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DISCUSSION PAPER SERIES

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ABSTRACT

Positive Health Externalities of Mandating Paid Sick Leave*

A growing economic literature studies the optimal design of social insurance systems and the empirical identification of welfare-relevant externalities. In this paper, we test whether mandating employee access to paid sick leave has reduced influenza-like-illness (ILI) transmission rates as well as pneumonia and influenza (P&I) mortality rates in the United States. Using uniquely compiled data from administrative sources at the state-week level from 2010 to 2018 along with difference-in-differences methods, we present quasi-experimental evidence that sick pay mandates have causally reduced doctor-certified ILI rates at the population level. On average, ILI rates fell by about 11 percent or 290 ILI cases per 100,000 patients per week in the first year.

JEL Classification: H23, H75, I12, I14, I18, J22, J38, J58

Keywords: sick pay mandates, population health, flu infection, negative externalities

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Introduction

The United States is one of very few developed countries that does not provide universal access to paid sick leave (Heymann et al. 2010; Schliwen et al. 2011, OECD 2020). In the U.S., employers have traditionally provided paid sick leave voluntarily, leading to highly unequal provision. Among low-income, part-time and service-sector workers, the majority cannot take paid sick leave (Bureau of Labor Statistics 2019). Moreover, many workers do not even have the right to take *unpaid* sick leave as the only federal law, the Family and Medical Leave Act (FMLA), solely covers workers who worked 1,250 hours in the last 12 months in businesses with more than 50 employees (United States Department of Labor 2019).

Over the past decade, however, several dozen cities and a dozen states have passed sick pay mandates. Sick pay mandates allow employees to first accumulate, and then use, a credit of sick days. For each 30 to 40 hours of work, workers earn 1 hour of paid sick leave which they can use for either their own or their relative’s sickness. Moreover, if the days needed for recovery exceed the personal credit of sick days, employees have the right to take unpaid sick days. Recent research has not found evidence that these sick pay mandates significantly reduce employment or wage growth (Pichler and Ziebarth 2020).

Economic labor models clearly suggest—and empirical evidence clearly shows—that employees will take more sick days when sick leave generosity increases (Johansson and Palme 2005; Maclean et al. 2020). This overall employee labor supply response can be decomposed into a change in presenteeism (“working sick”) as well as shirking behavior (Pichler and Ziebarth 2017). Supported by empirical evidence that working sick is a relevant real-word phenomenon (Susser and Ziebarth, 2016, DeRigne et al. 2016, Piper et al. 2017, Asfaw et al .2017, CDC 2020a), it follows that fewer contagious employees will work sick when they have access to paid sick leave. This paper estimates whether mandating paid sick leave, and thus increasing sick pay coverage, has a causal impact on doctor-certified influenza-like illness (ILI) rates as well as pneumonia and influenza (P&I) mortality rates at the population level.[1]

The World Health Organization estimates that, worldwide, seasonal influenza is responsible for 3 to 5 million cases of severe illnesses and up to 650 thousand respiratory deaths per year (WHO 2018).[2] A growing body of economic research studies how infections relate to human behavior, their socio-economic determinants and the effectiveness of public policies (Philipson 1996, Gilleskie 1998, Rossin-Slater et al. 2013, Ward 2014, Adda 2016, Oster, 2018, Carpenter and Lawler 2019). Presenteeism is an important channel through which infections spread, particularly in the United States where one quarter of all employees did not have access to paid sick leave in 2019 (BLS 2019). A lack of paid sick leave could also be a risk factor for the spread of the novel coronavirus. The bipartisan Families First Coronavirus Response Act provides up to two weeks of emergency sick leave due to the coronavirus and was just signed into law (United States Congress, 2020)

This paper empirically tests for the population health effect of increased access to paid sick leave. We use uniquely compiled and officially reported ILI cases from the Weekly U.S. Influenza Surveillance Report (ISR) as well as P&I mortality data from the National Center for Health Statistics Mortality Surveillance System. We collect all reported and officially confirmed ILI cases and deaths at the week level for 49 U.S. states and the District of Columbia from 2010 to 2018.[3]

[1] For an exact definition of these outcomes, please see below.

[2] In the United States and in the European Union, influenza vaccination rates have stagnated below 50 percent and, because of varying strains, the effectiveness of influenza vaccines has varied between 10 and 60 percent since 2004 (Blank et al. 2009, CDC 2018b, c).

[3] Florida did not report ILI data and is thus excluded from our analysis.

We then exploit the variation across states and over time in the implementation of state-level sick pay mandates over the past decade. Connecticut was the first state to pass a sick pay mandate in 2012.[4] District of Columbia (2014), California, Massachusetts, and Oregon (all 2015), Arizona, Vermont, Washington (all 2017), as well as Rhode Island and Maryland (2018) followed more recently.[5]

To our knowledge, this is one of the first papers to empirically examine the causal effects of sick pay mandates at the state level.[6] It is the first paper to estimate the impact of the mandates on official, doctor-certified ILI cases and P&I mortality rates. It is thus only partly comparable to—but reinforces—previous research. In the only study of sick pay mandates on infection rates that we are aware of, Pichler and Ziebarth (2017) use variation in the implementation of *city-level* mandates and find that *Google Flu* ILI rates decreased by a significant 16 percent as a result of the mandates in the first year. Google Flu contains measurement error that potentially downward bias the estimates which could explain potential differences in effect sizes. Another potential explanation for differences in effects sizes is that this study evaluates the impact when entire states, and not just cities, mandate sick leave, which relates to the epidemiological literature on herd immunity and how infections spread (Fine et al. 2011, Plans-Rubío 2012) as well as the economic literature on the positive externalities of vaccinations (Carpenter and Lawler 2019, White 2020).

[4] The District of Columbia also initially adopted a policy in 2008 that excluded temporary and tip employees, though this law was expanded to include these workers in 2014.

[5] Sick pay mandates in Michigan and New Jersey were implemented after the end of our data sample and are thus not evaluated here. More details on the specifics of each state law are in the Appendix in Table A1.

[6] Maclean, Pichler, and Ziebarth (2020) estimate the effects of state-level sick pay mandates on coverage rates, utilization and labor costs using the National Compensation Survey.

However, as we will show below, the first year effect sizes identified in this paper are very comparable to Pichler and Ziebarth (2017).

Methodologically, this paper exploits variation in the implementation of the mandates allowing us to compare outcomes in the ten “treatment states” to outcomes in control states without a mandate. As such, our difference-in-differences models compare (the difference in) ILI activity and P&I mortality in treatment versus control states at the same time and their relative difference before and after the enforcement of the mandates. Additionally, we illustrate the dynamic effects over time in “event studies” (cf. Wing et al. 2018). We find clear evidence that sick pay mandates reduce ILI infection rates at the population level. Our estimates show that the mandates reduced ILI activity by about 11 percent on average in the first year. We also find that the reduction in ILI activity increases cumulatively over time during the first three years after the law’s implementation. Consistent with the reduction in ILI infection rates and with the fact that P&I mortality is highly concentrated among the elderly (CDC 2020b), we find negative point estimates for the P&I mortality rates, but these estimates are much smaller in size and not statistically significant.

Overall, our findings provide novel and important insights into the optimal design of social insurance programs (Chetty and Finkelstein 2013; Hendren 2017; Luttmer and Samwick; 2018, Goodman-Bacon 2018; Fadlon and Nielsen 2019, Cabral et al. 2019). Specifically, these findings contribute to empirical work that strives to identify negative or positive externalities and interaction effects of social insurance programs, as these yield evidence for possible welfare improving program adjustments (Borghans et al. 2014; Lalive et al. 2015; Leung and O'Leary 2020). The results also provide a case study of how labor market policies can improve population health and reduce the spread of diseases by incentivizing sick employees to call in sick instead of working sick. In self-reports, 55 percent of American workers without sick pay coverage report having worked sick with a contagious disease (Kotok 2010). Compared to workers with sick pay coverage, those without sick pay coverage are also more likely to report to work sick and have financial difficulties (DeRigne et al. 2016, 2019). Our research shows that a relatively modest mandate with potentially bipartisan support can induce economic incentives that improve population health on a broad basis (cf. OECD 2020).

The last statement unexpectedly became true amidst the coronavirus pandemic. As one of the first policy responses, on March 18 2020, the Senate passed the bipartisan Families First Coronavirus Response Act (FFCRA) with 90-8 votes and President Trump signed it the same day. FFCRA contains two weeks of emergency sick leave funding for reasons related to the coronavirus. Moreover, it extended coronavirus-related paid family and medical leave by 10 weeks at two-thirds the employee's regular wage. However, the purpose of the paid leave is strictly limited to the coronavirus, exempts firms with more than 500 employees, and expires at the end of 2020 (U.S. Department of Labor 2020).

Data Collection and Measure of Influenza Activity

Our main data source is the Weekly U.S. Influenza Surveillance Report (ISR) produced by the Centers for Disease Control and Prevention (CDC 2019). The CDC publishes the weekly ISRs to inform the public about current influenza activity in the United States. Statistics are by state and type of illness, that is, influenza-like-illnesses (ILI) in outpatient settings, laboratory confirmed influenza-associated hospitalizations, and influenza/pneumonia mortality. We export weekly ILI activity by state from October 2010 to July 2018.

In particular, we use two measures of influenza activity as outcome variables in this paper. First, we use reported ILI rates from outpatient settings as the broadest available measure of influenza activity. Each week, about 2,600 participating providers in all U.S. states submit their confirmed ILI cases along with the number of outpatient visits to the CDC. The CDC's Influenza Division then prepares and publishes the weekly statistics and reports. ILI are defined as those where the patient presented with a fever (temperature of 100°F or greater), a cough and/or sore throat, and with no other known cause of illness other than influenza. One advantage of this measure is that it is a relatively comprehensive measure of influenza activity. Moreover, the statistical properties allow us to measure influenza activity in all states (except Florida) and all weeks of the year.[7] We normalize the number of medically attested ILI cases by the number of total patients seen for any reason among participating outpatient healthcare providers as reported by the states.[8] Overall, providers report more than 47 million outpatient visits each year (CDC 2019).

Second, we use reported pneumonia and influenza (P&I) mortality rates as a relatively narrow but very severe health outcome measure. Because influenza is rarely documented on death certificates, we examine P&I mortality (rather than influenza mortality alone).[9] Mortality rates are

[7] In our main models, we include observations from Washington D.C. but show that results are similar when these observations are excluded. In fact, excluding D.C. is our preferred specification because the introduction of the first D.C. sick pay mandate is not covered by our study period. We also include all calendar months, although influenza activity mainly occurs between October and May. When we exclude observations between June and September, months when influenza activity is low, the effect sizes are larger. See Figure 4 below for an effect heterogeneity analysis by month-of-the year.

[8] Neither the number of total patients (denominator) nor number of participating providers were affected by the implementation of paid sick pay mandates (see Appendix Table A3). Moreover, as the number of providers varies over time, normalizing by the patients seen (by these providers) is necessary to avoid changes in our ILI measures due to a changing number of providers reporting.

[9] There are multiple reasons for this. First, U.S. states are not required to report influenza illnesses or deaths among people age 18 and older. Second, people often die of influenza-related complications rather than influenza alone. In these instances, influenza is rarely documented on death certificates. Third, many people who die of influenza are not tested for influenza.

also reported at the state-week level. These numbers are based on ICD-10 multiple cause of death codes and come from the National Center for Health Statistics (NCHS) mortality surveillance data.[10] The NCHS collects death certificate data from state vital statistics offices for all deaths occurring in the United States and reports those numbers to the CDC.

Table A2 in the Appendix shows descriptive statistics. When averaged across all states and all years, in our main sample with a total of 20,319 state-week observations, we count 1.9 ILI cases per 100 patients. Due to the seasonality in influenza activity, the mean varies from 3.4 ILI cases per 100 patients during typical peak months of the influenza season (January and February) to 0.7 ILI cases per 100 patients between June and September. Moreover, the P&I mortality rate per 100,000 population varies between 0 and 4.2 over all state-week observations. The mean is 1.1 and the standard deviation 0.4.

Because it has been well documented that presenteeism and infections vary over the business cycle (Pichler 2015), we also collect (seasonally adjusted) data on the state-level unemployment rate by month of the year as reported by the Bureau of Labor Statistics (2018), see Table A2. We control for this variable in our econometric specifications. As shown in the Results section, the findings are very robust to controlling for the monthly unemployment rate.

In addition, we collect a number of variables on state-level characteristics from additional sources. Due to the fact that these variables could be predictors of infection spread, we control for them in some specifications. In particular, as shown in Table A2, these variables include the state population share with health insurance coverage (Kaiser Family Foundation 2020a), whether the state expanded Medicaid at the time (Kaiser Family Foundation 2020b), the population share

[10] P&I does not have to be the main or underlying cause of death. Up to 20 different multiple causes of death codes are reported.

above the age of 65 (CDC 2020c), whether the state had an influenza vaccination mandate for children (Child Care Influenza Immunization Action Coalition 2020), as well as the precipitation level (NOAA 2020). Moreover, we compile data on the share of the state population who receives a flu vaccine by month of the year for all years and separately for people ages 18 to 64 as well as those 65 years and older (CDC 2020c).[11]

While the ISR is the most comprehensive and most suitable data source for our research (that we are aware of), it has drawbacks (Wallinga 2018). First, our outcome measure of ILI cases per 100 patients only includes patients who saw a participating outpatient medical care provider. However, such reporting—conditional on having seen a doctor—would only be a threat to our identification strategy if it was correlated with the implementation of sick pay mandates. If patients recover at home instead of working sick as a result of the mandates, it would not affect the statistic. If patients went to the doctor instead of working sick as a result of the mandates, it would bias our reform impact estimate *toward zero*, and we would obtain a lower bound estimate that would still establish the public health benefits of sick pay mandates. However, as seen in Panel A of Table A3, the number of patients was not significantly affected by the mandates.[12] Thus, a possible bias is unlikely to be large.

Second, with respect to our outcome variable of P&I deaths per 100,000 population, states are not legally required to report influenza mortality specifically, and people often die of influenza-

[11] For the summer months of July and August, these data are not available due to the lack of flu vaccinations in these months.

[12] Unfortunately, we do not observe the universe of diagnoses and thus cannot specifically investigate possible changes in the composition of doctor visits as a result of the mandates. For example, it could be that ILI-related doctor visits decreased while doctor visits related to preventive care and check-ups increased, leaving the total number of visits unchanged (see, for example, DeRigne et al. 2017). Also, we note that the estimates in Panel A of Table A3 are fairly imprecise and the point estimate around 10% of the mean. If the mandates lead to a general increase in doctor visits, it could imply that we overestimate the decrease in ILI rates.

related complications. Therefore, influenza is rarely documented on death certificates. Moreover, because influenza tests are most accurate within a week of the onset of illness, many people who die of influenza are never actually tested for influenza. In conclusion, P&I deaths are very likely underreported, and this underreporting likely varies by state (and by season). However, in our econometrics models, we control for state fixed effects and 406 week-of-the-year fixed effects. These control variables net out time-invariant differences in reporting behavior across U.S. states, as well as common seasonal effects. While we have no formal method to test it, it is unlikely that reporting of death certificates systematically varies with the introduction of sick pay mandates.

Estimating Equation

If the identifying assumptions hold, our statistical model will identify the causal effect of implementing state-level sick pay mandates on ILI activity and P&I mortality in subsequent weeks and years. Because the data cover a substantial number of post-reform periods (for some states), we are also able to distinguish between short-, medium- and long-term effects.

To estimate a causal effect of the mandate, we run a difference-in-differences (DD) model that uses the ten states (including the District of Columbia) with sick pay mandates as treatment states and the remaining states as control states. Because the ten states implemented the mandates in a staggered fashion in different calendar years and different weeks within these calendar years, the design allows us to control for monthly and annual flu dynamics. We also implement the Bacon decomposition to test for how much the timing of the treatment as well as treatment heterogeneity matters in our context (Goodman-Bacon 2018). As we rely on ten treatment states that implemented the mandates at different points in time, it is rather unlikely that an unobservable exists which would be correlated with the mandate implementation and also affects flu dynamics at the same time. The absence of such a systematically correlated unobservable is an important assumption for causal identification. Moreover, the "common time trends assumption" states that outcome dynamics for treatment and control states need to follow a common time trend which would have continued in post-treatment periods in the counterfactual scenario without the treatment.

These identification assumptions are important because the DD model benchmarks the change in influenza activity in the treatment states with mandates against the change in influenza activity in the control states without mandates. Taking the first difference—comparing the outcome before and after states implemented a mandate—and subtracting the second difference over the same time period from control states, yields the DD model; formally:

$$y_{sw} = \beta Treat_s \times Law_w + \gamma_s + \delta_w + Unemp_{sm} + \varepsilon_{sw} \tag{1}$$

where y_{sw} stands for the ILI rate or P&I mortality rate in state s and week-of-year w. Seasonality in influenza activity is taken out by $12\, weeks_{2010} + 7\, years \times 52\, weeks_{2011-2017} + 30\ weeks_{2018} = 406$ week-of-the-year fixed effects (δ_w). These are 406 dummy variables that net out the average U.S. wide influenza activity in a specific week. State fixed effects γ_s control for structural, time-invariant, differences in influenza activity among states (Dalziel et al. 2018). We also control for the unemployment rate in state s and month-of-the-year m by including $Unemp_{sm}$. In our main specification, we estimate this model by Ordinary Least Squares.[13]

The binary treatment indicator $Treat_s$ equals one if a state s implemented a sick pay mandate by the end of the observation period, while the binary time indicator Law_w equals one for

[13] Taking the logarithm of the outcome variable to consider the normal distribution assumption provides very similar estimates.

calendar weeks in which the mandate was in effect. Then, the interaction term between the treatment and time indicator $Treat_s \times Law_w$ yields the DD estimator and the causal effect of the mandates on ILI rates and P&I mortality rates (Angrist and Pischke 2009, 2010).

We cluster the error term ε_{sw} at the state level (Bertrand et al. 2004) and weight all regression models with the state populations of the given year (see Table A2). Weighting ensures that more populous states receive a larger weight than less populous states (Solon et al. 2015).

To assess the plausibility of the identification assumption in this setting with ten treatment states, it is standard routine to plot so called "event studies." To produce an event study, the binary time indicator Law_w in Equation (1) is replaced by a continuous time indicator counting the weeks to and from the date when the mandate was implemented, $\sum_{t=-156}^{156} Law_t$, or from three years before up to three years after the mandate's implementation. The reference point is the week before the law was officially enacted.

When plotted as an event study, the DD model thus translates into a visual representation of six years of weekly coefficient estimates. The weekly estimates are all normalized with respect to the implementation of the sick pay mandates, or "event time." The visual representation of an event study allows the researcher to assess the credibility of the main identifying assumption for causal effects, namely the common time trends assumption, conditional on all control variables in Equation (1). An increasing or decreasing trend in the outcome variable prior to the mandates' implementation in the states that passed mandates would be a clear indication of a violation of this assumption. The event study design also allows differentiating between short-, medium-, and long-term policy effects by studying the dynamics of the weekly post-mandate coefficient estimates with their 95 percent confidence intervals.

Additionally, we produce maximum likelihood estimates for a spatial error model; that is, we allow the errors of the model to be spatially correlated (Colella et al. 2019). For these models, we have slightly fewer observations as the estimation requires a balanced sample.

Results

Table 1 shows the results for our first outcome variable, the ILI rate. Each of the four columns in each panel represents one DD model as in Equation (1). Panel A shows the results when we estimate high-frequency models at the state-week level; and Panel B shows the results when we aggregate and estimate the model at the state-month level. Panel C shows the results when we allow the error term of Equation (1) to be spatially correlated between states at the state-month level.

[Table 1: Impact of Sick Leave Mandates on ILI Rates]

The findings can be summarized as follows: First, according to the state-week level models in Panel A, when states mandate paid sick leave, ILI cases decreases by 0.53 per 100 patients per week, where the point estimate is statistically significant at the five percent level. Relative to the baseline ILI rate of 1.9 confirmed cases per 100 patients, this represents a decrease of 28 percent (column [1]). In other words, on average, we find 5,300 fewer ILI cases per one million patients per week at the population level as a result of the sick pay mandates in Arizona, California, Connecticut, Maryland, Massachusetts, Oregon, Rhode Island, Vermont, Washington, and the District of Columbia. Note, however, that the identified effect is an average over all states and available post-mandate periods which differ by state (for example, for Arizona, Maryland and Rhode Island, we observe only up to one post-mandate year).

Second, comparing the coefficient estimates for the four models in columns (1) to (4) of Panel A, we find that the estimate is very robust to either controlling for the unemployment rate (column [2]), or to controlling for a set of additional state-level background variables such as the uninsurance rate, Medicaid expansions, the population structure in the state, child vaccination requirements, and precipitation levels (column [3]). The estimate is also robust to excluding Washington D.C. from the estimates (column [4]). Controlling for the unemployment rate could be relevant if sick pay laws were primarily and systematically passed in times of low (or high) unemployment. Controlling for the additional set of state-level background variables could be relevant because the population structure, weather or health policies could be possibe determinants for the spread of diseases. Excluding Washington D.C. could be relevant as the state first passed a mandate in 2008 that excluded temporary and tipped employees. This mandate is outside our period of observation. In 2014, D.C. then expanded coverage to these employee groups. As seen, neither the unemployment rate, nor potential population, weather or health policy controls, or the exclusion of Washington D.C. play a confounding role in our estimates.

Third, comparing the estimates in Panel A to Panel B, we find that aggregating the data at a higher temporal level also produces very robust estimates. The point estimate in column (4) of Panel B is -0.0058 (or 0.58 fewer ILI cases per 100 patients), and thus almost identical to the -0.0055 estimate in column (1) of Panel A. The fact that data aggregation does not alter the findings indicates that seasonal effects are sufficiently controlled for in the model in Panel A. Moreover, it implies that short-term decreases in the ILI rate are not compensated for by over proportional increases in subsequent weeks. In other words, the decrease in the ILI rate persists over time and is not a short-term phenomenon because infections are simply postponed by a few weeks.

Fourth, comparing the estimates in Panel A and B to those in Panel C, we find that our results are robust to allowing for spatial correlation of the error term (Colella et al. 2019). The point estimate in column (1) of Panel C indicates that the number of ILI cases decreases by 0.56 per 100 patients. These results are very similar to the estimates in Panels A and B.

Next, we exclude treatment states entirely and assign a randomized pseudo treatment status among all other remaining states, which had not passed mandates. This falsification check also tests for spurious pattern in our data structure. Figure 1 shows the distribution of the placebo estimates (from 800 regressions) along with the coefficient estimate from column (4) in Table 1. The figure provides further evidence that we have not picked up confounding trends.

[Figure 1: Placebo Estimates]

Table 2 shows the result of the "Bacon decomposition" (Goodman-Bacon, 2018b). The Bacon decomposition is based on a balanced sample, thereby ensuring that changes in our state-week level sample composition do not drive the treatment effect. More important, the Bacon decomposition decomposes the treatment effect in a multi-treatment group DD setting into several two-group, two-period estimates. Because the treatment is staggered over time in a multi-treatment group DD setting, states that implemented mandates later serve as control states for the states that implemented mandates earlier. Further, states that always or never had a mandate during the period of observation can serve as control groups. The Bacon decomposition shows how much of the treatment effect is due to timing and these different control states ("Timing Groups") as compared to a “clean” control state where mandates were never implemented over the entire period of observation (“Never vs. timing”). As seen in Table 2, in our setting, the treatment effect stemming from a comparison of treatment states with states that were never treated is -0.0055 and has 82 percent of the total weight, whereas the treatment effect from differential treatment timing is much smaller (-0.0016) and has only 8 percent of the total weight. Moreover, the “within” variation only contributes 0.0003 to the treatment effect, reinforcing that controlling for the unemployment rate barely alters the coefficient estimate.

[Table 2: Bacon Decomposition]

A complementary check to test for whether pre-treatment trends exist (conditional on the covariates) is to plot event studies. Event studies help to visually assess whether there is evidence that the mandates were a reaction to changing trends in influenza activity. They also help to assess whether there is evidence for anticipation effects, that is, that companies changed their sick leave policies in anticipation of the new laws. Importantly, event studies also allow for a dynamic visual representation of the treatment effect over time. Although aggregating the data at the monthly level did not yield evidence for nonlinear dynamic effects; nevertheless, it could be that the short-term effects differ from the longer-term effects.

[Figures 2 and 3: Event Studies Showing the Impact of Sick Leave Mandates on ILI Rates]

Figure 2 shows the event study for the state-week level data and Figure 3 shows the event study for the state-month level data. First, the data patterns between the two figures are almost identical. However, the aggregation at the monthly level evens out some of the seasonal spikes and yields a smoother but quantitatively identical picture.

Second, as the x-axis shows pre-mandate influenza activity for up to three years (156 weeks or 36 months), it allows for a thorough assessment of whether the laws were implemented *in reaction to* changes in influenza activity. If this were the case, one would observe an increasing or decreasing influenza activity prior to the mandate's implementation. As seen, however, there is no evidence for such endogenous policy implementation. In the three years leading up to all ten state-level mandates, the solid black line fluctuates closely around the zero line on the y-axis and the dashed 95 percent confidence intervals include the zero line over basically the entire pre-period.

Third, after the implementation of the mandates, as indicated by the vertical black line on the x-axis, influenza activity trends downwards over virtually the entire three post-mandate years. The decrease in influenza activity appears to be linear and becomes statistically significant after a little more than one year, after which the ILI rate further falls by about 1 case per 100 patients, where the baseline rate is 1.9 cases per 100 patients.[14] The decrease in the first post-mandate year, which is identified by all 10 treatment states is 11 percent or 290 fewer ILI cases per 100,000 patients per week.

This cumulative decrease in ILI rates is consistent with the fact that a large share of about 40 percent of employees immediately gain the right to take unpaid sick leave (IMPAQ 2017). Maclean et al. (2020) find that the mandates increased the probability that employers provide paid sick leave by 13 percentage points from a baseline of 66 percent. Those workers first must earn and accumulate sick time before they can take it. Maclean et al. (2020) show that sick leave

[14] Because of the 311 point estimates in Figure 2, our statistical power is limited; not all weekly point estimates are statistically significant at conventional levels. However, the results in Table 1 show that the overall ILI rate decreased at a significance level of less than five percent.

utilization also increases linearly over the post-mandate years, which matches up closely with the evidence presented here.

Finally, Figures A1 and Table A4 in the Appendix show almost identical event studies and regression output when running a model that includes DC (Figure A1) as well as balancing the panel and shortening the event times to -13,…,0,…+13 weeks (Table A4). The findings are robust to these alterations and are also robust to including state time trends.

[Figure 4: Treatment Effect Heterogeneity by Month-of-the-Year]

Figure 4 plots treatment effect heterogeneity by month-of-the-year. The numbers on the x-axis indicate the month of the year, that is, "1" stands for January and 12 stands for December. The y-axis displays the treatment effect along with dashed 95 percent confidence intervals. Each point estimate stems from a separate model as in Equation (1) that is solely run on a sample that includes flu dynamics from the respective month-of-the-year. That means that the leftmost estimate compares flu dynamics in January after a state enacted a mandate to flu dynamics in the same state in January before the mandate was enacted, and to the differences in January flu dynamics across calendar years for control states without a mandate. As seen in Figure 4, in line with our priors, we find that the treatment effect is largest during the flu season and the months of January to April. The months of October and November—the beginning of some flu seasons—also show negative point estimates that are statistically significant at the 5 percent level, whereas the effects between May and August are negative but not statistically significant at conventional levels.[15]

[15] A possible explanation for the zero effect in December could be holiday season and the fact that the number of overall workdays are relatively low in December. Additionally, people may travel for leisure more in December which may contribute to the spread of diseases.

[Table 3: Impact of Sick Leave Mandates on P&I Death Rates]

Table 3 shows the results for our second outcome variable, the P&I mortality rate. The structure of the table follows the structure of Table 1. In Panel A, we report our four main regressions at the state-week level in columns (1) to (4). In Panel B, we report the results at the state-month level; and in Panel C, we additionally correct for spatially correlated error terms.

As seen, and in line with the findings in Table 1, in Panel A we consistently find negative point estimates of size 0.02 or about two percent of the mean. However, none of the four estimates is statistically different from zero. For example, the 90 percent confidence intervals of the model in column (4) of Panel A range from -0.051 to + 0.004 or from -4.5 percent of the mean to + 0.3 percent. The p-value is 0.165. In other words, only with a statistical probability of 83.5 percent, we can say that the state-level sick pay mandates decreased the P&I mortality rate by about two percent.

The eight additional models in Panel B and Panel C of Table 3 reinforce these estimates. Here we aggregate the data at the state-month level and hence find estimates that are four times as large (as the denominator is the state population and remains constant when aggregating).[16] However, all eight point estimates have negative signs, are very similar to the weekly estimates in percent terms, and are not statistically different from zero at conventional levels.

[Figure 5: Impact of Sick Leave Mandates on P&I Death Rates]

[16] This is not the case in Table 1, as the ILI case rate normalizes by *the number of patients* seen, which also roughly quadruples when aggregating at the monthly level. We cannot normalize the ILI cases by the population as the number of participating providers (and thus reported cases) varies from week to week and state to state.

Figure 5 shows the event study at the weekly level for our P&I mortality outcome. Not surprisingly and in line with Table 3, the pre-mandate point estimates fluctuate very closely and unsystematically around the zero line, whereas a majority of post-mandate estimates are negative but not statistically different from zero.

In the last robustness check, we assess whether our results are robust to assigning non-treated states with cities that have sick pay mandates partial treatment values, where we assign partial values according to the population living in these cities as a share of the state population. California, Maryland and Washington include cities that enacted mandates prior to the state-level mandates during our time period. Illinois, Minnesota, New York and Pennsylvania include cities that enacted mandates but the state had not enacted a mandate as of July 2018 (Asfaw et al. 2017, A Better Balance 2020). As seen in Table A5, although the point estimates decrease slightly in a non-statistically significant fashion, all results—for ILI rates and P&I mortality—remain robust when assigning the DD indicator $Treat_s \times Law_w$ partial values from 0.01 (CA) to 0.42 (NY) for time periods when solely city-level mandates were enacted.

Finally, one could argue that one limitation of this study is that we are unable to investigate underlying mechanisms hinting at *why* ILI rates fall. Possible channels could include, but are not are not limited to, (a) an increase in influenza vaccination as employees have more opportunities to seek health care, or (b) reduced co-worker or customer infections because sick employees can call in sick instead of working sick. Another possibility is (c) that the effect operates through sick children who can be supervised by their parents instead of being sent to childcare when parents gain access to paid sick leave.

While we are indeed unable to formally test channels (b) and (c), in Table A6, we re-run our main models but use the share of the state population that receives a flu vaccination every month as the dependent variable. Specifically, in Panel A, we investigate vaccinations among the working age population and, in Panel B, we investigate vaccinations among those above 65 years. As seen, none of the eight coefficient estimates are statistically different from zero and effect sizes are quite small. We thus conclude that we do not find evidence for a significant role of channel (a) and that the mandates do not lead to significant increases in flu vaccination rates.

Discussion and Conclusion

This research is the first to use official, medically attested data on influenza activity and mortality to test whether access to sick leave can reduce the spread of diseases and improve population health. We leverage quasi-experimental statistical methods that do not require randomized laboratory or field experiments, but nevertheless allow for the identification of causal effects. Specifically, under certain assumptions, one can identify the causal effect of sick pay mandates on influenza activity by comparing outcomes in states that implemented mandates to control states that did not implement mandates over the same time period.

Our findings show that mandating employers to provide employees with access to sick leave can reduce negative externalities through lower flu infection rates. In the first year after the mandates' implementation, ILI rates fell on average by 11 percent in states that provided employees with the possibility to earn and take sick days, relative to control states that did not. The impact of the law is monotonically and linearly increasing over time for those states who were the first to pass such employer mandates (California, Connecticut, DC, Massachusetts and Oregon). In addition, we find consistently negative point estimates for the impact of state-level sick pay mandates on pneumonia and influenza (P&I) mortality. Our preferred specification suggests that sick pay mandates could have reduced P&I mortality by about two percent. However, this estimate is not statistically different from zero at conventional levels. One explanation for the smaller mortality effect size could be that sick pay mandates primarily reduce presenteeism behavior among the working population and thus primarily reduce the transmission of influenza among the working population, whereas P&I deaths are primarily experienced by older people; 70 to 85 percent of flu related deaths occur in people above age 65 (CDC 2020b).

Overall, our findings are consistent with companion research showing that employees take sick days when they obtain access to sick leave. Specifically, Maclean et al. (2020) use BLS data from the National Compensation Survey and find that the mandates increase coverage rates by 13 percentage points from a baseline of 66 percent. They also find that newly covered employees take roughly two additional sick days as a result of the mandates. This equals 2,500 additional sick days per week per city of 1 million residents.[17] The reduction in ILI cases that we find in Table 1 translates into about 204 per week for a city with one million residents, where the baseline is 731 ILI cases. Although the percentage reduction may appear large, it is very plausible that 2,500 additional sick days translate into 204 prevented, doctor diagnosed, ILI cases.[18] In fact, the implied transmission rate is very much consistent with the findings and assumptions in the field of epidemiology (Cooper et al. 2006).

In conclusion, this paper contributes to the empirical literature on optimal social insurance designs by identifying positive health externalities of employer mandates. In addition to reducing

[17] This assumes that 50 percent of the population work and yields 0.13*500,000*2 days/52 weeks=2,500. The calculated reduction in sick cases assumes that every resident has about two doctor visits per year, or 38,461 patients per week. The ILI rate per 38,461 patients is 731.

[18] Our robustness checks that consider city-level mandates and estimate the impact on doctor visits do not yield much evidence that our estimates are substantially downward biased. However, even if the true decrease was, for example, 250 ILI cases instead of 204 (that is 22.5% more), our findings would still appear very reasonable. In that case, every tenth additionally sick day taken would have prevented one doctor-confirmed ILI case.

labor market inequalities, this research shows that mandating employers to give employees the opportunity to earn paid sick leave reduces ILI rates, and possibly also P&I mortality rates—although the effect size of the latter is much smaller and not statistically significant at conventional levels. Reduced ILI activity not just implies direct and immediate population health benefits, but also indirect benefits through avoided *in utero* infections of pregnant women, reduced prematurity and better long-term labor market outcomes of uninfected newborns (Almond 2006, Schwandt 2018). Moreover, economic studies have not found evidence that sick pay mandates negatively affect employment and wages in local labor markets (Pichler and Ziebarth 2020). This paper shows that sick pay mandates are effective in preventing the spread of infectious diseases that lead to hospitalizations and even death for at risk groups. While we did not find evidence that the mandates had an impact on flu vaccination rates, the precise mechanisms of how infections predominately spread warrant more research.

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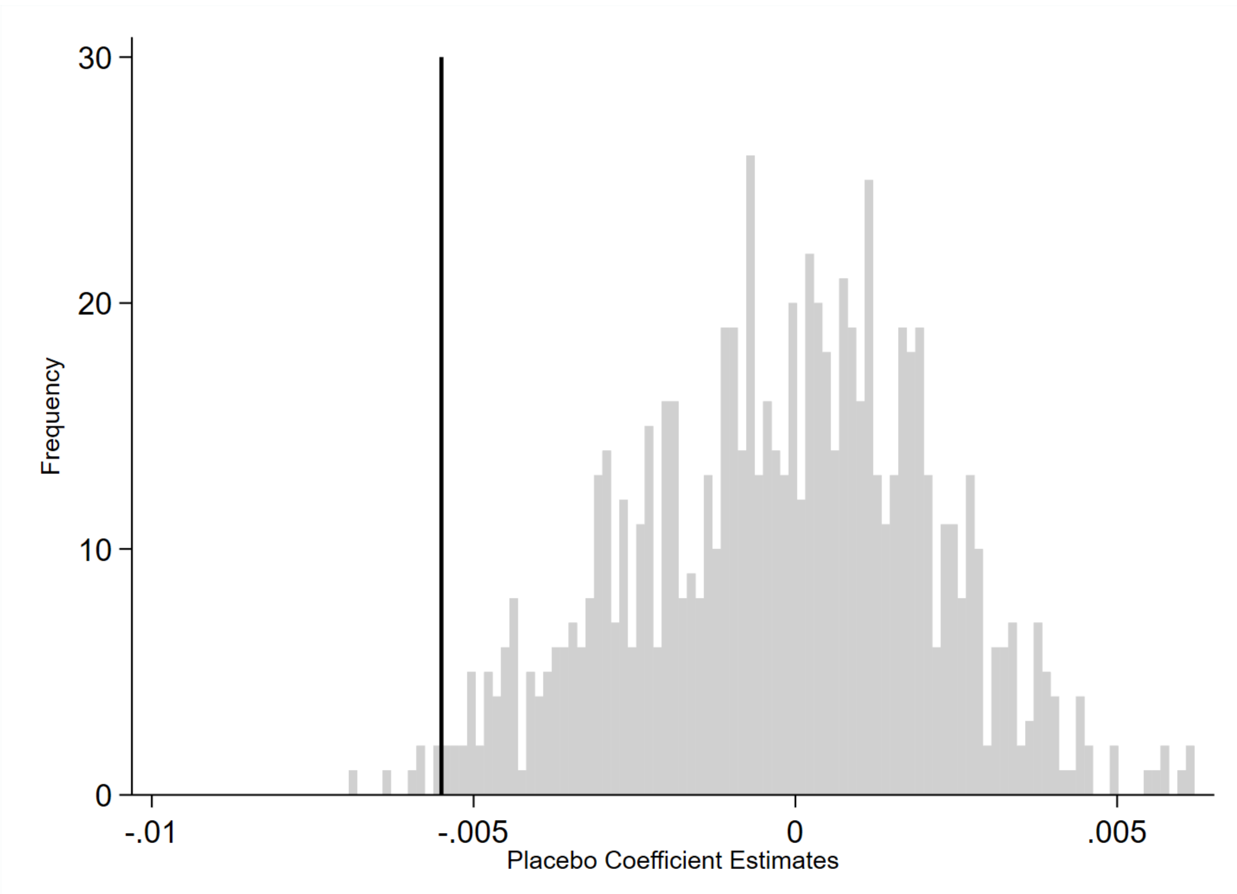
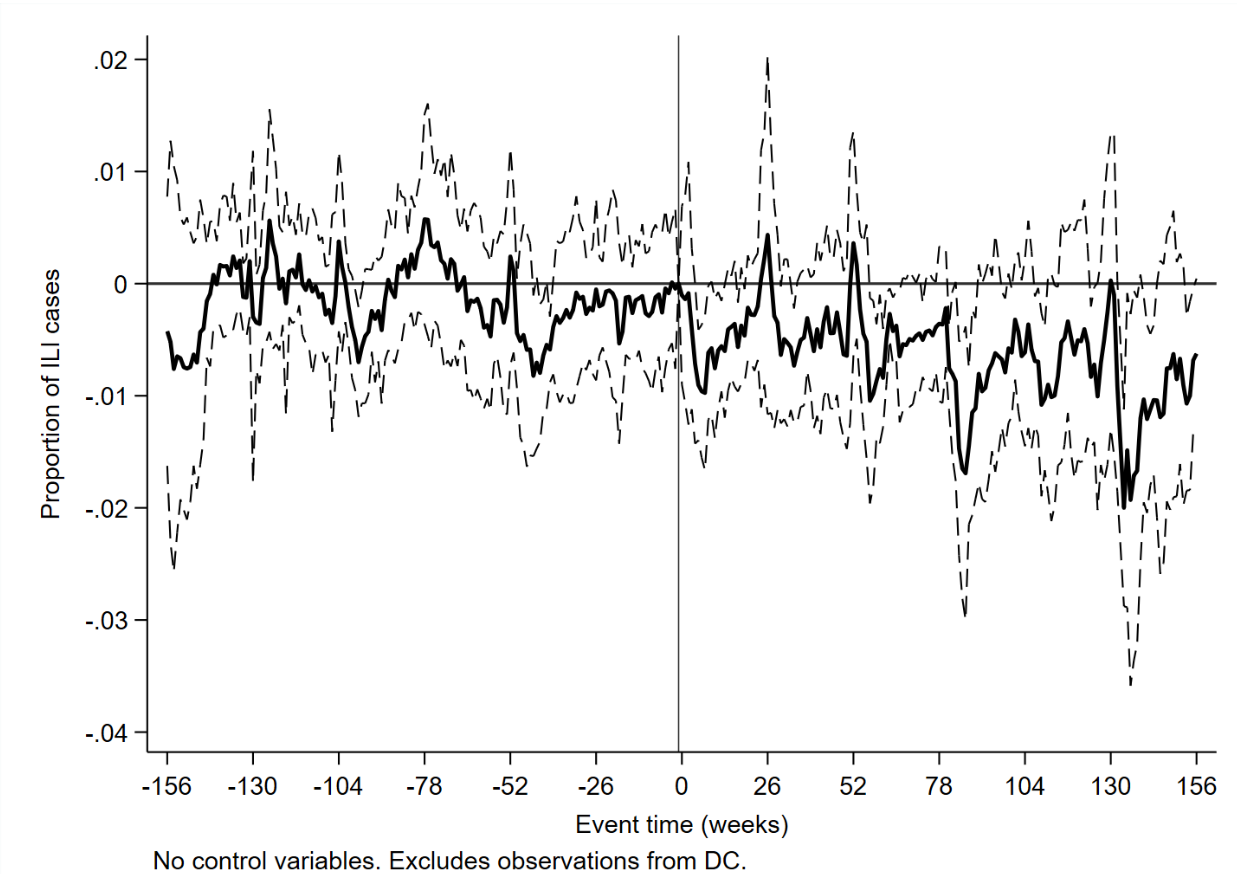
Tables and Figures

Figure 1. Distribution of Placebo Estimates of the Impact of Mandates on ILI Rates



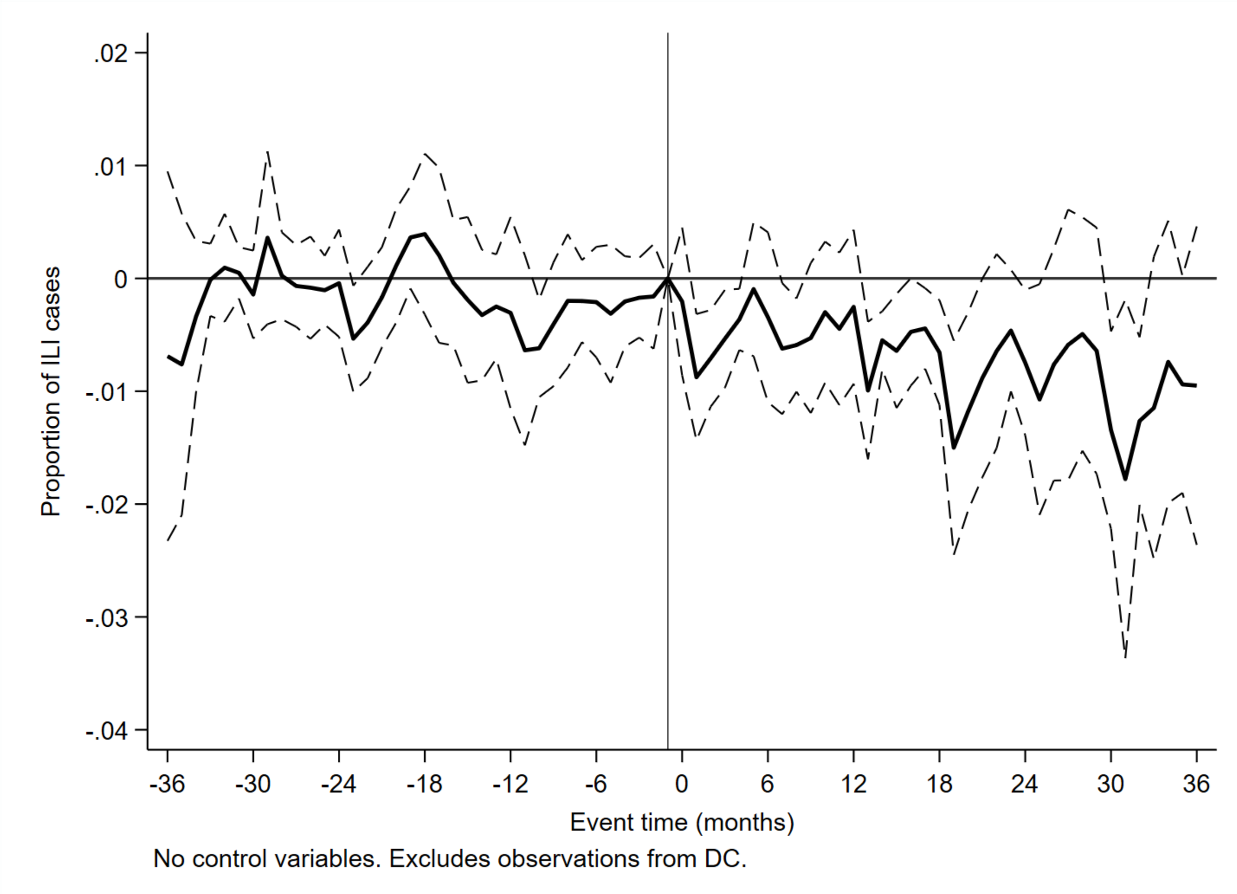
Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report. This figure plots the distribution of the estimated placebo regressions (n=800) that excluded treatment states and randomly assigned pseudo treatment states. The vertical black line denotes the coefficient estimate (-0.0055) from the main specification (Table 1, Panel A, Column 4).

Figure 2. Weekly Event Study of the Impact of Sick Leave Mandates on ILI Rates



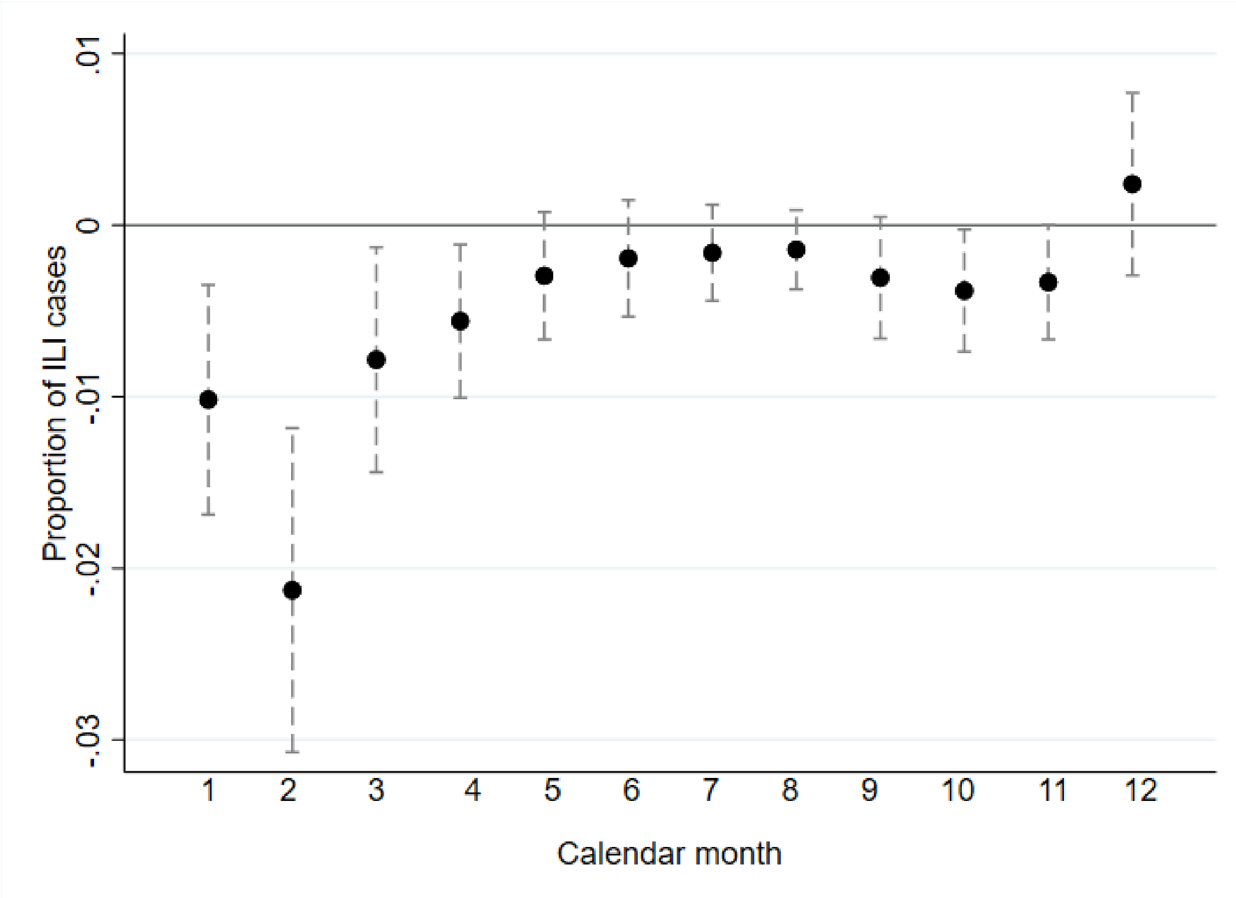
Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report. The figure is the equivalent event study of Table 1, Panel A, column (3) or Equation (1) with $\sum_{t=-156}^{156} Law_t$ plotted graphically, see main text for details. The dashed lines represent 95% confidence intervals. The x-axis illustrates the normalized time before and after the mandates became effective; the vertical line indicates the week prior to the effective date. The y-axis illustrates the change in the ILI rate. The model excludes observations from Washington D.C. See Figure A1 for an alternative specification that includes observations from Washington D.C.

Figure 3. Monthly Event Study of the Impact of Sick Leave Mandates on ILI Rates



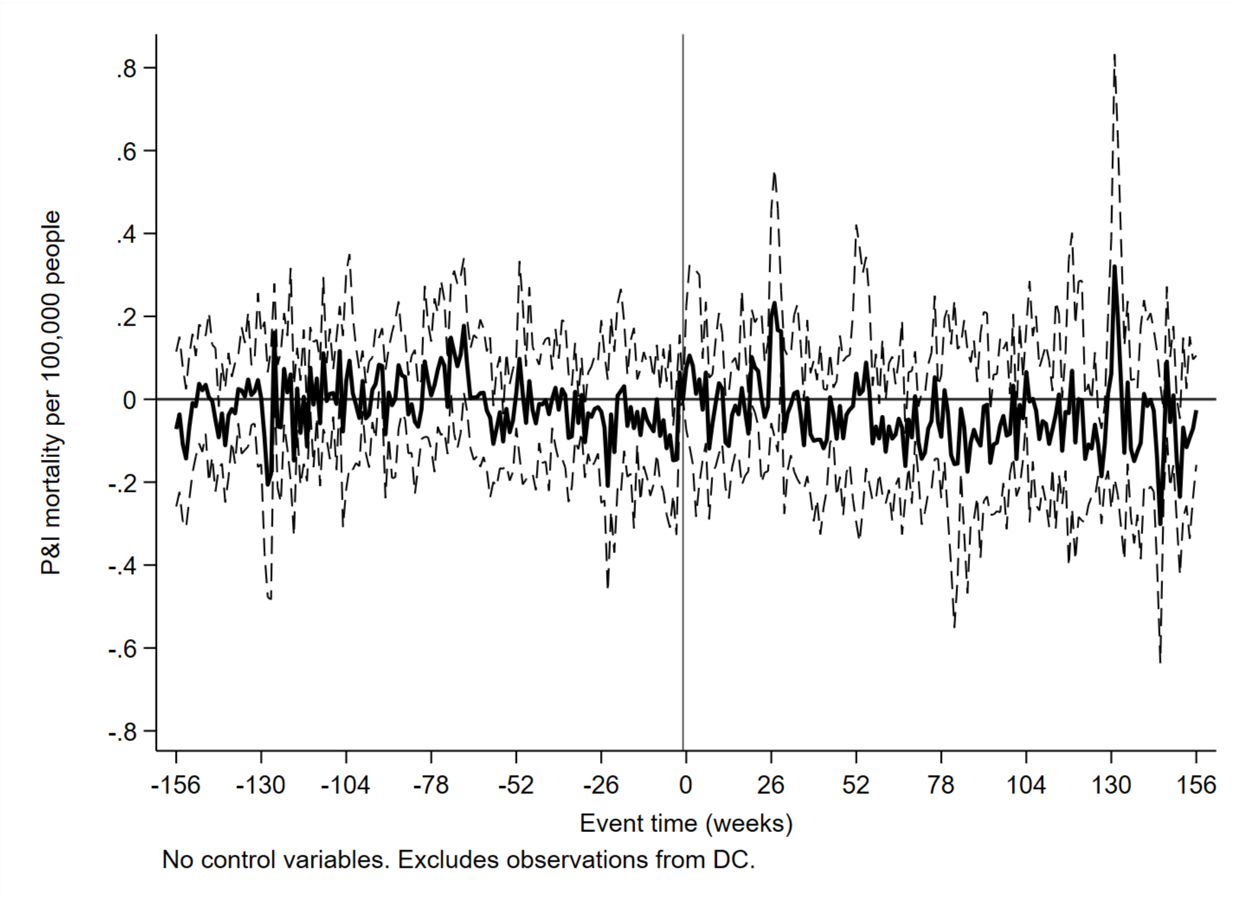
Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report. The figure is the equivalent event study of Table 1, Panel B, column (3) or Equation (1) with $\sum_{t=-36}^{36} Law_t$ plotted graphically, see main text for details. The dashed lines represent 95% confidence intervals. The x-axis illustrates the normalized time before and after the mandates became effective; the vertical line indicates the month prior to the effective date. The y-axis illustrates the change in the ILI rate. The model excludes observations from Washington DC.

Figure 4. Treatment Effect Heterogeneity by Month-of-the-Year



Sources: National Center for Health Statistics (NCHS) mortality surveillance data; Centers for Disease Control and Prevention. Each dot is from a separate regression as in Equation (1) and represents a treatment effect. On the x-axis, “1” stands for January, “2” stands for February and so on. All 12 models are run on samples that solely include observations from the respective month-of-the-year. The dashed gray lines represent 95% confidence intervals.

Figure 5. Weekly Event Study of the Impact of Mandates on P&I Mortality per 100,000 people



Sources: National Center for Health Statistics (NCHS) mortality surveillance data; Centers for Disease Control and Prevention. The figure is the equivalent event study of Table3, Panel A, column (3) or Equation (1) with $\sum_{t=-156}^{156} Law_t$ plotted graphically, see main text for details. The dashed lines represent 95% confidence intervals. The x-axis illustrates the normalized time before and after the mandates became effective; the vertical line indicates the week prior to the effective date. The y-axis illustrates the change in P&I mortality per 100,000 people. The model excludes observations from Washington D.C.

Table 1. Impact of Sick Leave Mandates on ILI Rates

	(1)	(2)	(3)	(4)
Panel A: State-Week Level				
Law Effective	-0.0053**	-0.0047**	-0.0042**	-0.0055**
	(0.0023)	(0.0018)	(0.0016)	(0.0022)
Unemployment		0.0006	0.0001	
		(0.0007)	(0.0006)	
Other controls	No	No	Yes	No
Mean	0.0187	0.0187	0.0187	0.0186
Change in percent	-28%	-25%	-22%	-29%
Observations	20,319	20,319	20,319	19,922
Panel B: State-Month Level				
Law Effective	-0.0056**	-0.0050**	-0.0044**	-0.0058**
	(0.0023)	(0.0019)	(0.0016)	(0.0022)
Unemployment		0.0006	0.0001	
		(0.0007)	(0.0006)	
Other controls	No	No	Yes	No
Mean	0.0188	0.0188	0.0188	0.0188
Change in percent	-30%	-27%	-23%	-31%
Observations	4,696	4,696	4,696	4,603
Panel C: State-Month Level (Spatial Error)				
Law Effective	-0.0056**	-0.0050**	-0.0043**	-0.0059**
	(0.0024)	(0.0020)	(0.0017)	(0.0023)
Unemployment		0.0006	0.0001	
		(0.0008)	(0.0006)	
Other controls	No	No	Yes	No
Mean	0.0193	0.0193	0.0193	0.0193
Change in percent	-29%	-26%	-22%	-31%
Observations	4,500	4,500	4,500	4,410

Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, National Center for Health Statistics mortality surveillance data, U.S. Census Bureau, U.S. Bureau of Labor Statistics, Kaiser Family Foundation, Child Care Influenza Immunization Action Coalition, NOAA, own calculations. Each column in each panel is one difference-in-differences model as in Equation (1), see main text for details. All regressions are weighted by the state populations. Column (3) includes as "other controls" the state population share with health insurance coverage, whether the state expanded Medicaid at the time, the population share above the age of 65, whether the state had an influenza vaccination mandate for children as well as the precipitation level, see Table A2 for summary statistics. Panel A estimates the models at the state-week level, Panel B estimates the models at the state-month level, Panel C estimates the random effects spatial error models at the state-month level. Column (4) excludes Washington D.C. but is otherwise identical to column (1) whereas column (2) controls for the unemployment rate. Standard errors in parentheses are clustered at the state level; *** p<0.01, ** p<0.05, * p<0.1

Table 2. Bacon Decomposition

	Coefficient	Weight
Timing Groups	-.0016	.0807
Never vs Timing	-.0055	.8239
Within	-.0003	.0953

Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, U.S. Census Bureau, U.S. Bureau of Labor Statistics, own calculations. Own data collection, own illustration. The decomposition controls for unemployment and is therefore directly comparable to Table 1, Panel A, column (2).

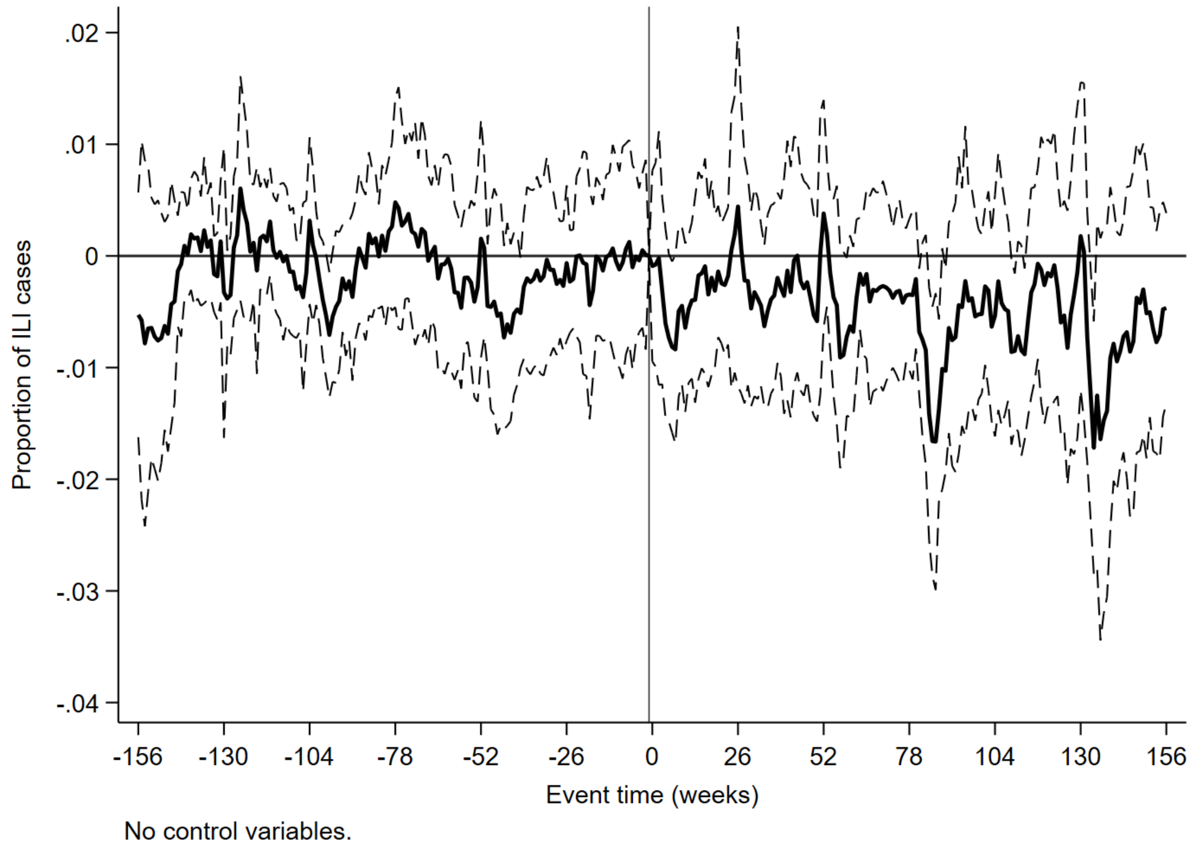
Table 3. Impact of Sick Leave Mandates on P&I Mortality per 100,000 population

	(1)	(2)	(3)	(4)
Panel A: State-Week Level				
Law Effective	-0.0231	-0.0209	-0.0232	-0.0232
	(0.0163)	(0.0214)	(0.0246)	(0.0165)
Unemployment		0.0021	0.0048	
		(0.0097)	(0.0077)	
Other controls	No	No	Yes	No
Mean	1.1349	1.1349	1.1349	1.1355
Change in %	-2.04%	-1.84%	-2.04%	-2.04%
Observations	20,328	20,328	20,328	19,921
Panel B: State-Month Level				
Law Effective	-0.0839	-0.0717	-0.0865	-0.0841
	(0.0729)	(0.0971)	(0.1101)	(0.0735)
Unemployment		0.0118	0.0255	
		(0.0432)	(0.0345)	
Other controls	No	No	Yes	No
Mean	4.9291	4.9291	4.9291	4.9317
Percent Change	-1.70%	-1.45%	-1.75%	-1.71%
Observations	4,700	4,700	4,700	4,606
Panel C: State-Month Level (Spatial Error)				
Law Effective	-0.0707	-0.0677	-0.0779	-0.0709
	(0.0766)	(0.0969)	(0.113)	(0.0772)
Unemployment		0.0029	0.013	
		(0.0423)	(0.0333)	
Other controls	No	No	Yes	No
Mean	4.8110	4.8110	4.8110	4.8137
Percent Change	-1.47%	-1.41%	-1.62%	-1.47%
Observations	4,500	4,500	4,500	4,410

Sources: National Center for Health Statistics (NCHS) mortality surveillance data; Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, National Center for Health Statistics mortality surveillance data, U.S. Census Bureau, U.S. Bureau of Labor Statistics, Kaiser Family Foundation, Child Care Influenza Immunization Action Coalition, NOAA, own calculations. Each column in each panel is one difference-in-differences model as in Equation (1), see main text for details. All regressions are weighted by the state populations. Column (3) includes as "other controls" the state population share with health insurance coverage, whether the state expanded Medicaid at the time, the population share above the age of 65, whether the state had an influenza vaccination mandate for children as well as the precipitation level, see Table A2 for summary statistics. Panel A estimates the models at the state-week level, Panel B estimates the models at the state-month level, Panel C estimates the random effects spatial error models at the state-month level. Column (4) excludes Washington D.C. Standard errors in parentheses are clustered at the state level; *** p<0.01, ** p<0.05, * p<0.1

Appendix

Figure A1. Event Study Robustness Check – No Control Variables, Includes D.C.



Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report. Own data collection, own illustration. The figure is the equivalent event study of Equation (1) with $\sum_{t=-156}^{156} Law_t$ plotted graphically, see main text for details. The dashed lines represent 95% confidence intervals. The x-axis illustrates the normalized time before and after the mandates became effective; the vertical line indicates the week prior to the effective date. The y-axis illustrates the change in the ILI rate. The model does not include any control variables.

Table A1. Overview of Sick Pay Mandates in Alphabetical Order

Location	Law Passed	Law Effective	Content
Arizona	Nov 8, 2016	July 1, 2017	all employees; 1 hour of paid sick leave for every 30 hours; firm specific 90 day accrual period if employment began after July 1, 2017 24 hours in firms ≤15; 40 hours in firms <15; own sickness or family member
California	Sept 19, 2014	July 1, 2015	all employees; 1 hour of paid sick leave for every 30 hours; 90 day accrual period; minimum 24 hours; own sickness or family member
Connecticut	July 1, 2011	Jan 1, 2012	full-time service sector employees in firms >49 employees (20% of workforce); 1 hour for every 40 hours; 680 hours accrual period (4 months); up to 5 days; own sickness or family member
District of Columbia	Dec 18, 2013	Feb 22, 2014 (retrosp. in Sep 2014)	all employees; 1 hour for every 87 hours (firms ≤24); 1 hour for every 43 hours (firms 25-99); 1 hour for every 37 hours (firms >99); 90 day accrual period; up to 24 hours (firms ≤24); up to 40 hours (firms 25-99); up to 56 hours (firms >100); own sickness or family member
Maryland	Jan 12, 2018	Feb 11, 2018	all employees working at least 12 hours per week in firms >14 employees; 1 hour for every 30 hours; 106 day accrual period; 40 hours; own sickness or family member
Massachusetts	Nov 4, 2014	July 1, 2015	all employees in firms >10 employees; 1 hour for every 40 hours; 90 day accrual period up to 40 hours; own sickness or family member
Oregon	June 22, 2015	Jan 1, 2016	all employees in firms >9 employees; 1 hour for every 30 hours; 90 day accrual period; up to 40 hours; own sickness or family member
Rhode Island	Sept 28, 2017	July 1, 2018	all employees; 1 hour of paid sick leave for every 35 hours; 90 day accrual period; 24 hours in firms >17 (2018); 24 hours in firms >17 (2019); 40 hours in firms >17 (2020 and after); own sickness or family member
Vermont	March 9, 2016	Jan 1, 2017	all employees working at least 18 hours per week; 1 hour of paid sick leave for every 52 hours; up to 1 year accrual period; 24 hours (2017-2018); 40 hours (2019 and after); own sickness or family member
Washington	Nov 8, 2016	Jan 1, 2017	all employees; 1 hour for every 40 hours; 90 day accrual period, own sickness or family member

Sources: Various, own collection; own illustration. The study uses all mandates listed for the evaluation, except for Maryland due to the late passage of the law. Washington D.C. is not included in our preferred specification as the law was originally implemented outside of the time period covered by the data in 2008. The original mandate excluded temporary and tip employees and was tightened in 2014 as listed above.

Table A2. Descriptive Statistics of Outcome and Control Variables

	N	Mean	Std. Dev.	Min	Max
ILI total	20,319	453	666	0	11,452
Total patients	20,319	20,238	17,447	15	112,599
ILI cases per patients	20,319	0.0187	0.0176	0	0.1942
Pneumonia & influenza deaths	20,328	147.762	131.431	0	971
Population	20,328	13,836,139	11,818,614	564,376	39,852,219
P&I mortality rate per 100,000 pop.	20,328	1.135	0.391	0	4.2392
Seasonally adj. unemployment (%)	20,328	6.339	2.068	2	13.7
Pop. share with insurance	20,328	0.886	0.048	0.76	0.98
Pop. share age 65+	20,328	0.142	0.018	0.077	0.206
Precipitation in inches	20,328	3.034	2.046	0.01	44
Child influenza vaccination mandate	20,328	0.101	0.301	0	1
Medicaid expansion	20,328	0.3716	0.483	0	1
Monthly New Vaccinations 18-64	4,700	3.044	4.347	0	27.5
Monthly New Vaccinations 65+	4,700	5.478	8.777	0	51

Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, National Center for Health Statistics mortality surveillance data, U.S. Census Bureau, U.S. Bureau of Labor Statistics, Kaiser Family Foundation, Child Care Influenza Immunization Action Coalition, NOAA, own calculations. P&I stands for "influenza and pneumonia." The table includes data from October 2010 to July 2018.

Table A3. Impact of Sick Leave Mandates on Number of Patients and Providers

	(1)	(2)	(3)	(4)
Panel A: State-Week Level, Outcome - Number of Patients				
Law Effective	1,097.2	1,206.7007	1,443.8	1,237.2
	(1,986.5)	(1,987)	(1,814.3)	(2,286.1)
Unemployment		263.2968	570.4528	
		(572.3)	(652)	
Other controls	No	No	Yes	No
Mean	12,316	12,316	12,316	12,499
Change in %	9%	10%	12%	10%
Observations	20,319	20,319	20,319	19,922
Panel B: **State-Week Level, Outcome - Number of Providers**				
Law Effective	0.4783	1.2364	1.6679	0.9312
	(3.8324)	(3.9448)	(4.0384)	(4.3722)
Unemployment		1.8225*	2.3970*	
		(1.0731)	(1.2794)	
Other controls	No	No	Yes	No
Mean	32.1347	32.1347	32.1347	32.7450
Change in %	1%	4%	5%	3%
Observations	20,350	20,350	20,350	19,943

Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, National Center for Health Statistics mortality surveillance data, U.S. Census Bureau, U.S. Bureau of Labor Statistics, Kaiser Family Foundation, Child Care Influenza Immunization Action Coalition, NOAA, own calculations. Each column in each panel is one difference-in-differences model as in Equation (1), see main text for details. All regressions are weighted by the state populations. Column (3) includes as "other controls" the state population share with health insurance coverage, whether the state expanded Medicaid at the time, the population share above the age of 65, whether the state had an influenza vaccination mandate for children as well as the precipitation level, see Table A2 for summary statistics. Panel A estimates the models at the state-week level on the number of total patients and Panel B estimates the models at the state-week level on the number of providers. Column (4) excludes observations from Washington D.C.

Table A4. Impact of Mandates on ILI Rates and P&I Mortality (Balanced Sample)

	(1)	(2)	(3)	(4)
Panel A: State-Week Level, Outcome - ILI Rates				
Law Effective	-0.0047**	-0.0047**	-0.0049**	-0.0046**
	(0.0020)	(0.0020)	(0.0020)	(0.0020)
Unemployment		-0.0004	-0.0007	
		(0.0006)	(0.0005)	
Other controls	No	No	Yes	No
Mean	0.0186	0.0186	0.0186	0.0186
Change in percent	-25%	-21%	-26%	-25%
Observations	16,475	16,475	16,475	16,448
Panel B: State-Week Level, Outcome - P&I Mortality per 100,000 population				
Law Effective	0.0164	0.0152	0.0149	0.0181
	(0.0370)	(0.0371)	(0.0375)	(0.0372)
Unemployment		-0.0159	-0.0096	
		(0.0110)	(0.0089)	
Other controls	No	No	Yes	No
Mean	1.1348	1.1348	1.1348	1.1349
Change in percent	1.44%	1.34%	1.31%	1.59%
Observations	16,898	16,898	16,898	16,871

Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, National Center for Health Statistics mortality surveillance data, U.S. Census Bureau, U.S. Bureau of Labor Statistics, Kaiser Family Foundation, Child Care Influenza Immunization Action Coalition, NOAA, own calculations. Each column in each panel is one difference-in-differences model as in Equation (1), see main text for details. All regressions are weighted by the state populations. Column (3) includes as "other controls" the state population share with health insurance coverage, whether the state expanded Medicaid at the time, the population share above the age of 65, whether the state had an influenza vaccination mandate for children as well as the precipitation level, see Table A2 for summary statistics. Panel A and B correspond to Panel A of Table 1 (ILI Rates) and Panel A of Table 3 (P&I Mortality), respectively, except they use balanced panels. That is, all event times are balanced and shortened from -13 weeks to +13 weeks. This excludes Rhode Island where the mandate was enacted on July 1, 2018, see Table A1.

Table A5. Impact of Mandates on ILI Rates and P&I Mortality (Incl. Partially Treated States)

	(1)	(2)	(3)	(4)
Panel A: State-Week Level, Outcome - ILI Rates				
Law Effective	-0.0047*	-0.0040*	-0.0035**	-0.0049*
	(0.0025)	(0.0020)	(0.0017)	(0.0025)
Unemployment		0.0006	0.0001	
		(0.0007)	(0.0006)	
Other controls	No	No	Yes	No
Mean	0.0187	0.0187	0.0187	0.0186
Change in percent	-25%	-21%	-19%	-26%
Observations	20,319	20,319	20,319	19,922
Panel B: State-Week Level, Outcome - P&I Mortality per 100,000 population				
Law Effective	-0.0148	-0.0116	-0.0126	-0.0149
	(0.0175)	(0.0221)	(0.0247)	(0.0177)
Unemployment		0.0032	0.0055	
		(0.0102)	(0.0079)	
Other controls	No	No	Yes	No
Mean	1.1349	1.1349	1.1349	1.1355
Change in percent	-1.30%	-1.02%	-1.11%	-1.31%
Observations	20,328	20,328	20,328	19,921

Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, National Center for Health Statistics mortality surveillance data, U.S. Census Bureau, U.S. Bureau of Labor Statistics, Kaiser Family Foundation, Child Care Influenza Immunization Action Coalition, NOAA, own calculations. Each column in each panel is one difference-in-differences model as in Equation (1), see main text for details. All regressions are weighted by the state populations. Column (3) includes as "other controls" the state population share with health insurance coverage, whether the state expanded Medicaid at the time, the population share above the age of 65, whether the state had an influenza vaccination mandate for children as well as the precipitation level, see Table A2 for summary statistics. Panel A and B equal Panel A of Table 1 (ILI Rates) as well as Panel A of Table 3 (P&I Mortality), except they assign partial treatment values to the main variable of interest for time periods when solely city-level mandates were enacted in a state. In total, this applies to seven states whose partial treatment values equal the population share of the state living in those cities, see main text for details and A Better Balance (2020).

Table A6. Impact of Sick Leave Mandates on New Flu Vaccinations

	(1)	(2)	(3)	(4)
Panel A: State-Month Level, Outcome - Monthly New Vaccinations ages 18-64				
Law Effective	0.1151	0.0584	0.0333	0.1171
	(0.0772)	(0.0675)	(0.0670)	(0.0774)
Unemployment		-0.0546*	-0.0362	
		(0.0286)	(0.0343)	
Other controls	No	No	Yes	No
Mean	3.044	3.044	3.044	3.043
Change in %	4%	2%	1%	4%
Observations	4,700	4,700	4,700	4,606
Panel B: State-Month Level, Outcome - Monthly New Vaccinations ages 65+				
Law Effective	0.1942	0.0900	0.0624	0.1950
	(0.1431)	(0.1232)	(0.1056)	(0.1445)
Unemployment		-0.1004***	-0.0650	
		(0.0365)	(0.0481)	
Other controls	No	No	Yes	No
Mean	5.478	5.478	5.478	5.478
Change in %	4%	2%	1%	4%
Observations	4,700	4,700	4,700	4,606

Sources: Centers for Disease Control and Prevention, Weekly U.S. Influenza Surveillance Report, National Center for Health Statistics mortality surveillance data, U.S. Census Bureau, U.S. Bureau of Labor Statistics, Kaiser Family Foundation, Child Care Influenza Immunization Action Coalition, NOAA, own calculations. Each column in each panel is one difference-in-differences model as in Equation (1), see main text for details. All regressions are weighted by the state populations. Column (3) includes as "other controls" the state population share with health insurance coverage, whether the state expanded Medicaid at the time, the population share above the age of 65, whether the state had an influenza vaccination mandate for children as well as the precipitation level, see Table A2 for summary statistics. The dependent variables in Panel A and B are the share of the state population who receives a flu vaccine by month of the year among those 18 to 64 years as well as among those above 64, respectively (CDC 2020c), also see Table A2 and main text. Column (4) excludes Washington D.C. Standard errors in parentheses are clustered at the state level; *** p<0.01, ** p<0.05, * p<0.1