



# The pros and cons of sick pay schemes: Testing for contagious presenteeism and noncontagious absenteeism behavior<sup>☆</sup>

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## ABSTRACT

This paper provides an analytical framework and uses data from the U.S. and Germany to test for the existence of contagious presenteeism and negative externalities in sick leave insurance. The first part exploits high-frequency Google Flu data and the staggered implementation of U.S. sick pay mandates to show, using a reduced-form framework, that population-level influenza-like disease rates decrease after employees gain access to paid sick leave. Next, a simple theoretical framework provides evidence on the underlying behavioral labor supply mechanisms. The model theoretically decomposes overall labor supply adjustments (“moral hazard”) into contagious presenteeism and noncontagious absenteeism behavior and derives testable conditions. The last part illustrates how to implement the model exploiting a German sick pay reform and administrative industry-level data on certified sick leave by diagnosis. The empirical test finds that the labor supply elasticity for contagious diseases is significantly smaller than for noncontagious diseases. Under the identifying assumptions of the model, this finding provides additional indirect evidence for the existence of contagious presenteeism.

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 Germany

*“Send me a bill that gives every worker in America the opportunity to earn seven days of paid sick leave. It’s the right thing to do. It’s the right thing to do.”*

Barack Obama  
 in his State of the Union Address (January 20, 2015)

*“I think the Republicans would be smart to get behind it.”*

Bill O’Reilly  
 in The O’Reilly Factor – Fox News (January 21, 2015)

## 1. Introduction

Besides Canada and Japan, the U.S. is the only industrialized country that does not provide universal access to paid sick leave (Heymann et al., 2009). Paid sick leave is different from Disability Insurance (which provides income replacement in case of permanent work disability) or Workers Compensation (which provides income replacement and medical benefits in case of work-related sickness). Paid sick leave gives employees the right to call in sick and receive sick pay due to work-unrelated short-term sickness; for example, due to the common cold or back pain.

In 2011, a third of U.S. full-time employees lacked sick leave coverage; among low-income and service sector employees, the uninsurance rates exceeded 80% (Susser and Ziebarth, 2016). However, support for sick leave mandates has grown substantially in the last decade. At the federal level, the *Healthy Families Act* (reintroduced in Congress in 2015) proposes the legal right for employees to earn one hour of paid sick leave per week, up to seven days per year. The epigraphs above suggest potential bipartisan support, but a decade of discussions in Congress has failed to deliver any concrete results. Instead, cities and states have taken steps and implemented mandates at the lower administrative level. On the city level, mandates were implemented in San Francisco (2007), Washington D.C. (2008), Seattle (2012), Portland (2014), New York City (2014), Philadelphia (2015), and several dozen other cities throughout this time. On the state level, Connecticut was first to implement a sick pay mandate for service sector workers in medium and large businesses in 2012. California, Massachusetts, and Oregon followed with more comprehensive mandates in 2015 and 2016.

One objective of this paper is to provide an analytical framework of the underlying labor supply reactions of employees when sick pay changes. Traditionally, behavioral adjustments to varying levels of insurance generosity have been labeled “moral hazard” in economics (Pauly, 1974; Nyman, 1999). We develop a simple model that decomposes overall employee labor supply adjustments into what we call “contagious presenteeism” and “noncontagious absenteeism.” Contagious presenteeism is when employees with a contagious disease (e.g., a common cold) go to work sick and spread the disease to co-workers, customers, and the general population. Such behavior induces negative externalities. Noncontagious

absenteeism is when employees without a contagious disease (e.g., back pain) call in sick. When sick pay changes, both behavioral responses work in opposite directions. For example, when employees obtain sick leave coverage, marginal employees with a common cold will call in sick and will not spread the disease because of sick pay. Contagious presenteeism decreases. On the other hand, marginal employees with back pain will call in sick because of sick pay. Noncontagious absenteeism increases.

One key element of our proposed theoretical mechanism is information frictions about contagiousness; if employers had perfect information, they could ban contagious employees from working. However, supported by intuition and empirical evidence (Pauly et al., 2008), employers have only incomplete information about employees’ contagiousness. For example, after the first occurrence of flu sickness symptoms, humans are contagious for 5 to 7 days (Centers for Disease Control and Prevention, 2016). Furthermore, the availability of over-the-counter drugs that suppress disease symptoms reinforces the spread of disease (Earn et al., 2014). Because of such information frictions, employers cannot fully internalize the negative externalities induced by contagious employees who work. Sick pay schemes then incentivize those contagious employees to stay at home but also induce noncontagious employees to call in sick more often.

While the traditional social insurance literature has considered behavioral adjustments to government regulation as generally undesirable, more recent papers also identify and emphasize welfare gains of such regulation. Welfare gains could be the value of leisure time (Fadlon and Nielsen, 2015), higher liquidity (Nyman, 2003; Chetty, 2008), or better job matches in case of unemployment benefits (van Ours and Vodopivec, 2008; Schmieder et al., 2012; Nekoei and Weber, 2017). This paper adds to this literature by identifying welfare gains of sick pay in the form of lower infection rates. However, we deliberately abstain from a normative welfare analysis. Because any social insurance involves welfare gains and losses, a welfare analysis would require implicitly or explicitly weighting these benefits and losses. Rather, we provide a positive analysis, and theoretically as well as empirically decompose the behavioral employee responses to variations in sick pay.

To our knowledge, this is the first attempt to define “contagious presenteeism” as a negative externality of a suboptimal provision of sick pay, and then to empirically identify it. The empirical identification of contagious presenteeism is particularly challenging because contagiousness is to a large degree unobservable and presenteeism (“going to work sick”) is to a large degree subjective. Therefore we propose two different empirical identification approaches using data from two countries: the U.S. with one of the least generous sick leave systems in the world, and Germany with one of the most generous sick leave systems in the world.

The first identifying test exploits high-frequency Google Flu data to estimate the impact of city-level sick pay mandates on influenza-like illness (ILI) rates in the U.S. The staggered implementation of sick pay mandates across metropolitan areas over time naturally leads to the estimation of standard difference-in-differences (DD) models. We make use of publicly available Google Flu data as they are available at the weekly level for 97 metropolitan areas over a long

time period. Using Google search queries and a specific algorithm, Google Flu retrospectively replicates the official ILI rates (which are not available to researchers at the city level). We first demonstrate the relevance of measurement error in the Google Flu data and verify that it is not correlated with the reforms. Then we show the following: After U.S. employees gained access to paid sick leave because of sick pay mandates, the ILI rate in the population decreased significantly. This finding yields strong reduced-form evidence for the existence of contagious presenteeism, and that such behavior becomes less common when employees are able to take sick days. The finding that sick pay mandates can result in fewer infections and lower influenza activity at the population level is highly policy relevant. It also has implications for specific workplace settings, such as schools or hospitals, where particularly vulnerable populations can be affected by the spread of diseases.

The second identifying test is closely linked to the analytical framework in the middle part of the paper. The identification hinges on one main (strong) assumption, and the data that we exploit are not ideal. However, the last part of the paper is still valuable as it illustrates how to carry out an alternative empirical test that has the potential to identify contagious presenteeism and noncontagious absenteeism. This last part exploits a policy reform that cut sick pay in the 1990s in Germany. Using administrative data aggregated at the industry level and variation in industry-specific sick pay regulations, we estimate labor supply elasticities by doctor-certified ICD disease categories. Within the context of our model, which (absent contagious presenteeism) assumes similar elasticities for contagious and noncontagious diseases, the difference in the aggregated labor supply elasticities is then a function of additional infections due to contagious presenteeism.

The next chapter discusses the history of sick leave as a social insurance. It also carves out contributions of this paper in the context of sick leave literature and literature on infectious diseases. More broadly, this paper contributes to social insurance literature (see above). Because a main theme of the paper is negative externalities, it also relates to papers that identify externalities in the context of UI (Lalive et al., 2015; Marinescu, 2017), endogenous private insurance (Chetty and Saez, 2010), or unhealthy consumption goods (O'Donoghue and Rabin, 2006).

Section 3 describes the U.S. sick pay mandates in more detail and provides reduced-form evidence that they reduced ILI rates. The middle part of the paper presents our theoretical framework and Section 5 implements the closely linked empirical test using German data. The final section concludes.

## 2. Paid sick leave history and literature

Historically, paid sick leave was part of the first social insurance system in the world. The first public health insurance legislation included sick leave benefits as part of the benefit package. Under Otto von Bismarck, the *Sickness Insurance Law of 1883* introduced social health insurance in Germany, which included 13 weeks of paid sick leave along with coverage for medical bills. At the time, the costs for paid sick leave exceeded half of all health care costs due to the limited availability of (expensive) medical treatments in the nineteenth century (Busse and Riesberg, 2004). Today, every European country has some form of universal access to paid sick leave.

Opponents of paid sick leave mandates argue that such a social insurance benefit would encourage shirking behavior. Moreover, forcing employers to provide sick pay via mandates or new taxes would dampen job creation and hurt employment. However, using synthetic control group methods and similar variation than this paper, Pichler and Ziebarth (2016a) find no evidence that the early U.S. sick pay mandates had an economically relevant effect on wages

or employment. A general argument against government-mandated sick leave argues that, if coverage were optimal, the private market would ensure that employers would provide such benefits.

In addition to inequality and worker well-being concerns, one rationale for sick pay mandates is public health promotion. When workers lack access to paid sick leave, they may go to work despite being sick. Particularly in professions with direct customer contact, presenteeism can induce negative externalities and infections of co-workers and customers. Given the low influenza vaccination rates of around 40% in the U.S. and 10 to 30% in the EU (Centers for Disease Control and Prevention, 2014; Blank et al., 2009), workplace presenteeism is one important channel through which infectious diseases spread. Worldwide, seasonal influenza epidemics result in 3 to 5 million severe illnesses and an estimated 250 to 500 thousand deaths; in the U.S., the flu-associated death count ranges from 3 to 49 thousand per year (World Health Organization, 2014; Centers for Disease Control and Prevention, 2016). Moreover, recent evidence suggests that influenza during pregnancy can lead to negative outcomes for exposed offspring (Schwandt, 2017).

This paper is one of the first to study the effects of sick pay mandates in the U.S. (Ahn and Yelowitz, 2015; Pichler and Ziebarth, 2016a; Stearns and White, 2016, are exceptions). Existing studies find that employees adjust their workplace attendance to variations in sick pay (Johansson and Palme, 1996; Johansson and Palme, 2005; De Paola et al., 2014; Ziebarth and Karlsson, 2010; Ziebarth and Karlsson, 2014; Dale-Olsen, 2014; Fevang et al., 2014). Other papers in the sick leave literature identify general determinants (Barmby et al., 1994; Markussen et al., 2011), investigate the impact of probation periods (Riphahn, 2004; Ichino and Riphahn, 2005), culture (Ichino and Maggi, 2000), gender (Ichino and Moretti, 2009; Gilleskie, 2010), income taxes (Dale-Olsen, 2013), and unemployment (Askildsen et al., 2005; Nordberg and Røed, 2009; Pichler, 2015). There is also research on the impact of sickness on earnings (Sandy and Elliott, 2005; Markussen, 2012).

In particular, this paper extends the small economic literature on presenteeism in the workplace (Aronsson et al., 2000; Chatterji and Tilley, 2002; Brown and Sessions, 2004; Johns, 2010; Böckerman and Laakkonen, 2010; Markussen et al., 2012; Hirsch et al., 2015; Ahn and Yelowitz, 2016). Although various definitions of presenteeism exist (Simpson, 1998), it most commonly refers to going to work despite being sick. Pauly et al. (2008) ask 800 U.S. managers about their views on employee presenteeism with chronic and acute diseases. Pichler (2015) provides evidence for the hypothesis that presenteeism is procyclical due to a higher workload during economic booms. And Barmby and Larguem (2009) exploit daily absence data from a single employer and estimate absence determinants as well as transmission rates of contagious diseases, linking the estimation approach to an economic model of absence behavior.

This paper also adds to the literature on the determinants and consequences of infectious diseases, vaccinations, and epidemics (Mullahy, 1999; Bruine de Bruin et al., 2011; Uscher-Pines et al., 2011; Ahn and Trogdon, 2015). For example, Maurer (2009) models supply and demand side factors of influenza immunizations, whereas Karlsson et al. (2014) empirically assess the impact of the 1918 Spanish flu on economic performance in Sweden. Stoecker et al. (2016) find an 18% increase in influenza deaths for the elderly in counties whose teams participate in the Super Bowl. Their findings suggest that large events are an important underlying mechanism for influenza transmission. Adda (2016) shows that reductions in interpersonal contacts, e.g., through school closures or the shut-down of public transportation, reduce transmission rates.

Although related and sometimes combined in laws, sick pay schemes differ crucially from parental leave schemes (Gruber, 1994; Ruhm, 1998; Waldfogel, 1998; Ruhm, 2000; Rossin-Slater et al., 2013; Lalive et al., 2014; Carneiro et al., 2015; Thomas, 2015; Dahl et al., 2016) due to the negative externalities induced by contagious

presenteeism in combination with information frictions about the type and extent of the disease.

### 3. Evidence from U.S. sick leave reforms

The U.S. has one of the least generous sick leave systems of all industrialized countries. Using high-frequency data from Google Flu at the weekly level over more than a decade, this section assesses the impact of city-level sick pay mandates on influenza-like disease rates in the U.S.

#### 3.1. The U.S. sick leave landscape

About half of the U.S. workforce lacks access to paid sick leave, particularly low-income employees in the service sector (Heymann et al., 2009; Susser and Ziebarth, 2016). Appendix Table A1 provides a summary of recent sick pay reforms at the city level. The details of the bills differ from city to city but all sick pay schemes are employer mandates. Small firms are sometimes exempt. Employees “earn” paid sick leave credit (typically one hour per 30–40 h worked) up to nine days per year; this credit rolls over to the next calendar year if unused. Because employees need to accrue sick days, most sick pay schemes explicitly state a 90 day accrual period. However, the right to take *unpaid* sick leave is part of most sick pay mandates. Note that gaining the right to take unpaid leave can be seen as a normalization and represents an increase in sick leave benefits because the right to take unpaid leave decreases the likelihood of being dismissed when calling in sick. Also note that these sick pay mandates do not require (or allow) employers to require doctors’ notes as a precondition for taking sick leave (Polsky, 2016; New York City Consumer Affairs, 2016).

As Table A1 shows, San Francisco was the first city to mandate paid sick leave on February 5, 2007. Washington, D.C., followed on November 13, 2008, and extended the coverage to temporary workers and tipped employees effective February 22, 2014. Seattle (September 1, 2012), Portland (January 1, 2014), New York City (April 1, 2014), Newark (May 29, 2014) Philadelphia (May 13, 2015), and Oakland (March 2, 2015) followed.

#### 3.2. Using Google Flu data to test for changes in infections

We use Google Flu Trends data at the city level for each week from 2003 to 2015. Google search queries can be used via an algorithm to mimic actual ILI rates very accurately (Carneiro and Mylonakis, 2009; Ginsberg et al., 2009). Because the governmental Center for Disease Control and Prevention (CDC) does not provide ILI rates at the city level, we use Google Flu data to test for city-level changes in infection rates as a result of sick pay mandates. We use the data as provided by Google Flu (2015).

Our Google Flu sample contains the weekly ILI rates of all major U.S. cities—81 in total—from 2003 to 2015 (Appendix Table A2). We include data for most cities starting September 28, 2003. The end date for all cities is July 26, 2015.<sup>1</sup> This results in 48,333 city-week observations. We also create a second sample that aggregates the data at the monthly level and has 11,157 city-month observations.

##### 3.2.1. Outcome variable

Google Flu replicates the official CDC ILI rate per 100,000 doctor visits via an (unknown) algorithm. The mean for the city sample

is 1913 ILI cases per 100,000 doctor visits. We take the natural logarithm of this measure as dependent variable.

The reason for the normalization of the ILI rate by doctor visits is that its CDC benchmark measure has the same normalization. Consequently, the ILI rate can be interpreted as the “diagnosed” ILI rate. As U.S. sick pay mandates do not require (or allow) a doctor’s note, doctor visits should not increase due to the mandates. If that was nevertheless the case, our estimates would represent lower bounds.

##### 3.2.2. Treatment and control groups

Table A1 (Appendix) lists all cities that implemented sick pay mandates between 2006 and 2015. All seven cities listed and Washington D.C. are treated units and the remaining metropolitan areas in Appendix A2 are control units.

In addition to Google Flu data, we use data from the Bureau of Labor Statistics (BLS, 2015) to control for the monthly unemployment rate in each city. The unit of observation in the BLS data equals the unit of observation in the Google Flu data. Accordingly, we merge in the BLS unemployment rates at the city-month level with the Google Flu data.

##### 3.2.3. Assessing Google Flu measurement error

Lazer et al. (2014) report that Google Flu would overestimate actual ILI rates. The media eagerly picked up the story. By googeling “Google Flu”, one finds reports about the “Epic Failure of Google Flu.” This section assesses whether measurement error in the Google Flu data could be a serious threat to our main findings. Note that the original ambition of Google Flu was to *predict* epidemic outbreaks earlier and faster than the governmental CDC. Given Lazer et al. (2014) and the media reports, Google obviously accepted that this objective may have been overly ambitious. However, we exploit Google Flu *retrospectively* to test for changes in infection rates and do not intend to make any predictions.

First of all, even if systematic over- or underestimation occurred, it should not be a threat to our estimates as long as this bias is not correlated with the introduction of sick pay mandates at the city level. Our main model is a rich fixed effects specifications with 80 city and 617 week-year fixed effects that net out time-variant seasonal trends in influenza activity as well as time-invariant regional effects.

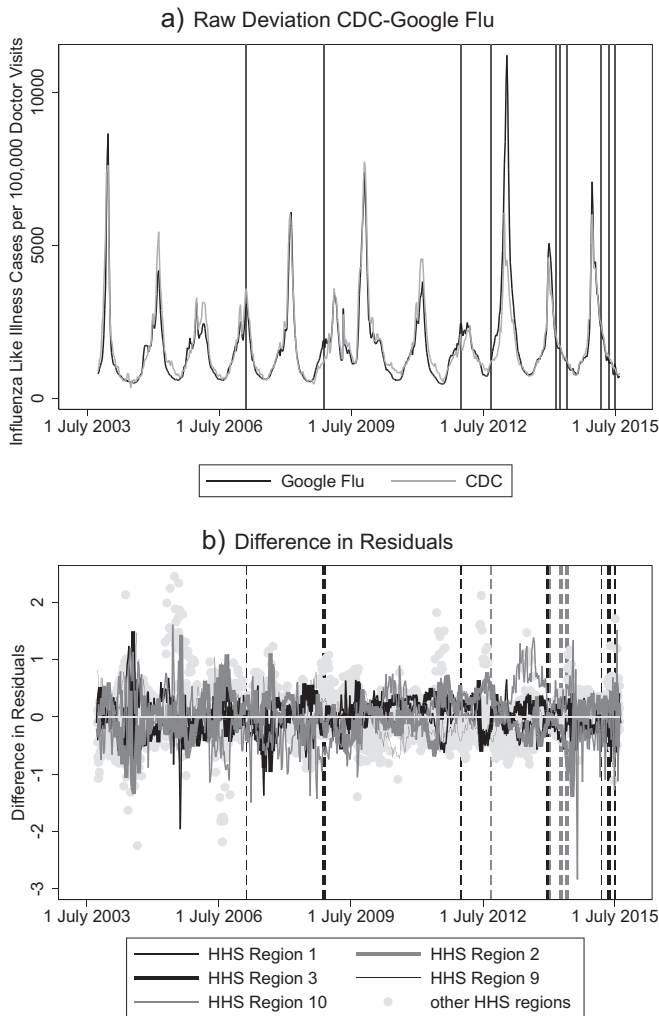
Second, one could argue that more people were searching for sick leave information after the laws were implemented. However, even if this was the case, Google Flu would simply overestimate the true ILI rate after the mandates and our treatment effects would be downward biased and lower bounds. Below we conduct several robustness checks to check for the validity of this hypothesis.

Third, we formally test whether there is evidence that Google Flu over- or underreports are correlated with the reforms. For this test, we acquired official CDC data on ILI cases per 100,000 doctor visits. These data are available on the weekly level and the level of the 10 HHS regions (but not at the city level). We aggregate our Google Flu data and construct an equivalent dataset. Fig. 1a plots both time series. The vertical lines represent the implementation of *all* sick pay mandates during this time, both city and state mandates (e.g. Connecticut (Jan 1 2012), California and Massachusetts (July 1 2015)). As seen, one does *not* observe any trend in the measurement error, but single spikes here and there, some of which represent an overestimation of the true ILI rate. Particularly striking is the huge spike in the second half of 2012 that triggered the media debates about the “Epic Failure of Google Flu” (Lazer et al., 2014). However, as seen, this seems to have been a single outlier that is not particularly worrisome in our model with week fixed effects.

Fig. 1b plots the difference in residuals between both datasets (CDC vs. Google Flu) after regressing each ILI rate on 617 week and 9 HHS region fixed effects. In other words, Fig. 1b provides a visual assessment of the differences in the remaining variation by week and region after netting out seasonal and regional effects. The thin solid

<sup>1</sup> We omit New Orleans, which has missing information due to Hurricane Katrina. We also omit cities that were not treated through a city mandate but a state mandate, and we omit cities that are in close proximity of the treated cities due to potential spillovers. These 15 disregarded cities are gray in Table A2.





**Fig. 1.** Google Flu measurement error.

**Note:** Panel a shows official ILI cases per 100,000 doctor visits as reported by the CDC and by Google Flu. CDC data are available at the weekly level for 10 HHS regions. Panel b plots the difference in residuals between the two datasets (CDC vs. Google Flu). Residuals are calculated for both datasets separately by regressing the ILI rate on a set of 617 week fixed effects and 9 HHS region fixed effects. The lines and dots represent different HHS regions which include the treatment regions. The vertical lines represent the implementation of the sick pay mandates. HHS Region 1 includes Connecticut and Massachusetts, HHS Region 2 New York City and Newark City, HHS Region 3 Philadelphia and DC, HHS Region 9 California and HHS Region 10 Oregon and Seattle.

black line represents HHS Region 1 which includes the treatment states Connecticut and Massachusetts. The corresponding dashed vertical line represents the date when the sick pay mandates were implemented in both states. Equally constructed are the thick black and gray colored lines and dots. As seen, there is no visual evidence of any systematic correlation between week-region measurement errors and the implementation of sick pay mandates. This visual assessment is confirmed when we regress the differences in residuals on a treatment-time indicator: With 6191 region-week observations, the point estimate is 0.0247, positive and not statistically significant (standard deviation: 0.0697).

As final robustness checks, we scrap [Google Trends \(2017\)](#) data at the week-city level using the search terms (i) flu shot, (ii) back pain, and (iii) gun shows. Using the search frequencies for these search terms as outcome variables, we re-run our standard model. The (non-significant) findings are discussed in the results section. We also

estimate placebo models pretending other major cities in the same state were treated instead of the real ones.

### 3.3. Changes in influenza activity when employees gain sick pay coverage

#### 3.3.1. Parametric difference-in-differences model

The staggered implementation of sick pay schemes across space and over time naturally leads to the estimation of the following standard difference-in-differences (DD) model:

$$\log(y_{it}) = \phi \text{TreatedCity}_i \times \text{LawEffective}_t + \delta_t + \gamma_i + \text{Unemp}_{im} + \mu_{it} \quad (1)$$

where  $\log(y_{it})$  is the logarithm of the [Google Flu \(2015\)](#) rate in city  $i$  in week of the year  $t$ .  $\gamma_i$  are 81 city fixed effects and  $\delta_t$  is a set of 617 week fixed effects over almost 12 years.  $\text{TreatedCity}_i$  is a treatment indicator which is one for cities that implemented a sick pay mandate between 2003 and 2015, see [Table A1](#). The interaction with the vector  $\text{LawEffective}_t$  yields the binary variable of interest. The interaction term is one for cities and time periods where a sick pay scheme was legally implemented (see [Table A1](#), column (3)). In addition to the rich set of city and time fixed effects, we control for the monthly BLS unemployment rate at the city level,  $\text{Unemp}_{im}$ . The standard errors are routinely clustered at the city level ([Bertrand et al., 2004](#)) but the standard errors obtained by bootstrapping are very similar (available upon request). Thus this empirical specification allows us to estimate  $\phi$ —the effect of mandated sick pay on the population ILI rate.

**Event study.** To produce an event study graph, we replace the binary  $\text{LawEffective}_t$  time indicator with one that continuously counts the number of days until (and from) a law became effective—from  $T = -24$  months to  $T = 0$  and  $T = +24$  months. This allows us to net out, normalize and graphically plot changes in flu rates, relative to when the laws were implemented. Event studies also help assessing whether there is any evidence for confounding factors or an endogenous implementation of the laws, for example, as a reaction to pre-existing trends.

#### 3.3.2. Empirical results

**Event study graphs.** We start the results section with the event study graphs in [Fig. 2](#). Panel a plots the coefficients of the continuous time indicators counting the weeks before and after the laws became effective in each city. Recall that the coefficient estimates are net of city fixed effects and week-year fixed effects, i.e., correct for common influenza seasonalities across U.S. metropolitan areas. Panel a shows the result for the unbalanced panel. There is very little trending in the two years before the mandates became effective; most coefficient estimates are not statistically different from zero and fluctuate only slightly around the zero line. Also, there is not much evidence for anticipation effects.

Immediately after employees gained the right to take paid and unpaid sick leave, the ILI rate decreases significantly. The estimates for  $T = +18$  months and beyond lack precision because they are solely based on San Francisco (2007), D.C. (2008), and Seattle (2012). New York City's comprehensive bill became effective April 1, 2014—about one year and four months before the end of our observation period at the end of July 2015. Portland's bill took effect in January 2014, and Newark's bill at the end of May 2014.

Hence, the observed rebound of infection rates to the zero line is determined by a lack of precision and the early experiences in San Francisco, D.C. and Seattle. More important, the rebound may be driven by the confounding effect of the Great Recession for San Francisco (it is well documented that fear of unemployment increases presenteeism (cf. [Pichler, 2015](#); [Schön, 2015](#))). We test this hypothesis by excluding San Francisco from the sample and re-running the

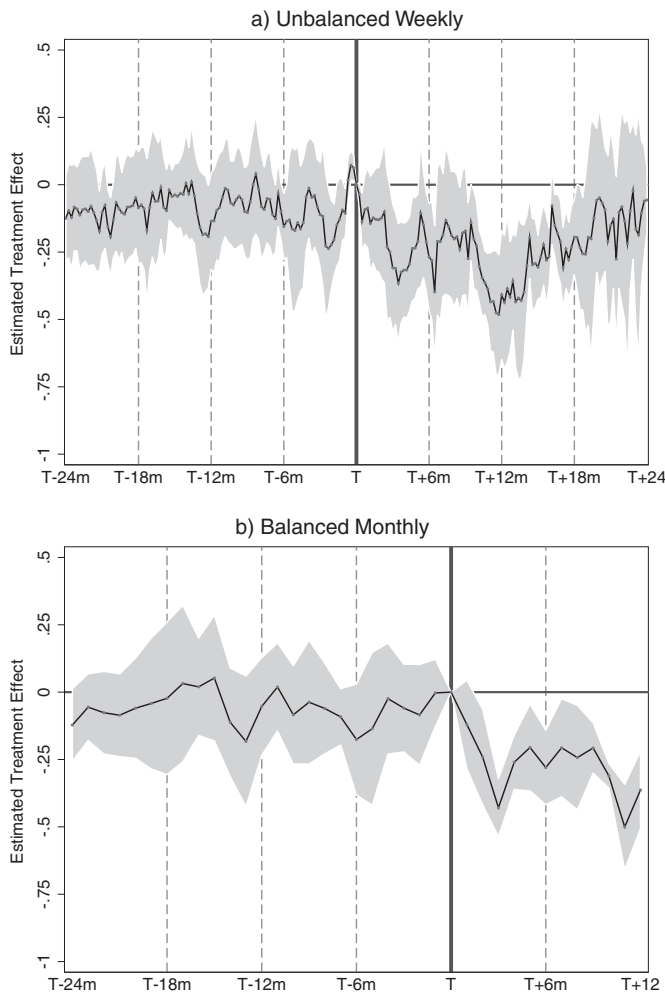


Fig. 2. Event study—effect of city-level sick pay mandates on ILI rates.

model. Panel a of Appendix Fig. A1 shows that, indeed, the rebound effect was at least partially driven by the Great Recession that kicked in 2008.

The rebound effect could also be due to the unbalanced nature of the sample and the fact that fewer units of observation remain in the sample, the further we move to the right of  $T=0$  in Fig. 2. Therefore, as additional robustness checks, we (a) construct a balanced panel where all cities are included at every point in time of the event study (this excludes Oakland and Philadelphia). In addition, we (b) aggregate the data at the city-month level to reduce noise and the impact of single outliers. Panel b of Fig. 2 shows the event study for the balanced sample at the monthly level, and Panel b of Fig. A1 shows the event study for unbalanced panel at the monthly level.

Both additional event studies largely confirm our main findings. The balanced panel event study at the monthly level shows no trending in the two pre-reform years and then a clear decrease in ILI rates in the first year after the laws became effective (Fig. 2, Panel b). The unbalanced event study at the monthly level (Fig. A1, Panel b) shows the same pattern as the main event study in Panel a of Fig. 2.

Overall, the event study graphs illustrate a clear and significant decrease in ILI rates at the population level after employees gained sick leave coverage. This finding suggests that sick leave coverage induces some sick and contagious employees to call in sick instead of going to work sick, thereby reducing contagious presenteeism and infection rates.

**Main DD results.** Panel A of Table 1 shows the main regression results of the DD model in Eq. (1). Every column represents one

model where the first two columns represent the standard model. The only difference between even and uneven columns is that the former additionally control for the monthly BLS unemployment rate. We find that controlling for the monthly unemployment rate barely alters the results (the same is true when we generate and control for additional covariates scraping Google Trends (2017)).

The main  $TreatedCity \times LawEffective$  coefficients in the main model in first two columns provide negative coefficient estimates that are significant at the 5% level. The literal interpretation would be that the ILI rate per 100,000 doctor visits decreases by about 6% when employees gain access to paid (and unpaid) sick leave. This is a weighted average over all seven treated U.S. cities in Table A1 as well as a mix of short- and medium-term estimates. For three cities (NYC, Portland, Newark), we cover more than a year of post-reform influenza activity, and for three other cities (SF, DC, Seattle), we cover at least three years of post-reform influenza rates. Here, the baseline period is the entire pre-reform period since 2003 whereas the baseline period of the event studies is the first week when the mandates became effective.

To test for anticipation effects, columns (3) and (4) of Panel A in Table 1 make use of city-specific dates indicating when the laws were passed by the city legislature. Up to one year elapsed between the passing and implementation of the laws (Table A1). It could be that firms voluntarily implemented sick pay schemes ahead of the official due date. However, as seen, columns (3) and (4) do not provide much evidence that this was the case—the coefficients shrink in size to about 3% and are not statistically significant any more.

The models in columns (5) and (6) make use of city-specific dates indicating when the accrual period was over. As discussed, all laws require employees to “earn” their sick days. Employees accrue one hour of paid sick leave per 30 or 40 h of work, i.e., per full-time work week. In addition, all laws specify a minimum accrual period of typically 90 days before employees can take paid sick leave for the first time. Assuming that the first paid sick day can be taken after 12 full work weeks, each earning employees one hour of sick pay, then full-time employees can take 1.5 paid sick days after 90 days. Note that the option to take unpaid sick leave is typically part of these sick pay mandates.<sup>2</sup> Letting the data speak, we can say that the decrease in flu rates increases by one percentage point to  $-7\%$  in columns (5) and (6) of Panel A, suggesting that paid sick leave can be more effective in reducing contagious presenteeism than unpaid sick leave.

**Placebo estimates and robustness checks.** Panel B of Table 1 provides placebo estimates. We run almost exactly the same models as in Panel A. However, instead of the treated cities we assign a placebo treatment to “neighboring” cities in the same state. To avoid the possibility of confounding spillovers, we require a distance of at least 100 miles between the treated and placebo city, which results in 13 placebo cities for the 7 treatment cities (see notes to Table 1). Panel B of Table 1 shows that none of the six placebo estimates is statistically significant.

Next, we scrap three search terms using Google Trends (2017) data for the same set of U.S. cities and the same time horizon on the weekly level. Unfortunately, not all 81 cities in Table A2 were available on Google Trends on a weekly basis for the entire time period. Consequently, the eight models in Table 2 have slightly smaller sample sizes (but still enough statistical power). Moreover, the raw search

<sup>2</sup> The Family and Medical Leave Act of 1993 (FMLA) covers employees with 1250 h of work in the past year and at locations with at least 50 employees with unpaid leave in case of pregnancy, own disease, or disease of a family member (e.g. Tominey, 2016). Jorgensen and Appelbaum (2014) find that 49 million U.S. employees are ineligible for FMLA, 44% of all private sector employees. The findings in Susser and Ziebarth (2016) also suggest that many low-wage and service sector employees are either not aware of this right, or—more likely—not covered by it. The majority of employees without access to firm-provided sick pay likely gained access to both paid and unpaid sick leave through the mandates listed in Table A1.

**Table 1**  
Effect of city-level sick pay mandates on ILI rates 2003–2015.

	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: Main estimates</b>						
TreatedCity × LawEffective	−0.0629** (0.0240)	−0.0604** (0.0231)				
TreatedCity × LawPassed			−0.0307 (0.0249)	−0.0278 (0.0252)		
TreatedCity × ProbationOver					−0.0704** (0.0298)	−0.0682** (0.0286)
<b>Panel B: Placebo estimates</b>						
TreatedCity × LawEffective	−0.0072 (0.0296)	−0.0100 (0.0303)				
TreatedCity × LawPassed			−0.0403 (0.0279)	−0.0428 (0.0282)		
TreatedCity × ProbationOver					−0.0057 (0.0275)	−0.0089 (0.0279)

Source: Google Flu (2015), own calculation and illustration. Standard errors in parentheses are clustered at the city level. The dependent variable is always the logarithm of the ILI rate per 100,000 doctor visits as reported by Google Flu (2015). All regressions contain week-year fixed effects and city fixed effects as in Eq. (1). Each column represents one model, estimated by OLS. N = 48,333 for Panel A and 48,505 for Panel B. Even numbered columns additionally control for the local monthly unemployment rate (BLS, 2015). Treated cities in Panel A are listed in Table A1 together with the dates for *LawEffective*, *LawPassed* and *ProbationOver*. The placebo cities in Panel B are cities from the same state but which are at least 100 miles distant. Specifically, we assign placebo treatments to Los Angeles, San Diego, San Jose, Irvine, Fresno and Sacramento, CA (instead of San Francisco and Oakland, CA); Spokane, WA instead of Seattle, WA; Albany, Buffalo, and Rochester, NY (instead of New York City); Eugene, OR instead of Portland, OR; and Pittsburgh and State College, PA (instead of Philadelphia, PA). The entire sample of cities considered is in Table A2.

\* p < 0.1.  
\*\* p < 0.05.  
\*\*\* p < 0.01.

data contains many zeros which is why we run a generalized linear model with a log link as suggested by Silva and Tenreiro (2006).

Specifically, as a benchmark, we replicate our main specification, but use this smaller subsample of cities and a generalized log link linear model with the ILI rate in levels as dependent variable. Columns (1) and (2) of Table 2 show that the results are almost identical to those in the first two columns of Panel A in Table 1.

Columns (3) and (4) use Google searches for “flu shots” as outcome measure. This is due to concerns that employers could have “induced” their employees to get flu shots as a result of the reforms. However, the point estimates are not statistically significant. The same is true for “back pain” in columns (5) and (6)—a noncontagious disease—as well as “gun shows” in columns (7) and (8)—an entirely unrelated search term.

We also experimented with adding these three additional variables (flu shots, back pain, and gun shows) as covariates to our main model. However, these search variables were not significantly correlated with ILI rates and the estimated treatment effects hardly changed.

Summing up, all results together provide evidence that sick pay reduces contagious presenteeism which reduces ILI rates. For noncontagious diseases such as back pain, on the other hand, we do not find evidence for a reduced incidence rate.

**Discussion of effect sizes.** Our main models in the first two columns of Table 1 suggest reductions in population-level ILI rates

by between 6 and 7% after sick leave mandates were implemented at the city level. Our model in the next section will provide theoretical evidence of the underlying labor supply mechanisms: marginal employees with contagious diseases will call in sick and stay at home when the costs of taking sick leave decrease. Consequently, social interactions of contagious employees decrease and hence the infection rate in the population.

According to Susser and Ziebarth (2016), 35% of full-time employees and 45% of all employees were not covered by firm-specific sick leave policies in 2011. Given the current population-employment ratios (BLS, 2016), this means that roughly 20% of the population gained access to sick leave coverage when cities passed such mandates (assuming that treated cities had average coverage rates). The model above thus estimates Intent-to-Treat (ITT) effects at the city level which represent an average over all 8 cities and post-reform periods in the sample.

Per week and over the time period considered in this paper, the CDC counted an average of 1655 confirmed ILI cases per 100,000 doctor visits (Centers for Disease Control and Prevention, 2016). Because, in the US, the average number of doctor visits is about three (Centers for Disease Control and Prevention, 2017), this implies that a county with a population of 100,000 has 4965 ILI cases per year, or close to 100 per week.

Thus, our back-of-the-envelope calculation suggests that implementing a sick pay mandate in a metropolitan area with 1 million

**Table 2**  
Effect of city-level sick pay mandates on ILI rates and alternative outcomes 2003–2015.

	ILI rate		Flu shots		Back pain		Gun shows	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatedCity × LawEffective	−0.061*** (0.023)	−0.060*** (0.022)	−0.135 (0.083)	−0.134 (0.083)	0.100 (0.085)	0.101 (0.085)	−0.134 (0.155)	−0.136 (0.156)
N	39,510	39,510	39,510	39,510	39,510	39,510	39,510	39,510

Source: Google Flu (2015) and Google Trends (2017), own calculation and illustration. Standard errors in parentheses are clustered at the city level. The dependent variable is shown in the column header. In the first two columns, it is the ILI rate as reported by Google Flu (2015). All regressions contain week-of-year fixed effects and city fixed effects as in Eq. (1). Each column represents one generalized linear model with a log link. Even numbered columns additionally control for the local monthly unemployment rate (BLS, 2015). *TreatedCity* is a treatment indicator which is one for all cities listed in Table A1. The sample of cities considered is in Table A2.

\* p < 0.1.  
\*\* p < 0.05.  
\*\*\* p < 0.01.

inhabitants (i) provides sick leave coverage for 200,000 employees, and (ii) prevents the transmission of around 3000 ILI cases per year ( $4965 \times 10 \times 0.06 = 2979$ ).

#### 4. Identifying contagious presenteeism and negative externalities

After having provided reduced-form evidence that sick leave coverage reduces the ILI rate at the population level, this section provides an analytical framework that illustrates the underlying behavioral mechanisms.

##### 4.1. Modeling contagious presenteeism and noncontagious absenteeism behavior

We extend and build upon a mix of standard work-leisure models to theoretically study the absence behavior of workers (Allen, 1981; Brown, 1994; Barmby et al., 1994; Brown and Sessions, 1996; Gilleskie, 1998). Our model focuses on the trade-off between employee absenteeism and presenteeism behavior. In particular, it focuses on negative externalities due to contagious diseases. These externalities stem from incomplete employer information about the existence and the degree of contagiousness of employees' diseases. Because contagiousness is not perfectly observable by employers (and customers), employers cannot prevent contagious employees from working and thus cannot fully internalize the externalities. However, we abstain from explicitly modeling the employer side and the firm level below.<sup>3</sup> We also abstain from analyzing general equilibrium labor market effects. To ease the exposition of the model, we first focus on a static model with myopic workers. Thus every period workers decide whether to go to work or not, depending on the implied utilities. In the context of this model, we analyze how sick pay affects worker behavior and infections. In Appendix B, we relax the assumption of workers being myopic.

A representative worker's utility in the current period reads

$$\mu(\sigma, c, l), \quad (2)$$

where  $u$  represents the current utility,  $c \geq 0$  represents current consumption, and  $l \geq 0$  represents current leisure. Utility increases in consumption and leisure over the whole domain. The sickness level is continuous but bounded,  $\sigma \in [0, 1]$ . For  $\sigma = 0$  workers are in perfect health. Their maximum sickness level is  $\sigma = 1$ . Utility decreases over the whole domain of sickness.

Moreover, we assume

$$\frac{\partial^2 \mu}{\partial \sigma \partial l} > 0 \quad \text{and} \quad \frac{\partial^2 \mu}{\partial \sigma \partial c} \leq 0. \quad (3)$$

The first cross derivative implies that leisure (or recuperation) time is more valuable the more severe the disease. The second cross derivative implies that consumption is less valuable the more severe the disease (or entirely independent).

Hours of work are defined as  $h > 0$  and  $T$  is the total amount of time available. Workers consume their entire income from work  $w$  or sick pay  $s = \alpha w$ ,  $\alpha \in [0, 1]$ .<sup>4</sup> If contagiousness and sickness were perfectly observable, we would obtain the first best solution. However, due to information frictions, the employer offers a contract

with payments  $w$  and  $s$  depending on whether workers are giving up their leisure time  $T - h$  or not.

The utility differential between work and work absence can be written as  $\Delta = \mu(\sigma, w, T - h) - \mu(\sigma, s, T)$ . For  $\Delta > 0$  workers will come to work, while they will stay at home for  $\Delta < 0$ . To ensure that the worker works when in perfect health, we assume  $\Delta > 0$  for  $\sigma = 0$ .

For a given replacement rate  $\alpha$ , the sickness reservation level  $\sigma_\alpha^*$  can then be derived from<sup>5</sup>

$$\mu(\sigma^*, w, T - h) - \mu(\sigma^*, \alpha w, T) = 0. \quad (4)$$

If  $\sigma > \sigma_\alpha^*$  workers call in sick. They work if  $\sigma < \sigma_\alpha^*$ .

##### 4.1.1. Changes in sick pay

$F(\sigma)$  is the cumulative density function of  $\sigma$  for  $\sigma > 0$ . Assuming a worker population of size one, at any point in time, a share of  $p$  workers is sick ( $\sigma > 0$ ) and a share of  $1 - p$  workers is healthy ( $\sigma = 0$ ). It follows that for a given replacement rate  $\alpha$ , the share of workers at work  $P$  is the sum of healthy workers and workers with  $\sigma < \sigma_\alpha^*$ , thus  $P = 1 - p + pF(\sigma_\alpha^*)$ . Workers with  $\sigma > \sigma_\alpha^*$  call in sick and the share of workers on sick leave  $A$  is therefore  $A = p(1 - F(\sigma_\alpha^*))$ .

How do changes in sick pay affect the sickness reservation level  $\sigma_\alpha^*$ ? Applying the implicit function theorem to Eq. (4), we obtain (see Appendix B1)

$$\frac{\partial \sigma_\alpha^*}{\partial \alpha} < 0. \quad (5)$$

More sick pay decreases the sickness reservation level. *Ceteris paribus*, more workers call in sick.

Next, we analyze how the shares of absent and present workers change when sick pay changes. We focus on absent workers but the exact opposite holds for present workers as both shares add up to 1. Taking the total derivative of  $A$  with respect to the replacement rate, we obtain

$$\frac{dA}{d\alpha} = \frac{\partial p}{\partial \alpha}(1 - F(\sigma_\alpha^*)) + p \frac{\partial(1 - F(\sigma_\alpha^*))}{\partial \alpha}. \quad (6)$$

For now, we assume that the share of sick workers,  $p$ , is exogenous and thus  $\frac{\partial p}{\partial \alpha} = 0$ . Because of Eq. (5), the second term of Eq. (6) is positive and the share of workers on sick leave increases as sick pay increases. Traditionally, this effect is labeled moral hazard. Workers take more sick leave when sick pay becomes more generous and vice versa.

##### 4.1.2. Two types of diseases and negative externalities due to "contagious presenteeism"

Next, let us assume that two types of (mutually exclusive) diseases exist: (1) contagious diseases denoted by subscript  $c$  (e.g., flu) and (2) noncontagious diseases denoted by subscript  $n$  (e.g., back pain). The share of sick workers equals the sum of workers with contagious and noncontagious diseases  $p = p_n + p_c$ .<sup>6</sup>

As for noncontagious diseases,  $p_n$  is exogenous. Thus, changes in sick pay will only result in behavioral labor supply changes  $\frac{dA_n}{d\alpha} = p_n \frac{\partial(1 - F_n(\sigma_\alpha^*))}{\partial \alpha}$ .

As for contagious diseases,  $p_c$  depends on the share of contagious workers who work. Henceforth, "contagious presenteeism" is when workers with contagious diseases work. Here, the first term of Eq. (6),

<sup>3</sup> This could include employer signaling, peer effects at the firm level, or discrimination against unhealthy workers.

<sup>4</sup> Note that  $w$  also includes career opportunity costs. Scoppa and Vuri (2014) find that absent workers are at a higher risk of getting laid off. Also, workers who call in sick a lot may not get promoted and may not receive bonus payments. Thus, even a nominal replacement rate of 100% (like in Germany) implies  $\alpha < 1$ .

<sup>5</sup> To avoid unnecessary brackets for function values, we write  $\sigma^*(\alpha) = \sigma_\alpha^*$ . There exists a unique  $\sigma_\alpha^*$  because of Eq. (3) and as utility increases in consumption and leisure and decreases in sickness.

<sup>6</sup> In principle, noncontagious diseases represent a special case of contagious diseases, where infections are equal to zero.



representing additional infections, becomes relevant as well; however, it is outside the scope of this paper to model the transmission rate of contagious diseases explicitly (Philipson, 2000; Pichler, 2015). As shown in Eq. (5), more sick pay leads to a lower sickness reservation level and thus fewer infectious workers will work. When fewer contagious workers work who could infect coworkers or customers, infections and the associated negative externalities decrease. It thus holds  $\frac{\partial p_c}{\partial \alpha} < 0$  for the first term of Eq. (6).

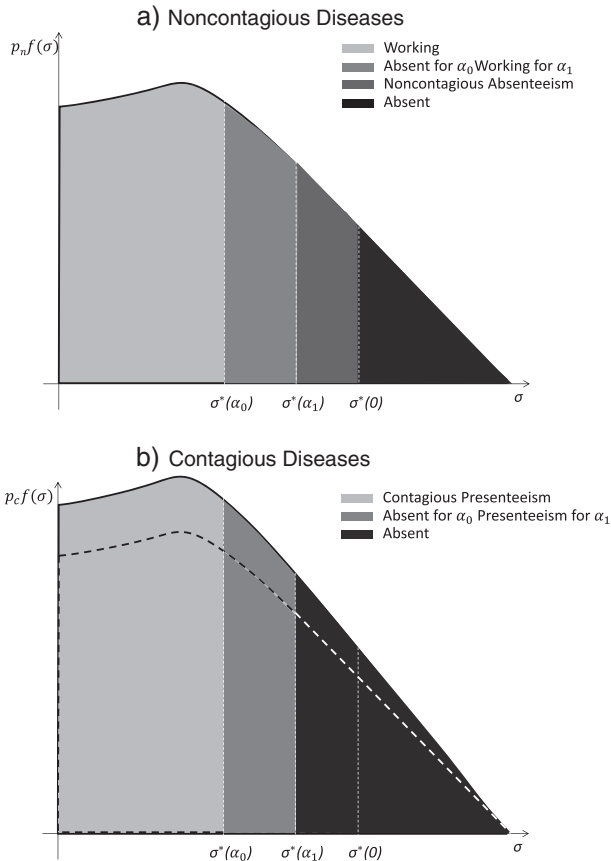
The behavioral labor supply response (the second term in Eq. (6), commonly called moral hazard, can be decomposed into (a) the change in contagious presenteeism and (b) the change in noncontagious absenteeism:

$$p \frac{\partial (1 - F_c(\sigma^*))}{\partial \alpha} = p_c \underbrace{\frac{\partial (1 - F_c(\sigma^*))}{\partial \alpha}}_{>0} + p_n \underbrace{\frac{\partial (1 - F_n(\sigma^*))}{\partial \alpha}}_{>0} \quad (7)$$

It is not surprising that moral hazard is increasing in sick pay. However, the key insight here is that this increase coincides with a decrease in contagious presenteeism which leads to a (normatively desirable) reduction in infections.

#### 4.1.3. Changes in sick pay and sick leave: graphical representation

To simulate the German sick pay reform of 1996 in the next section, Fig. 3 illustrates the effects of a cut in sick pay. Panel a shows the case for noncontagious diseases. Initially with  $\alpha = \alpha_0$ ,



**Fig. 3.** Graphical representation and classification of working and absent employees. **Note:** Panel a shows the share of employees with a noncontagious disease. After the sick pay cut from  $\alpha = \alpha_0$  to  $\alpha = \alpha_1$  with  $\alpha_0 > \alpha_1$ , noncontagious absenteeism decreases. The black area shows individuals who are on sick leave even without sick pay ( $\alpha=0$ ). Panel b shows the case for contagious diseases. A sick pay cut increases contagious presenteeism and  $p_c$ , represented by the outward shift of the curve.

the share of workers who engage in noncontagious absenteeism behavior—indicated by the sum of the two dark gray areas—is quite large. As sick pay decreases to  $\alpha = \alpha_1$ , more workers with noncontagious diseases come to work and the noncontagious absenteeism rate decreases.

Panel b shows the case for contagious diseases. As sick pay decreases, more workers with contagious diseases come to work and contagious presenteeism increases. Because of additional infections, the share of individuals with a contagious disease,  $p_c$ , increases, as represented by the outward shift of  $p_c f(\sigma)$ .

#### 4.1.4. Changes in sick pay and moral hazard: analytical derivation

$\beta_{nt} = \frac{A_{nt} - A_{n0}}{A_{n0}}$  denotes the percentage change in noncontagious absenteeism when sick pay decreases from  $\alpha = \alpha_0$  to  $\alpha = \alpha_t$  with  $\alpha_0 > \alpha_t$ .  $\beta_{nt}$  thus represents the cumulative reform effect at  $t$  or formally

$$\beta_{nt} = \frac{1}{A_{n0}} p_n (F_n(\sigma^*) - F_n(\sigma^*)) < 0. \quad (8)$$

Similarly,  $\beta_{ct} = \frac{A_{ct} - A_{c0}}{A_{c0}}$  denotes the percentage change in contagious absenteeism when sick pay decreases

$$\beta_{ct} = \frac{1}{A_{c0}} \left( \underbrace{p_{c0} (F_c(\sigma^*) - F_c(\sigma^*))}_{<0} + \underbrace{(p_{ct} - p_{c0}) (1 - F_c(\sigma^*))}_{>0} \right). \quad (9)$$

The first element is negative and represents the increase in contagious presenteeism due to a cut in sick pay. It represents the decrease in absenteeism and is a function of the initial share of workers with a contagious disease,  $p_c = p_{c0}$ . The second element represents the increase in absenteeism due to additional infections as a result of the increase in contagious presenteeism (with  $p_c = p_{ct} > p_0$ ). Depending on the share of newly infected individuals due to additional contagious presenteeism, the increase in sick leave due to additional infections can offset or even overcompensate the decrease in sick leave due to the sick pay cut (cf. Stearns and White, 2016). For example, if—at the firm level—one additional worker exhibits contagious presenteeism due to a sick pay cut, then the net effect of the sick pay cut would be zero if this additional worker infected one additional co-worker who then called in sick.

To empirically identify contagious presenteeism, we need one additional assumption:

$$\frac{F_n(\sigma^*) - F_n(\sigma^*)}{1 - F_n(\sigma^*)} = \frac{F_c(\sigma^*) - F_c(\sigma^*)}{1 - F_c(\sigma^*)}. \quad (10)$$

This implies that the share of workers between the two indifference points is identical for contagious and noncontagious diseases. In other words: if one is willing to assume that the share of marginal compliers is identical across the two disease types, then new infections are identified by comparing the labor supply elasticities for contagious and noncontagious diseases. This assumption would be violated if the disutilities differed systematically between contagious and noncontagious diseases. If, for example, all contagious diseases were mild (and the labor supply elasticities for those diseases large) and all noncontagious diseases severe (and the labor supply elasticity small), the assumption would be violated.

Using Eq. (10), we can rewrite  $\beta_{ct}$  as

$$\beta_{ct} = \beta_{nt} + \frac{1}{A_{c0}} ((p_{ct} - p_{c0}) (1 - F_c(\sigma^*))). \quad (11)$$

As seen,  $\beta_{ct}$  and  $\beta_{nt}$  only differ by the share of newly infected individuals, weighted by the share of workers on sick leave prior to the

sick pay cut. Moreover, as seen in Eq. (9), this share is positive and thus, under the existence of contagious presenteeism, it holds that  $\beta_{nt} < \beta_{ct}$ .

Note that by definition,  $\beta_{nt} < 0$ . However, in case of contagious diseases, the sign of  $\beta_{ct}$  is ambiguous. For a very contagious disease,  $\beta_{ct}$  might become positive. Therefore the sign of  $\beta_{ct}$  remains an empirical question.

Finally, the total percentage change in the absence rate,  $\beta_t = \frac{A_t - A_0}{A_0}$ , is

$$\beta_t = \frac{1}{A_0} \left( p_n \left( F_n(\sigma_{\alpha_0}^*) - F_n(\sigma_{\alpha_t}^*) \right) + p_{c0} \left( F_c(\sigma_{\alpha_0}^*) - F_c(\sigma_{\alpha_t}^*) \right) + (p_{ct} - p_{c0})(1 - F_c(\sigma_{\alpha_t}^*)) \right). \quad (12)$$

The Appendix discusses the assumptions of the model and potential model extensions.

#### 4.2. Identifying contagious presenteeism and negative externalities empirically

The paper uses two empirical tests from two different countries to provide (indirect) evidence on the existence of contagious presenteeism.

##### 4.2.1. Using population-level influenza rates to identify contagious presenteeism

In Section 3, we applied a reduced-form test of whether infections decreased after workers gained access to sick pay in the U.S. Sick pay mandates increase sick pay which, according to our model and a rich literature (Section 3.1), increases the absence rate ( $\frac{\partial \sigma_{\alpha}^*}{\partial \alpha} < 0$ ).

Furthermore, our model predicts that access to sick pay reduces contagious presenteeism, which reduces infections. In this subsection, we denote the share of individuals who have a contagious disease under no sick pay coverage with  $p_c = p_{c0}$ . When sick pay is mandated, the share of individuals with a contagious disease will be different; formally we have now  $p_c = p_{ct}$  with  $p_{ct} < p_{c0}$  once sick pay is introduced. The reduction in contagious diseases,  $\phi_t$ , is thus

$$\phi_t = p_{ct} - p_{c0}. \quad (13)$$

In Section 3 we empirically tested whether  $\phi_t = 0$ ; i.e., whether sick pay coverage reduces infectious diseases at the population level. We found  $\phi_t < 0$  yielding empirical evidence for a reduction in contagious presenteeism.

##### 4.2.2. Using disease-specific sick leave rates to identify contagious presenteeism

In the next section, we will directly implement the model and its identifying test. To do so, we need data on sick leave behavior and variation in sick pay that affects different groups of workers. Then we can estimate the causal effect of changes in sick pay on workplace absenteeism and empirically identify  $\beta_t$ .

Moreover, if one can empirically identify two different disease categories,  $c$  and  $n$ , and the share of workers who call in sick due to contagious and noncontagious diseases, one could test whether  $\beta_{nt} < \beta_{ct}$ , as suggested by Eq. (11). Under the identifying assumption in Eq. (10), the differential  $\beta_{ct} - \beta_{nt}$  would then identify additional infections due to contagious presenteeism. These represent negative externalities under lower sick pay.

## 5. Evidence from German sick leave reforms

Our first results section, Section 3, provided reduced-form evidence from the U.S., a country where many workers have no sick pay coverage. We found that more sick pay coverage reduces the ILL

rate at the population level. Then, Section 4 developed a simple theoretical illustration of the potential underlying worker reactions to changes in sick pay, and how these relate to changes in infections. The model section also proposed an identifying test of changes in infections and contagious presenteeism, given a (rather strong) main identifying assumption. The main purpose of this final result section is to illustrate how to implement the proposed test of Section 4 in practice.

The data that we use in this section are not ideal and may not meet the high standards required for causal identification in other settings. However, the data have several strengths: (a) they cover a reform that cut sick pay in Germany, a country with a very generous federal sick pay mandate. (b) They allow us to identify treatment and control groups that were affected differently by the reform. (c) Most important, they include normalized sick leave episodes by type of disease. Moreover, the disease type is not based on self-reports, but on “official” doctor diagnoses.

### 5.1. The German employer sick pay mandate

Germany has one of the most generous universal sick leave systems in the world. The system is predominantly based on employer mandates. In Germany, employers are mandated to continue wage payments for up to six weeks per sickness episode.<sup>7</sup> When employees fall sick and want to take sick leave, they have to inform their employer immediately about their sickness and the expected duration. From the fourth day of a sickness episode, a doctor's note is legally required, but employers can ask for a note from day one. Note that the sickness itself remains confidential. Employees just have to inform their employer that they are sick, not why; the standardized public insurance form for doctors' notes does not indicate the type of disease, which is only confidentially transmitted to the sickness fund. This is important because the model assumes that the type of disease is unobservable to the employer.

### 5.2. The sick pay cut at the end of 1996

In 1996, the center-right government passed a *Bill to Foster Growth and Employment*, effective October 1, 1996. Ziebarth and Karlsson (2010) and Pichler and Ziebarth (2016b) discuss the institutional details. The most important provision of the bill reduced the minimum statutory sick pay level from 100% to 80% of foregone wages. We solely focus on the implementation at the industry level among private sector employees who were covered by collective agreements.

We reviewed all collective agreements that existed during the time of the sick pay reforms and categorized industries accordingly (Ziebarth and Karlsson, 2010; Hans Böckler Stiftung, 2014). One can distinguish three different groups:

**Group I** includes the construction sector whose collective agreement covered about 1.1 million private sector workers. When the law became effective at the end of 1996, the existing collective agreement did not include any explicit provision on sick pay,

<sup>7</sup> From the seventh week onward, sick pay is disbursed by the “sickness funds” and lowered to 70% of foregone gross wages for those who are insured under Statutory Health Insurance (SHI). SHI long-term sick pay was cut from 80 to 70% of foregone gross wages in 1997. See Ziebarth (2013) for details and evidence that this (moderate) cut did not induce significant behavioral reactions among the long-term sick.

which is why all federal regulations applied at the time of the bill's implementation. A negotiated compromise between unions and employers validated the cut but only for the first three days of a sickness episode. The new agreement became effective July 1, 1997.

**Group II** includes about 4.4 million employees and is quantitatively the largest group. It counts 11 industries as specified in Table C2, among them the steel, textile and automobile industry. Union leaders in these industries managed to maintain the symbolically important 100% sick pay level. However, in return, they agreed to exclude paid overtime from the basis of calculation for sick pay. Hence employees with many overtime hours experienced sick pay cuts.

**Group III** includes seven industries (see Table C2). These industries' collective agreements specified 100% sick pay already pre-reform. In contrast to Group II, these industries did not exclude overtime payments from the basis of calculation. Hence the 4 million employees in these industries serve as control group.

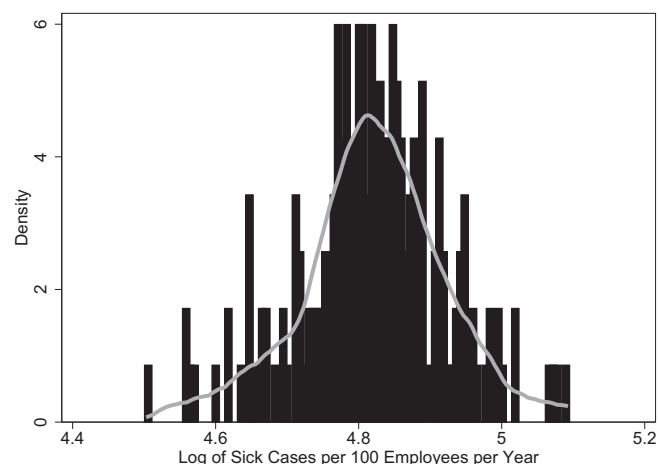


Fig. 4. Distribution of logarithm of sick leave cases per 100 workers and year.

### 5.3. Using data on disease-specific sickness absence to test for elasticity differences

In Germany, information on certified sickness absence—including diagnoses—are collected by the nonprofit SHI sickness funds. In 1995, a total of 960 SHI sickness funds existed; 72% of them were company-based health plans (German Federal Statistical Office, 2014; Schmitz and Ziebarth, 2017; Pilny et al., 2017). Employees covered by these health plans were likely also covered by binding collective agreements (Pichler and Ziebarth, 2016b).

The Federal Association of Company-Based Sickness Funds (BKK Dachverband) publishes annually sick leave statistics of their employed enrollees who are mandatorily insured under SHI (Bundesverband der Betriebskrankenkassen (BKK), 2004).<sup>8</sup> The *Krankheitsartenstatistik* reports aggregated sick leave episodes separately by gender, age group, industry, and main diagnosis according to the *International Classification of Diseases (ICD)*. We collected and digitized information from annual reports between 1994 and 1998 to study the effects of the sick pay cut.<sup>9</sup> Note that the data are only available at the industry level for 12 main disease categories as defined by the ICD; unfortunately, the data are not available at the individual level and we have no discretion over how to categorize the disease groups. The descriptives of the disease groups used are in Table C1 (Appendix).

As seen, in total, we count 1080 observations. Each observation represents one industry, year and sickness category. We make use of 5 years and 18 industries, which adds up to 90 industry-year observations per sickness category.

Our outcome variable is the sick leave rate which measures sick cases per 100 workers. We take the logarithm of each variable, mainly because  $\beta_{nt}$  and  $\beta_{ct}$  (Eqs. (8) and (9)) are expressed in percent and we would like to link the model to the empirical part as closely as possible. Fig. 4 shows this dependent variable and a relatively symmetric, close to normal, distribution. The untransformed sick cases per 100 workers variable has a mean of 126, implying 1.26 sick leave

cases per year and worker across all industries and years. However, the variation ranges from 90 to 163 (Table C1).

The largest sick leave disease category is *respiratory diseases*, ICD codes J00–J99, making up 37 sick cases per 100 workers, or 29% of all cases. We can infer from *separate* statistics on “the most common singular diagnoses” of the *Krankheitsartenstatistik* that a third of all *respiratory diseases* are due to “bronchitis (J20)” and a quarter is due to “influenza (J09).” Another fifth are caused by “acute upper respiratory infections (J06).” Unfortunately, the relevant data by industry are not reported at this fine level but only at the level of the main disease category, i.e., in this case *respiratory diseases*. We conclude that *respiratory diseases* are a mixed category and include contagious as well as noncontagious diseases.

The second largest sick leave disease category is *musculoskeletal diseases* (M00–M99), with 15 cases per 100 workers, or almost 12% of all cases. These have the reputation to be particularly prone to shirking behavior. The main subcategory in this group is “dorsalgia - back pain (M54)” making up 70% of all musculoskeletal cases. Below we use *musculoskeletal diseases* as the main representative category for noncontagious diseases.

Next in terms of their incidence relevance is *injuries and poisoning* (S00–T98, 20%), *digestive diseases* (K00–K93, 14%), followed by *infectious diseases* (A00–B99, 5%). Infectious diseases mainly include “viral infections (B34)” and “infectious gastroenteritis (A09).” Over 80% of all *infectious diseases* fall in these two subcategories. Below we use *infectious diseases* as the main representative category for contagious diseases.

### 5.4. Nonparametric graphical evidence

Fig. 5 shows the “Development of Sick Leave Rates by Treatment Groups over Time.” Fig. 5a displays the total sick leave rate, Fig. 5b displays musculoskeletal diseases, and Figs. 5c and d display respiratory and infectious diseases, respectively. The reference year, 1994, is indexed as 100 and the black vertical bar indicates the reform.

Fig. 5 serves two main purposes: To examine the plausibility of the common time trends assumption, and to illustrate the main findings and help understand how they identify contagious presenteeism within the context of our model. Musculoskeletal sick leave cases (e.g., back pain, Fig. 5b) represent the category “non-infectious diseases”, whereas infectious sick leave cases (Fig. 5d) represent the category “infectious diseases” in our model. Respiratory sick leave cases (Fig. 5c) is a mixed category.

Fig. 5 shows the following: First, in general, the data support the common time trend assumption. Despite some minor spikes here

<sup>8</sup> BKKs are not obliged to contribute to the *Krankheitsartenstatistik*. However, the overwhelming majority does, probably out of tradition to contribute to this important statistic that has been existing since 1976. In 2013, more than 90% of all mandatorily insured BKK enrollees were covered by the *Krankheitsartenstatistik* (BKK, 2004; German Federal Statistical Office, 2014). There is no evidence that this share varied systematically due to the reforms.

<sup>9</sup> We cannot use earlier data due to a lack of measurement consistency. Although the data contain information on the duration of sickness spells by disease groups, we decided to not exploit this information as the theoretical predictions of the reforms on the duration of spells are ambiguous.

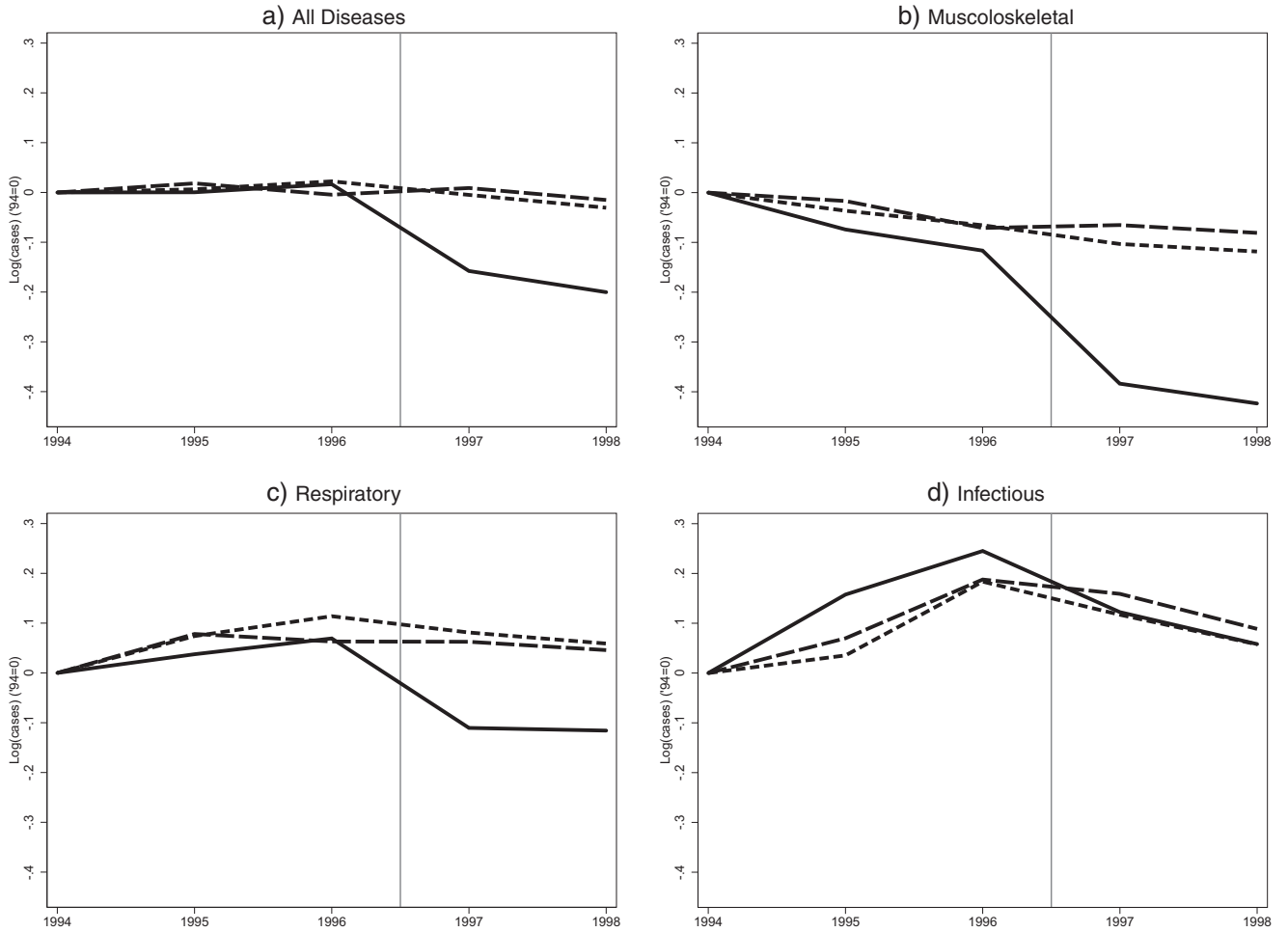


Fig. 5. Development of sick leave rates by treatment groups over time.

**Note:** The solid line represents **Group I** whose sick pay was cut from 100% to 80% at the end of 1996. The short dashed line represents **Group II** who experienced a “soft cut.” The long dashed line represents **Group III** whose sick pay was not cut. For more information about the sick pay reforms, see main text.

and there, all three groups in the four graphs develop in a relatively parallel manner over the pre-reform years.

Second, with the exception of infectious diseases, the other three graphs provide strong evidence of a significant reform effect for treatment Group I. Fig. 5a, plotting the general sick leave rate, shows a 20% decrease post-reform. This is in line with the other two studies evaluating this reform using SOEP data (Ziebarth and Karlsson, 2010; Puhani and Sonderhof, 2010).

Third, the effect of the soft sick pay cut—excluding overtime from the basis of calculation—for treatment Group II was probably minor. In any case, the aggregated industry-year level data are not powerful enough to exploit this reform element to identify contagious presenteeism.

Fourth, as for musculoskeletal diseases (“back pain”) in Fig. 5b—our noncontagious disease category: the decrease is almost twice as large and around –40% for treatment Group I. As for respiratory diseases in Fig. 5c—the mixed category that also includes flu and common colds: the decrease is only around –10%.

Finally, as for infectious diseases in Fig. 5d—the only clean contagious disease category: we do not observe much evidence of a reform effect.

Summing up, (i) here is clear evidence that the sick leave rate decreased significantly following the sick pay cut in Germany,  $\beta_t < 0$ . In addition, we (ii) find a large decrease in back pain cases, suggesting that shirking may have decreased,  $\beta_{nt} < 0$ .

(iii) The annual aggregated labor supply elasticity for contagious diseases appears to be smaller (close to zero) than the elasticity for noncontagious diseases. Thus, there is evidence that  $\beta_{nt} < \beta_{ct}$  holds up. (iv) The increase in presenteeism appears to slightly outweigh additional infections  $\beta_{ct} < 0$ . Finally, because (v)  $\beta_{nt} - \beta_{ct} < 0$ , there is evidence that the sick pay cut may have increased infections through more contagious presenteeism behavior.

##### 5.5. Parametric difference-in-differences model

Next we estimate a conventional parametric DD model separately for the different disease categories:

$$\log(y_{it}) = \beta_1 \text{GroupI}_i \times \text{post} + \beta_2 \text{GroupII}_i \times \text{post} + \delta_t + \gamma_i + \mu_{it} \quad (14)$$

where  $\log(y_{it})$  is the sick leave rate for industry  $i$  in year  $t$ .  $\gamma_i$  are 18 industry fixed effects and  $\delta_t$  are 4 year fixed effects. The standard errors are clustered at the industry level. The reference period is 1994 to 1996.

$\text{GroupI}_i$  and  $\text{GroupII}_i$  are binary treatment indicators which are one for industries that were affected by the sick pay reform (Table C2). Group I experienced a sick pay cut from 100% to 80%, while Group II experienced a “soft cut”—with paid overtime excluded



from the basis of calculation. Group III was not affected, serving as the control group. Thus  $\beta_1$  identifies the effect of the sick pay cut for Group I relative to Group III (over the time period 1997–1998 vs. 1994–1996). Moreover,  $\beta_2$  identifies the effect of excluding paid overtime for Group II relative to Group III (over the same time period).

#### 5.5.1. Decomposing total labor supply adjustments via disease-specific elasticities

**Estimating  $\hat{\beta}_t$ ,  $\hat{\beta}_{nt}$ , and  $\hat{\beta}_{ct}$ .** Table 3 shows the results of the DD model using the four different outcome variables in Fig. 5, plus sick leave due to *injuries and poisoning*. Each column is one model as in Eq. (14). For illustrative purposes, we only display  $\beta_1$  and  $\beta_2$ . Note that the empirical models are closely linked to the theoretical model in Section 4. For example,  $\beta_1$  in the first row of the first column of Table 3 estimates  $\beta_t$  in Eq. (12).

We summarize Table 3: First, the overall sick leave rate decreased by about 18% when sick pay was cut from 100 to 80% (column (1)). This is  $\beta_t$  in Eq. (12) and represents the total labor supply effect, traditionally called “moral hazard.” As seen,  $\beta_1$  is highly significant and clearly smaller than zero. Related to the decrease in sick pay of about 20%, the sickness rate elasticity with respect to the replacement rate would be about 1.

Second, as already inferred from Fig. 5, all  $\beta_2$  interaction effects are imprecisely estimated and relatively small in size. Hence, in a regression framework with industry and year fixed effects, we cannot identify any significant changes in the sick leave rate for Group II. This is at least partly due to the coarse industry-year data.

Third, musculoskeletal diseases, representing the noncontagious disease category  $n$  in our model, decreased overproportionally by 30% (column (4)). This fits the common perception that the labor supply of this category is particularly elastic and prone to shirking behavior. Eq. (8) of our model illustrates the analytical derivation of  $\beta_{nt}$ , which is represented by  $\beta_1$  in column (4) of Table 3. It is the decrease in noncontagious absenteeism when sick pay decreases.

Finally, infectious diseases, representing the contagious disease category  $c$  in our model, decreased underproportionally by an estimated 8% (column (2)). The estimate represents  $\beta_{ct}$  in Eq. (9). Note that this estimate is likely upward biased, because the pre-reform time trends for infectious diseases are not entirely parallel for all three groups (Fig. 5d). The unbiased estimate likely tends toward zero. In any case, while the findings suggest that  $\hat{\beta}_{nt} < \hat{\beta}_{ct}$ , (as suggested by Eq. (11)), the findings also suggest that  $\beta_t < 0$ , implying that the sick pay cut reduced overall sickness absence.

**Further results and robustness checks.** Column (5) is a robustness check because 50% of all *injury and poisoning* absences are due

to workplace accidents (BKK, 2004). The bill that cut sick pay, however, exempted workplace accidents from the cuts. Indeed, as seen by  $\beta_1$  in column (5), the *injury and poisoning* absence rate decreased underproportionally by 11%.

In addition, column (3) also shows underproportional decreases in the sick leave rate for *respiratory diseases* by 16%. Recall that *respiratory diseases* is a mixed category with infectious and noninfectious diseases and includes common colds and flu.

#### 5.5.2. Directly testing $\beta_{nt} = \beta_{ct}$

To directly test the model prediction  $\beta_{nt} = \beta_{ct}$ , see Eq. (11), we now pool all disease categories and estimate a triple difference model in Table C3. The triple difference model is similar to Eq. (14), but pools all disease groups and enriches it with triple interaction terms  $\lambda_1 \text{GroupI}_i \times \text{post} \times \text{Dis}_d$  (in addition to all two-way interactions), where  $\text{Dis}_d$  is a vector of disease indicators.  $\lambda$  then indicates how the reform effect for each disease category differs from the baseline effect.

Column (1) of Table C3 replicates column (4) of Table 3 focusing on *musculoskeletal diseases*, our proxy for noncontagious diseases.

Column (2) adds the contagious category *infectious diseases* and includes 180 industry-year observations. With *musculoskeletal diseases* as the baseline category, the two triple interaction terms  $\text{GroupI}_i \times \text{post} \times \text{Infectious}$  and  $\text{GroupII}_i \times \text{post} \times \text{Infectious}$  directly test  $\beta_{nt} = \beta_{ct}$ . We find a highly significant  $\hat{\beta}_{ct} - \hat{\beta}_{nt} = 21.4$  percentage points. This suggests that the decrease in the contagious sick leave rate was a significant 21.4 percentage points smaller than the decrease in the noncontagious sick leave rate (8.2 vs. 29.6%, see columns (2) and (4) of Table 3 and Fig. 5b and d).

Column (3) additionally adds *respiratory diseases* to the data. As above, the triple interaction terms identify the differential effect relative to *musculoskeletal diseases*. We find that the decrease in the respiratory sick leave rate is about 14 percentage points smaller than then back pain baseline. This difference is significant at the 10% level.

## 6. Conclusion

Empirically identifying presenteeism behavior is extremely challenging, yet crucial to testing for the negative externalities associated with contagious presenteeism, a main economic justification for publicly provided sick pay. Contagious presenteeism occurs when employees with infectious diseases go to work sick and infect coworkers and customers. Such behavior is a public health issue and one driving force of the spread of contagious diseases. If contagion is unobservable, which is usually the case at the beginning of sickness

**Table 3**  
Effect of sick pay cut on sick cases per 100 workers by disease groups.

	All diseases (1)	Infectious (2)	Respiratory (3)	Musculosk. (4)	Inj. & pois. (5)
Group I $\times$ post	−0.177*** (0.042)	−0.082** (0.038)	−0.156*** (0.046)	−0.296*** (0.063)	−0.109*** (0.032)
Group II $\times$ post	−0.020 (0.056)	−0.024 (0.059)	0.000 (0.058)	−0.033 (0.078)	−0.027 (0.059)
Number of industries	18	18	18	18	18

Source: BKK (2004), own calculation and illustration. Standard errors in parentheses are clustered at the industry level. The descriptive statistics are in the Appendix (Table C1). Each column represents one model as in Eq. (14), estimated by OLS, i.e., all models include industry and year fixed effects. The dependent variables are logarithms of the normalized sick cases per 100 workers. Column (1) uses the total number of sick cases as dependent variable, column (2) solely uses infectious sick cases, and so on. Inj. & pois. in column (5) stands for “Injuries and Poisoning.” For more data information, see Section 5.3. Group I’s sick pay was cut from 100 to 80% at the end of 1996. Group II’s sick pay was also cut but the cut was smaller. For more information about the sick pay reforms, see Section 5.2.

\*  $p < 0.1$ .  
\*\*  $p < 0.05$ .  
\*\*\*  $p < 0.01$ .

episodes, then state regulation may reduce market inefficiencies by mandating employers to provide monetary incentives for employees to stay home when sick. If such monetary incentives work, as economic theory and empirical studies strongly suggest, then public sick pay schemes reduce contagious presenteeism and the spread of diseases.

To our knowledge, this study is the first that theoretically derives and empirically implements two tests for the existence of contagious presenteeism and negative externalities in sickness insurance schemes.

First, using standard DD reduced-form methods, we analyze the staggered implementation of employer sick pay mandates at the city level in the U.S.—a country without generous sick pay coverage. Using Google Flu data, we show that influenza-like illness rates decreased significantly when employees gained access to paid sick leave. However, almost half of all U.S. employees do not have access to sick leave insurance. Through the U.S. sick pay mandates, about 20 thousand employees per 100 thousand population gain coverage for themselves and their children. Our estimates suggest that the relatively comprehensive laws at the level of eight major U.S. cities helped prevent about 3000 influenza-like illness cases per year, per 1 million population. Infections rates may further decrease in the medium to long-run when employees have accrued larger amounts of paid sick days.

The middle part of the paper provides a theoretical framework illustrating the behavioral employee reactions to changes in sick pay coverage. The model defines different possible cases of workplace absence behavior under contagious and noncontagious continuous sickness levels. As such, we can decompose classical “moral hazard” into noncontagious absenteeism and contagious presenteeism behavior. The former does not imply negative health spillovers, whereas the latter does. Marginal employees with contagious diseases call in sick instead of working sick when provided with sick leave coverage. We also derive testable conditions for the overall labor supply effect under sickness insurance as well as for its decomposed elements.

Finally, we make use of a German sick pay reform and administrative physician-certified sick leave data at the industry-level to illustrate how one can implement our proposed empirical test for the existence of contagious presenteeism. Under the main, relatively strong, identifying assumption that the first order labor supply reactions are similar for contagious and noncontagious diseases, we also find evidence for the existence of contagious presenteeism in Germany. However, in case of Germany, with one of the most generous sick leave systems worldwide, the reduction in noncontagious absenteeism was clearly larger than the increase in the infectious sick leave rate due to contagious presenteeism when sick pay was cut from a baseline level of 100%.

Researchers could exploit different settings and use our proposed method, or variants of it, to test for the existence and the degree of contagious presenteeism, noncontagious absenteeism, and the overall labor supply adjustments to changes in sick pay. Important fields of applications include contagious presenteeism by teachers or school children; for example, induced by teacher sick pay schemes or parental sick pay schemes that may or may not cover sickness of children. Schools are important sources for the spread of contagious diseases. Another relevant setting would be at the firm level to test for contagious presenteeism behavior by employees with a high degree of customer contact. As a last example, contagious presenteeism behavior by health care workers can be life-threatening for patients and minimized by optimized sick pay schemes. Note that our test can be carried out using many different types of data, including school-level, firm-level data, or hospital-level data. Ideally, one would want to exogenously vary sick pay schemes (e.g., in field experiments) and then measure changes in noncontagious absenteeism and contagious presenteeism behavior.

More research is needed to better understand how exactly contagious presenteeism leads to infections of coworkers and customers and how it affects overall workplace productivity. Firm-level and employee-level compensation strategies to dampen sickness-related productivity losses are also fruitful and relevant research questions.

## Appendix A. United States

**Table A1**

Overview of employer sick pay mandates at the city level in the U.S.

Region	Law passed	Law effective	Content
(1)	(2)	(3)	(4)
San Francisco, CA	Nov 7, 2006	Feb 5, 2007	All employees including part-time and temporary; 1 h of paid sick leave for every 30 h worked; up to 5 to 9 days depending on firm size; for own sickness or family member; 90 days accrual period
Washington, DC	May 13, 2008	Nov 13, 2008	‘Qualified employees’; 1 h of paid sick leave for every 43 h, 90 days accrual period; up to 3 to 9 days depend. on firm size; own sickness or family; no health care or restaurant workers
Seattle, WA	Dec 18, 2013 Sep 12, 2011	Feb 22, 2014 Sep 1, 2012	Extension to 20,000 temporary workers and tipped employees All employees in firms with >4 full-time employees; 1 h for every 30 or 40 h worked; up to 5 to 13 days depending on firm size, for own sickness or family member; 180 days accrual period
New York, NY	June 26, 2013 Jan 17, 2014	April 1, 2014	Employees with >80 h p.a in firms >4 employees or 1 domestic worker; 1 h for every 30 h; Up to 40 h; own sickness or family member; 120 days accrual period
Portland, OR	March 13, 2013	Jan 1 2014	Employees with >250 h p.a. in firms >5 employees; 1 h for every 30 h; up to 40 h; own sickness or family member
Newark, NJ	Jan 29, 2014	May 29, 2014	All employees in private companies; 1 h of for every 30 h; 90 days accrual period; up to 24 to 40 h depending on size; own sickness or family
Philadelphia, PA	Feb 12, 2015	May 13, 2015	Employees in firms >9 employees; 1 h of paid sick leave for every 40 h; 90 days accrual period; up to 40 h; own sickness or family member
Oakland, CA	Nov 4, 2014	March 2, 2015	Employees in firms >9 employees; 1 h of paid sick leave for every 30 h; 90 days accrual period; up to 40 to 72 h depending on firm size; own sickness or family member

Source: several sources, own collection, own illustration.

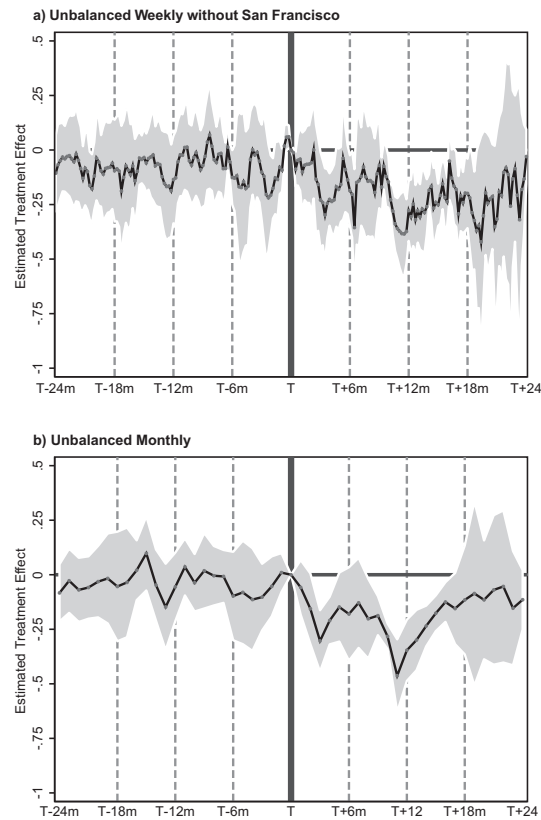


Fig. A1. Robustness checks for event studies.

Table A2

Cities Included in Weekly Google Flu Data.

City	Month	Day	Year
Albany, NY	9	28	2003
Albuquerque, NM	10	12	2003
Anchorage, AK	10	17	2004
Arlington, VA	9	28	2003
Atlanta, GA	9	28	2003
Austin, TX	9	28	2003
Baltimore, MD	9	28	2003
Baton Rouge, LA	9	26	2004
Beaverton, OR	12	14	2003
Bellevue, WA	11	30	2003
Berkeley, CA	9	19	2004
Birmingham, AL	9	28	2003
Boise, ID	10	3	2004
Boston, MA	9	28	2003
Buffalo, NY	10	19	2003
Cary, NC	9	26	2004
Charlotte, NC	9	28	2003
Chicago, IL	9	28	2003
Cleveland, OH	9	28	2003
Colorado Springs, CO	9	19	2004
Columbia, SC	10	10	2004
Columbus, OH	9	28	2003
Dallas, TX	9	28	2003
Dayton, OH	11	23	2003
Denver, CO	9	28	2003
Des Moines, IA	10	17	2004
Durham, NC	9	28	2003
Eugene, OR	10	17	2004
Fresno, CA	12	7	2003
Ft Worth, TX	10	3	2004
Gainesville, FL	10	12	2003
Grand Rapids, MI	10	3	2004

**Table A2** (continued)

City	Month	Day	Year
Greensboro, NC	11	14	2004
Greenville, SC	10	24	2004
Honolulu, HI	9	28	2003
Houston, TX	9	28	2003
Indianapolis, IN	9	28	2003
Irvine, CA	10	3	2004
Irving, TX	9	28	2003
Jackson, MS	11	14	2004
Jacksonville, FL	10	3	2004
Kansas City, MO	9	28	2003
Knoxville, TN	10	3	2004
Las Vegas, NV	9	28	2003
Lexington, KY	9	26	2004
Lincoln, NE	10	31	2004
Little Rock, AR	10	3	2004
Los Angeles, CA	9	28	2003
Lubbock, TX	10	17	2004
Madison, WI	9	28	2003
Memphis, TN	10	24	2004
Mesa, AZ	11	7	2004
Miami, FL	9	28	2003
Milwaukee, WI	9	28	2003
Nashville, TN	9	28	2003
New York, NY	9	28	2003
Newark, NJ	9	28	2003
Norfolk, VA	9	28	2003
Oakland, CA	9	28	2003
Oklahoma City, OK	9	28	2003
Omaha, NE	9	28	2003
Orlando, FL	9	28	2003
Philadelphia, PA	9	28	2003
Phoenix, AZ	9	28	2003
Pittsburgh, PA	9	28	2003
Plano, TX	10	16	2005
Portland, OR	9	28	2003
Providence, RI	10	17	2004
Raleigh, NC	9	28	2003
Reno, NV	10	24	2004
Reston, VA	11	28	2004
Richmond, VA	9	28	2003
Rochester, NY	9	28	2003
Roswell, GA	11	23	2003
Sacramento, CA	9	28	2003
Salt Lake City, UT	9	28	2003
San Antonio, TX	9	28	2003
San Diego, CA	9	28	2003
San Francisco, CA	9	28	2003
San Jose, CA	9	28	2003
Santa Clara, CA	9	28	2003
Scottsdale, AZ	10	24	2004
Seattle, WA	9	28	2003
Somerville, MA	9	28	2003
Spokane, WA	1	16	2005
Springfield, MO	10	30	2005
St Louis, MO	9	28	2003
St Paul, MN	9	28	2003
State College, PA	9	5	2004
Sunnyvale, CA	9	28	2003
Tampa, FL	9	28	2003
Tempe, AZ	9	28	2003
Tucson, AZ	9	28	2003
Tulsa, OK	9	28	2003
Washington, DC	9	28	2003
Wichita, KS	9	26	2004

Source: Google Flu (2015), own collection, own illustration. The table indicates the first observation period and all cities included in the study. The last observation period is July 26, 2015 for the whole sample. Treated cities are in bold. Cities in gray are excluded because they were covered by a state, not a city mandate, or because they were in close proximity to the treated cities (Bellevue, WA is 10 miles away from Seattle, WA. Baltimore, MD is 40 miles away from Washington, DC)



## Appendix B. Model

### B1. Derivation of Eq. (5)

With the implicit function theorem applied to Eq. (4), one obtains

$$\frac{\partial \sigma^*}{\partial \alpha} = \frac{\frac{\partial u(\sigma, \alpha w, T)}{\partial c} \frac{\partial c}{\partial \alpha}}{\frac{\partial u(\sigma, w, T-h)}{\partial \sigma} - \frac{\partial u(\sigma, \alpha w, T)}{\partial \sigma}}. \quad (B1)$$

The denominator can then be rewritten as the sum of

$$- \left( \underbrace{\frac{\partial \mu(\sigma, \alpha w, T)}{\partial \sigma} - \frac{\partial \mu(\sigma, \alpha w, T-h)}{\partial \sigma}}_{>0(3)} \right),$$

and

$$\left( \underbrace{\frac{\partial \mu(\sigma, w, T-h)}{\partial \sigma} - \frac{\partial \mu(\sigma, \alpha w, T-h)}{\partial \sigma}}_{\leq 0(3)} \right).$$

Thus the expression is negative and the inequality follows directly from a positive numerator and a negative denominator due to  $\alpha \in [0, 1]$ ,  $T-h < T$  and Eq. (3). The numerator is positive because utility increases in the replacement rate  $\alpha$ . And the denominator is negative because of the assumption that leisure time is more valuable when sick (Eq. (3)).

### B2. Discussion of assumptions and possible model extensions

In addition to the assumptions discussed in the main text (incomplete information about contagiousness, similar disutility and labor supply for contagious and noncontagious diseases,...), we discuss additional implicit model assumptions below.

First, we abstract away from savings to simplify the model. In Eq. (3), we assume that the marginal utility of consumption depends on the sickness level. The higher the sickness level, the lower the marginal utility of consumption. However, with savings, sick workers could transfer consumption into the future. In that case, Eq. (4) would imply a rather strong assumption. However, we could simply allow for savings and then (realistically) assume  $\frac{\partial^2 u}{\partial \sigma \partial c} = 0$  (independence of sickness and marginal consumption utility).

Second, we discussed the possibility that sick leave could affect workers' future wages, including a higher likelihood of dismissal. However, we do not model this mechanism explicitly. The benefits of doing it are limited and it would make the model unnecessarily complex. Similarly, sick leave could directly affect future health and productivity. The effects could go in both directions (fully recovering from a disease could increase future health and productivity; however, workers might also lose firm-specific human capital). In addition, these mechanisms likely differ by disease and occupation and would require to model complex effect heterogeneity.

Third, to keep the model tractable, so far, we have implicitly assumed that workers optimize their utility by only considering the current time period, i.e., they are myopic. However, sick leave in  $t$  can affect sickness in  $t+1$  because workplace absences reduce the likelihood of an infection. We have not modeled this channel explicitly yet. When considering this possibility, the expected utility in  $t+1$  conditional on being absent or present in  $t$  (see Eq. (4)) becomes

$$\mu_t(\sigma^*, w, T-h) + \beta E[\mu_{t+1} | \text{present}_t] - \mu_t(\sigma^*, \alpha w, T) - \beta E[\mu_{t+1} | \text{absent}_t] = 0 \quad (B2)$$

where  $\beta$  is the discount rate.

Next we derive how the sickness reservation level responds to changes in sick pay as in Eq. (B1). First, in this case, the numerator equals the derivative of Eq. (B2) with respect to  $\alpha$ . Multiplied by  $(-1)$ , it reads

$$\frac{\partial \mu(\sigma, \alpha w, T)}{\partial c} \frac{\partial c}{\partial \alpha} + \frac{\partial \beta E[\mu_{t+1} | \text{absent}_t]}{\partial \alpha} - \frac{\partial \beta E[\mu_{t+1} | \text{present}_t]}{\partial \alpha} > 0 \quad (B3)$$

As before,  $\frac{\partial u(\sigma, \alpha w, T)}{\partial c} \frac{\partial c}{\partial \alpha}$  is positive. In addition, the effects of sick pay on  $E[\mu_{t+1} | \text{absent}_t]$  are negligible because slightly more sick pay in  $t$  is unlikely to affect  $\mu_{t+1}$ .<sup>10</sup> Finally,  $\frac{\partial \beta E[\mu_{t+1} | \text{present}_t]}{\partial \alpha} < 0$  because, *ceteris paribus*, increasing sick pay decreases contagious presenteeism and thus the likelihood of future infections decreases. Therefore the numerator of Eq. (B1) becomes smaller.

In words: if workers were fully rational and expected to meet many sick co-workers at work after a sick pay cut, they might prefer to stay at home in order to avoid infections. However, as it is very difficult to predict infections, abstracting away from this mechanism appears to be a realistic assumption. Moreover, as the flu season extends over several months, it is unrealistic that workers call in sick over a longer period of time to avoid infections. Thus,  $\frac{\partial \beta E[\mu_{t+1} | \text{present}_t]}{\partial \alpha}$  is smaller than  $\frac{\partial \mu(\sigma, \alpha w, T)}{\partial c} \frac{\partial c}{\partial \alpha}$ , which means that the numerator represented by Eq. (B3) remains

<sup>10</sup> An example for a large term could be: medicine only becomes affordable once sick pay exceeds a certain threshold and this medicine would cure the disease only in combination with sickness absence.

positive. Overall, it implies that workers' direct/first order response (more sick pay leads to more consumption when sick) is stronger than indirect/second order response (more sick pay increases the likelihood of being infected).

Second, the denominator equals the derivative of Eq. (B2) with respect to  $\sigma$

$$\frac{\partial \mu(\sigma, w, T - h)}{\partial \sigma} - \frac{\partial u(\sigma, \alpha w, T)}{\partial \sigma} + \frac{\partial \beta E[\mu_{t+1} | \text{present}_t]}{\partial \sigma_t} - \frac{\partial \beta E[\mu_{t+1} | \text{absent}_t]}{\partial \sigma_t} < 0 \quad (\text{B4})$$

The assumption of myopic workers implies  $\frac{\partial \beta E[\mu_{t+1} | \text{present}_t]}{\partial \sigma_t} - \frac{\partial \beta E[\mu_{t+1} | \text{absent}_t]}{\partial \sigma_t} = 0$ . However, a higher sickness level today will likely decrease expected utility tomorrow as individual health is correlated over time. This decrease in expected utility is most likely even larger if workers work sick instead of recovering at home. Thus the entire denominator as shown in Eq. (B4) becomes even more negative when we relax the myopia assumption.

In sum, relaxing the assumption of worker myopia results in a positive numerator (Eq. (B3)) and a negative denominator (Eq. (B4)); the extended version of Eq. (B1) remains negative and more workers call in sick when sick leave increases.

## Appendix C. Germany

**Table C1**

Descriptive statistics of sick leave measures.

Variable	Mean	Std. dev.	Min.	Max.	N
Total sick cases per 100 workers	125.813	12.968	90.269	162.834	90
Total log(cases)	4.829	0.106	4.503	5.093	90
Infectious sick cases per 100 workers	5.881	0.981	3.896	9.698	90
Infectious log(cases)	1.759	0.159	1.36	2.272	90
Respiratory sick cases per 100 workers	36.59	4.099	26.34	50.049	90
Respiratory log(cases)	3.594	0.113	3.271	3.913	90
Digestive sick cases per 100 workers	18.141	1.964	13.356	24.049	90
Digestive log(cases)	2.892	0.108	2.592	3.18	90
Musculoskeletal sick cases per 100 workers	14.549	3.596	6.824	23.485	90
Musculoskeletal log(cases)	2.646	0.258	1.92	3.156	90
Injury sick cases per 100 workers	25.031	4.926	9.752	34.379	90
Injury log(cases)	3.194	0.25	2.278	3.537	90

Source: BKK (2004), own calculations and illustration. Descriptives are weighted by the annual number of industry-specific sickness fund enrollees.

**Table C2**

Number of enrollees per industry and treatment group.

Industry and classification	Mean	Std. dev.
<b>Group I</b>		
Construction	31,693	13,913
<b>Group II</b>		
Steel	114,236	8831
Textile	33,034	11,579
Mechanical engineering	162,364	35,168
Automobile	311,173	42,239
Ship and aerospace	26,090	7698
Electrical engineering, optics	251,284	27,410
Wood and paper	33,375	6146
Printing	21,111	3080
Food and hospitality	27,074	4518
Trade	135,812	35,417
Banking and insurance	85,851	15,263
<b>Group III</b>		
Chemical	233,675	63,750
Oil	11,572	2218
Glass	36,484	7417
Energy and water	39,670	8399
Postal and transportation	513,328	138,192
Public administration	317,137	63,066

Source: Bundesverband der Betriebskrankenkassen (BKK) (2004), own calculation and illustration.

**Table C3**  
Effect of sick pay on sick leave—pooled regressions.

	(1)	(2)	(3)
	Musculoskeletal	Musculoskeletal, infectious	Muscul., infect. respiratory
Group I × post	−0.296*** (0.063)	−0.296*** (0.062)	−0.296*** (0.062)
Group II × post	−0.033 (0.078)	−0.033 (0.078)	−0.033 (0.077)
Group I × post × Infectious		0.214*** (0.073)	0.214*** (0.072)
Group II × post × Infectious		0.009 (0.097)	0.009 (0.096)
Group I × post × Respiratory			0.141* (0.077)
Group II × post × Respiratory			0.033 (0.096)
Observations	90	180	270

Source: BKK (2004), own calculation and illustration. Standard errors in parentheses are clustered at the industry-disease level. The descriptive statistics are in Table C1. The regressions are based on Eq. (1). The model in the first column equals the fifth column of Table 3. The model in the second column pools the two categories musculoskeletal and infectious sick leave, where musculoskeletal sick leave is the reference group. The third column additionally adds respiratory sick leave. All regressions are estimated by OLS and include industry, disease and year fixed effects. The dependent variables are logarithms of the normalized sick leave cases per 100 workers. For more information on how the variables were generated, see Section 5.3.

\*  $p < 0.1$ .

\*\*  $p < 0.05$ .

\*\*\*  $p < 0.01$ .

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