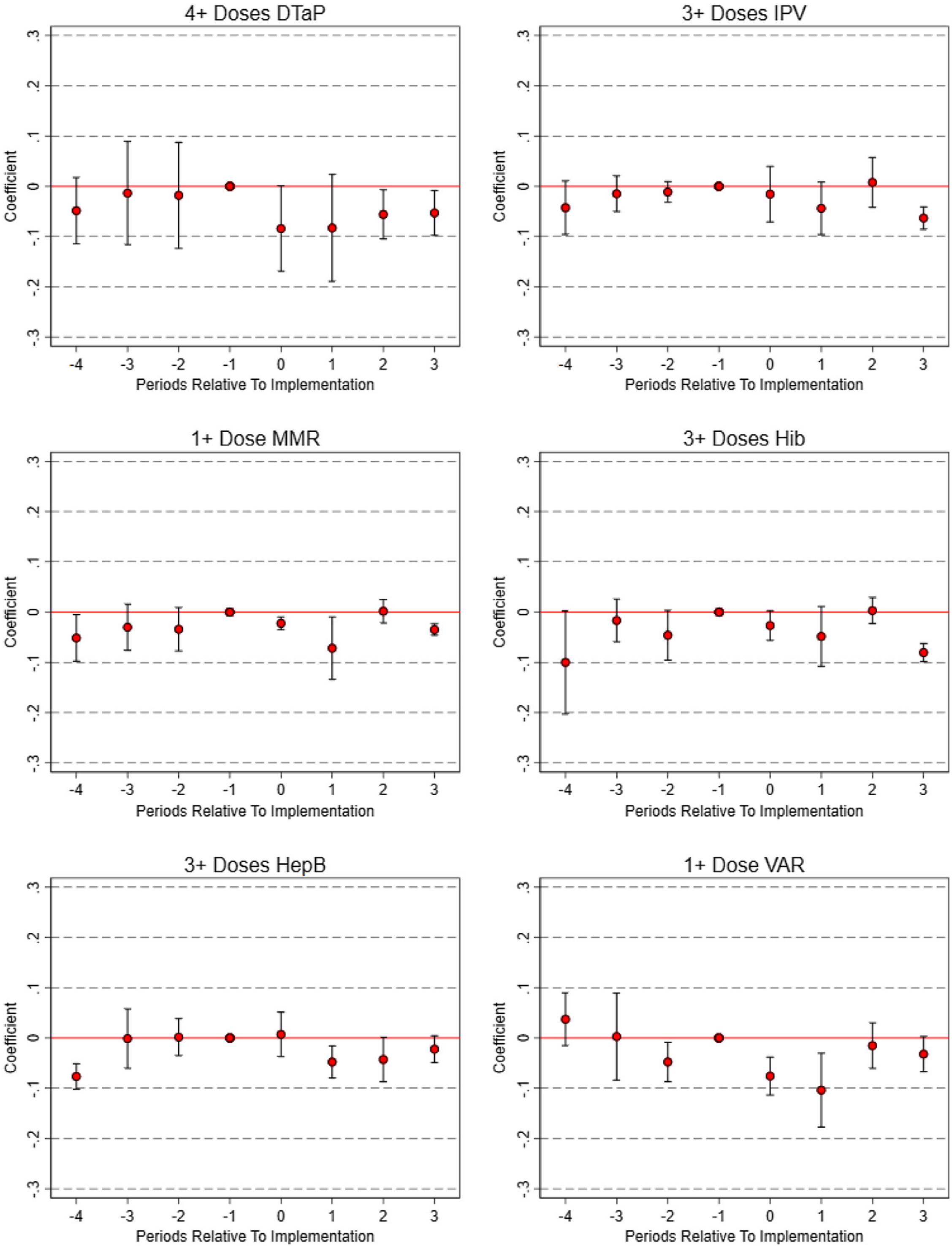


Fig. A.6. Event study estimates of the effects of PBE on early childhood vaccination rates by vaccination for non-Hispanic Black children, NIS-Child (1999–2006). This figure presents estimates from a regression-based event study analysis. Vertical bars represent the 95% confidence interval around each point estimate. The year preceding policy implementation (event time −1) is the omitted category. The dependent variables are each dummy variables equal to one if the child completed each of the following, respectively: ≥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). 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.

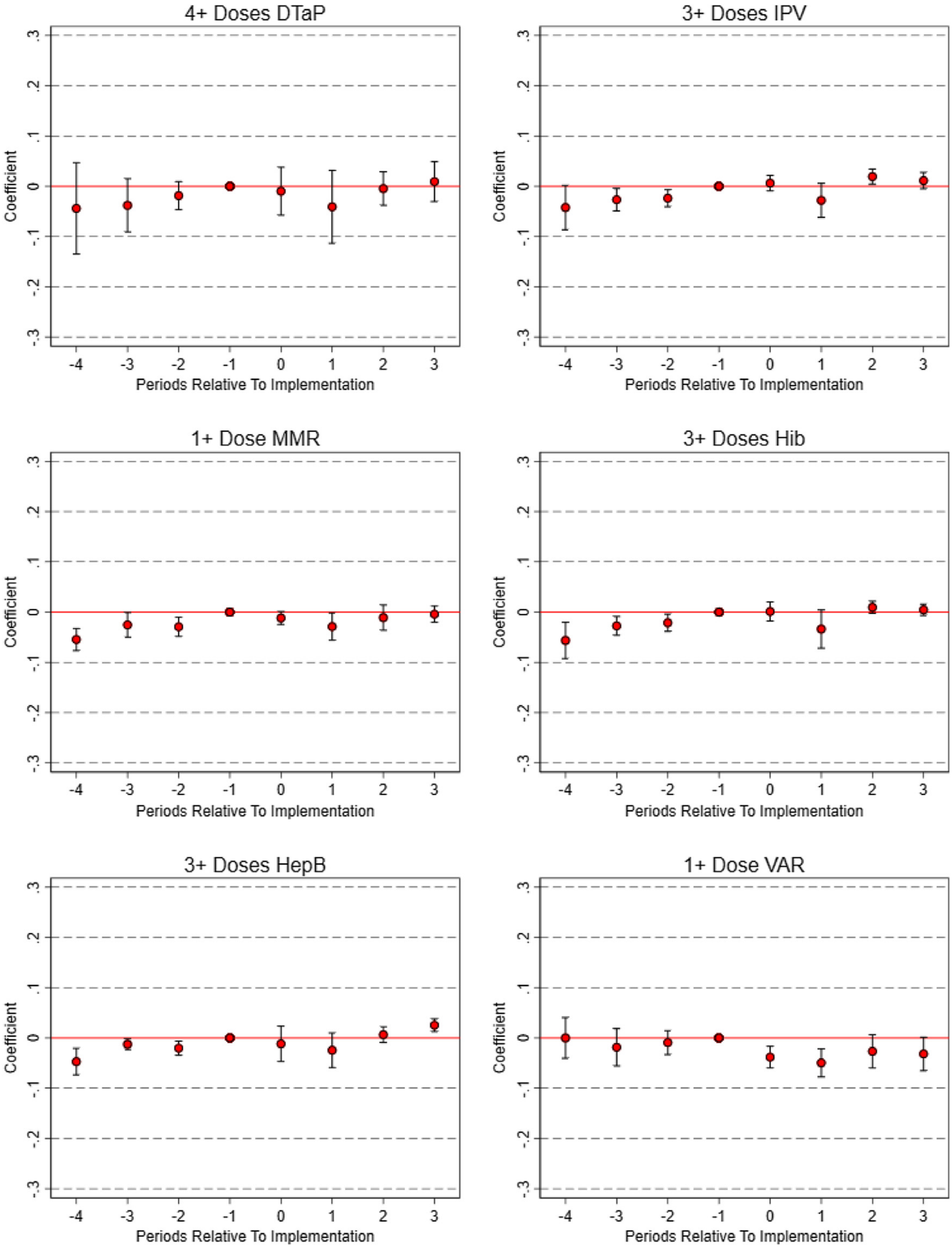


Fig. A.7. Event study estimates of the effects of PBE on early childhood vaccination rates by vaccination for low income children, NIS-Child (1999–2006). This figure presents estimates from a regression-based event study analysis. Vertical bars represent the 95% confidence interval around each point estimate. The year preceding policy implementation (event time −1) is the omitted category. The dependent variables are each dummy variables equal to one if the child completed each of the following, respectively: ≥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). 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.3
Laws establishing vaccine requirements for secondary schools, 1999–present.

34 states included in analysis							
State	HepB	Tdap		MCV		HPV	
AL		2010	Gr 6[b]				
AK	2001	2009	Gr 6–9				
AR	**2000**	**2014**	**≥11 yrs**[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	Gr 6[d]
FL	Y	2009	Gr 7[b]				
GA		2014	Gr 7[c]	2014	Gr 7[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	≥12 yrs[a]	2019	Gr 7[b]		
KY	2001	2011	Gr 6	2018	11–15 yrs[a]		
MD	2007	2014	Gr 7[b]	2014	Gr 7[b]b		
MA	Y	2011	Gr 7[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	11 yrs[a]				
NJ	2004	2008	Gr 6	2008	Gr 6		
NM	Y	2007	Gr 7[b]	2019	Gr 7		
NY	2000	2007	Gr 6–12	2016	Gr 7[b]		
NC	2005	2015	Gr 7	2015	Gr 7		
RI	2000	2009	Gr 7	2009	Gr 7	2015	Gr 7[b]
SC	Y	2013	Gr 7				
SD		2016	Gr 6	2016	Gr 7[c]		
TN	2002	2010	Gr 7				
TX	**2000**	**2009**	**Gr 7**	**2009**	**Gr 7**[b]		
VA	2001	2006	Gr 7			2008	Gr 6[d]
WV		2012	Gr 7, 12	2012	Gr 7		
WY	Y	2010	Gr 7				

This table summarizes vaccine requirements for secondary schools in the 34 states included in the treatment (bold 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	Gr 6[a]	2008	Gr 6[a]	
CA	Y	2011	Gr 7			
ID	Y	2011	Gr 7[a]	2011	Gr 7[a]	
LA	2009	2009	Gr 6[a]	2009	Gr 6	
ME		2017	Gr 7	2018	Gr 7	
MI	2002	2010	Gr 7[b]	2010	Gr 7	
MN	2001	2014	Gr 7[a]	2014	Gr 7[a]	
MO	Y	2010	Gr 8[a]	2015	Gr 8	
ND	Y	2014	Gr 7	2018	Gr 7–10	
OH	2006	2012	Gr 7–9	2016	Gr 7	
OK	Y	2011	Gr 7[a]			

Table A.4 (*Continued*)

17 states excluded from analysis

State	HepB	Tdap		MCV		HPV
OR	2000	2008	Gr 7[a]			
PA	2002	2011	Gr 7	2011	Gr 7	
UT	Y	2007	Gr 7	2015	Gr 7	
VT	Y	2008	Gr 7			
WA	2008	2007	Gr 6[a]			
WI	Y	2008	Gr 6[a]			

This table 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] Mandate implemented progressively in successive grades, e.g. Gr 7 in 2010, Gr 7–8 in 2011, and Gr 7–12 in 2016.

[b] Mandate also applies to new entrants and students changing school districts.

Table A.5
Descriptive statistics, National Immunization Survey-Child (NIS-Child) sample.

	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: 19–23 months	0.299	0.294	0.303	0.308	0.305
Age: 24–29 months	0.344	0.345	0.33	0.347	0.34
Age: 30–35 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: ≥4 doses	0.843	0.865	0.801	0.818	0.801
IPV: ≥3 doses	0.91	0.92	0.887	0.902	0.891
MMR: ≥1 dose	0.921	0.928	0.907	0.913	0.904
Hib: ≥3 doses	0.939	0.949	0.923	0.929	0.917
HepB: ≥3 doses	0.914	0.922	0.903	0.905	0.899
VAR: ≥1 dose	0.794	0.784	0.791	0.809	0.773

Estimates in this table 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 (Fig. 1). Sample includes 99,464 children with adequate provider data. Low income assigned based on annual household income below $25,000.

Table A.6
Descriptive statistics, Stanford Education Data Archive (SEDA) sample.

	Control	Treatment	All
% Black in grade	0.154	0.103	0.146
	(0.225)	(0.147)	(0.216)
% Hispanic in grade	0.0750	0.355	0.117
	(0.114)	(0.273)	(0.180)
% Asian in grade	0.0123	0.00896	0.0118
	(0.0264)	(0.0163)	(0.0251)
% American Indian in grade	0.0196	0.00485	0.0174
	(0.0910)	(0.00694)	(0.0840)
% FRPL	0.548	0.599	0.556
	(0.173)	(0.139)	(0.169)
% Bachelors+	0.198	0.173	0.194
	(0.0940)	(0.0713)	(0.0914)
% SNAP	0.191	0.201	0.193
	(0.101)	(0.0961)	(0.100)
Unemployment rate	0.0454	0.0378	0.0442
	(0.0190)	(0.0169)	(0.0189)
% <19 uninsured	7.668	13.82	8.598
	(3.281)	(5.256)	(4.263)

Notes: This table reports means with standard deviations in parentheses. All time-varying county-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 Fig. 1. These data depict the full sample for all counties with ELA scores, though the descriptive statistics are nearly identical if we restrict to those with math scores.

Table A.7
Difference-in-differences estimates of the effects of PBE on early childhood vaccination rates by race and income, allowing for group-specific trends, 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**	−0.026*	−0.017	−0.026	−0.018	−0.009
	(0.013)	(0.013)	(0.012)	(0.024)	(0.024)	(0.019)
PBE × Black		−0.073***			−0.077***	
		(0.014)			(0.012)	
PBE × low income			−0.038***			−0.037***
			(0.014)			(0.013)
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
Group-specific trends	NO	RACE	INCOME	YES	RACE	INCOME
Observations	99,464	99,464	99,464	99,464	99,464	99,464

In this table, we present estimates from the standard DD model in Eq. (1) along with models that allow for group-specific (either Black or low income) trends.
The combined 6-vaccine series includes ≥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).
Fig. 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8
Difference-in-differences estimates of the effects of PBE on early childhood vaccination rates by race and income, dropping Arkansas, 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	-0.035^{***}	-0.028^{**}	-0.016	-0.046^{***}	-0.038^{***}	-0.027^{**}
	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)
PBE × Black		-0.087^{***}			-0.085^{***}	
		(0.004)			(0.004)	
PBE × low income			-0.043^{***}			-0.042^{***}
			(0.006)			(0.006)
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	97,461	97,461	97,461	97,461	97,461	97,461

In this table, we present estimates from the standard DD model in Eq. (1) but drop Arkansas.
The combined 6-vaccine series includes ≥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).
Fig. 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9
Difference-in-differences estimates of the effects of PBE on early childhood vaccination rates by race and income, dropping cohorts that straddle the treatment, 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	-0.042^{**}	-0.036^{**}	-0.029^{*}	-0.080^{**}	-0.074^{**}	-0.067^{**}
	(0.016)	(0.016)	(0.016)	(0.031)	(0.032)	(0.028)
PBE × Black		-0.075^{***}			-0.074^{***}	
		(0.007)			(0.007)	
PBE × low income			-0.029^{***}			-0.029^{***}
			(0.010)			(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	74,763	74,763	74,763	74,763	74,763	74,763

In this table, we present estimates from the standard DD model in Eq. (1) but drop cohorts that straddle the treatment on either side (Cohorts C3 and C4 in the timeline in Fig. 5).
The combined 6-vaccine series includes ≥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).
Fig. 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10
Difference-in-differences estimates of the effects of PBE on early childhood vaccination rates by vaccine, NIS-Child (1999–2006).

	(1)	(2)	(3)	(4)	(5)	(6)
Child received ≥ 4 doses DTaP						
PBE	0.021***	0.028***	0.037***	−0.002	0.006	0.014
	(0.006)	(0.006)	(0.005)	(0.021)	(0.021)	(0.018)
PBE × Black		−0.088***			−0.087***	
		(0.016)			(0.016)	
PBE × low income			−0.037***			−0.037***
			(0.008)			(0.007)
Child received ≥ 3 doses IPV						
PBE	0.019***	0.022***	0.024***	0.009	0.013	0.015**
	(0.004)	(0.004)	(0.006)	(0.009)	(0.008)	(0.007)
PBE × Black		−0.042***			−0.041***	
		(0.008)			(0.009)	
PBE × low income			−0.012**			−0.013***
			(0.005)			(0.005)
Child received ≥ 1 dose MMR						
PBE	0.010***	0.013***	0.021***	−0.002	0.001	0.009
	(0.003)	(0.003)	(0.005)	(0.006)	(0.005)	(0.006)
PBE × Black		−0.036***			−0.036***	
		(0.007)			(0.007)	
PBE × low income			−0.024**			−0.024**
			(0.009)			(0.009)
Child received ≥ 3 doses Hib						
PBE	0.016***	0.020***	0.021***	−0.004	0.000	0.001
	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)	(0.005)
PBE × Black		−0.043***			−0.042***	
		(0.006)			(0.006)	
PBE × low income			−0.011*			−0.011*
			(0.006)			(0.006)
Child received ≥ 3 doses HepB						
PBE	0.022***	0.025***	0.031***	−0.009	−0.005	0.001
	(0.007)	(0.006)	(0.005)	(0.012)	(0.012)	(0.010)
PBE × Black		−0.037***			−0.037***	
		(0.012)			(0.012)	
PBE × low income			−0.022**			−0.021**
			(0.008)			(0.008)
Child received ≥ 1 dose VAR						
PBE	−0.045***	−0.042***	−0.039***	−0.032***	−0.029***	−0.026***
	(0.013)	(0.013)	(0.014)	(0.009)	(0.009)	(0.009)
PBE × Black		−0.035***			−0.034***	
		(0.012)			(0.011)	
PBE × low income			−0.014			−0.013
			(0.010)			(0.009)
State-specific trends	NO	NO	NO	YES	YES	YES

In this table, we present estimates from the standard DD model in Eq. (1).
The dependent variables are each dummy variables equal to one if the child completed each of the following, respectively: ≥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).
Fig. 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11
Difference-in-differences estimates of the effects of PBE on the number of early childhood vaccination initiated by race and income, NIS-Child (1999–2006).

	# Vaccines in combined 4:3:1:3:3:1 series initiated (≥1 dose)					
	(1)	(2)	(3)	(4)	(5)	(6)
PBE	−0.038***	−0.031**	−0.011	−0.055**	−0.047*	−0.027
	(0.013)	(0.013)	(0.016)	(0.025)	(0.024)	(0.020)
PBE × Black		−0.081***			−0.079***	
		(0.023)			(0.023)	
PBE × low income			−0.062***			−0.062***
			(0.021)			(0.020)
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 this table, we present estimates from the standard DD model in Eq. (1) but change the dependent variable to equal the number of vaccines in the series initiated.
The combined 6-vaccine series includes ≥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).
Fig. 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12
Difference-in-differences estimates of the effects of PBE on early childhood vaccination rates by vaccine, NIS-Teen (2010–2016).

	(1) ≥3 doses HepB	(2)	(3) ≥1 dose MMR	(4)	(5) ≥1 dose VAR	(6)
PBE	−0.007	−0.007	−0.001	−0.001	−0.033***	−0.032***
	(0.006)	(0.006)	(0.004)	(0.004)	(0.011)	(0.011)
N	81004	81004	81004	81004	81004	81004
State-specific trends	NO	NO	NO	YES	YES	YES

In this table, we present estimates from the standard DD model in Eq. (1), though we observe children from 2010 to 2016 and code their vaccines completed by age six despite them being older at the time of observation. Thus, we control for both year of observation and year the individual was two in this specification.
The dependent variables are each dummy variables equal to one if the child completed each of the following by age six, respectively: ≥1 dose of measles, mumps, rubella (MMR); ≥3 doses of hepatitis B (HepB); and ≥1 dose of varicella (VAR).
Fig. 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, SEDA (2009–2015).

	All enrolled (1)	Non-Hispanic Black (2)	Low income (3)
Panel A: ELA			
PBE	−0.006	−0.050	−0.017
	(0.010)	(0.030)	(0.018)
Observations	42,518	19,214	39,738
Panel B: Math			
PBE	−0.030	−0.079***	−0.038
	(0.026)	(0.018)	(0.034)
Observations	38,392	16,968	36,206

In this table, we present estimates from the standard DD model in Eq. (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.
Fig. 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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage restricting sample, SEDA (2009–2015).

	All enrolled (1)	All enrolled (2)	All enrolled (3)
Panel A: ELA			
PBE	−0.008	−0.012	−0.010
	(0.012)	(0.012)	(0.011)
Observations	42,518	19,214	39,738
SAMPLE	ALL	Non-Hispanic Black	Low income
Panel B: Math			
PBE	−0.027	−0.042**	−0.028
	(0.017)	(0.019)	(0.017)
Observations	38,392	16,968	36,206
SAMPLE	ALL	Non-Hispanic Black	Low income

In this table, we present estimates from the standard DD model in Eq. (2) that restricts the sample for all enrolled students to those in counties where non-Hispanic Black students and economically disadvantaged students have an achievement measure, respectively.
The SEDA data include 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.
Fig. 1 highlights those states included in the treatment or the control group.
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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, grade 5 only, SEDA (2009–2015).

	All enrolled (1)	Non-Hispanic Black (2)	Low income (3)
Panel A: ELA			
PBE	−0.023**	−0.050	−0.035**
	(0.010)	(0.037)	(0.015)
Observations	14,236	6466	13,363
Panel A: Math			
PBE	−0.003	−0.045*	−0.009
	(0.020)	(0.024)	(0.020)
Observations	13,164	5816	12,450

In this table, we present estimates from the standard DD model in Eq. (2), but it only includes Grade 5.
The SEDA data include 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. The outcomes are the CS scale for all enrolled students in the county (column 1), for only non-Hispanic Black students within the county (column 2), or only economically disadvantaged students within the county (column 3) for a given grade and year. The latter two are only defined when there are enough students in that group to report and calculate a CS scale. Low income is based on state classification of economic disadvantage for students, which is very highly correlated with free and reduced-price lunch status.
Fig. 1 highlights those states included in the treatment or the control group.
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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Table A.13).
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, grade 6 only, SEDA (2009–2015).

	All enrolled (1)	Non-Hispanic Black (2)	Low income (3)
Panel A: ELA			
PBE	0.001	−0.027	−0.009
	(0.016)	(0.025)	(0.014)
Observations	14,173	6390	13,249
Panel A: Math			
PBE	−0.022	−0.047**	−0.032*
	(0.018)	(0.018)	(0.018)
Observations	13,122	5764	12,375

In this table, we present estimates from the standard DD model in Eq. (2), but it only includes Grade 6.
The SEDA data include 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. The outcomes are the CS scale for all enrolled students in the county (column 1), for only non-Hispanic Black students within the county (column 2), or only economically disadvantaged students within the county (column 3) for a given grade and year. The latter two are only defined when there are enough students in that group to report and calculate a CS scale. Low income is based on state classification of economic disadvantage for students, which is very highly correlated with free and reduced-price lunch status.
Fig. 1 highlights those states included in the treatment or the control group.
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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Table A.13).
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, grade 7 only, SEDA (2009–2015).

	All Enrolled (1)	Non-Hispanic Black (2)	Low income (3)
Panel A: ELA			
PBE	0.018	−0.011	−0.009
	(0.020)	(0.012)	(0.021)
Observations	14,109	6358	13,126
Panel A: Math			
PBE	−0.094***	−0.106***	−0.092***
	(0.022)	(0.016)	(0.022)
Observations	12,106	5388	11,381

In this table, we present estimates from the standard DD model in Eq. (2), but it only includes Grade 7.
The SEDA data include 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. The outcomes are the CS scale for all enrolled students in the county (column 1), for only non-Hispanic Black students within the county (column 2), or only economically disadvantaged students within the county (column 3) for a given grade and year. The latter two are only defined when there are enough students in that group to report and calculate a CS scale. Low income is based on state classification of economic disadvantage for students, which is very highly correlated with free and reduced-price lunch status.
Fig. 1 highlights those states included in the treatment or the control group.
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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Table A.13).
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, dropping Arkansas, SEDA (2009–2015).

	All enrolled (1)	Non-Hispanic Black (2)	Low income (3)
Panel A: ELA			
PBE	−0.003	−0.063***	−0.027*
	(0.012)	(0.018)	(0.014)
Observations	41,081	18,434	38,351
Panel A: Math			
PBE	−0.022	−0.073***	−0.042*
	(0.021)	(0.022)	(0.025)
Observations	36,955	16,185	34,819

In this table, we present estimates from the standard DD model in Eq. (2), but it drops Arkansas.
The SEDA data include 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. The outcomes are the CS scale for all enrolled students in the county (column 1), for only non-Hispanic Black students within the county (column 2), or only economically disadvantaged students within the county (column 3) for a given grade and year. The latter two are only defined when there are enough students in that group to report and calculate a CS scale. Low income is based on state classification of economic disadvantage for students, which is very highly correlated with free and reduced-price lunch status.
Fig. 1 highlights those states included in the treatment or the control group.
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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Table A.13).
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.19
Difference-in-differences estimates of the effects of PBE on English language arts and mathematics test scores by race and economic disadvantage, dropping cohorts that straddle the treatment, SEDA (2009–2015).

	All enrolled (1)	Non-Hispanic Black (2)	Low income (3)
Panel A: ELA			
PBE	−0.022	−0.050*	−0.041**
	(0.016)	(0.027)	(0.017)
Observations	30,230	13,740	28,424
Panel A: Math			
PBE	−0.042	−0.086***	−0.054**
	(0.025)	(0.027)	(0.026)
Observations	27,476	12,198	25,950

In this table, we present estimates from the standard DD model in Eq. (2), but we drop the cohorts straddling the treatment year (Cohorts C3 and C4).
The SEDA data include 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. The outcomes are the CS scale for all enrolled students in the county (column 1), for only non-Hispanic Black students within the county (column 2), or only economically disadvantaged students within the county (column 3) for a given grade and year. The latter two are only defined when there are enough students in that group to report and calculate a CS scale. Low income is based on state classification of economic disadvantage for students, which is very highly correlated with free and reduced-price lunch status.
Fig. 1 highlights those states included in the treatment or the control group.
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, and HepB vaccines; and state decisions to expand Medicaid eligibility.
In separate specifications, models additionally control for state-specific linear cohort trends (Appendix Table A.13).
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.20
Difference-in-differences estimates of the effects of PBE on pertussis (whooping cough) incidence, (2000–2016).

	0–4 YOs (1)	5–9 YOs (2)	0–4 YOs (3)	5–9 YOs (4)
PBE	150.108	77.041	125.058	29.053
	(193.199)	(87.905)	(150.091)	(47.251)
Observations	509	475	509	475
State FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Time-varying controls	YES	YES	YES	YES
State-specific trends	NO	NO	YES	YES

In this table, we present estimates from a standard state panel DD model, but the outcomes are pertussis incidence for ages 0–4 (Columns 1 and 3) and 5–9 (Columns 2 and 4). In periods where some in that age group would be treated and others would be in the control group (for example, 3 year-olds in 2003 and 4 year-olds in 2004), we drop those years of data. Data come directly from the CDC.
Estimated effects are interpreted as the increase in incidence. The mean for 0–4 year-olds is 95.5, and the distribution ranges from 0 through 2303. The mean for 0–4 year-olds is 46.0, and the distribution ranges from 0 through 1372.
All models include controls for potentially confounding changes in state policy, i.e. school-entry mandates for the HepA, HepB, VAR and PCV vaccines.
Robust standard errors (reported in parentheses) are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

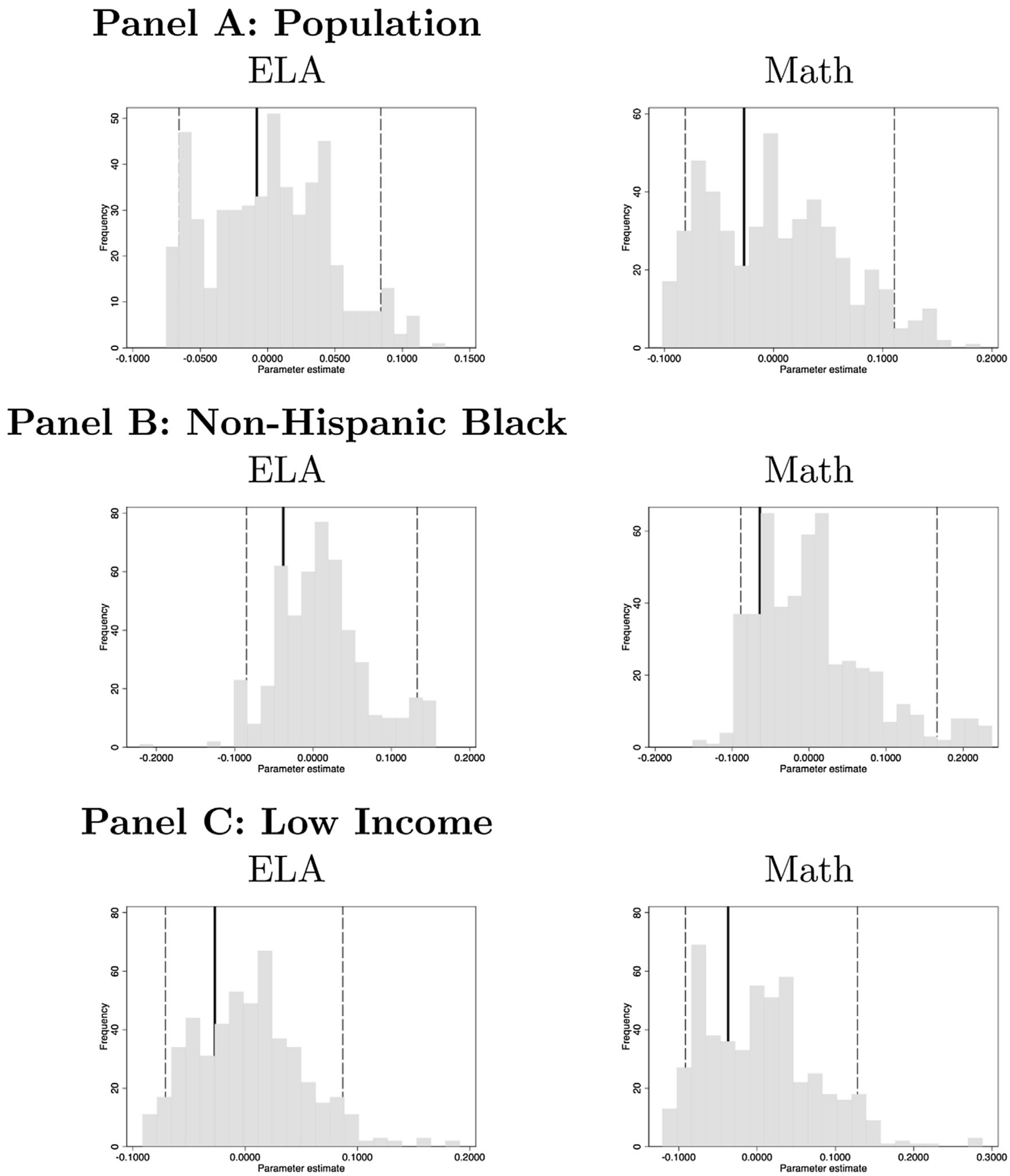
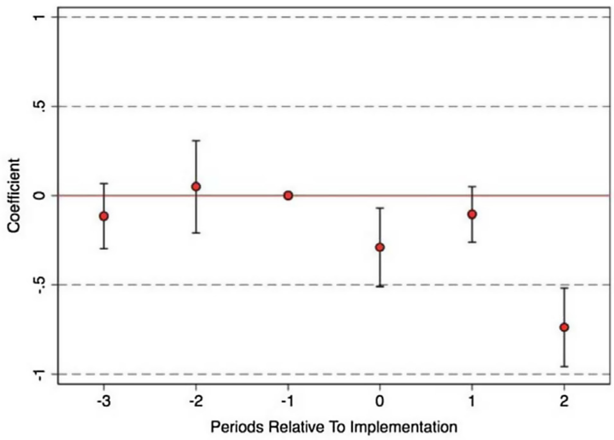


Fig. A.8. Difference-in-differences estimates of the effects of PBE on academic achievement by race and income (placebo tests, TX and AR vs all control state pairs). The histograms in this figure summarize the results of our falsification tests. We compare the estimated treatment effect for TX and AR (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 academic achievement measure in standard deviation units for English/Language Arts (ELA) and Mathematics. Low income is assigned based in the economically disadvantage label in SEDA, which very closely correlates with free and reduced-price lunch receipt. 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, middle school vaccination requirements, and county-level controls, as well as county, year, and cohort fixed effects.

A. Overall



B. Low-income

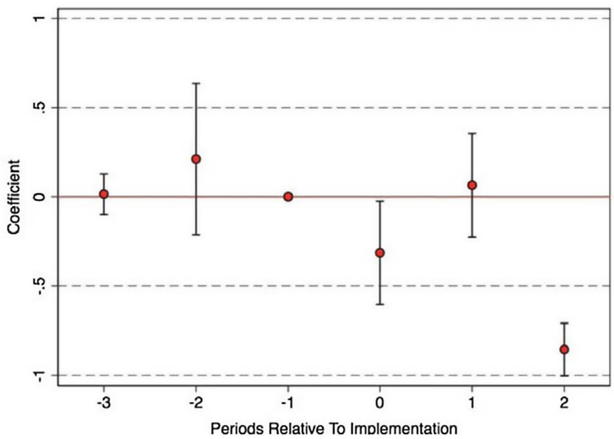
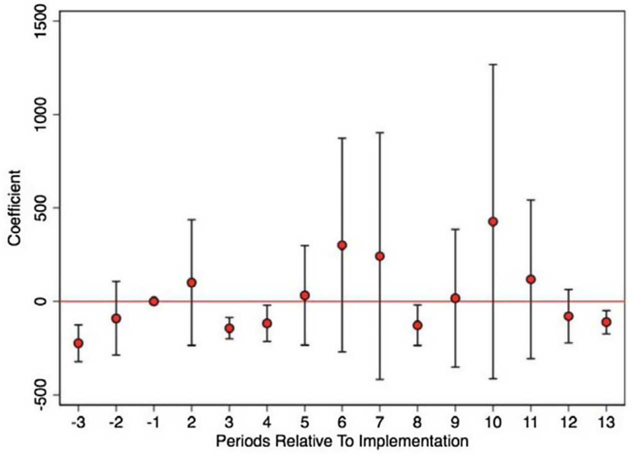


Fig. A.9. Event STUDY ESTIMATES OF THE EFFECTS Of PBE on well-child visits (2003). This figure presents estimates from a regression-based event study analysis. Vertical bars represent the 95% confidence interval around each point estimate. The year preceding policy implementation (event time −1) is the omitted category. In Panel A we report the overall effect, and in Panel B we show the effect for children who come from families below 200% of the federal poverty line. Each estimate compares children across treatment and control states at ages that would and would not have been affected by the policy. Event time $t + 2$ represents those under 1 year of age; $t + 1$ represents those between 1 and 2; $t = 0$ represents those between 2 and 3; $t - 1$ (the excluded period) represents those between 3 and 4; $t - 2$ represents those between 4 and 5; $t - 3$ represents those 5 years of age. The outcome of interest is the number of children in the age band that were reported ill with pertussis according to the CDC. 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.

A. 0–4 YOs



B. 5–9 YOs

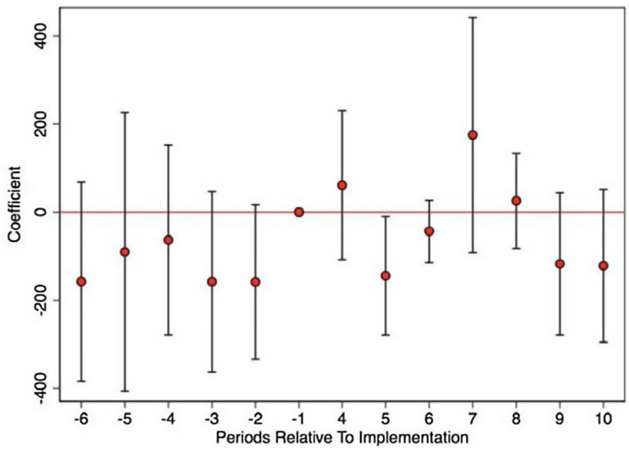


Fig. A.10. Event study estimates of the effects of PBE on pertussis (whooping cough) incidence (2000–2016). This figures presents estimates from a regression-based event study analysis. Vertical bars represent the 95% confidence interval around each point estimate. The year preceding policy implementation (event time −1) is the omitted category. In Panel A (when the outcome is 0–4 year old disease incidence), we omit 2003 and 2004, the two years following the policy because 3 and 4 year olds would have already been vaccinated, whereas 0 through 2 year olds would have been subject to the policy. In Panel B (when the outcome is 5–9 year old disease incidence), we omit 2006, 2007, 2008, and 2009. This is because during these four years some of the 5–9 year olds would not have been subject to the treatment in 2003. In 2010, all 5–9 year olds would have been treated. In Panel B, we additionally have more pre-treatment periods, as older children were likely already vaccinated prior to the beginning of the PBE. The outcome of interest is the number of children in the age band that were reported ill with pertussis according to the CDC. 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.

Appendix B. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jhealeco.2021.102464.

References

Abrevaya, J., Mulligan, K., 2011. Effectiveness of state-level vaccination mandates: evidence from the varicella vaccine. J. Health Econ. 30 (5), 966–976.

Anderberg, D., Chevalier, A., Wadsworth, J., 2011. Anatomy of a health scare: education, income and the MMR controversy in the UK. J. Health Econ. 30 (3), 515–530.

Bauch, C.T., Bhattacharyya, S., 2012. Evolutionary game theory and social learning can determine how vaccine scares unfold. PLOS Comput. Biol. 8 (4), e1002452.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? Quart. J. Econ. 119 (1), 249–275.

Bharadwaj, P., Løken, K.V., Neilson, C., 2013. Early life health interventions and academic achievement. Am. Econ. Rev. 103 (5), 1862–1891.

Blank, N.R., Caplan, A.L., Constable, C., 2013. Exempting schoolchildren from immunizations: states with few barriers had highest rates of nonmedical exemptions. Health Aff. 32 (7), 1282–1290.

Brown, D.W., Kowalski, A.E., Lurie, I.Z., 2019. Long-Term impacts of childhood medicaid expansions on outcomes in adulthood. Rev. Econ. Stud.

Carpenter, C.S., Lawler, E.C., 2019. Direct and spillover effects of middle school vaccination requirements. Am. Econ. J.: Econ. Policy 11 (1), 95–125.

Carrell, S.E., Hoekstra, M.L., 2010. Externalities in the classroom: how children exposed to domestic violence affect everyone's kids. Am. Econ. J.: Appl. Econ. 2 (1), 211–228.
Carrell, S.E., Hoekstra, M., Kuka, E., 2018. The long-run effects of disruptive peers. Am. Econ. Rev. 108 (11), 3377–3415.
CDC, 1999. Ten great public health achievements – United States, 1900–1999. MMWR 48 (12), 241–243.
CDC, 2011. Ten great public health achievements – United States, 2001–2010. MMWR 60 (19), 619–623.
Chang, L.V., 2016. The effect of state insurance mandates on infant immunization rates. Health Econ. 25 (3), 372–386.
Chang, L.V., 2018. Information, education, and health behaviors: evidence from the MMR vaccine autism controversy. Health Econ. 27 (7), 1043–1062.
Chay, K.Y., Guryan, J., Mazumder, B., 2009. Birth Cohort and the Black-White Achievement Gap: The Roles of Access and Health Soon After Birth. National Bureau of Economic Research Working Paper 15078.
Cohodes, S.R., Grossman, D.S., Kleiner, S.A., Lovenheim, M.F., 2016. The effect of child health insurance access on schooling: evidence from public insurance expansions. J. Human Resour. 51 (3), 727–759.
Donald, S.G., Lang, K., 2007. Inference with difference-in-differences and other panel data. Rev. Econ. Stat. 89 (2), 221–233.
Duncan, G.J., Murnane, R., 2011. Whither Opportunity? Rising Inequality, Schools, and Children's Life Chances. Russell Sage Foundation.
Fahle, E.M., Shear, B.R., Kalogrides, D., Reardon, S.F., Chavez, B., Ho, A.D., 2019. Stanford Education Data Archive: Technical Documentation (Version 3.0). Stanford Center for Education Policy Analysis Working Paper 16-09.
Fisher, R.A., 1935. The Design Of Experiments.
Goodman-Bacon, A., 2018. Difference-in-Differences with Variation in Treatment Timing. National Bureau of Economic Research Working Paper 25018.
House Research Organization. Texas House of Representatives, 2003. Conscientious objection to immunization. Interim News 78 (1), 1–6.
Hoynes, H., Schanzenbach, D.W., Almond, D., 2016. Long-run impacts of childhood access to the safety net. Am. Econ. Rev. 106 (4), 903–934.
Isen, A., Rossin-Slater, M., Walker, W.R., 2017. Every breath you take every dollar you'll make: the long-term consequences of the Clean Air Act of 1970. J. Polit. Econ. 125 (3), 848–902.
Lawler, E.C., 2017. Effectiveness of vaccination recommendations versus mandates: evidence from the hepatitis A vaccine. J. Health Econ. 52, 45–62.
Lee, B.R., Feaver, S.L., Miller, C.A., Hedberg, C.W., Ehresmann, K.R., 2004a. An elementary school outbreak of varicella attributed to vaccine failure: policy implications. J. Infect. Dis. 190 (3), 477–483.
Lee, G.M., Lett, S., Schauer, S., LeBaron, C., Murphy, T.V., Rusinak, D., Lieu, T.A., Massachusetts Pertussis Study Group, 2004b. Societal costs and morbidity of pertussis in adolescents and adults. Clin. Infect. Dis. Off. Publ. Infect. Dis. Soc. Am. 39 (11), 1572–1580.
Luca, D.L., 2016. The Effects of Mandatory Vaccination Laws on Childhood Health and Adult Educational Attainment. Social Science Research Network SSRN Scholarly Paper ID 1987898, Rochester, NY.
Miller, S., Wherry, L.R., 2019. The long-term effects of early life medicaid coverage. J. Human Resour. 54 (3), 785–824.
National Conference of State Legislatures, 2020. States With Religious and Philosophical Exemptions From School Immunization Requirements.
Olive, J.K., Matthews, K.R.W., 2016. How Too Much Freedom of Choice Endangers Public Health: The Effect of Nonmedical Exemptions from School-Entry Vaccinations in Texas. Rice University's Baker Institute for Public Policy, Houston, Texas, Policy Brief No. 10.13.16.
Omer, S.B., Richards, J.L., Ward, M., Bednarczyk, R.A., 2012. Vaccination policies and rates of exemption from immunization, 2005–2011. N. Engl. J. Med. 367 (12), 1170–1171.
Omer, S.B., Pan, W.K.Y., Halsey, N.A., Stokley, S., Moulton, L.H., Navar, A.M., Pierce, M., Salmon, D.A., 2006. Nonmedical exemptions to school immunization requirements: secular trends and association of state policies with pertussis incidence. JAMA 296 (14), 1757–1763.
Orenstein, W.A., Hinman, A.R., 1999. The immunization system in the United States: the role of school immunization laws. Vaccine 17, S19–S24.
Oster, E., 2018. Does disease cause vaccination? Disease outbreaks and vaccination response. J. Health Econ. 57, 90–101.
Patel, M., 2019. National update on measles cases and outbreaks – United States, January 1–October 1, 2019. MMWR Morbid. Mortal. Wkly. Rep. 68.
Phadke, V.K., Bednarczyk, R.A., Salmon, D.A., Omer, S.B., 2016. Association between vaccine refusal and vaccine-preventable diseases in the United States. JAMA 315 (11), 1149–1158.
Reardon, S.F., Ho, A.D., Shear, B.R., Fahle, E.M., Kalogrides, D., DiSalvo, R., 2017a. Stanford Education Data Archive (Version 2.0).
Reardon, S.F., Kalogrides, D., Ho, A.D., 2017b. Linking U.S. School District Test Score Distributions to a Common Scale. Stanford Center for Education Policy Analysis Working Paper 16-09.
Richwine, C.J., Dor, A., Moghtaderi, A., 2019. Do Stricter Immunization Laws Improve Coverage? Evidence from the Repeal of Non-medical Exemptions for School Mandated Vaccines. National Bureau of Economic Research Working Paper 25847.
Rota, J.S., Salmon, D.A., Rodewald, L.E., Chen, R.T., Hibbs, B.F., Gangarosa, E.J., 2001a. Processes for obtaining nonmedical exemptions to state immunization laws. Am. J. Public Health 91 (4), 645–648.
Rota, J.S., Salmon, D.A., Rodewald, L.E., Chen, R.T., Hibbs, B.F., Gangarosa, E.J., 2001b. Processes for obtaining nonmedical exemptions to state immunization laws. Am. J. Public Health 91, 645–648.
Safi, H., Wheeler, J.G., Reeve, G.R., Ochoa, E., Romero, J.R., Hopkins, R., Ryan, K.W., Jacobs, R.F., 2012. Vaccine policy and arkansas childhood immunization exemptions: a multi-year review. Am. J. Prev. Med. 42 (6), 602–605.
Salmon, D.A., Sapsin, J.W., Teret, S., Jacobs, R.F., Thompson, J.W., Ryan, K., Halsey, N.A., 2005. Public health and the politics of school immunization requirements. Am. J. Public Health 95 (5), 778–783.
Schaller, J., Schulkind, L., Shapiro, T.M., 2017. The Effects of Perceived Disease Risk and Access Costs on Infant Immunization. National Bureau of Economic Research Working Paper 23923.
Smith, P.J., Chu, S.Y., Barker, L.E., 2004. Children who have received no vaccines: who are they and where do they live? Pediatrics 114 (1), 187–195.
Wooldridge, J.M., 2006. Cluster-Sample Methods in Applied Econometrics: An Extended Analysis.