Supplementary Appendix

Supplement to: Carrera M, Lawler EC, White C. Population Mortality and Laws Encouraging Influenza Vaccination for Hospital Workers.

APPENDIX

Appendix Section 1. Details on Data Sources and Variable Construction

Mortality rates were constructed from restricted-access microdata from the National Vital Statistics System multiple cause of death mortality files. For each death, these files included the month of death, state of residence, and up to 21 cause of death codes. A death was categorized as related to Pneumonia & Influenza (P&I) if any of the 21 cause of death codes indicated pneumonia or influenza (ICD-9 codes: 480-488; ICD-10 codes: J9-J18). Non-P&I causes of death are based on National Center for Health Statistics (NCHS) categorization of underlying cause of death, according to the 39-cause recode that is available as of 1999. Categories were mutually exclusive, and non-P&I categories excluded deaths with a secondary cause of death for P&I.

Age-specific population data was obtained from the Surveillance, Epidemiology and End Results Program (SEER) data. SEER data are annual and were linearly interpolated to the monthly level to avoid discontinuous jumps at the beginning of each year. These population data were used as the denominators for calculating P&I mortality rates by age group. In all analyses we specify our outcome variable as the natural log of the P&I mortality rate per 100,000 population.

We obtained information on state laws regarding influenza vaccination in long term care facilities or influenza vaccination in childcare facilities, to be used as covariates (CDC Public Health Law Program and the Immunization Action Coalition, respectively).

We also obtained data on vaccine match rates from the CDC's annual influenza season summaries, which are compiled from the CDC's virologic surveillance system. The match rate is defined as the percentage of strains characterized by the CDC that are included (as exact matches) in the vaccine for that season.

We obtained data on adult (age 18+) influenza and pneumococcal vaccine coverage through the Behavioral Risk Factor Surveillance System (BRFSS) for the years 1995 to 2017, although the relevant vaccination questions were not included in the survey for influenza-years 1995/96, 1997/98 and 1999/00. These data have information on vaccination receipt, the respondent's age, and their state of residence (but not occupation), which allow us to construct measures of vaccine coverage at the age-state-influenza year level. Our measure of influenza vaccination is derived from the self-reported variable that asks if the individual received an influenza vaccine in the past 12 months. Given the 12 month look back period of this survey question and the fact that influenza vaccines are most commonly administered during the months of September to December,[1] we assign individuals surveyed during January through August of year *t* to the influenza-year that began in year *t-1*, and drop individuals that were surveyed between September and December. Influenza vaccination coverage rates for each influenza-year are constructed as the share of surveyed individuals in a given state that reported receiving the influenza vaccine during the relevant influenza-year, weighted by the provided BRFSS sample adult person weights. As we do not have information on occupation in this dataset, these vaccination coverage rates are for the general adult population rather than the population of hospital workers.

Data on adolescent influenza vaccination coverage were obtained from the 2008-2017 waves of the National Immunization Survey – Teen (NIS-Teen); infant influenza vaccination coverage data were

obtained from the 2003-2017 waves of the National Immunization Survey-Child. The NIS-Teen survey targets adolescents between 13 and 17 years of age, and includes information on the adolescent's state of residence, as well as a provider-verified measure of if an adolescent surveyed during year *t* received an influenza vaccine dose during the *t-1/t* influenza season.

The NIS-Child survey is a counterpart to the NIS-Teen, and targets infants between 19 and 35 months of age. These data similarly include information on the infant's state of residence, however, unlike the NIS-Teen, these data do not consistently report information on if an infant surveyed during year *t* received an influenza vaccine dose during the *t-1/t* influenza season. Therefore, to construct our measure of infant influenza vaccination coverage, we utilized information on the number of influenza vaccine doses the infant had received during their life, and we restricted our sample to the youngest set of infants surveyed: 19-23 month olds. We then constructed an indicator variable that equaled one if the infant had ever received an influenza vaccine dose, and that equaled zero otherwise. Since infants are not recommended to receive any influenza vaccine until the age of 6 months, by restricting our sample to 19-23 month olds our vaccine measure should primarily capture influenza doses received during the previous 13-17 months, thus allowing us to more closely approximate a measure of influenza vaccination during the *t-1/t* influenza season.

From the NIS-Child dataset we also constructed a measure of pneumococcal vaccination among infants. Since infants are recommended to have received 4 doses of the pneumococcal conjugate vaccine by the age of 15 months, our measure of pneumococcal vaccination is an indicator variable that is equal to one if the infant is up-to-date on their pneumococcal vaccine series at the time of the survey, and is equal to zero otherwise.

Appendix Section 2. Details of State Healthcare Worker Influenza Vaccination Laws

Language in the table below regarding the health care facilities and the set of workers covered by the laws is drawn from state statutes and regulations, which vary widely in their specificity. For example, some states specify that the law applies simply to all hospitals (e.g Oklahoma, Tennessee), whereas some specify all general acute care hospitals (e.g. California, Nebraska) or all acute general hospitals and special hospitals (Maryland). The laws in some states extend beyond hospitals; for example, in Colorado it applies to all licensed hospitals, hospital units, ambulatory surgical centers and long-term care facilities, and in Rhode Island it applies to all health care facilities. Within a given health care facility, there is also variation as to whether the statute applies to all employees (e.g. Oklahoma, California, Nebraska), or only all health care workers (e.g. Colorado, Rhode Island), or some intermediate version such as all employees with direct patient contact (Maine). As noted in the main text, federal facilities (including those overseen by the Veterans Health Administration (VHA)) are not bound by these state laws. Notably, effective November 2017 (after the end of our sample period), the VHA implemented a directive that all heath care workers are to receive the influenza vaccine annually or wear a face mask during the influenza season.[2] Prior to this directive, only 4% of VHA hospitals reported hospital level requirements that health care workers receive the influenza vaccine.[3]

We also graphically present the timing of the law adoption across states, and how that coincides with our sample period, in the figure below.

Figure S1: Hospital Worker Influenza Vaccination Laws, by State

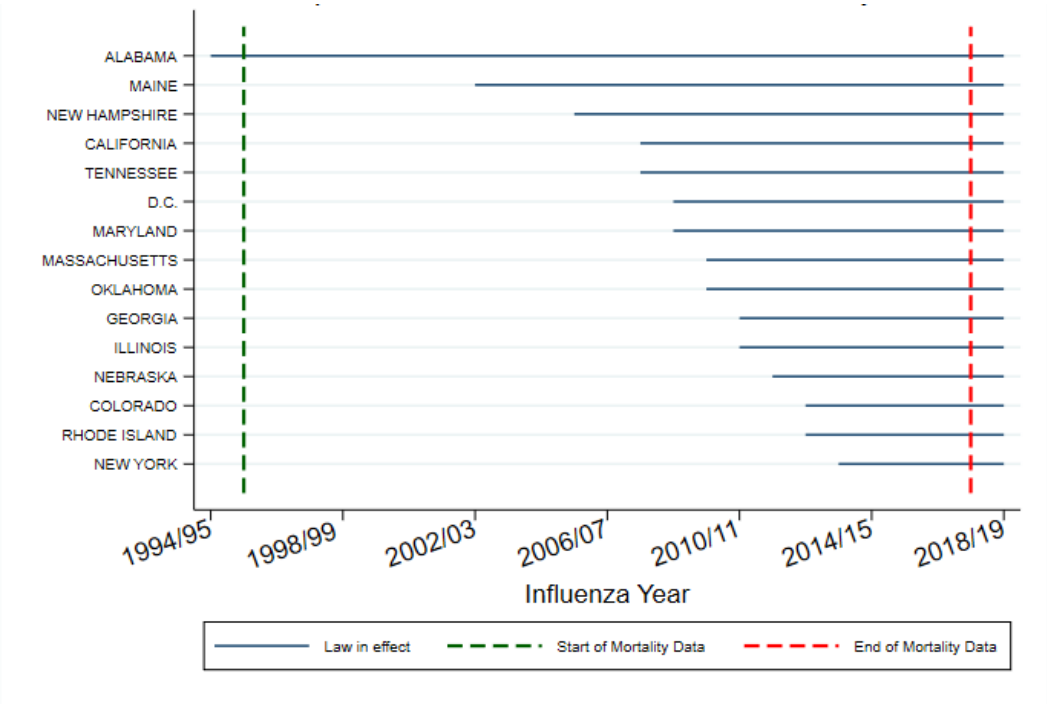


Table S1: Details on State Law Content

State	First Season	Law Type	Summary
Alabama	1994/95	Vaccination Offered	All hospitals are required to establish vaccine policies for all employees that are consistent with CDC recommendations.
Maine	2002/03	Vaccination or Declination Required	All designated healthcare facilities are required to offer the influenza vaccine to employees with direct patient contact. Formal documentation of vaccine declination is required.
New Hampshire	2005/06	Vaccination Offered	All hospitals, residential care facilities, adult day care facilities, and assisted living facilities are required to provide all employees annual influenza immunizations. Employees may decline to consent to the vaccine for any reason, and exemptions are available for medical contraindications and religious beliefs.
California	2007/08	Vaccination or Declination Required	All general acute care hospitals are required to offer the influenza vaccine onsite to all employees, at no cost. Employees may decline the vaccine for any reason, but formal documentation of vaccine declination is required.
Tennessee	2007/08	Vaccination or Declination Required	All hospitals are required to offer the influenza vaccine to all staff and independent practitioners, at no cost. Employees may decline the vaccine for any reason, but formal documentation of vaccine declination is required if it is declined for a reason other than medical contraindication.
Maryland	2008/09	Vaccination or Declination Required	All acute general hospitals and special hospitals are required to offer the influenza vaccine to all staff and independent practitioners. Reasons for vaccine declination must be formally documented.
Washington D.C.	2008/09	Vaccination or Declination Required	All hospital employees and other persons with direct patient contact must be immunized in accordance with CDC standards.
Massachusetts	2009/10	Vaccination or Declination Required	Hospitals must provide the influenza vaccine to all personnel, at no cost. Employees may decline the vaccine for any reason, and exemptions are available for medical contraindications and religious beliefs. Formal documentation of vaccine declination is required.
Oklahoma	2009/10	Vaccination or Declination Required	All hospitals are required to offer the influenza vaccine onsite to all employees, at no cost. Employees may decline the vaccine for any reason, but formal documentation of vaccine declination is required if it is declined for a reason other than medical contraindication.
Georgia	2010/11	Vaccination Offered	All hospitals are required to offer the influenza vaccine to all health care workers and other employees that have direct contact with patients, at no cost.
Illinois	2010/11	Vaccination or Declination Required	All health care settings must offer the influenza vaccine to all health care employees. Employees may decline the vaccine for any reason, and exemptions are available for medical contraindications and religious beliefs. Formal documentation of vaccine declination is required.
Nebraska	2011/12	Vaccination Offered	All general acute care hospitals are required to offer the influenza vaccine onsite to all employees. Employees may decline the vaccine for any reason.
Colorado	2012/13	Vaccination or Declination + Mask Required	All licensed hospitals, hospital units, ambulatory surgical centers and long-term care facilities must provide an annual influenza vaccine to all healthcare workers. Healthcare workers are required to show proof of immunization or a valid medical exemption. Healthcare workers without proof of immunization must wear a mask during influenza season when in direct contact with patients.

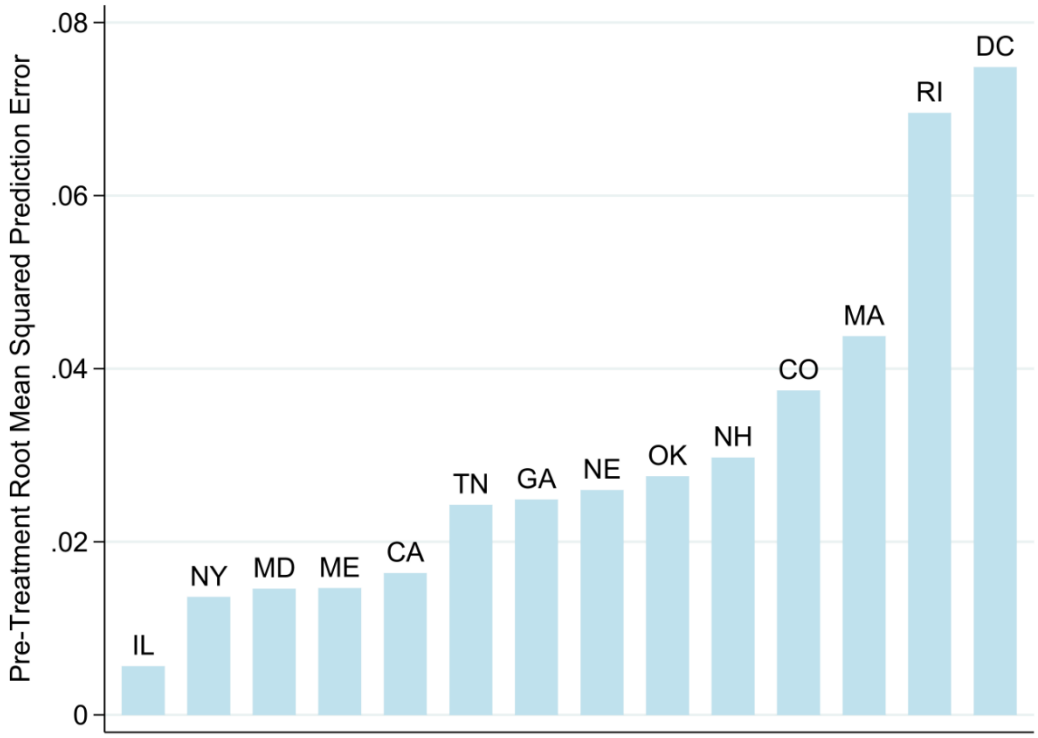
Rhode Island	2012/13	Vaccination or Declination + Mask Required	All health care facilities must provide the influenza vaccine at no cost to the health care worker. Health care workers are required to receive the annual influenza vaccine, although may decline for any reason and medical exemptions are available. Any workers who do not receive the vaccine must wear a mask during influenza season when in direct contact with patients. Formal documentation of vaccine declination is required.
New York	2013/14	Vaccination or Declination + Mask Required	Health care personnel are required to receive the annual influenza vaccine, although may decline for any reason and medical exemptions are available. Any workers who do not receive the vaccine must wear a mask during periods when influenza is prevalent and when in areas where patients may be present. Formal documentation of vaccine declination is required.

Appendix Section 3. Details on the Synthetic Control Estimation Method

We conduct a synthetic control analysis in which we estimate a separate treatment effect for each of the 14 states that adopted a vaccination law between 2002 and 2014. We construct a synthetic counterfactual for each of the 14 treated states, where the synthetic counterfactual is a weighted average of the P&I mortality rate of the set of states that *never* adopt a vaccination law (i.e., the "donor pool"). To obtain these weights, we match on the pre-treatment values of the outcome variable (log P&I mortality per 100,000 population) in each influenza year prior to treatment. For example, California was treated in the 2007/08 influenza-year, and thus we matched on the P&I mortality rate in each influenza-year from 1995/96 (the first year in our sample period) through 2006/07. The synthetic control weights are those that minimize the pre-treatment root mean squared prediction error (RMSPE), and these weights were obtained using the *synth* command in Stata.

The estimated treatment effect for each treated state is the average treatment-control difference in the post-treatment period. P-values for the estimates were constructed via randomization inference using the distribution of the ratio of post-treatment root mean squared prediction error (RMSPE) to pre-treatment RMSPE.[4] The post-treatment RMSPE is a measure of the size of the treatment effect, and the pre-treatment RMSPE is a measure of pre-treatment goodness of fit. Therefore, achieving a small p-value requires both good pre-treatment fit and a relatively large post-treatment difference between treatment and control. Figure S2 displays the pre-treatment RMSPE for each of the 14 treatment states. The large pre-treatment RMSPE for Washington DC, for example, implies that these estimates are relatively less reliable and thus an extremely large treatment effect would be necessary achieve a small p-value. Confidence intervals for these estimates cannot be constructed without the additional assumption that treatment is randomly assigned (which would obviate the need for a synthetic control design).

Figure S2: Pre-treatment Root Mean Squared Prediction Error, by State



Note: Each bar represents the pre-treatment root mean squared prediction error (RMSPE) for the synthetic control procedure described in Appendix Section 3. The pre-treatment RMSPE is a measure of how well the synthetic control procedure is able to match trends in the treated state to its synthetic control (lower values indicate better fit). Synthetic control estimates are more reliable for states with better pre-treatment fit.

Appendix Section 4. Details on Two-Way Fixed Effects Estimation Method

4.1 Main Specification (Table 1)

To estimate the effect of state laws regarding influenza vaccination for health care workers on P&I mortality rates we perform a state-level longitudinal data analysis and estimate two-way fixed effects models in which we control for national time fixed effects and state fixed effects, as well as state-specific linear time trends. Specifically, we estimate the following multivariate linear regression model:

$$Y_{st} = \beta_0 + \beta_1 Law_{sy} + \beta_2 X_{st} + \delta_s + \delta_t + \delta_s \times t + \varepsilon_{st}$$

where Y_{st} is the natural log of the P&I mortality rate per 100,000 population in state *s* and year-month *t*. We used the natural log of the mortality rate to ensure that predicted values from the regression would not take on impossible values (i.e., negative mortality rates), although the main estimates are nearly identical when we used the mortality rate in levels instead of logs. β_1 is the coefficient of interest, where Law_{sy} is a binary variable indicating the presence of any state law regarding health care worker influenza vaccination in state s and influenza-year y. X_{st} represents a vector of indicators for the presence of state laws regarding influenza vaccination in long term care facilities and childcare facilities.

δ_s are state fixed effects, which control for observable and unobservable time-invariant differences in log P&I mortality rates across all states. In other words, δ_s control for all cross-sectional differences across states. δ_t are year-month fixed effects, which allow for differences in log P&I mortality rates over time that are common to all states (e.g., the common component of a particularly bad influenza season). Finally, $\delta_s \times t$ are state-specific linear time trends, which allow for time-varying differences across states to evolve linearly over time. In the trend difference specification below we formally evaluate the extent to which there were statistically different trends in P&I mortality across states during the period prior to law adoption. While the evidence from these analyses (discussed elsewhere) suggests that there were not meaningful differences in pre-trends, we conservatively choose to include state-specific linear time trends in our preferred specification. We also report estimates excluding these trends, however.

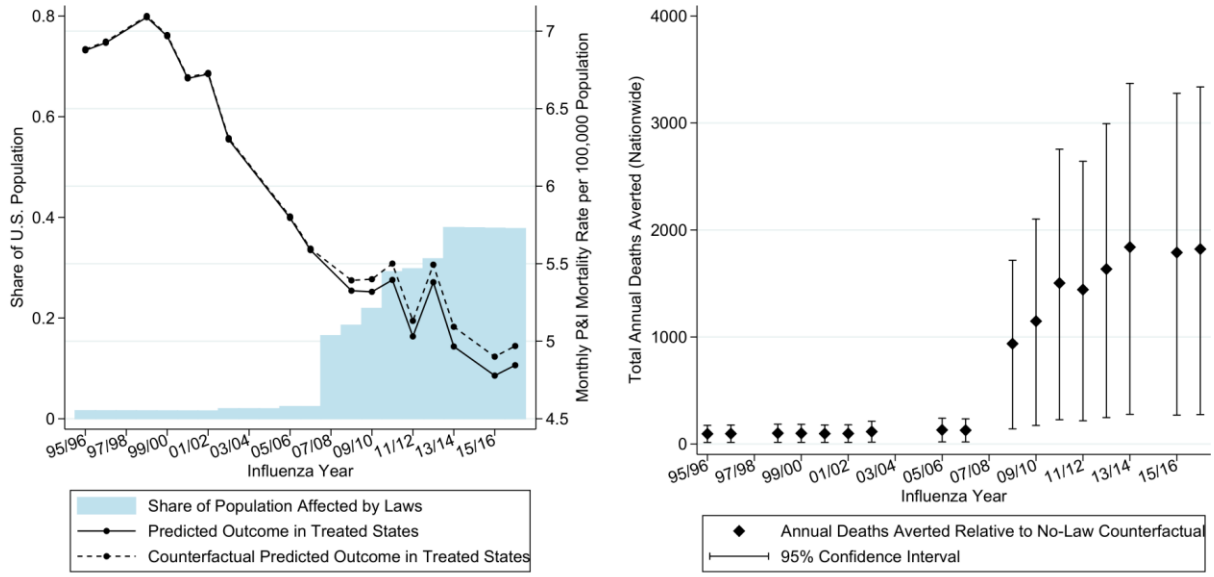
This approach estimates the change in the log P&I mortality rate before and after a state implements a vaccination law, relative to the changes in states that implemented laws in different years or not at all. Interpreting the estimates as causal effects of the law requires assuming that log P&I mortality rates would have evolved similarly in treatment and control states (i.e. with parallel trends) in the absence of the new laws.

Regressions are weighted by the mean state population over the sample period and standard errors are clustered at the state level to allow for autocorrelation in the errors within states. Additionally, since the proportion of treated states is low (14 out of the 50 states plus D.C.), as a robustness check we also report in Table S2 p-values obtained from a non-parametric bootstrap procedure in which resampling was done at the state level and was stratified within the two intervention groups (law adopter or non-adopter).[5, 6]

For interpretation, we also present in Figure S3 two plots that graphically show the aggregate impact of the laws in each influenza-year in our sample. In the left panel, we plot two sets of predicted values that

were generated using the results from the estimation of our main two-way fixed effects model: (1) P&I mortality rates for each influenza year for states that adopted a law, assuming the actual timing of law adoption; and (2) the predicted counterfactual P&I mortality rates for the set of states that adopted a law, had they never adopted a law. Thus, the gap between the two lines represents the estimated reduction in the P&I mortality rate that occurred in treated states in each influenza year as a result of law adoption. We note that although our treatment effect represents a permanent level shift in the P&I mortality rate, in the figure the gap between the predicted average P&I mortality rate and the counterfactual grows over time due to the fact that more states are adopting laws (as shown by the light blue bars representing the share of population affected by the laws). The right panel calculates the implied number of deaths averted due to the laws in each year. To calculate this, we first estimated the number of predicted deaths and counterfactual predicted deaths for each state, year, and month (i.e., the estimates in the left panel, but at a more granular level). We then estimated the number of deaths averted in each state, year and month as the difference predicted deaths and counterfactual predicted. We then summed over all treated states and months to generate the aggregated, annual estimates displayed in the right panel.

Figure S3: Predicted Trends in P&I Mortality Rate per 100,000 Population, and Deaths Averted Due to Laws



Note: The left panel uses the main specification (described in Appendix Section 4.1 and estimated in the first row of Table 1) to generate two sets of predicted values: (1) P&I mortality rates per 100,000 population for each influenza year for states that adopted a law, assuming the actual timing of law adoption (solid line); and (2) the predicted counterfactual P&I mortality rates per 100,000 population for the set of states that adopted a law, had they never adopted a law (dashed line). Thus, the gap between the two lines represents the estimated reduction in the monthly P&I mortality rate that occurred in treated states each influenza year as a result of law adoption. The population share represents the percent of the US population subject to these laws in a given influenza-year. The right panel calculates the implied number of deaths averted due to the laws in each year. To calculate this, we first estimated the number of predicted deaths and counterfactual predicted deaths for each state, year, and month (i.e., the estimates in the left panel, but at a more granular level). We then estimated the number of deaths averted in each state, year and month as the difference predicted deaths and counterfactual predicted. We then summed over all treated states and months to generate the aggregated, annual estimates displayed in the right panel.

4.2 Role of Negative Weights in the Two-Way Fixed Effects Model

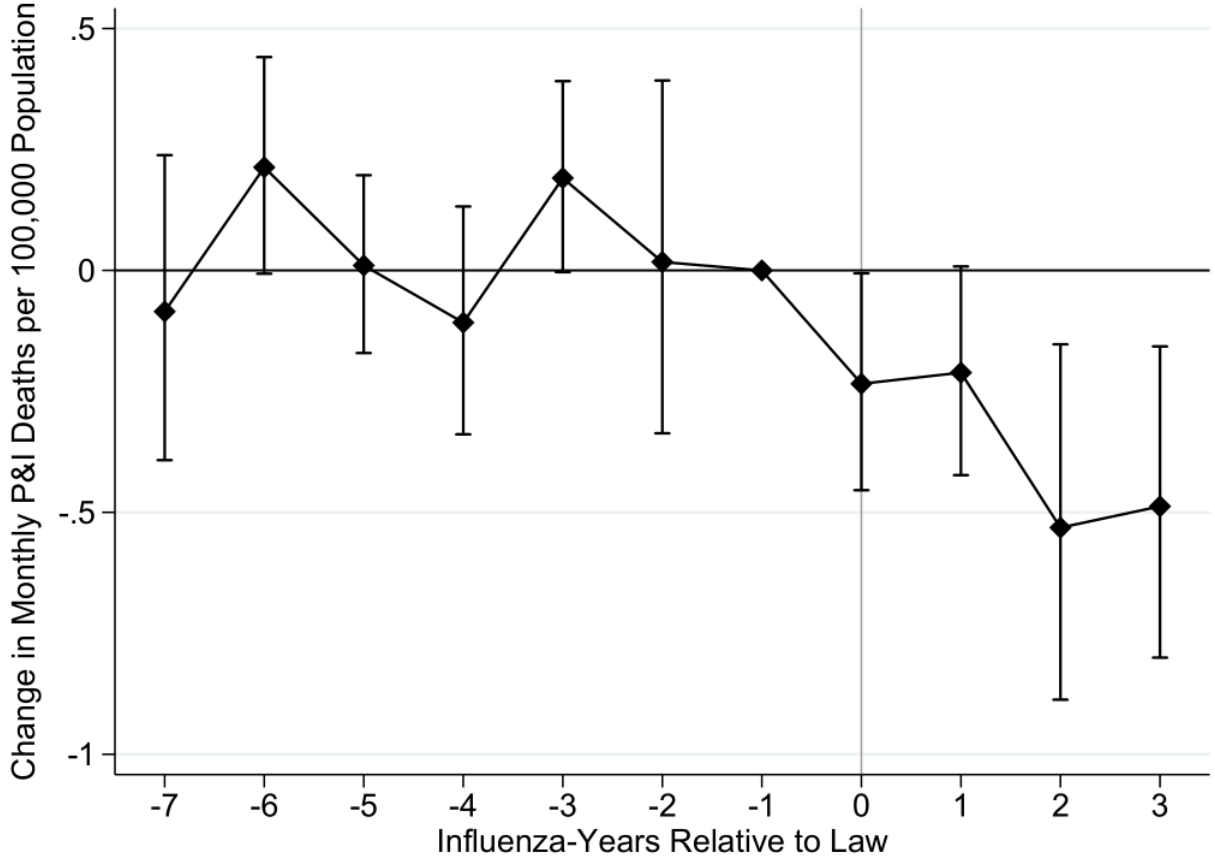
One important limitation of the standard two-way fixed effect model is that the resulting estimate of the average treatment effect on the treated (ATT) represents a weighted average of many treatment effects. In an empirical setting such as ours, in which states are adopting laws at different points in time, there is the potential for the weights to be negative, which can induce bias in the estimates of the average treatment effects.[7-9] To mitigate concern regarding the role of this bias in our estimates of the average treatment effect, we do several key things.

First, we implement a test for the potential influence of negative weights, as proposed by de Chaisemartin and d'Haultfoeuille (2020). Using the Stata command *twowayfeweights* we find that our ATT is the weighted sum of 112 estimated average treatment effects. Of those, 104 estimates receive a positive weight, and only 8 receive a negative weight. Additionally, the sum of negative weights is -0.021, representing a very small contribution to the overall ATT estimate, as the total of *all* weights sums to one. Thus, this test suggests that there is not likely to be substantial bias in our estimated ATT due to negative weights.

Second, we show that our findings are robust to the "DIDm" estimator proposed by de Chaisemartin and d'Haultfoeuille (2020), which is also not subject to the concern of negative weights. This result is reported in Table 2 in the main text. We note that we do not use the DIDm estimator as our main model, as it faces other empirical limitations. Specifically, the DIDM estimator only allows for a single treatment variable (i.e., we cannot interact the treatment with subgroups, as in our heterogeneity analysis). Furthermore, given the large number of time periods in the monthly data, we can only use the DIDm estimator on annual-level data in our setting. These issues make it difficult to test for statistical differences in the effects of treatment across groups (especially across peak and non-peak influenza months). Like our main specification, the DIDm estimator controls for state-specific linear time trends and indicators for the presence of state laws regarding vaccination in long-term care facilities and childcare centers.

Finally, we estimate the version of the DIDm estimator that estimates dynamic treatment effects over time, both before and after treatment, as suggested by de Chaisemartin and d'Haultfoeuille (2020). We present the findings from this analysis below in Figure S4.

Figure S4: Estimated Effects of State Laws on Pneumonia & Influenza Mortality Rates by Year Relative to Law Implementation, de Chaisemartin and d'Haultfoeuille (2020) DIDm Estimator



Notes: Reported estimates represent the differential change relative to the year prior to law adoption (t=-1) in the P&I mortality rate per 100,000 population, in adopting versus non-adopting states. Like our main specification, the DIDm estimator controls for state-specific linear time trends and indicators for the presence of state laws regarding vaccination in long-term care facilities and childcare centers. The seven-year pre-period (t=-7 to t=-1) and four year post-period (t=0 to t=3) were chosen because these were the maximum number of years of pre- and post-treatment data that were available for all states implementing laws. Bars represent 95% confidence intervals.

4.3 Trend Difference Specification

To quantitatively evaluate the extent to which we can rule out important differences in trends between treatment and control states, we estimate the following trend difference specification:[10]

$$Y_{st} = \gamma_0 + \theta EverAdopt_s \times t + \sum_{j=0}^{4} \gamma_j \, Law_{sy,j} + \delta_s + \delta_t + \varepsilon_{st}$$

As in our main models, Y_{st} is the natural log of the P&I mortality rate per 100,000 population in state s and year-month t; δ_s are state fixed effects; and δ_t are year-month fixed effects. The coefficients on $Law_{sy,j}$ are the estimates of the treatment effect. The $Law_{sy,j}$ variables represent a set of indicators for time relative to policy adoption. Note that in this model we only estimate post-treatment effects (i.e., $j = 0, j = 1, \ldots, j \geq 4$), as this allows us to estimate our trend difference parameter based on the pre-treatment differences. $EverAdopt_s$ is an indicator variable equal to 1 if a state ever adopts a hospital worker vaccination law and is 0 otherwise, thus θ represents the difference in slope between ever adopting and never adopting states during the period prior to adoption.

4.4 Heterogeneity Analyses (Table 1)

The version of the two-way fixed effects model that flexibly allows for differential effects of the laws for each year following law adoption is described by the following equation:

$$Y_{st} = \gamma_0 + \sum_{j=0}^{4} \gamma_j \, Law_{sy,j} + \beta X_{st} + \ \delta_s + \delta_t + \ (\delta_s \times t) + \ \ \varepsilon_{st}$$

In this model, the coefficients on $Law_{sy,j}$ are the coefficients of interest. The $Law_{sy,j}$ variables represent a set of indicators for time relative to policy adoption. For example, if $Law_{sy,j=2}$ equals one, this implies that a law was implemented two years prior to influenza-year y in state s. The years prior to law adoption are omitted as the reference group, so all estimates are measured as relative to the years prior to adoption of the laws. Note that because of the staggered timing of adoption of these laws across states, relative event time (i.e., years relative to law adoption) is identified separately from the common effects across all states in a given calendar month-year (captured by δ_t). For example, in this model, 1 year post adoption occurs in 2003 for Maine, 2006 for New Hampshire, 2008 for California and Tennessee, etc. Thus, we can estimate effects of the law, on average, for each year post-adoption, independently of secular trends over calendar time.

There are at least 4 years of post-policy data (i.e., $j = 0$ through $j = 3$) for all states adopting laws. The γ_j coefficient estimates are identified off of the same number of states for all coefficients $\gamma_{j=0}$ through $\gamma_{j=3}$, and as such these are the estimates we report. The variable at the extreme (i.e., $Law_{sy,j=4}$) is defined slightly differently, and represents 4 *or more* years post law adoption (and it is identified off of a smaller set of states compared to the other estimates).

To test for potential differences in the effect of the implementation of hospital worker influenza vaccination laws on log P&I mortality across peak versus non-peak influenza season we augment our baseline two-way fixed effects model as follows:

$$Y_{st} = \beta_0 + \beta_1 Law_{sy} + \beta_2 (Law_{sy} \times Season_t) + \ \ \beta_3 X_{st} + \ \delta_{s,season} + \delta_t + (\delta_{s,season} \times t \ ) + \ \varepsilon_{st}$$

In this specification, $Season_t$ is an indicator variable that is equal to 1 if a given state-month-year observation is during peak influenza season (defined as December through March), and is zero otherwise; all other variables are as defined in our baseline model. We used this definition for peak influenza months rather than a definition based on ex-post observed prevalence to ensure that the sample was not selected based on a variable that could have been affected by the treatment. Relative to the baseline model we have additionally included in this specification the following interaction term: $(Law_{sy} \times Season_t)$, such that β_2 captures the differential effect the state laws have on mortality during peak influenza season, relative to the effect on mortality during non-peak seasons (captured by β_1). Relative to the baseline model, we have also allowed controls to vary across peak and non-peak seasons. Specifically, $\delta_{s,season}$ are state-by-season fixed effects (in place of state fixed effects), the inclusion of which controls for all factors specific to a state and season (e.g., California during peak season) that do not vary over years.

($\delta_{s,season} \times t$) are state-by-season linear time trends (in place of state linear time trends), which allows for P&I mortality in each state to follow a different linear trend during peak versus non-peak influenza season. We note that the $Season_t$ indicator variable does not separately enter into the regression specification, as it would be colinear with the included month-year fixed effects. In this specification, the effect of the law on P&I mortality during non-peak influenza season is given by β_1; the effect on P&I mortality during *peak* influenza season is given by $\beta_1 + \beta_2$.

We next test for potential differences in the effect of the implementation of hospital worker influenza vaccination laws on log P&I mortality across non-elderly (18-64 year olds) versus elderly (65+ year olds) using the following similarly augmented regression model:

$$Y_{ast} = \beta_0 + \beta_1 Law_{sy} + \beta_2 (Law_{sy} \times Elderly_a) + \beta_3 X_{st} + \delta_{s,a} + \delta_{t,a} + (\delta_{s,a} \times t) + \varepsilon_{ast}$$

In this specification, in order to compare the effects of the law across different age groups, we define an age-group specific P&I mortality rate. Specifically, our outcome variable Y_{ast} is the P&I mortality rate for age group *a* (either non-elderly or elderly) in state *s* and year-month *t*. In this model, $Elderly_a$ is an indicator variable that is equal to 1 if a given P&I mortality rate observation is for the elderly age group, and is zero otherwise; all other variables are as defined in our baseline model. Relative to the baseline model we have additionally included in this specification the following interaction term: ($Law_{sy} \times Elderly_a$) such that β_2 captures the differential effect the state laws have on mortality among the elderly, relative to the effect for the non-elderly (captured by β_1). Relative to the baseline model, we have also allowed controls to vary across age groups. Specifically, $\delta_{s,a}$ are state-by-age fixed effects (in place of state fixed effects), the inclusion of which controls for all factors specific to a state and age group (e.g., age 65+ population in California) that do not vary over time. $\delta_{t,a}$ are time-by-age group fixed effects (in place of time fixed effects), the inclusion of which controls for all factors specific to a time period and age group (e.g., age 65+ population during January 2012) that do not vary across states. ($\delta_{s,a} \times t$) are state-by-age group linear time trends (in place of state linear time trends), which allows for P&I mortality in each state and age group to follow a different linear trend. We note that the $Elderly_a$ indicator variable does not separately enter into the regression specification, as it would be colinear with the included state-by-age fixed effects. In this specification, the effect of the law on P&I mortality for the non-elderly is given by β_1; the effect on P&I mortality for the elderly is given by $\beta_1 + \beta_2$.

Table S2: Change in Log Monthly Pneumonia and Influenza Deaths Associated with State Laws Regarding Hospital Worker Influenza Vaccination, with Bootstrapped P-Values

Outcome: Log P&I Mortality Rate (Monthly Deaths per 100,000 Population)*	**Difference in Log P&I Mortality per 100,000 Population (95% CI)**	**P-value**	**Bootstrap P-value§**
Average treatment effect on the treated across all months, ages, and states †	-0.025 (-0.047 to -0.004)	0.022	0.028
Heterogeneity across Years since Law			
Effect for first year of law (t=0)	-0.013 (-0.044 to 0.018)	0.365	0.410
Effect for first year after law (t=1)	-0.027 (-0.054 to 0.000)	0.041	0.054
Effect for second year after law (t=2)	-0.032 (-0.062 to -0.002)	0.029	0.034
Effect for third year after law (t=3)	-0.055 (-0.092 to -0.018)	0.002	0.003
Heterogeneity across Peak vs. Non-Peak Months †			
Effect for Non-Peak Months	-0.019 (-0.043 to 0.005)	0.099	0.121
Effect for Peak Months	-0.038 (-0.063 to -0.014)	0.002	0.002
Differential Effect for Peak vs. Non-Peak ‡	-0.019 (-0.036 to -0.003)	0.017	0.022
Heterogeneity across Elderly vs. Non-Elderly			
Effect for Age <65	-0.006 (-0.035 to 0.024)	0.655	0.696
Effect for Age 65+	-0.026 (-0.047 to -0.006)	0.013	0.013
Differential Effect for Age 65+ vs. Age <65	-0.020 (-0.047 to 0.007)	0.110	0.141

* P&I Mortality Rate denotes the Pneumonia & Influenza mortality rate, calculated as the number of deaths per month with any pneumonia/influenza diagnosis per 100,000 population. Age-specific mortality rates were calculated using age-specific populations. There were 9,672 state-year-month observations in the sample.

† Estimates test for differences in the effects of the laws across groups. For example "Peak vs. Non-Peak Months" tests for the difference between the effect of the laws during peak months and the effect of the laws during non-peak months (i.e., Peak minus Non-Peak).

‡ Peak Months refer to December-March, and Non-Peak months refer to April-November.

§ Bootstrapping was achieved through a non-parametric bootstrapping procedure with 200 iterations. Resampling was done at the state level and was stratified within the two intervention groups (state law or no state law).

Appendix Section 5. Falsification Analyses

It is possible that there are unobservable state time-varying confounders, such as other vaccination policies or attitudes regarding vaccination, that are driving the observed changes in P&I mortality. To examine this possibility, we estimate two-way fixed effects models analogous to our baseline specification using data from NIS-Child, NIS-Teen, and BRFSS, to examine the association of the hospital worker influenza vaccination laws with changes in infant, adolescent, and adult influenza and pneumococcal vaccinations, respectively. Details on these data sources are discussed in Appendix Section 1, above. Given that hospital workers make up approximately 2 percent of the adult population, we would not expect to detect changes in the overall rate of vaccination coverage among adults if the laws only influenced uptake among hospital workers and were not correlated with other unobservable changes that broadly impact adult vaccination. Similarly, hospital worker vaccination laws should have no effect on infant and adolescent vaccination rates.

Specifically, for analyses of infant and adolescent vaccination, we use individual level vaccination measures from NIS-Child and NIS-Teen, respectively, and estimate the following multivariate logistic regression model:

$$Y_{isy} = \beta_0 + \beta_1 Law_{sy} + \beta_2 X_{sy} + \delta_s + \delta_y + \delta_s \times \mathrm{y} + \varepsilon_{isy}$$

where Y_{isy} is an indicator variable that is equal to 1 if child *i*, residing in state *s*, received a given vaccine (either influenza or pneumococcal for infants, or influenza, for adolescents) during influenza-year *y.* As in our main specification, β_1 is the coefficient of interest, where Law_{sy} is a binary variable indicating the presence of any state law regarding hospital worker influenza vaccination in state s and influenza-year y. X_{st} represents a vector of indicators for the presence of state laws regarding influenza vaccination for workers in long term care facilities and childcare facilities.

As in our main specification, δ_s are state fixed effects, which control for observable and unobservable time-invariant differences in vaccination across all states. δ_y are influenza-year fixed effects, which allow for differences in vaccination uptake across influenza years that are common to all states. Finally, $\delta_s \times \mathrm{y}$ are state-specific linear time trends at the annual level, which allow for time-varying differences across states to evolve linearly over time. For these specifications, time fixed effects and trends are at the year level (as opposed to year-month level) because this is the level of granularity available in the data. We separately estimate models for infants and for adolescents, as the determinants of vaccination for those groups are likely different. Regressions are weighted using the relevant sample weights provided by NIS-Child and NIS-Teen and standard errors are clustered at the state level. For these regressions, all samples are limited to influenza-years with a well-matched vaccine (similar to the main analysis), though the results are not sensitive to this restriction. The results from these analyses are presented in Appendix Table S3 below.

For analyses of adult vaccination, we construct measures of adult vaccine coverage at the age-state-influenza-year level from BRFSS data and estimate the following multivariate linear regression model:

$$Y_{sy} = \beta_0 + \beta_1 Law_{sy} + \beta_2 X_{sy} + \delta_s + \delta_y + \delta_s \times \mathrm{y} + \varepsilon_{sy}$$

where Y_{sy} is the natural log of the vaccination coverage rate for a given vaccine (influenza or pneumococcal) and age group in state *s* during influenza-year *y*. Due to the fact that the hospital worker vaccination laws are associated with reductions in mortality only among the elderly, in addition to considering the overall adult vaccination coverage rate, we also separately consider vaccination coverage rates for non-elderly (18-64 years old) and elderly (65+ years old) adults. All other variables in the specification are as defined previously. Regressions are weighted by mean state population, and standard errors are clustered at the state level. The sample is limited to influenza-years with a well-matched vaccine (similar to the main analysis), though the results are not sensitive to this restriction. The results from these analyses are presented in Appendix Table S4 below.

In Table S6 we test whether the laws had an impact for any non-P&I cause of death. These estimates were calculated using a Poisson model: the outcome was the total number of deaths for 38 cause of death categories and the sample was limited to peak influenza months only. The Poisson model was used due to the fact that there were many state-year-month observations with zero deaths for certain causes. Similar to the main specification, the following equation was used to calculate the monthly change in the mortality rate in levels: $(\exp(Monthly\ Change\ in\ Logs) - 1) \times Baseline\ Mean$. The cause of death categories follow the 39-cause recode utilized in the National Vital Statistics System mortality files, and estimates for deaths due to one category (Syphilis) were omitted because the model failed to converge. The sample was limited to influenza-years in which the 39-cause recode was available and deaths were categorized using ICD10 codes (1999/00-2016/17). The P&I category represents deaths with any diagnosis (underlying diagnosis or up to 20 secondary diagnoses) for P&I. Other categories represent deaths with an underlying cause of death for the other 37 categories, and exclude deaths with a secondary cause of death for P&I. P-values were corrected for multiple hypothesis testing using the Bonferroni and Simes procedures.[11,12] The 95% confidence intervals account for multiple hypothesis testing and were calculated the formula described in Altman and Bland (2011, BMJ).[13] The confidence intervals utilized the Simes procedure because valid confidence intervals cannot be calculated when the p-value equals one (which frequently occurs with the Bonferroni correction)

Table S3: Effects of State Laws Regarding Hospital Worker Influenza Vaccination on Influenza Vaccination Coverage Rates Among Adolescents and Infants

Outcome:	Baseline Mean in Adopting States	Change in Child's Probability of Vaccination (95% CI)	P-value
NIS-Teen			
Influenza Vaccination, age 13-17 years	0.193	-0.011 (-0.029 to 0.007)	0.240
NIS-Child			
Influenza Vaccination, age 19-23 months	0.470	-0.011 (-0.053 to 0.031)	0.610
Pneumococcal Vaccination, age 19-35 months	0.681	-0.005 (-0.009 to 0.019)	0.449

Notes: Each observation in these analyses represents an individual child, and the outcome variables are indicator variables equal to 1 if the child had received the specified vaccine by the time of survey and are equal to zero otherwise. The NIS-Teen regression includes 162,601 individual observations from the 2008-2017 sample waves; The NIS-Child regression includes 53,964 individual observations aged 19-23 months, and 185,653 individual observations aged 19-35 months from the 2003-2017 sample waves. More details on the data and construction of the outcome variables is given in Appendix Section 1. All samples are limited to influenza-years with a well-matched vaccine (as in the main analysis), though the results are not sensitive to this restriction. Regressions include state fixed effects, influenza-year fixed effects, state linear time trends, and the following time-varying covariates: long-term care vaccination laws, and childcare vaccination laws. Estimates were obtained via logistic regression and marginal effects assumed the mean values for all covariates. Survey weights were used to weight the regressions, and standard errors were clustered at the state level to allow for within-state dependence over time.

Table S4: Effects of State Laws Regarding Hospital Worker Influenza Vaccination on Influenza and Pneumococcal Vaccination Coverage Rates

Outcome:	Baseline Mean in Adopting States	Change in State Adult Vaccination Rate (95% CI)	P-value
Influenza Vaccination Rate			
All Adults	0.328	0.000 (-0.005 to 0.006)	0.909
Age 18-64	0.259	0.000 (-0.004 to 0.005)	0.840
Age 65+	0.689	-0.009 (-0.033 to 0.016)	0.472
Pneumococcal Vaccination Rate			
All Adults	0.219	-0.001 (-0.004 to 0.001)	0.307
Age 18-64	0.14	-0.000 (-0.001 to 0.001)	0.700
Age 65+	0.604	-0.010 (-0.025 to -0.005)	0.200

Notes: Each regression includes 765 state-by-year observations. The sample is limited to influenza-years with a well-matched vaccine (as in the main analysis), though the results are not sensitive to this restriction. Regressions include state fixed effects, influenza-year fixed effects, and the following time-varying covariates: long-term care vaccination laws, and childcare vaccination laws. Observations are at the state-year level, and estimates are obtained via linear regression using the log vaccination rate as the outcome. Similar to the main estimates in Table 1, changes in levels (i.e., the probability of vaccination) were calculated using following equation: $(\exp(Change\ in\ Logs) - 1) \times Baseline\ Mean$. Regressions were weighted by mean state population and standard errors were clustered at the state level to allow for within-state dependence over time.

Appendix Section 6. Sensitivity Analyses

Table 2 shows results from a series of sensitivity analyses based on the all-months all-age model of Table 1.

The first set of estimates reported vary only controls for potential time-varying confounders in our regression. Our main estimates include state and year-month fixed effects as well as covariates and state-specific linear time trends, corresponding to the most conservative estimate of the association with mortality. In Panel A of Table 2, we show how the estimates change when covariates and state trends are omitted.

Next, in Panel B of Table 2 we show the estimate for the full set of sample years, including the five influenza-years with match rates below 50% which were dropped from our main specification. This results in a smaller estimated effect which is not statistically significant at the 5% level (p=0.076). We also show estimates dropping H1NI influenza-years.

In Panel C of Table 2 we show that our main estimate is similar when estimated with a Poisson regression model designed to deal with count data (implemented via Stata command *xtpoisson)*. For this model our outcome variable is a count of the number of P&I deaths in a given state-month, as opposed to the natural log of the mortality rate. For comparability with the other coefficient estimates derived from log-linear models (which were transformed to be interpreted as the difference in *levels* between states with influenza laws and the counterfactual), we computed the implied difference in P&I mortality rates from the Poisson regression. Specifically, we used the Poisson regressions to construct the predicted (i.e., expected) number of deaths (and then mortality rates) for treated states after the laws were in place, both using the actual treatment status and a counterfactual in which the laws were never implemented. We then calculated the population-weighted average difference in predicted mortality rates between treated state-month observations and the counterfactual.

Finally, in Panel D of Table 2 we show that our results are robust to estimating the DIDm estimator proposed by de Chaisemartin and d'Haultfoeuille (2020). Because this estimator requires the use of annual data, we also provide in Panel D an estimate from our main two-way fixed effect model that similarly uses annual data. As discussed in Appendix Section 4.2 above, the DIDm estimator is not subject to the concern of negative weights, as with the standard two-way fixed effect estimate of the average treatment effect.[7]

Appendix Section 7: Calculation of the E-value

As a test for the sensitivity of our results to unmeasured confounding, we calculate the E-value for several specifications of our two-way fixed effects model.[14] These results are presented in Table S5 below. Specification (1) is based on our main specification in the paper, which uses data at the monthly level. This specification also uses the raw standard deviation of the outcome variable in the E-Value formula. Because the E-value is intended to assess the potential effect of unmeasured confounders, Haneuse, VanderWeele and Arterburn (JAMA, 2019) point out that it is appropriate to adjust for measured confounders in calculating the standard deviation of the outcome.[15]

In specification (2), the raw standard deviation is replaced by "residualized" standard deviation, which is the standard deviation of the residuals from a regression of the outcome on all control variables (state fixed effects, time fixed effects, state-specific linear time trends, and indicators for other laws). Specifications (3) and (4) use annual data in place of monthly data. There is much greater variation in the outcome in monthly compared to annual data. Thus, using annual data substantially limits the scope for unmeasured confounding to bias the estimates. Because the treatment (state laws) is at the annual level, the main estimates do not rely on the use of monthly data and the main estimates are very similar in specifications that use monthly versus annual data. The reason monthly data was used in the main specification was only to allow the flexibility to test for differences across different months (i.e., peak versus non-peak months).

We calculate an E-value of 3.419 using the estimated residual standard deviation from our two-way fixed effects linear regression on annual data. This implies that for an unobserved time-varying confounding variable to be fully responsible for the association we estimate between state laws and P&I mortality, that unobserved variable would need have a 3.4-fold association with *both* P&I mortality, after adjusting for state and year and all control variables, and with state law passage.

Table S5: Test for Sensitivity of Findings to Unmeasured Confounding

Specification	Regression Coefficient	Regression Coefficient SE	SD(Outcome)	E-Value and its 95% Confidence Interval
(1) Monthly Data, Raw SD	-0.0253	0.0107	0.2817	1.389 (1.327 to 1.448)
(2) Monthly Data, Residualized SD	-0.0253	0.0107	0.1878	1.514 (1.457 to 1.570)
(3) Annual Data, Raw SD	-0.0295	0.0113	0.2070	1.536 (1.484 to 1.587)
(4) Annual Data, Residualized SD	-0.0295	0.0113	0.0387	3.419 (3.348 to 3.490)

Notes: Specification (1) is based on our main specification in the paper, which uses data at the monthly level. This specification also uses the raw standard deviation of the outcome variable in the E-Value formula. In specification (2), the raw standard deviation is replaced by "residualized" standard deviation, which is the standard deviation of the residuals from a regression of the outcome on all control variables (state fixed effects, time fixed effects, state-specific linear time trends, and indicators for other laws). Specifications (3) and (4) use annual data in place of monthly data.

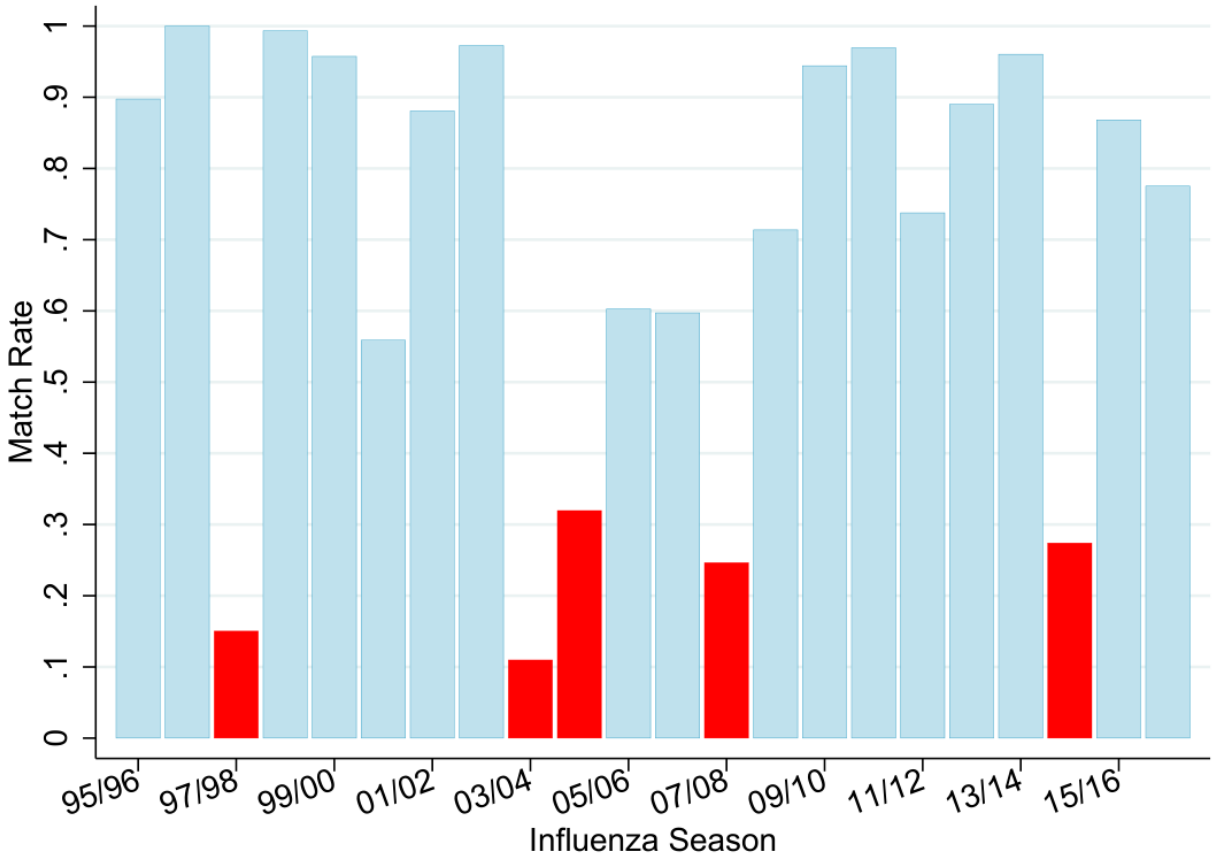
Appendix Section 8. Details on Construction of Figure 1A

National level influenza vaccination coverage rates by occupation and influenza-year are constructed from the IPUMS National Health Interview Survey (NHIS) database, 1997-2017 survey waves.[16] We focus on vaccination coverage rates among employed individuals, and thus restrict our sample to the set of individuals who report their employment status in the past 1 to 2 weeks as "working" or "with job, but not at work." Individuals are classified into one of the following three mutually exclusive industry groups based on the provided 1995 industry of employment code: hospital sector (1995 industry code: 1910), non-hospital health services (1995 industry code: 1920), or non-health services (all others). We focus on the vaccination rates of hospital employees in this figure because all fourteen laws applied to them. Since six of the fourteen laws also applied to healthcare workers outside of hospitals, however, for the comparison group of non-affected workers we exclude all healthcare workers.

Our measure of influenza vaccination is derived from the self-reported variable that asks if the individual received an influenza vaccine in the past 12 months. Given the 12 month look back period of this survey question and the fact that influenza vaccines are most commonly administered during the months of September to December,[1] we assign individuals surveyed during quarters 1-3 of year *t* to the influenza-year that began in year *t-1*, and assign individuals surveyed during quarter 4 of year *t* to the influenza-year that began in year *t*. Influenza vaccination coverage rates for each influenza-year are constructed as the share of surveyed individuals in a given industry group that reported receiving the influenza vaccine during the relevant influenza-year, weighted by the provided NHIS sample adult person weights. As state identifiers are not provided in the publicly available NHIS, our vaccination coverage rates for each occupation are at the national level.

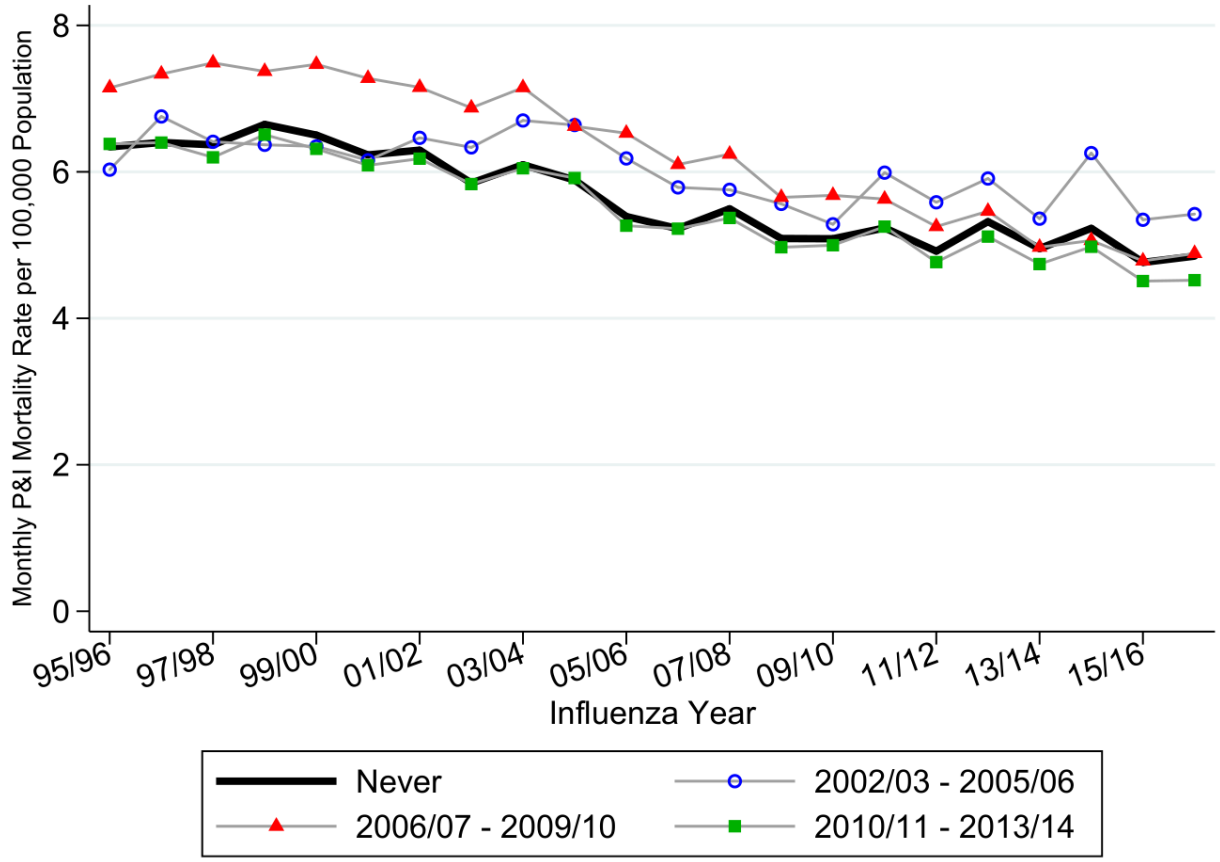
The variable capturing exposure of hospital workers to a hospital influenza vaccination law in a given influenza-year is constructed using data from the IPUMS Current Population Survey (CPS) database, 1997-2017 survey waves.[17] The CPS contains information on both occupation and state of residence (but not influenza vaccination), thus for each year we can construct a measure of the share of all hospital workers in the nation that are exposed to a state vaccination law. In order to construct this variable we first create weighted counts of individuals at the state and year level who report that they were employed last year and report their industry of employment as hospitals (1990 industry code: 831). For each year we then calculate the fraction (number of treated hospital workers)/(total number hospital workers), in which individuals surveyed in year *t* are considered treated by the law if their state had adopted a law that was in effect for the influenza-year starting in year *t*.

Figure S5: Vaccine Match Rates by Influenza Season



Note: The vaccine match rate is the percentage of virus strains characterized by the CDC that are included in each the season's vaccine. Seasons with match rates below 50% are dropped from the sample in the main specification and are highlighted here in red. Note that the match rate is not well defined for the 2008/09 influenza season due to the H1N1 pandemic. Specifically, the 2008/09 vaccine was well-matched to the seasonal epidemic for that season (illustrated on the plot), but the H1N1 pandemic arrived in April and a subsequent monovalent vaccine was developed (not illustrated on the plot). The pandemic strain was included in the 2009/10 seasonal influenza vaccine, and the vaccine was well-matched to the strains that circulated during the seasonal epidemic (illustrated on the plot).

Figure S6: Mean P&I Mortality Rate per 100,000 Population Across States, Grouped by Year of Implementation of Hospital Worker Influenza Vaccination Law



Notes: Each line represents the mean monthly P&I mortality rate per 100,000 population for the group of states that adopted a hospital worker influenza vaccination law during the period denoted in the figure legend.

Table S6: Change in Monthly Deaths Associated with State Laws Regarding Hospital Worker Influenza Vaccination by Cause of Death

Outcome: Number of Deaths by Cause	Change in monthly P&I deaths per 100,000 population (95% CI)	Bonferroni Corrected P-Value	Simes Corrected P-Value
Pneumonia & Influenza	-.250 (-.465 to -.035)	.023	.023
Cerebrovascular Disease	-.134 (-.278 to .010)	.134	.067
Peptic Ulcer	-.011 (-.025 to .003)	.383	.117
Accidents – Motor Vehicle	-.094 (-.211 to .023)	.467	.117
Chronic Lower Respiratory Disease	-.079 (-.199 to .041	1	.198
Cancer – Cervix/Ovary	.016 (-.008 to .039)	1	.198
Diabetes	-.068 (-.175 to .039)	1	.216
Ischemic Heart Disease	-.377 (-.972 to .217)	1	.216
Heart Disease - Other	-.104 (-.267 to .060)	1	.216
Hypertensive Heart Disease	-.054 (-.139 to .031)	1	.216
Cancer – Urinary Tract	-.020 (-.057 to .016)	1	.277
Conditions Pregnancy/Childbirth	.003 (-.003 to .010)	1	.277
Alzheimer's	.040 (-.066 to .147)	1	.47
Tuberculosis	-.002 (-.009 to .004)	1	.47
Chronic Liver Disease and Cirrhosis	-.018 (-.070 to .033)	1	.493
Essential Hypertension	.009 (-.030 to .049)	1	.655
Other External Causes	-.025 (-.159 to .110)	1	.732
Accidents - Other	.051 (-.227 to .329)	1	.732
HIV	-.015 (-.100 to .069)	1	.732
Cancer – Trachea/Bronchus/Lung	-.035 (-.273 to .204)	1	.789
Other Diseases	-.039 (-.356 to .277)	1	.819
Cancer - Leukemia	.005 (-.036 to .047)	1	.819
Cancer - Pancreas	-.008 (-.075 to .059)	1	.819
Congenital Disease	-.003 (-.030 to .024)	1	.819
Assault (Homicide)	-.017 (-.158 to .123)	1	.819
Sudden Infant Death Syndrome	.004 (-.026 to .033)	1	.819
Cancer - Prostate	.005 (-.032 to .042)	1	.819
Nephritis, etc.	-.010 (-.093 to .073)	1	.819
Conditions in the Perinatal Period	-.006 (-.056 to .043)	1	.819
Cancer - Stomach	-.006 (-.055 to .043)	1	.819
Cancer - Lymphoma	.004 (-.027 to .035)	1	.819
Cancer - Colon	-.006 (-.108 to .095)	1	.908
Unclassified Diseases	.027 (-.756 to .810)	1	.95
Circulatory System - Other	.001 (-.037 to .039)	1	.961
Intentional Self-Harm (Suicide)	-.002 (-.079 to .075)	1	.961
Cancer - Other	.004 (-.142 to .150)	1	.961
Atherosclerosis	.001 (-.035 to .037)	1	.961
Cancer - Breast	-.001 (-.034 to .032)	1	.961

Note: These estimates were calculated using a Poisson model and the outcome was the total number of deaths for 38 cause of death categories and the sample was limited to peak influenza months only. The Poisson model was used due to the fact that there were many state-year-month observations with zero deaths for certain causes. Similar to the main specification, the following equation was used to calculate the monthly change in the mortality rate in levels: $(\exp(Monthly\ Change\ in\ Logs) - 1) \times Baseline\ Mean$. The cause of death categories follow the 39-cause recode utilized in the National Vital Statistics System mortality files, and estimates for deaths due to one category (Syphilis) were omitted because the model failed to converge. The sample was limited to influenza-years in which the 39-cause recode was available and deaths were categorized using ICD10 codes (1999/00-2016/17). The P&I category represents deaths with any diagnosis (underlying diagnosis or up to 20 secondary diagnoses) for P&I. Other categories represent deaths with an underlying cause of death for the other 37 categories, and exclude deaths with a secondary cause of death for P&I. P-values were corrected for multiple hypothesis testing using the Bonferroni and Simes procedures.[11,12] The 95% confidence intervals account for multiple hypothesis testing and were calculated the formula described in Altman and Bland (2011, BMJ).[13]The confidence intervals utilized the Simes procedure because valid confidence intervals cannot be calculated when the p-value equals one (which frequently occurs with the Bonferroni correction).

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