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THE PROS AND CONS OF SICK PAY SCHEMES:
TESTING FOR CONTAGIOUS PRESENTEEISM AND NONCONTAGIOUS ABSENTEEISM BEHAVIOR

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The Pros and Cons of Sick Pay Schemes: Testing for Contagious Presenteeism and Noncontagious Absenteeism Behavior
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ABSTRACT

This paper provides an analytical framework and uses data from the US and Germany to test for the existence of contagious presenteeism and negative externalities in sickness insurance schemes. The first part exploits high-frequency Google Flu data and the staggered implementation of U.S. sick leave reforms to show in a reduced-from 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 mechanisms. The model theoretically decomposes overall behavioral 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 German sick pay reforms and administrative industry-level data on certified sick leave by diagnoses. It finds that the labor supply elasticity for contagious diseases is significantly smaller than for noncontagious diseases. Under the identifying assumptions of the model, in addition to the evidence from the U.S., this finding provides indirect evidence for the existence of contagious presenteeism.

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The Pros and Cons of Sick Pay Schemes: Testing for Contagious Presenteeism and Noncontagious Absenteeism Behavior‡

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Abstract

This paper provides an analytical framework and uses data from the US and Germany to test for the existence of contagious presenteeism and negative externalities in sickness insurance schemes. The first part exploits high-frequency Google Flu data and the staggered implementation of U.S. sick leave reforms to show in a reduced-from 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 mechanisms. The model theoretically decomposes overall behavioral 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 German sick pay reforms and administrative industry-level data on certified sick leave by diagnoses. It finds that the labor supply elasticity for contagious diseases is significantly smaller than for noncontagious diseases. Under the identifying assumptions of the model, in addition to the evidence from the U.S., this finding provides indirect evidence for the existence of contagious presenteeism.

Keywords: Sickness Insurance, Paid Sick Leave, Presenteeism, Contagious Diseases, Infections, Negative Externalities, Absenteeism, U.S., Germany

JEL classification: I12, I13, I18, J22, J28, J32

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"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

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. Although various definitions exist (Simpson 1998), going to work despite being sick is commonly referred to as "presenteeism." Particularly in professions with direct customer contact, presenteeism in combination with contagious diseases leads to negative externalities and infection spillovers for co-workers and customers. Given the low influenza vaccination rates of around 40 percent in the U.S. and 10-30 percent 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. After the first occurrence of flu sickness symptoms, humans are contagious for 5-7 days (Centers for Disease Control and Prevention 2016). Over-the-counter (OTC) drugs that suppress symptoms, but not contagiousness, promote the spread of disease in cases

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of presenteeism and noninsured workplace absenteeism (Earn et al. 2014). Worldwide, seasonal influenza epidemics alone lead to 3-5 million severe illnesses and an estimated 250,000-500,000 deaths; in the U.S., flu-associated annual deaths range from 3,000 to 49,000 (World Health Organization 2014; Centers for Disease Control and Prevention 2016).

Historically, paid sick leave was actually one of the first social insurance pillars worldwide; this policy was included in the first federal health insurance legislation. Under Otto van 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. The costs associated with paid sick leave initially made up more than half of all program costs, given the limited availability of (expensive) medical treatments in the nineteenth century (Busse and Riesberg 2004). Today, virtually every European country has some form of universal access to paid sick leave—with varying degrees of generosity.

Opponents of universal paid sick leave point to the fact that such social insurance systems would encourage shirking behavior and reduce labor supply. Moreover, forcing employers to provide sick pay via mandates or new taxes would dampen job creation and hurt employment. A final argument against government-mandated paid sick leave states that, when coverage is optimal, the private market would ensure that employers voluntarily provide such benefits.

The U.S. is the only industrialized country worldwide without universal access to paid sick leave (Heymann et al. 2009). Half of all American employees have no access to paid sick leave, particularly low-income and service sector workers (Lovell 2003; Boots et al. 2009; Susser and Ziebarth 2016). However, support for sick leave mandates in the U.S. has grown substantially in the last decade. On the city level, sick leave schemes have been implemented in San Francisco, Washington D.C., Seattle, Philadelphia, Portland, and New York City, among other cities. On the state level, Connecticut was the first state to introduce a sick leave scheme in 2012 (for service sector workers in non-small businesses). California, Massachusetts, and Oregon followed in 2015. At the federal level, reintroduced in Congress in March 2013, the Healthy Families Act foresees the introduction of universal paid sick leave for up to seven days per employee and year. The epigraphs above demonstrate the support among Democrats and conservatives alike.

As discussed, one economic argument for paid sick leave hinges crucially on the existence of negative externalities and presenteeism with regard to contagious diseases. Despite being of tremendous relevance, empirically proving the existence of presenteeism with contagious diseases is extremely difficult, if not impossible, because contagiousness is generally unobservable. Several empirical papers evaluate the causal effects of cuts in sick pay and find that employees adjust their labor supply in response to such cuts (Johansson and Palme 1996, 2005; De Paola et al. 2014; Ziebarth and Karlsson 2010, 2014; Dale-Olsen 2014; Fevang et al. 2014).[1] Traditionally, behavioral adjustments to varying levels of insurance generosity is labeled 'moral hazard' in economics (Pauly 1974, 1983; Arnott and Stiglitz 1991; Nyman 1999; Newhouse 2006; Felder 2008; Bhattacharya and Packalen 2012). However, in the case of sick leave, being able to disentangle shirking behavior from presenteeism is crucial in order to derive valid policy conclusions.

One main objective of this paper is to provide an analytical framework that illustrates the underlying behavioral mechanisms when sick pay generosity changes. It decomposes the overall labor supply adjustments into what we call 'contagious presenteeism' as well as 'noncontagious absenteeism.' The paper applies two different approaches to (indirectly) test for the existence of contagious presenteeism, and associated negative externalities. To our knowledge, this paper is the first in the economic literature to define and test for the existence of contagious presenteeism. The empirical tests exploit variation in the generosity of sick pay for one of the most generous sick leave systems in the world, Germany, and one of the least generous sick leave systems in the world, the US. 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. One key element of our proposed theoretical mechanism is private information about the type of disease that workers contract. Supported by intuition and empirical evidence (Pauly et al. 2008), employers have only incomplete information about employees' contagiousness and do not fully internalize the negative externalities induced by the spread of contagious diseases to coworkers and customers. Sick pay schemes incentivize contagious employees to stay at home but also induce noncontagious employees to engage in absenteeism behavior.

The first part of this paper exploits high-frequency Google Flu data to evaluate the impact of U.S. sick pay mandates on influenza-like disease rates at the population level. The staggered implementation of several sick pay schemes at the regional level in the U.S. naturally leads to the estimation of standard difference-in-differences (DD) models. Although the U.S. sick pay schemes vary in their comprehensiveness, and some have exemptions reducing the effectiveness

[1] Other papers in the literature on sickness absence looked at and decomposed general determinants (Barmby et al. 1994; Markussen et al. 2011), investigated 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).

of lowering infection rates, we can show the following: When U.S. employees gain access to paid sick leave, the general flu rate in the population decreases significantly. This finding yields strong reduced-form evidence for the existence of contagious presenteeism. It suggests that a reduction in contagious presenteeism occurs when sick pay coverage increases, resulting in fewer infections and lower influenza activity. This paper is one of the first to study the introduction of sick pay mandates in the U.S. (Ahn and Yelowitz 2015; Pichler and Ziebarth 2016, are two exceptions). It is also one of the first economic papers to exploit high-frequency data from Google Flu Trends, a rich data set that assesses influenza activity on a weekly basis starting in 2003.

The second part of the paper provides an analytical framework that illustrates the underlying behavioral mechanisms when employees gain access to paid sick leave. The simple model decomposes traditional 'moral hazard' into noncontagious absenteeism and contagious presenteeism. The model allows us to provide a very concise definition of what we mean by contagious presenteeism: workplace attendance while having a contagious disease. Negative externalities can then be identified by assessing changes in infections after changes in sick pay. The model predicts that changes in sick pay generosity induce changes in the two (potentially undesired) behaviors that work in opposite directions: noncontagious absenteeism and contagious presenteeism. We explicitly refrain from a normative welfare analysis, which would require to weight these two phenomena, depending on societal preferences. Rather, we provide a positive analysis and the first approach to theoretically define and empirically identify these countervailing effects. Note that the theory and empirical sections do not hinge on whether the sick pay scheme is mandated by the government.

The final part of the paper serves as an illustration of how to estimate our model and the proposed test for contagious presenteeism. It exploits two German policy reforms that varied the level of sick pay. Using administrative data aggregated at the industry level and variation in industry-specific sick pay regulations, sick pay cuts from 100 to 80% of foregone wages reduced overall sickness rates by about 20%. This is in line with the standard predictions of our model and the previous literature (Johansson and Palme 1996, 2005; Ziebarth and Karlsson 2010; De Paola et al. 2014; Ziebarth and Karlsson 2014; Fevang et al. 2014). Next, and more importantly, we analyze the labor supply effects by certified disease categories. In line with the theoretical model implications, we find disproportionately large labor supply adjustments for musculoskeletal diseases ('back pain'). Meanwhile, the labor supply adjustments in case of infectious diseases are significantly smaller. Within the context of our model and under the assumption of similar labor supply elasticities for contagious and noncontagious diseases, the differences between the small labor supply effects for contagious diseases and the large labor supply effects for noncontagious diseases are a function of additional infections due to contagious presenteeism. Additional infections increase sick leave rates of infectious diseases and countervail decreases due to lower sick pay. Thus, when mandated sick pay is lowered, policymakers have to consider the trade-off between the short-run effect of a reduction in noncontagious absenteeism vs. an increase in contagious presenteeism leading to a higher infection rate and more relapses in the medium-run.

Obviously, this paper is close in spirit to papers that estimate causal labor supply effects of changes in sick pay levels (Johansson and Palme 1996, 2002, 2005; Hesselius et al. 2009; Ziebarth 2013; Ziebarth and Karlsson 2010, 2014). However, none of these papers estimates labor supply effects by disease groups or estimates effects on contagious disease rates. In particular, this paper extends the small economic literature on presenteeism at the workplace (Aronsson et al. 2000; Chatterji and Tilley 2002; Brown and Sessions 2004; Pauly et al. 2008; Barmby and Larguem 2009; Johns 2010; Böckerman and Laukkanen 2010; Markussen et al. 2012; Pichler 2015; Hirsch et al. 2015; Ahn and Yelowitz 2016). With one exception, none of the empirical studies on presenteeism just cited identifies or intends to identify causal effects of sick leave schemes on presenteeism. The exception is Markussen et al. (2012) who study the impact of partial absence certificates on what they label 'presenteeism.' However, they define presenteeism very broadly—as a general increase in labor supply when activation requirements become tighter. 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. 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 nicely to an economic model of absence behavior.

This paper also adds to the literature on the determinants and consequences of infectious diseases, epidemics and vaccinations (Mullahy 1999; Bruine de Bruin et al. 2011; Uscher-Pines et al. 2011; Ahn and Trogdon 2015; Stoecker et al. 2016; Adda 2016). For example, Maurer (2009) models supply and demand side factors of influenza immunization, 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 percent increase in influenza deaths for the elderly in counties whose team participate in the Super Bowl. Their findings suggest that influenza transmissions at gatherings related to large spectator events are the underlying mechanism. Adda (2016) shows that reductions in inter-personal contacts, e.g. through school closures or the closure of public transportation networks, reduce transmission rates.

2 Evidence from U.S. Sick Leave Reforms

Whereas Germany has one of the most generous sick leave systems worldwide, the U.S. represents one of the least generous systems. Using high-frequency data from Google Flu at the weekly level over more than a decade, this section assesses the impact of U.S. sick pay mandates on influenza-like disease rates at the population level (Google 2015).

2.1 The U.S. Sick Leave Landscape

The U.S. is the only industrialized country without universal access to paid sick leave. About half of the 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 comprehensive summary of recent sick pay reform at the city and state level. The details of the bills differ from city to city and state to state but, basically, all sick pay schemes represent employer mandates. Mostly small firms are exempt or face less restrictions. Employees "earn" paid sick pay credit (typically one hour per 40 hours worked) up to nine days per year, and this credit rolls over to the next calendar year if unused. Because employees need to accrue sick pay credit, 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 schemes. Note that gaining the right to take unpaid leave can be seen as a normalization and equals an increase in sick leave benefits because the right to take unpaid leave decreases the likelihood of being fired when calling in sick.

As Table A1 shows, San Francisco was the first city to introduce paid sick leave on February 5, 2007. Washington, D.C., followed on November 13, 2008, and extended its sick pay in February 22, 2014 to temporary workers and tipped employees. Seattle (September 1, 2012), Portland (January 1, 2014), New York City (April 1, 2014), and Philadelphia (May 13, 2015) followed. Connecticut (January 1, 2012) was the first US state to pass a sick leave mandate; however, it only applied to service sector employees in non-small businesses and covered solely 20 percent of the workforce. Very recent newly introduced schemes in California (July 1, 2015), Massachusetts (July 1, 2015), and Oregon (Jan 1, 2016) are significantly more comprehensive (see Table A1).

2.2 Exploiting Google Flu Trends Data to Test for Changes in Infections: 2003–2015

We exploit weekly Google Flu Trends data at the city and state level from 2003 to 2015 to test for changes in influenza rates following the introduction of sick pay schemes (Google 2015). Google provides these data in processed form. The basic idea is that Google search queries can be used to predict and replicate actual influenza infection rates. It has been shown that Google Flu Trends accurately estimates weekly influenza activity in each region of the U.S. (Carneiro and Mylonakis 2009; Ginsberg et al. 2009).

We use two main Google Flu Trends samples. The first sample contains the weekly flu rates of all major U.S. cities—97 in total—from 2003 to 2015, as listed in columns (1) and (2) of Appendix Table A2. The specific start dates are also listed in Table A2.[2] We include data for most cities starting September 28, 2003. The end date for all cities is July 26, 2015. For our first sample of U.S. metropolitan areas, this results in 49,560 city-week observations. The second sample contains all U.S. states and counts 30,141 state-week observations.

Generated outcome variable.

We use the data that is provided by Google (2015), aggregated at the regional week level. Google Flu recalculates search queries into influenza-like illnesses (ILI) per 100,000 doctor visits.[3] The mean for the city sample is 1,913 and for the state sample 1,703. We take the natural logarithm of this variable as dependent variable.

Hence the dependent variable can be interpreted as "diagnosed influenza-like illnesses (ILI)." Because—unlike in Germany—the U.S. sick pay mandates do not require a doctor's note in order to take sick pay, one would not expect that doctor visits increase due to the sick pay reforms. However, even if that was the case, it still would not be a main threat to our estimates—our estimate of the decrease in influenza-like activity would then represent a lower bound.

Treatment and control groups.

Appendix Table A1 provides the list of cities and states that implemented sick pay schemes between 2006 and 2015. When using our first sample of cities, all seven listed major cities and Washington, D.C., belong to the treatment group and all other cities to the control group. Analogously, the five states that implemented sick pay schemes so far—District of Columbia, Connecticut, California, Massachusetts, and Oregon—belong to the treatment group in the second sample with state-week observations.

In addition to Google Flu Trends data, we use data from the Bureau of Labor Statistics (BLS 2015) to control for monthly unemployment rates in our model. The unit of observation in the BLS

[2] We omit the city of New Orleans, which was missing variables of interest due to Hurricane Katrina. In the first sample, we also omit the cities that were not treated through a city mandate but through a state mandate and are already included in the second sample.

[3] The original purpose for recalculating this measure by Google was to be able to compare the search queries to a meaningful measure.

data is equal to the unit of observation in the Google Flu Trends data. Accordingly, we merge in BLS monthly unemployment rates at the level of the cities and states as reported in Table A2.

Assessing Google Flu Measurement Error.

Lazer et al. (2014) reports that Google Flu Trends would overestimate actual influenza rates. The media eagerly picked up the story and googeling Google Flu, one finds reports about the "Epic Google Flu Failure." Appendix B assesses whether measurement error in the Google Flu data is a serious threat to our main findings.

First of all, even if systematic over- or underestimation occurs, it should not be a threat to our estimates as long as the bias is not correlated with the introduction of sick pay schemes at the regional level. Our main model is a rich fixed effects specifications with region and 617 week-year fixed effects that net out time-variant seasonal trends in influenza activities and time-invariant region specifics. Also note that the original ambition of Google Flu was to *predict* epidemic outbreaks earlier and faster than the Centers for Disease Control and Prevention (CDC). Given Lazer et al. (2014) and the media reports, Google obviously accepted that this may have been overly ambitious. However, we exploit Google Trends *retrospectively* to test for regional changes in infection rates and do not intend to make any predictions.

Appendix B reports the results of our testing procedure. First, we acquired CDC data on confirmed influenza-like cases. These data are available on the weekly level and the level of the 10 HHS regions (but not at the city or state level necessary to study the effects in this paper); the data are normalized per 100,000 doctor visits. We aggregate and construct an equivalent dataset with the Google Flu data. Figure B6a plots both two time series. The vertical lines represent the implementation of sick pay mandates. 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 flu rate. Particular striking is the huge spike in the second half of 2012 that triggered the media debates about the "Epic Google Flu failure." However, as seen, this seems to have been a single outlier that is not particularly worrisome in our model context with week fixed effects—-as long as it is not correlated with the implementation of sick pay mandates.

Figure B6b plots the difference in residuals between both datasets (CDC vs. Google Flu) after regressing each flu rate on 617 week and 9 region fixed effects. In other words, Figure B6b provides a visual assessment of the difference in the remaining variation by week and region after netting out seasonal and regional effects. The thin sold black line represents HHS Region 1 that includes the treatment states Connecticut and Massachusetts. The corresponding dashed vertical line represents the date when the sick pay mandate was 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 6,191 region-week observations, the point estimate is 0.0247, positive and not statistically significant (standard deviation: 0.0697).

2.3 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:

$$log(y_{it}) = \phi TreatedCity_i \times LawEffective_t + \delta_t + \gamma_i + Unemp_{it} + \mu_{it} \tag{1}$$

where $log(y_{it})$ is the logarithm of the reported Google (2015) Flu rate in city i in week of the year t. γ_i are 83 city fixed effects and δ_t is a set of 617 week fixed effects over almost 12 years. $TreatedCity_i$ is a treatment indicator which is one for cities that implemented a sick pay scheme between 2003 and 2015, see Table A1. The interaction with the vector $LawEffective_t$ yields the binary variable of interest. The interaction 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 provided unemployment rate at the city level, $Unemp_{ci}$. The standard errors are routinely clustered at the city level. Thus this empirical specification allows us to estimate ϕ_t, i.e., the effect of the influenza-like disease rate through the introduction of mandated sick pay as defined above.

State level estimation. Our second specification estimates the entire model at the state-week level. The idea is to capture the effects of the sick pay mandates in the District of Columbia, Connecticut, California, and Massachusetts (see Table A1). Accordingly, we use our second Google Flu Trends sample covering weekly state level data from 2003 to 2015; all i subscripts in Equation (1) now represent states, not cities.

Event study. Lastly, to plot an event study graph, we replace the binary $LawEffective_t$ time indicator with one that continuously counts the number of days until (and from) a law became effective—from -720 days to 0 and +720 days. 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 as a reaction to pre-existing trends.

Changes in Influenza Activity When Employees Gain Sick Pay Coverage

Evidence from City Mandates. We begin by discussing the estimation results of the DD model in Equation (1). Table 1 shows the findings for our first sample of U.S. cities from 2003 to 2015. Every column represents one model where the first two columns represent the standard model. The only difference between evenly and unevenly numbered columns is that the evenly numbered columns additionally control for the monthly unemployment rate at the city level.

Comparing the $TreatedCity \times LawEffective$ coefficient estimates in the first two columns, we see that controlling for the monthly unemployment rate barely alters the results—a finding that likewise holds up for columns (3) - (6). Importantly, the first two columns provide negative coefficient estimates that are significant at the 5 percent level. The literal interpretation would be that influenza-like illnesses (ILI) per 100,000 doctor visits decrease by about 5.5 percent when employees gain access to paid (and unpaid) sick leave. It is also worthwhile to emphasize that this is a weighted estimate over all seven U.S. cities that implemented sick pay mandates, and that these are short- to 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 postreform influenza rates.

[Insert Table 1 about here]

The models in columns (3) and (4) now replace the city-specific dates indicating when the laws became effective in *LawEffective* (Column (2), Table A1) with the city-specific dates indicating when the laws were *passed* by the city legislature (*LawPassed*). As column (3) of Table A1 shows, the time span between when the laws were passed and when they became effective amounts up to one year. It is at least imaginable that private firms voluntarily implemented sick pay schemes ahead of the official 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 percent and are not statistically significant any more.

Lastly, the models in columns (5) and (6) use time indicators that only become one after the probation or accrual period has been passed (*LawProbation*). As discussed, all laws require employees to "earn" their sick pay. Employees accrue one hour of paid sick leave per 30 or 40 hours of work, i.e., per full-time work week (Table A1). In addition, all laws specify a minimum accrual period of typically 90 days that needs to elapse 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.[4] Letting the data speak, we can say that the decrease in flu rates increases by one percentage point to -6.5 percent in columns (5) and (6), suggesting that paid sick leave coverage is more effective in reducing contagious presenteeism than unpaid sick leave coverage.

Discussion of Effect Sizes. The models in Table 1 suggest reductions in population-level ILI by between 5.5 and 6.5 percent when sick leave mandates are implemented. Our model in Section 3 provides evidence of the underlying mechanism: more employees with a contagious disease will call in sick and stay at home when they gain access to sick leave insurance.

According to Susser and Ziebarth (2016), 35 percent of full-time employees and 45 percent of all employees are not covered by firm-specific sick leave policies in the U.S. Given the current population-employment ratios (BLS 2016), this means that roughly 20 percent of the population gain access to sick leave coverage when cities pass such mandates. Per week and over the time period considered in this paper, the CDC counted on average 1,655 ILI per 100,000 doctor visits (Centers for Disease Control and Prevention 2016). Taken together, the numbers suggest that U.S. sick leave mandates provide coverage for about 20,000 employees per 100,000 population. Our estimates suggest that the sick pay coverage for 20K employees helps preventing the transmission of around (1655×0.06) 100 ILI per 100,000 population and week. Combined with estimates based on self-reports, according to which about 1,000 employees per 100,000 population would work sick every week (Susser and Ziebarth 2016), the effect sizes are very compatible and reasonable.

Event Study Graphs. Figure 1a shows the Event Study Graph for the model in Table 1. Here we plot the coefficient estimates that replace the binary time indicators in *LawEffective* with continued time indicators counting the days 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 all major U.S. metropolitan areas. Figure 1a demonstrates very little trending in the two years before the sick pay schemes became effective.

[4] The Family and Medical Leave Act of 1993 (FMLA) covers employees with 1,250 hours 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 US employees are ineligible for FMLA, 44 percent 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 by Table A1.

The coefficient estimates are not statistically different from zero and fluctuate only slightly around the zero line. In line with columns (3) and (4), there is not much evidence for anticipation effects.

Immediately after all employees gained access to paid and unpaid sick leave, the infection rates decrease significantly by up to 20 percent. Note that the estimates past 480 days following the law lack precision because they are solely based on the experiences in San Francisco (2007), D.C. (2008), and Seattle (2012). New York City's comprehensive bill became effective April 1, 2014—about one year and fours 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 fact that one seems to observe a long-term rebound of infection rates to the zero line is determined by a lack of precision and the early experiences in San Francisco (2007), DC (2008 and 2014), and Seattle (2012). More importantly, 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). We test this hypothesis by excluding San Francisco from the sample and re-running the city-level model. Appendix Figure A5 shows that, indeed, the observed rebound effect in Figure 1a was very likely driven by the Great Recession in started in 2008.

Overall, the city event study graphs nicely illustrates the clear and significant decrease in ILI rates at the population level after employees found sick leave coverage. These findings suggest that sick and contagious employees stayed at home to recover instead of going to work, thereby reducing contagious presenteeism and decreasing infection rates.

[Insert Figure 1 about here]

Evidence from State Mandates. The setup of Table 2 follows Table 1. The only difference is that we now estimate the DD models at the state-week level. States in the treatment group are now D.C. (2008 and 2014), Connecticut (2012), California (2015), and Massachusetts (2015). However, unfortunately, the bills in California and Massachusetts only became effective July 1, 2015 and our Google Flu Trends observation period ends at the end of July 2015. Hence estimates outside the 26 day postreform window are exclusively driven by Connecticut and D.C. In addition, as a reminder, Connecticut's law only covers service sector employees in non-small businesses which represent about 20% of the workforce. The first DC law was also quite lax. Because effectively reducing infection rates requires comprehensive measures and preventing infections for as many susceptibles as possible (Vynnycky and White 2010), and because two important states are only briefly covered in the summer months following the law, we expect the state level estimates to be less pronounced.

[Insert Table 2 about here]

In line with our expectations, we identify a marginally significant decrease in ILI rates of about 2.5 percent following the laws in D.C., Connecticut, California, and Massachusetts (Columns (1) and (2)). Again, there is not much evidence that a significant amount of employers (who did not provide paid sick leave previously) provided sick pay voluntarily between the passage of the law and its implementation. The size of the coefficients in columns (3) and (4) are attenuated, only around -1 percent, and not statistically significant. The same is true for the estimates in columns (5) and (6) which are solely based on D.C. and Connecticut because the end of the official accrual period (90 days) lies outside of our window of observation for California and Massachusetts.

The event study in Figure 1b provides a clearer picture. While the two-year period before the reform implementation shows estimates that fluctuate consistently around the zero line and are never significantly different from zero, the infection rates slightly trend downward in the postreform period. However, the estimates are partly noisy and lack statistical power. Again, recall that only the first 26 days are based on evidence from four states, while all other postreform estimates are exclusively based on the patchy Connecticut bill and the two step introduction in D.C.

3 Identifying Contagious Presenteeism and Negative Externalities

After having provided very reduced-form evidence that access to paid sick leave can reduce the influenza-like disease rate at the population level, this section provides an analytical framework that illustrates the underlying behavioral mechanisms.

3.1 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 (Brown 1994; Barmby et al. 1994; Brown and Sessions 1996; Gilleskie 1998). While additional arguments for or against the provision of sick pay exist, our model focuses on the trade-off between absenteeism and presenteeism behavior and negative externalities in form of infections resulting from information asymmetries.[5] Since we construct a model of individual behavior we omit the i subscript in order to simplify notation. We specify the individual utility function as

[5] In particular, we abstain from modeling the employer's side and effects on the firm level. This could include employer signaling (or adverse selection) effects, peer effects, or discrimination against identifiable unhealthy workers (e.g., obese workers). We also abstain from analyzing general equilibrium labor market effects.

$$u_t(\sigma_t, c_t, l_t) \tag{2}$$

where u_t represents the utility of a worker at time t, c_t stands for consumption, and l_t for leisure and both consumption and leisure lead to a higher utility, i.e., we assume that utility is increasing in consumption and leisure over the whole domain. The current sickness level is σ_t, with larger values of σ_t representing a higher degree of sickness and thus decreasing utility over the whole domain of sickness. Furthermore, we assume that the sickness level is private information of the worker and unknown by the firm.

Moreover, in terms of the cross derivatives we assume

$$\frac{\partial^2 u_t}{\partial\sigma\partial l} > 0 \text{ and } \frac{\partial^2 u_t}{\partial\sigma\partial c} \leq 0. \tag{3}$$

The first expression implies that leisure or recuperation time becomes more valuable for higher values of sickness and the opposite holds true for consumption. In time periods with high levels of σ_t, i.e., when the worker is very sick, utility is mostly drawn from leisure or recuperation time rather than consumption.

With h defining hours of contracted work and T the total amount of time available—and assuming that workers are not saving but consuming their entire income from work w_t or sick pay s_t—one can write the utility difference between working and (sickness) absence formally as

$$u_t(\sigma_t, w_t, T-h) - u_t(\sigma_t, s_t, T) = 0 \tag{4}$$

In most countries sick pay is not a flat monetary amount but rather a replacement rate of the current wage. Hence we substitute sick pay with $s_t = \alpha_t w_t$ in the equation above (with $\alpha_t \in [0,1]$).[6] Moreover, workers are paid based on their average productivity and, approximating reality, we assume rigid wages and thus a time invariant wage level w.

From equation (4), we may then calculate the indifference point $\sigma^*(\alpha_t)$ for a given replacement rate α_t.[7] Hence if $\sigma_t > \sigma^*(\alpha_t)$ workers will be absent, while they will be present if $\sigma_t < \sigma^*(\alpha_t)$. The latter can be thought of the "normal" state under which the great majority, 80-90 percent of all workers, fall every day. The value of $\sigma^*(\alpha_t)$ where workers are indifferent solely depends on *(i)*

[6]Notice that the wage may also include nonmonetary benefits, such as more job security. For instance, Scoppa and Vuri (2014) find that workers who are absent more frequently face higher risks of dismissal. Thus even in countries with nominally full replacement, in our model, this might translate to a replacement rate smaller than one.

[7]Notice that due to our assumption of a utility increasing in consumption and leisure and decreasing in the sickness level over the whole domain there is a unique $\sigma^*(\alpha_t)$, where the worker is indifferent between work and absence.

the amount of money workers lose while on sick leave, $(1-\alpha_t)w$, and *(ii)* the contracted amount of working hours h and total time available T.

Finally, applying the implicit function theorem to equation (4) the partial derivative of the indifference sickness level $\sigma^*(\alpha_t)$ with respect to the replacement rate reads:

$$\frac{\partial \sigma^*(\alpha_t)}{\partial \alpha} = \frac{\frac{\partial u_t(\sigma_t,\alpha_t w,T)}{\partial c}\frac{\partial c}{\partial \alpha}}{\frac{\partial u_t(\sigma_t,w,T-h)}{\partial \sigma}-\frac{\partial u_t(\sigma_t,\alpha w,T)}{\partial \sigma}} < 0, \tag{5}$$

where the inequality follows directly from a positive numerator and a negative denominator due to $\alpha_t \in [0,1]$, $T-h<T$ and the assumption about cross derivatives given in equation (3).

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).[8] More precisely, we assume that there always exist three fractions of workers: a first share of workers, $1-q-p_t$, who are healthy; a second share , q, who have a noncontagious disease, $\sigma_t = \sigma_{nt}$; and a third share, p_t, who have a contagious disease, $\sigma_t = \sigma_{ct}$. In the latter two cases, the disutility due to sickness σ_t is determined by the density function $f(\sigma)$. Thus, whereas the level of σ_t determines the decision of the worker to stay home or not, this additional characteristic determines whether the disease is contagious.[9]

The share of workers being affected by a contagious disease, p_t, changes over time depending on infections in the previous period, as outlined below. On the other hand, the share of workers affected by noncontagious diseases, q, is time invariant.[10]

Importantly, both the severity of the disease and the "disease type" drawn by the worker are not perfectly observable by the employer. This is an important, yet realistic, assumption and drives the main mechanism below. It allows us to abstract away from a hypothetical scenario where employers can unambiguously and always identify workers with contagious diseases and simply send them home to avoid infections. The information friction assumption is very reasonable given that diseases and contagiousness—especially at the beginning of a disease when humans are already contagious—are mostly unobservable for the employer (and also the employee)

[8]In principle, noncontagious diseases represent a special case of contagious diseases, where infections are equal to zero. Moreover, (diseases with) relapses can also be considered as a special case of contagious diseases, where the level of contagiousness is fairly low, as individuals "infect" only themselves.

[9]We also assume that, conditional on being sick ($\sigma > 0$), the shares of disease types (p_t and q) are independent of the density of the sickness level $f(\sigma)$.

[10]Note that we abstract away from competing risks. While substitution might take place, we assume it is of a small enough margin not to be of major relevance.

and subject to very incomplete monitoring. Note that most infectious diseases are contagious for several days before definite symptoms are observable. The availability and popularity of OTC drugs suppressing disease symptoms reinforce the unobservability assumption (Earn et al. 2014). Also note that, for our model to work, it is not necessary to assume that employees know their disease type.

Given $\sigma^*(\alpha_t)$ and assuming a worker population of size one, we can now define the sick leave rate A_t as the share of individuals absent from work:

$$A_t = A_{ct} + A_{nt} = (p_t + q) \int_{\sigma^*(\alpha_t)}^{1} f(\sigma) d\sigma; \tag{6}$$

similarly, the share of workers present at work is given by

$$P_t = (1 - p_t - q) + (p_t + q) \int_{0}^{\sigma^*(\alpha_t)} f(\sigma) d\sigma. \tag{7}$$

Given the replacement rate α_t, a share of workers

$$\pi_t(\alpha_t) = p_t \int_{0}^{\sigma^*(\alpha_t)} f(\sigma) d\sigma \tag{8}$$

is contagious but present at work. We define $\pi_t(\alpha_t)$ as *contagious presenteeism*. One economic purpose of providing paid sick leave is to provide financial incentives for sick workers to call in sick, such that infections caused by contagious presenteeism are minimized.

As seen, the share of workers with contagious presenteeism behavior who transmit diseases to their coworkers and customers equals $\pi_t(\alpha_t)$. Following a standard SIS (susceptible-infected-susceptible) endemic model (Ross 1916; Kermack and McKendrick 1927), the transmission of diseases via contagious presenteeism depends on three factors: *1)* the share of contagious workers working (the infected) π_t, *2)* the share of noncontagious individuals who can be infected (the susceptibles) $S_t = (1 - p_t - q) + q \int_{0}^{\sigma^*(\alpha_t)} f(\sigma) d\sigma$, and *3)* the transmission rate of the disease which we denote with r.[11] Therefore the share of individuals with contagious diseases is an increasing function of these three elements, formally $p_t(\pi_t, S_t, r)$. Thus contagious workers who show up at the workplace trigger the negative externalities that sick pay schemes intend to minimize.

[11] It is outside the scope of this paper to model the transmission rate of contagious diseases explicitly (Philipson 2000; Barmby and Larguem 2009; Pichler 2015).

Severely Sick Workers and the Definition of 'Moral Hazard'

If $\sigma_t > \sigma^*(0)$, workers are too sick to work and would stay home—even under a replacement rate of zero. This can be thought of as a state where people are either lying in bed with extremely high fever and heavy, acute, flu symptoms (as an example for a contagious disease), or lying in bed after chemotherapy because of cancer (as an example for a noncontagious disease). Empirically, one can estimate that about 3-5 percent of all workers fall into this category on a given day. In Germany, on a given workday, about 4 percent of the workforce is on sick leave. During the flu season, each day 1.5 percent are on sick leave due to colds and flu (Techniker Krankenkasse 2015).

When employees gain access to sick pay ($\alpha_t > 0$), a share of marginal workers will call in sick as a result of their sick pay (workers with $\sigma^*(\alpha_t) < \sigma_t < \sigma^*(0)$). These individuals would work, if there was no sick pay and it is rational for them to now be absent from work. In the domain of noncontagious diseases, we refer to this behavior as noncontagious absenteeism. The share of employees with noncontagious diseases who call in sick as a result of sick pay at any point in time and for a given sick pay replacement level α_t equals

$$\omega(\alpha_t) = q \int_{\sigma^*(\alpha_t)}^{\sigma^*(0)} f(\sigma) d\sigma. \tag{9}$$

As work productivity is difficult to measure, we do not model it explicitly. However, as for noncontagious diseases and from a welfare perspective, working—even if associated with lower productivity due to sickness—would be generally preferred to sickness absence and zero work output under quite weak assumptions. Formally, denote with $\delta(\sigma^*(0))$ the sickness-related productivity losses for workers that are just indifferent between going to work and staying at home at a replacement rate of zero, i.e., $\sigma^*(0)$. If worker utility and firm profits had similar weights, then as long as $\sigma^*(0)\alpha_t w > \delta(\sigma^*(0))$, working would dominate sickness absence. This condition compares the consumption utility of sick leave benefits with the productivity losses of a noncontagious worker. Abstracting away from time inconsistent behavior of employees, e.g. which could induce unintended long-term health damages, sickness absence would only be preferred if the productivity losses or consumption utility losses due to sickness were very large. The 'Con' of sick pay schemes thus implies that $\sigma^*(0)\alpha_t w > \delta(\sigma^*(0))$, and thus working would be preferred to sickness absence for noncontagious diseases, as long as the disease is not too severe $\sigma_t < \sigma^*(0)$.

Finally, we define the overall behavioral effect ['moral hazard'] as the sum of noncontagious absenteeism and contagious presenteeism behavior [12]

$$\rho_t(\alpha_t) = \omega(\alpha_t) + \pi_t(\alpha_t). \tag{10}$$

Proposition 1. *Under a sick pay scheme and given the existence of contagious as well as noncontagious diseases, there exists a fraction of contagious workers π_t who engage in presenteeism. Contagious workers who go to work induce negative externalities because they infect coworkers and customers. Likewise, there exists a fraction of noncontagious workers who call in sick due to sick leave benefits, ω. The overall behavioral labor supply adjustment, ρ_t, is the sum of noncontagious absenteeism and contagious presenteeism behavior.*

Contagious diseases lead to contagious presenteeism and infections. This negative externality can be seen as one economic justification for sick pay mandates. The extent of the negative externality depends on the contagiousness of the disease. In the context of our model, presenteeism is not harmful per se, but rather the negative externalities triggered by contagious presenteeism.

Changes in Sick Pay and Labor Supply: Graphical Representation

To simplify and simulate the German sick pay reform of 1996 in the next section, we assume (without loss of generality) that sick pay is high in the base year ($t = 0$) and is exogenously cut after one year in $t = y_1$. Equation (5) yields $\frac{\partial \sigma^*(\alpha)}{\partial \alpha} < 0$. Hence, a decrease in the replacement rate increases σ^* and more workers work: the sick leave rate decreases.

But how do contagious presenteeism and noncontagious absenteeism behavior change? Noncontagious absenteeism decreases because $\sigma^*(\alpha_{y_1}) > \sigma^*(\alpha_0)$. Moreover, contagious presenteeism increases for the same reason. Thus it remains ambiguous what happens to the overall behavioral effect ρ_t because the first component, contagious presenteeism, increases while the second component, noncontagious absenteeism, decreases.

Proposition 2. *Given the existence of contagious as well as noncontagious diseases, a sick pay cut increases contagious presenteeism, which induces negative externalities through infections of coworkers and customers. At the same time, a sick pay cut reduces noncontagious absenteeism. A priori, the overall behavioral effect, defined as the sum of both behaviors, is ambiguous. Anal-*

[12] Similar to Einav et al. (2013), moral hazard is strictly speaking not a hidden action in our context, since it is perfectly observable whether an employee is present or not. It is rather hidden information that employees have about their personal sickness level and their type of sickness.

ogously, an increase in sick pay decreases contagious presenteeism and increases noncontagious absenteeism behavior.

[Insert Figure 2 about here]

Figure 2 shows a graphical representation of Proposition 2. Panel A depicts the situation for noncontagious diseases. Initially, the share of employees who engage in noncontagious absenteeism—indicated by the sum of the two dark gray areas—is quite large. As sick pay decreases, more workers with noncontagious illnesses come to work and the absenteeism rate decreases.

Panel B depicts the situation for contagious diseases. As sick pay decreases, contagious presenteeism increases, meaning more workers with contagious illnesses come to work. Because of additional infections, the share of individuals with a contagious disease, p_t, increases, as represented by the outward shift of the density function.

Changes in Sick Pay and Moral Hazard: Analytical Derivation

Noncontagious diseases. $\frac{A_{n0}-A_{nt}}{A_{n0}} = \beta_{nt}$ denotes the percentage change in the sick leave rate of noncontagious diseases, when sick pay decreases and after t time periods have passed. Thus β_{nt} represents the cumulative reform effect at time t, or formally

$$\beta_{nt} = \frac{1}{A_{n0}} \left(q \int\limits_{\sigma^*(\alpha_0)}^{1} f(\sigma) d\sigma - q \int\limits_{\sigma^*(\alpha_t)}^{1} f(\sigma) d\sigma \right) = \frac{1}{A_{n0}} \left(q \int\limits_{\sigma^*(\alpha_0)}^{\sigma^*(\alpha_t)} f(\sigma) d\sigma \right). \tag{11}$$

In the case of noncontagious disease, we can write

$$\beta_{nt} = \frac{1}{A_{n0}} \left(\omega(\alpha_0) - \omega(\alpha_t) \right). \tag{12}$$

Contagious diseases. Similarly $\frac{A_{c0}-A_{ct}}{A_{c0}} = \beta_{ct}$ denotes the percentage change in the sick leave rate of contagious diseases, when sick pay decreases and after t time periods.

$$\beta_{ct} = \frac{1}{A_{c0}} \left(p_0 \int\limits_{\sigma^*(\alpha_0)}^{1} f(\sigma) d\sigma - p_t \int\limits_{\sigma^*(\alpha_t)}^{1} f(\sigma) d\sigma \right). \tag{13}$$

This expression can be rewritten as

$$\beta_{ct} = \frac{1}{A_{c0}} \left(\left(\pi_0(\alpha_t) - \pi_0(\alpha_0) \right) - \left((p_t - p_0) \int\limits_{\sigma^*(\alpha_t)}^{1} f(\sigma) d\sigma \right) \right), \tag{14}$$

where the first element corresponds to the increase in contagious presenteeism due to the sick pay cut (and the corresponding decrease in the absence rate)—related to the initial share of workers with a contagious disease, p_0. The second element corresponds to the increase in the absence rate due to additional infections as a result of the increase in contagious presenteeism.

As described, additional infections increase the infection rate, p_t. As seen in ***Proposition 2***, more contagious workers work after sick pay is cut. Furthermore, as more noncontagious workers work as well, the number of susceptibles increases. Both effects result in more infections. Depending on the magnitude of newly infected individuals, the increase in sickness absence due to infections offsets the decrease due to additional contagious presenteeism, at least partly. 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 on the overall sick leave rate would be zero if this additional worker infected one additional co-worker who then called in sick.

Next, we contrast the two offsetting behavioral forces, where β_{ct} and β_{nt} can be rewritten as:

$$\beta_{ct} = \beta_{nt} - \frac{1}{A_{c0}} \left((p_t - p_0) \int_{\sigma^*(\alpha_t)}^{1} f(\sigma) d\sigma \right). \tag{15}$$

Being able to rewrite the percentage change in the sick leave rate of contagious diseases as an expression of the percentage change in the sick leave rate of noncontagious diseases is only possible due to the assumption of equal densities $f(\sigma)$ across the two disease groups. Section 2 already provided some evidence for the existence of new infections through contagious presenteeism. This assumption allows us to identify the magnitude of new infections by comparing differences in sickness absence changes over different disease groups.

Accordingly, the behavioral adjustments of the two disease groups, β_{ct} and β_{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. Thus, under the existence of contagious presenteeism, it holds that $\beta_{nt} > \beta_{ct}$.

Finally, note that by definition, $\beta_{nt} > 0$. However—in case of contagious diseases—the sign of β_{ct} is ambiguous. For a very contagious disease, β_{ct} might become negative. Therefore the sign of β_{ct} remains an empirical question which will be assessed below.

Hypothesis 1 *After a sick pay cut, the noncontagious absenteeism rate decreases ($\beta_{nt} > 0$). The sign of the absence rate for contagious diseases, β_{ct}, remains ambiguous because additional absences due to new infections might outweigh the immediate decrease in the absence rate due to the sick pay cut. The difference $\beta_{nt} - \beta_{ct}$ indicates additional absences due to new infections.*

Finally, we denote the overall percentage change in the absence rate with $\beta_t = \frac{\Delta A}{A_0}$:

$$\beta_t = \frac{1}{A_0}\left((\omega(\alpha_0) - \omega(\alpha_t)) + (\pi_0(\alpha_t) - \pi_0(\alpha_0)) - \left((p_t - p_0)\int\limits_{\sigma^*(\alpha_t)}^{1} f(\sigma)d\sigma\right)\right). \tag{16}$$

The next subsection discusses how these effects can be empirically identified in order to quantify the change in shirking and in new infections following a change in sick pay coverage.

Identifying Contagious Presenteeism and Negative Externalities Empirically

Using Population-Level Influenza Rates to Identify Contagious Presenteeism

Section 2 exploited the implementation of U.S. sick pay mandates across space and over time in order to test for decreased contagious presenteeism and infections after the introduction of sick pay. We use Google Flu Trends data at the weekly regional level from 2003 to 2015 to estimate the effect of sick pay mandates. Providing employees with paid sick leave is equivalent to increasing sick leave benefit levels which, according to our model and a rich literature (Johansson and Palme 1996, 2005; De Paola et al. 2014; Ziebarth and Karlsson 2010, 2014; Dale-Olsen 2014; Fevang et al. 2014), unambiguously increases sick leave utilization ($\frac{\partial\sigma^*(\alpha)}{\partial\alpha} < 0$).

Our model would predict that access to sick pay coverage reduces contagious presenteeism (***Proposition 2***). This leads to a reduction in the share of individuals infected by a contagious disease. Assume there is no sick pay at time zero ($t = 0$), and that sick pay is introduced after one year ($t = y_1$). Then the reduction in contagious diseases at t, ϕ_t, can be defined as

$$\phi_t = (p_t - p_0)f(\sigma). \tag{17}$$

In Section 2 we empirically tested whether $\phi_t < 0$; i.e., whether sick pay coverage reduces the incidence rate of infectious diseases in the population. We found $\phi_t < 0$ which yields empirical evidence for a reduction in contagious workplace presenteeism because employees have been covered by paid sick leave schemes.

Furthermore, access to paid sick leave increases noncontagious absenteeism and decreases contagious presenteeism behavior (***Hypothesis 1***). The finding of a subsequent decrease in infection rates are thus a direct implication of our model and yields strong evidence for a decrease in contagious workplace presenteeism.

Using Disease-Specific Sick Leave Rates to Identify Contagious Presenteeism

To directly implement the model, one needs data on sick leave behavior, an exogenous sick pay reform and different groups of affected workers. Then one can empirically estimate the causal effect of the change in sick pay on the share of workers who call in sick. In the notation above, we thus empirically identify β_t.

Moreover, if one can empirically identify two different disease categories, c and n, and the share of workers who call in sick with certified sickness due to contagious and noncontagious diseases, one could carry out a statistical test to check if $\beta_{nt} > \beta_{ct}$. In other words, one could test if a sick-pay-cut induced decrease in sick leave is larger for disease categories, n as compared to c, which would yield evidence for an increased spread of contagious diseases via an increase in contagious presenteeism behavior.

Proposition 4a. *Given the existence of a reform that exogenously varied sick pay and sick leave data on differently affected employees, one can econometrically test if* $\beta_t > 0$*, i.e., if the labor supply adjustment with respect to a sick pay cut is positive and, if so, how large it is.*

Proposition 4b. *Given the availability of data for contagious and noncontagious sick leave rates, one can estimate* β_{nt} *and* β_{ct}*. The size of* β_{nt} *is informative for the relevance of noncontagious absenteeism behavior.* β_{ct} *represents both the increase in contagious presenteeism and in additional sick leave due to infections triggered by contagious presenteeism behavior.*

Proposition 4c. *Lastly, one can econometrically test if* $\beta_{nt} > \beta_{ct}$ *(Hypothesis 1), i.e., whether the labor supply adjustment is larger for noncontagious than for contagious diseases. The size of the differential illustrates additional infections that lead to additional sick leave as a result of contagious presenteeism. These represent negative externalities under lower sick pay.*

The next section exploits German sick pay reforms and data on disease-specific sick leave rates to illustrate how one can implement the second proposed test that, under the model assumptions, empirically identifies noncontagious absenteeism and contagious presenteeism behavior.

4 Evidence from German Sick Leave Reforms

4.1 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. In other words, employers have to provide 100 percent sick pay from the first day of a period of sickness without benefit caps.[13]

In the case of illness, employees are obliged to inform their employer immediately about both the sickness and the expected duration. From the fourth day of a sickness episode, a doctor's certificate is required. However, employers have the right to ask for a doctor's note from day one of a spell, and many employees voluntarily submit doctors' notes from day one. Note that the sickness itself remains confidential. Employees just have to inform their employer *that* they are sick, not why, and the standardized form for the doctor's note does not indicate the type of disease, which is confidentially transmitted to the sickness fund. This is important because the model assumes that the type of disease is unobservable to the employer.

If the sickness lasts more than six continuous weeks, the doctor needs to issue a different certificate. From the seventh week onward, sick pay is disbursed by the health insurers (called "sickness funds") and lowered to 70 percent of foregone gross wages for those who are insured under Statutory Health Insurance (SHI).[14]

4.2 The Policy Reforms of 1996 and 1999

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. Panel A of Table C1 in the appendix summarizes how the bill altered the federal employer mandate. The most important provision of the bill reduced the minimum statutory sick pay level from 100 percent to 80 percent of foregone wages.[15] In addition to Table C1, Ziebarth and Karlsson (2010, 2014) provide more details on the regulatory changes and affected employee groups. This paper solely focuses on the implementation at the industry level among private sector employees who were covered by collective agreements.

Ongoing union pressure made employer associations in various industries—through collective agreements—to voluntarily provide sick pay on top of the statutory regulations. Further, the question of whether employees in specific industries were entitled to claim 100 percent or 80 per-

[13] In principle, there is no limit to the frequency of sick leave spells. However, if employees fall sick again due to the same illness after an episode of six weeks, the law explicitly states that they are only again eligible for employer-provided sick pay if at least six months have been passed between the two spells or twelve month have been passed since the beginning of the first spell. This paragraph intends to avoid substitution of long-term spells by short-term spells.

[14] Two additional benefit caps limit long-term sick pay. The first cap is 90 percent of the net wage, and the second cap is the contribution ceiling up to which contribution rates have to be paid.

[15] In addition to this bill, another bill cut SHI long-term sick pay from the seventh week onward from 80 percent to 70 percent of forgone gross wages. Ziebarth (2013) shows that this second bill did not induce significant behavioral reactions among the long-term sick.

cent of their salary during sickness episodes was determined by existing collective agreements and their legal interpretation. Some existing agreements explicitly, but probably coincidentally, stated that sick pay would be 100 percent, while others did not mention sick pay at all. In the former case, sick pay would remain 100 percent despite the decrease in the generosity of the employer mandate, whereas in the latter case, sick pay would decrease to 80 percent until a revised agreement was negotiated.

Review of collective agreements. We reviewed all collective agreements that existed during the time of the sick pay reforms and categorized industries (Hans Böckler Stiftung 2014). Overall, one can distinguish three different groups and industries: Panel B of Table C1 provides the provisions at the industry level and our categorization.

Group I is composed of the construction sector, whose collective agreement covered about 1.1 million private sector workers. When the law was passed in 1996, the existing collective agreement did not include any explicit provision on sick pay, which is why the entire federal regulations applied to the construction sector at the time of the bill's implementation. A negotiated compromise between unions and employers resulted in a new agreement which became effective July 1, 1997. This new agreement specified that the cut in the replacement rate would only be applied during the first three days of a sickness episode.[16]

Group II counts at least 4.4 million covered employees and is quantitatively the largest group. It includes 11 industries as specified in the notes to Table C1, among them the steel, textile and automobile industry. Union leaders in these industries managed to maintain the symbolically important 100 percent sick pay level. However, in return, they agreed to exclude paid overtime from the basis of calculation for sick pay, which effectively means that employees with a significant amount of overtime hours experienced sick pay cuts.[17]

[16]In 1997 a minimum wage in the construction sector was introduced. Theoretically a wage increase should also lead to a reduction in sickness absence. Blien et al. (2009) and Rattenhuber (2011) find a positive impact of the law on wages in East Germany. Whereas we cannot ultimately rule out an impact on sick leave rates, we consider it very unlikely that over proportional wage increases for low-wage blue collar construction workers in East Germany are the major driver of the large effects identified below. In any case, they are no threat to the illustration of the application of our testing procedure.

[17] There are several reasons why this type of sick pay cut may be of minor relevance: *(a) Fraction of Employees Effectively Affected.* As representative SOEPGroup (2008) data show, among BKK insurees (which our main data set is composed of), only 19% had paid overtime hours in 1998, the average being 4 hours per week. *(b) Size of Cut.* Whereas a decrease in the base rate to 80% would reduce net sick pay by €280 per month (in 1998 values), the exclusion of paid overtime would only lead to a net cut of €110 per month, conditional on working overtime and getting paid for it. *(c) Salience of Cut.* While maintaining the 100% replacement level had a high symbolic meaning for unions, the indirect reductions in sick pay were not communicated as openly, and it is questionable if every employee was aware of them. *(d) Affected individuals.* One could suspect that employees with paid overtime hours might be highly motivated employees in leading positions with a low number of sick days and a low propensity to shirk. However, as the SOEP shows, employees with paid overtime had on average 10 sick days per year while those *without* paid overtime hours had only 4.7 sick days.

Group III is composed of seven industries, all of which stated in their collective agreements that they would maintain 100 percent sick pay. Moreover, in contrast to **Group II**, these industries did *not* exclude overtime payments from the basis of calculation. Hence the 4 million employees covered by these agreements serve as control group in the evaluation of the 1997 sick pay cut.

Reversal of Main Sick Pay Cut 1999 and Remaining Changes

After the federal election was won by the new center-left coalition in 1998, as a reaction to the 1996 bill, the *Bill for Social Insurance Corrections and to Protect Employee Rights* was passed and became effective January 1, 1999. It increased federally mandated sick pay again from 80 percent to 100 percent. However, as Table C1 illustrates, while the main provision was reversed, two minor—but potentially important—details made the new arrangements less generous than sick pay coverage prior to October 1996. And in combination with the meanwhile negotiated collective agreements, they affected the three groups in Table C1 differently.

First, the four week waiting period—introduced in October 1996—was maintained. This implies that new employees have not been eligible for paid sick leave during the first month of their employment. However, to our knowledge no collective agreement had excluded the application of this waiting period, meaning that none of the three groups was affected by this provision in 1999. Second, the 1999 bill explicitly stated that paid overtime would be excluded from the basis of calculation. This provision was not part of the 1996 reform bill; it was probably a reaction to the many collective agreements that had implemented such a provision at the industry level in 1997 and 1998. However, because no industry in Group I and III of Table C1 excluded paid overtime voluntary in their 1997/1998 agreements, ironically, **Group III**'s sick pay became *less* generous as a result of the 1999 center-left bill.

4.3 Exploiting Administrative Data on Disease-Specific Sickness Absence: 1994–2004

In Germany, information on certified sickness absence—including diagnoses—are collected by the nonprofit SHI sickness funds covering 90 percent of the population (*Gesetzliche Krankenversicherungen GKV*). In 1995, before the first reform, switching between health plans was not possible and employees were assigned to company-based health plans (*Betriebskrankenkassen, BKKs*) if their employer offered such plans (similar to the employer-sponsored health plans in the U.S. but with mandatory enrollment). In 1995, a total of 960 SHI sickness funds existed, and 690 or 72 percent of them were company-based health plans (German Federal Statistical Office 2014). Employees covered by these health plans were likely also covered by binding collective agreements. (Eibich et al. 2012; Schmitz and Ziebarth forthcoming).

The Federal Association of Company-Based Sickness Funds (*BKK Dachverband*) annually publishes sick leave statistics of their 4.8 million enrollees (19 percent of all private sector employees) who are mandatorily SHI insured and gainfully employed (Bundesverband der Betriebskrankenkassen (BKK) 2004).[18] The *Krankheitsartenstatistik* reports both the incidence as well as the length of sickness spells by gender, age group, diagnoses according to the *International Classification of Diseases (ICD)*, and industry. We collected and digitized information from annual reports between 1994 and 2004.[19] Note that the data are only published by the main disease category as defined by the ICD; we have no discretion on how to categorize the different disease groups. The descriptive statistics are in the appendix, Table C2.

In total, we count 1,188 observations, where each observation represents one industry and year as well as the diagnosed sickness category. More specifically, we count 11 years and 18 industries, which adds up to 198 industry-year observations per diagnosis category.

Generated sick leave variables. Our outcome variable is the sick leave rate. This variable counts the number of certified sickness episodes per 100 enrollees (*sick cases per 100 enrollees*). We transform each dependent variable by taking the logarithm. The log-transformation is mainly done because β_{nt} in equation (11) as well as β_{ct} in equation (13) are expressed in percent and we would like to link the model to the empirical part as closely as possible.

Figure 3a shows the distribution of *total sick cases per 100 enrollees* and Figure 3b its logarithm. In both cases we observe relatively symmetric, close to normal, distributions. The untransformed plain variable has a mean of 125, implying 1.25 sick leave cases per year and enrollee across all industries and years. However, the variation ranges from 90 to 163 (Figure 3a and Table C2).

[Insert Figure 3 about here]

Looking at the disease categories and their incidence rates, one finds that the largest disease group is *respiratory diseases*, ICD codes J00-J99, contributing 29 percent of all cases. Within this

[18]Although, strictly speaking, BKKs are not legally obliged to contribute to the *Krankheitsartenstatistik*, the overwhelming majority does, probably simply out of tradition to contribute to this important statistic that has been existing since 1976. In 2013, more than 90 percent 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 systematically varied due to the reforms.

[19]We cannot use earlier data due to a lack of consistency that goes back to an earlier reform. 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.

group, a third of all cases are due to "bronchitis (J20)", while a quarter is due to "influenza (J09)." Moreover, another fifth are caused by "acute upper respiratory infections (J06)."

The second largest disease group with almost 20 percent of all cases is *musculoskeletal diseases* (M00-M99), which have the reputation to be particularly prone to shirking behavior. The most noteworthy subcategory in this group is "dorsalgia - back pain (M54)" making up 70 percent of all musculoskeletal cases.

Next in terms of their incidence relevance are *digestive diseases* (K00-K93, 14 percent), *injuries and poisoning* (S00-T98, 11 percent), followed by *infectious diseases* (A00-B99, 6 percent). The most common digestive disease is "noninfective gastroenteritis (K52, 45 percent)". Infectious diseases are mainly made up of "viral infections (B34)" and "infectious gastroenteritis (A09)." Together over 80 percent of all cases coded as infectious diseases fall in these two subcategories.

4.4 Nonparametric Graphical Evidence

Figure 3 shows the "Development of Normalized Sick Leave Cases by Treatment Groups" over time. Figure 4a shows the development for the overall *Sick leave rate*, Figure 4b looks at musculoskeletal diseases, and Figures 4c and d plot diseases of the respiratory system as well as infectious diseases. In addition to being normalized by the number of enrollees, these graphs are also adjusted with respect to the reference year 1994, which is indexed as 100. The two black vertical bars indicate the official implementation dates of the decrease and increase in sick pay generosity, respectively. The representation in Figure 4 serves two main purposes: *1)* to examine the plausibility of the common time assumption, and *2)* to anticipate and visually illustrate the main findings and help understand how they identify the model in the second section. Musculoskeletal sick leave cases (e.g., back pain, Figure 4b) represent the category "non-infectious diseases" in our model in the second section, whereas infectious sick leave cases (Figure 4d) represent the category "infectious diseases" in our model. Respiratory sick leave cases (Figure 4c) is a mixed category.

[Insert Figure 4 about here]

Overall, Figure 4 shows us the following: First, in general the data support the common time trend assumption. Despite some minor spikes here and there, it is obvious that all three groups in the four graphs develop in a pretty parallel manner over the 11 years without reform. In the graphs, this is the case for the time periods before 1997 and after 2000. In particular Figure 4d—showing infectious diseases—illustrates a remarkably parallel development (and does not provide any graphical evidence for a reform effect).

Second, with the exception of infectious diseases, the other three graphs provide strong evidence of a significant reform effect for Group I (see Table C1). Immediately after the reform implementation, we observe a 20 percent decrease in the sick leave rate for the overall disease category.[20] As for musculoskeletal diseases—the noncontagious disease category—the decrease is almost twice as large and around -40% for Group I, suggesting strong increases in shirking behavior. As for respiratory diseases—the mixed disease category that also includes flues and common colds—the decrease is only around -10%. Finally, as for infectious disease—the contagious disease category—we do not observe much evidence for any reform effect.

Third, the gap between the differently affected groups unambiguously, not but entirely, closes after 2000. This suggests that the behavioral reaction after the reversal of the sick pay cut kicks in delayed, probably due to the relatively low media coverage when the law was reversed. Moreover, there is evidence for time persistence or habit formation in sick leave behavior, since the regulations were again identical for all three groups post-1999 (Table C1). However, we still observe significant differences in between the three groups, even as late as 2004.

Fourth, the reaction to the soft sick pay cut—excluding overtime from the basis of calculation—was obviously asymmetric. Figure 4 does not provide much evidence that excluding overtime affected Group II's behavior in 1997 and 1998. However, the graphical evidence suggests that the very same measure had a significant impact on Group III post 1999.[21]

Relating these findings to our model above, one can summarize that *(i)* there is clear evidence for a significant and persistent decrease in the absence rate following a sick pay cut, $\beta_t > 0$ (Proposition 4a). Similarly, sick leave rates increase when the system becomes more generous. *(ii)* the labor supply adjustment of contagious diseases is smaller (and in fact close to zero) than the adjustment of noncontagious diseases and thus Proposition 4c, $\beta_{nt} > \beta_{ct}$, holds up. In addition, we find a large decrease in shirking $\beta_{nt} > 0$ whereas the increase in presenteeism outweighs additional infections $\beta_{ct} > 0$ (Proposition 4b). Finally, because *(iii)* $\beta_{nt} - \beta_{ct} > 0$, the German sick pay cut also led to an increase in infections (Proposition 4b).

[20] This is in line with the two other existing studies evaluating this reform using SOEP data (Ziebarth and Karlsson 2010; Puhani and Sonderhof 2010)

[21] There are two potential explanations for this finding. *1) Relevance of Relative Changes.* The decrease in sick pay at the end of 1996 was heatedly debated in German society and led to strikes. The main (media) focus was clearly on the decrease in the overall sick pay level. It is plausible that Group II did not react since the main reference point mattered here, which was the decrease in the default federal level. About 50 percent of all employees experienced a decrease in the level to 80 percent (Ridinger 1997; Jahn 1998). Hence the exclusion of overtime pay was, relatively seen, negligible for affected workers. It may not even have been noticed by the affected employees. After unions managed to negotiate the general sick pay level to remain at 100 percent, they marketed and emphasized this success accordingly—but either did not mention, or heavily down played the overtime cut. In 1999, by contrast, the exclusion of paid overtime was the only regulatory change that made employees worse off. *2) LATE.* Since the model identifies the Local Average Treatment Effect (LATE), it could simply be that paid overtime was more relevant for Group III than for Group II.

4.5 Parametric Difference-in-Differences Model

We now estimate the following conventional parametric DiD model separately for different disease categories:

$$
\begin{aligned}
log(y_{it}) = \gamma_i + \beta_0 + &\beta_1 GroupI_i \times' 97 -' 98 + \beta_2 GroupI_i \times' 99 -' 04 + \\
&\beta_3 GroupII_i \times' 97 -' 98 + \beta_4 GroupII_i \times' 99 -' 04 + \\
&+ \delta_t + \mu_{it}
\end{aligned} \tag{18}
$$

where $log(y_{it})$ stands for one of our dependent sick leave measures for industry i at time t. γ_i are 17 industry fixed effects and δ_t 10 year fixed effects. The standard errors are routinely clustered at the industry level. We interact the treatment indicators as defined below with two time period dummy variables *'97-'98* and *'99-'04*. The reference period is the years 1994 to 1996.

$GroupI_i$ as well as $GroupII_i$ are binary treatment indicators. As for the 1996 reform, Group I experienced a sick pay cut from 100 percent to 80 percent, while Group II underwent a soft sick pay cut—with paid overtime excluded (Table C1). Group III was not affected, serving as the control group. Thus β_1 identifies the effect of the sick pay cut for Group I relative to Group III and the years 1997/1998 and relative to the time between 1994 and 1996. Moreover, β_3 identifies the effect of excluding paid overtime for Group II in 1997/1998 relative to the pre-reform period.

As for the 1999 reform, the main pay level was increased again for Group I, but overtime excluded from the basis of calculation. Group II was not affected and serves as control group. Group III experienced a soft cut (Table C1). Thus, β_2 identifies the post-1999 level effect, relative to pre-1997 levels, or the joint effect of the two reforms for Group I. Moreover, the difference $\beta_2 - \beta_1$ identifies the effect of the increase in sick pay levels from 80 percent to 100 percent after 1999 relative to 1997/1998. In contrast, β_3-β_4 identifies the effect of the overtime exclusion for Group III in the post-1999 era relative to pre-1999. Recall that overtime was excluded for Group II in 1997 while nothing happened to Group III, whereas in 1999, overtime was excluded for Group III while nothing happened to Group II. Consequently, $-\beta_4 + \beta_3$ identifies the estimate of the 1999 overtime exclusion for Group III. Hence, we differentiate three different groups over three different time periods but only need to estimate four relevant parameters. Since the outcome measures are in logarithms, β_1 to β_4 directly provide the reform-related change of the outcome variable in percent.

Disease-Specific Labor Supply Adjustments: Decomposing the Total Labor Supply Effects

Estimating $\hat{\beta}_t$, $\hat{\beta}_{nt}$, and $\hat{\beta}_{ct}$. Table 3 shows the results of the DiD model in Equation (18) using different outcome variables: the logarithm of *sick cases per 100 enrollees* by the disease categories *total, musculoskeletal, infectious, respiratory,* and *injuries & poisoning*. Each column is one model as in Equation (18). For illustrative purposes, we solely show the coefficients of β_1 to β_4 and suppress the remaining ones. In the row below, we display the results of an F-test $\beta_2 - \beta_1 = 0$ to test for the effect of the level increase for Group I relative to Group III in 1999. As discussed in the previous section, the empirical models closely identify the theoretical model. For example, β_1 in the first row of the first column of Table 3 estimates β_t in Equation (16) and tests ***Proposition 4a***. The finding is then cross-checked by $\beta_2 - \beta_1 = 0$ which likewise test ***Proposition 4a*** using the increase in sick pay as as an exogenous source of variation.

Note that the overtime exclusion, or "soft sick pay cut" as we call it, essentially also tests ***Proposition 4a*** and the size and sign of β_t in Equation (16) since any variant of making the sick pay less generous could be interpreted as a decrease in sick pay. However, we believe that the best suited coefficient estimates to test ***Propositions 4a–c*** are the ones resulting from $GroupI_i \times' 97 -' 98$—the β_1s for the different disease categories. These are the effects of the initial reduction in the sick pay replacement rate from 100 percent to 80 percent in 1997/1998. However, we double and cross-check the consistency and plausibility of these main β_1 findings using the effects of the increase in the replacement rate from 80 percent to 100 percent in 1999 ($\beta_2 - \beta_1$), the exclusion of overtime for Group II in 1997 (β_3) and Group III and 1999 ($\beta_3 - \beta_4$), as well as the overall development of the sick leave rates from 1999 to 2004—when the system as a whole was more restrictive—relative to 1994 to 1996 (β_2; β_4).

[Insert Table 3 about here]

One can summarize the following from Table 3: First, during the time when sick pay was cut to 80 percent, in 1997 and 1998, we find overall decreases in the sickness rate by about 22 percent (β_1 in column (1)).[22] This reflects $\hat{\beta}_t$ in Equation (16), i.e., the total labor supply effect. As seen, β_1 is highly significant and clearly larger than zero, which confirms ***Proposition 4a***. Related to the decrease in sick pay of 20 percent, one obtains a sickness rate elasticity with respect to the replacement rate of about one. Decreases of about 20 percent are also found for the "mixed" infectious and noninfectious category of respiratory diseases (column [3]).

[22] This is just an approximation. The exact effect in percent is $100 \cdot (exp(X)1)$, i.e., 24.6 percent.

Second, musculoskeletal diseases represent the noncontagious disease category n in our model in the second section. Following the sick pay cut, the sick leave rate of musculoskeletal diseases decreased overproportionally by 34 percent (column [4], β_1). The overproportional decrease for musculoskeletal diseases, which is composed of 70 percent back pain cases, fits the common perception that the labor supply of this category is particularly elastic and prone to shirking behavior. Equation (11) of our model illustrates the analytical derivation of β_{nt}, β_{nt}, which is represented by β_1 in column (4) of Table 3, equals the decrease in noncontagious absenteeism as sick pay decreases.

Third, infectious diseases, ICD-10 codes A00-B99, represents the contagious disease category c in our model. The estimate stands for the β_{ct} in our model in Equation (13). As β_1 in column (2) of Table 3 shows, the infectious disease rate fell underproportionally by an estimated 15 percent as a response to the sick pay cut in 1997/1998. Note that this estimate is likely upward biased, since the pre-1997 common time trend for infectious diseases is not 100 percent clean, as Figure 4d nicely illustrates. The unbiased estimate likely tends toward zero. In any case, while the findings suggest that $\beta_{nt} > \beta_{ct}$ as formulated in *Hypothesis 1* and *Proposition 4c*, it is also clear that $\hat{\beta}_t > 0$ holds, meaning that the reform led to a decrease in overall sickness absence.

Further Results and Robustness Checks. The labor supply effect in column (5) of Table 3 serves as a robustness test since 50% of all *injuries & poisoning* absences are due to workplace accidents (BKK 2004). The first bill that cut sick pay, however, excluded sick leave due to workplace accidents from the cuts (see Table C1). Indeed, as see by β_1 in column (5), the *injuries & poisoning* absence rate decreased underproportionally by almost exactly half the rate than the overall rate, namely by 11.2 percent instead of 22 percent.

Second, the β_2 estimate provides the change in sickness rates in the post-1999 era relative to the pre-1997 era for Group I. Meanwhile, the F-test, β_2 - $\beta_1 = 0$, yields the effect of the increase in the replacement rate to 100 percent in 1999. Thus β_2 reflects the long-term impact after a series of reforms that made the overall system more restrictive and shows a decrease of 13.5 percent at the 10 percent significance level for *all diseases*. β_2 - β_1 is highly significant for all but infectious diseases. Column (1) suggests that the overall rate increased by 8.4 percent after the reversal. Column (4) confirms the findings above and suggests that *musculoskeletal diseases*, i.e. back pain, reacted overproportionally with an increase of 19.1 percent following the increase in sick pay to 100 percent.

Third, all separate β_3 and β_4 estimates are imprecise and relatively small in size meaning that—in a regression framework that employs industry and year fixed effects—we are unable to detect significant sick leave rate changes in response to the mild sick leave cuts that excluded overtime from the basis of calculation. However, this is at least partly a function of the statistical power that our data offer. Note that all coefficients carry the expected sign and most magnitudes lie around 3 to 5%.

Does the Decrease in Noncontagious Absenteeism Outweigh the Externalities of Contagious Presenteeism?

Estimating β_{nt} - β_{ct}. To directly test the model predictions, we now pool all disease categories and estimate a triple difference model. *Proposition 4c* allows us to directly carry out the following statistical tests $\beta_{nt} = \beta_{ct}$. The triple difference model is similar to the one in Equation (18) above but pools all disease groups and adds additional triple interaction terms like $\lambda_1 GroupI_i \times' 97 -' 98 \times Dis_d$, $\lambda_2 GroupI_i \times' 99 -' 04 \times Dis_d$ etc. to the model, where Dis_d represents a vector of disease indicators. The estimates for λ then directly indicate how the reform effect for every disease category differs from the baseline disease effect.

Table C4 shows the results of this triple difference model. Column (1) of Table C4 simply replicates column (4) of Table 3 focusing on *musculoskeletal diseases*, our proxy for noncontagious diseases.

Column (2) adds the main contagious disease category *infectious diseases* and has thus twice as many observations (396 industry-year estimates). With *musculoskeletal diseases* as the baseline category, the four triple DiD interaction terms 1) $GroupI_i \times' 97 -' 98 \times Infectious$, 2) $GroupI_i \times' 99 -' 04 \times Infectious$, 3) $GroupII_i \times' 97 -' 98 \times Infectious$, and 4) $GroupII_i \times' 99 -' 04 \times Infectious$ directly test ***Hypothesis 1*** ($\beta_{nt} = \beta_{ct}$). What Table 3 already suggested can now be tested with statistical certainty in column (2) of Table C4: $\hat{\beta}_{ct}$ - $\hat{\beta}_{nt}$ = 19.3 percentage points, meaning that the decrease in the contagious sick leave rate was a significant 19.3 percentage points smaller than the decrease in the noncontagious sick leave rate (14.8 percent vs. 34.1 percent, see columns [2] and [4] of Table 3). Again, this is likely an underestimate since we likely overestimate β_{ct}. Figures 4b and 4d illustrate very nicely and even more clearly than Table C4 that there was basically no behavioral reaction for *infectious diseases* while one observes substantial behavioral reactions for *musculoskeletal diseases*.

Column (3) additionally adds *respiratory diseases* to the data set. While not all *respiratory diseases* are contagious, this category contains "influenza (J09)", commonly referred to as the flu. As above, the four triple interaction terms identify the differential effect relative to the baseline category *musculoskeletal diseases*. Although we lack statistical power, there is suggestive evidence that the respiratory sick leave rate decreased by about 13 percent less than the noncontagious baseline.

5 Conclusion

Empirically identifying presenteeism behavior is extremely challenging, yet crucial in order to test for one major economic justification for publicly provided sick pay: the negative externalities associated with contagious presenteeism. Contagious presenteeism refers to the phenomenon when employees with infectious diseases go to work sick and infect co-workers and customers. Such behavior is a major 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 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, and economic theory as well empirical studies strongly suggest that they do, 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 and state level in the U.S.—the industrialized country with the least generous sick pay coverage. Using Google Flu Trends data, we show that influenza-like disease rates decrease significantly when employees gain access to paid sick leave. Almost half of all U.S. employees do not have access to sick leave insurance. Through the US sick pay mandates, about 20K employees per 100K population gain sick leave coverage for themselves and their children. Our estimates suggest that the relatively comprehensive laws at the level of seven major U.S. cities helped preventing about 100 influenza-like infections per week and 100K population. Infections rates may further decrease in the medium to long-run when employees have accrued larger amounts of paid sick days.

The next 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 also decompose classical 'moral hazard' into noncontagious absenteeism and contagious presenteeism behavior. The former does not imply negative health spillovers, whereas case 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 absence insurance and its decomposed elements.

Finally, we use two German sick pay reforms 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 identifying conditions, we indeed find indirect evidence for the existence of contagious presenteeism. However, assuming that the identifying assumptions hold up, we also show that—in Germany with one of the most generous sick leave systems worldwide—the reduction in noncontagious absenteeism was larger than the increase in the infectious disease rate (due to contagious presenteeism) when sick pay was cut from a baseline level of 100 percent.

Researchers could exploit different settings and our proposed methods, or variants of it, to test for the existence and the degree of contagious presenteeism, noncontagious absenteeism, and the overall labor supply adjustments. Important fields of applications include contagious presenteeism by teachers or school kids, e.g., induced by teacher 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 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 but potentially 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 the generosity of the sick pay scheme under investigation, then measure changes in noncontagious absenteeism and contagious presenteeism behavior, and then readjust until both undesirable employee behaviors are minimized.

More research is also needed in order 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.

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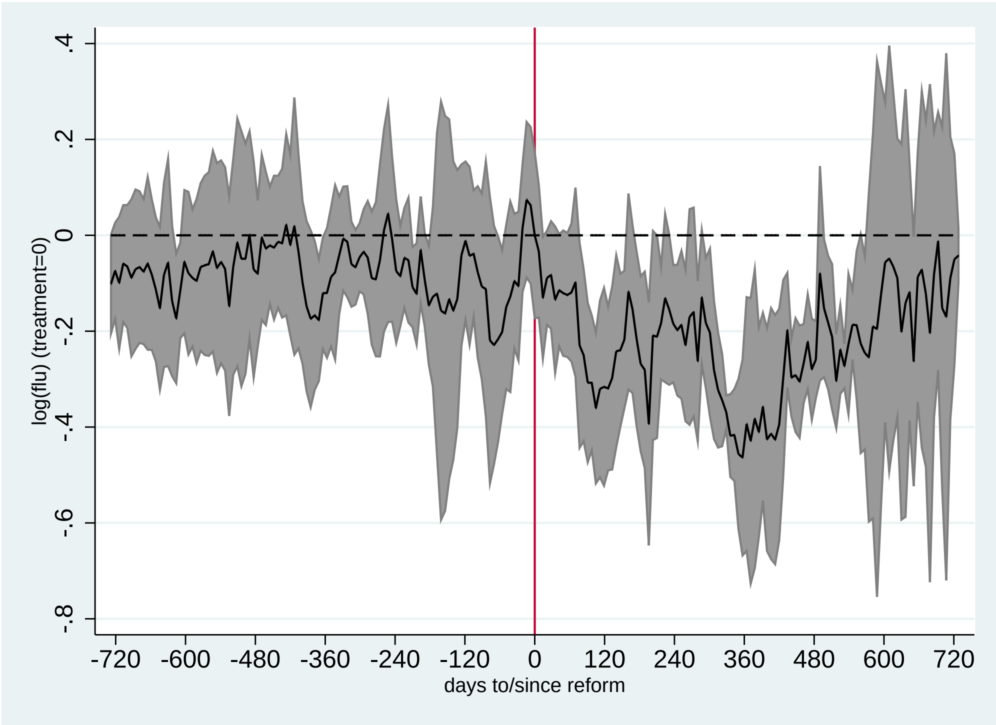
Figures and Tables

Figure 1 Event Study—Effect of Sick Pay Mandates in

Panel A: Cities



Panel B: States

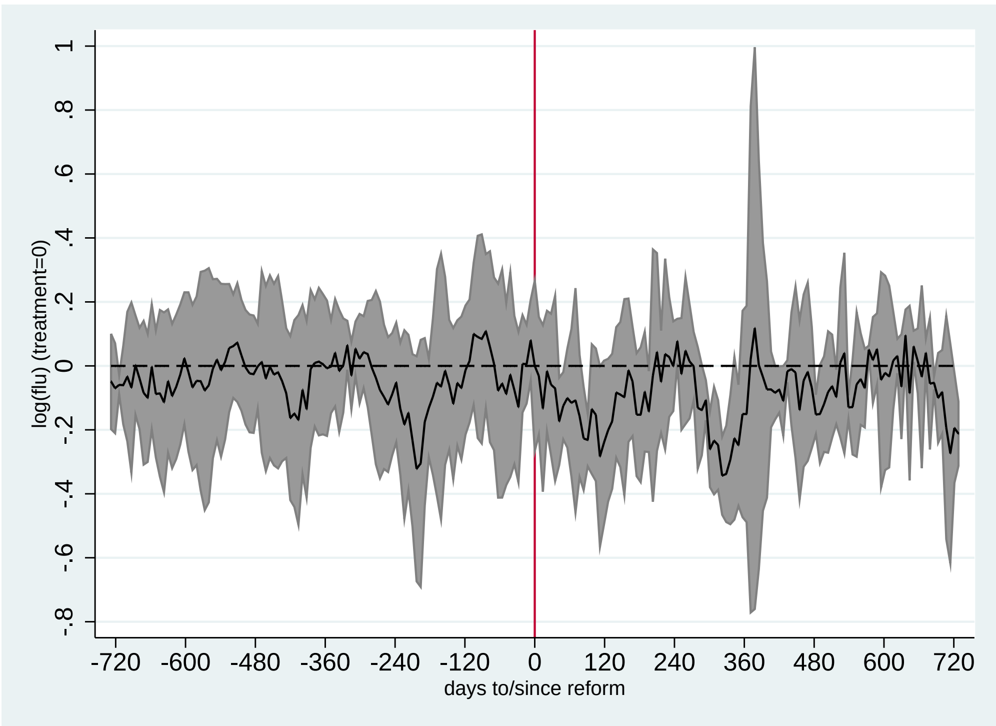
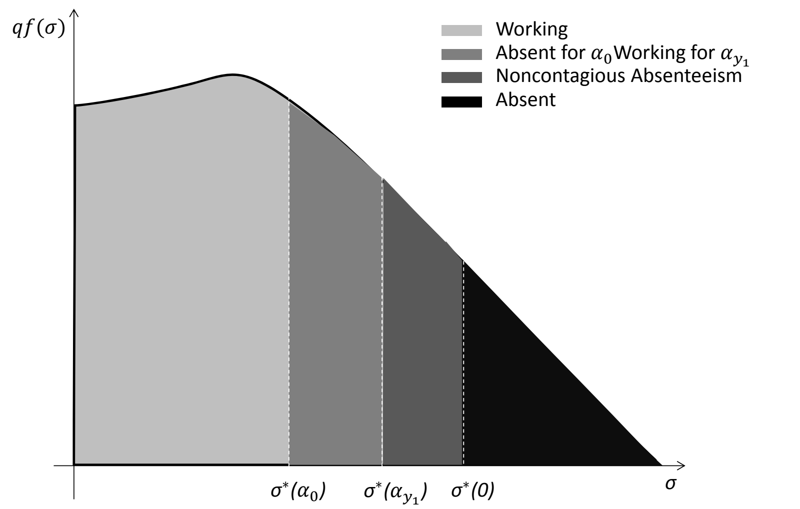
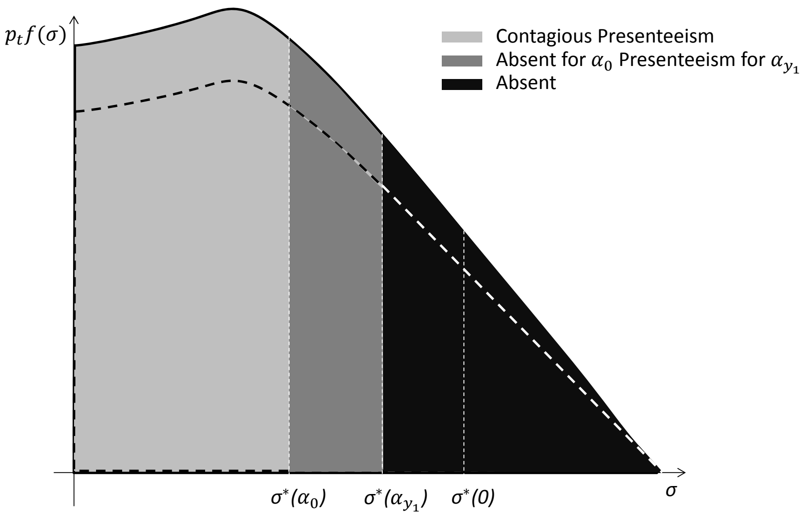


Figure 2 Graphical Representation and Classification of Shares of Employees Working and on Sick Leave

Panel A: Noncontagious Diseases



Panel B: Contagious Diseases



Panel A shows the share of employees who draw a noncontagious disease. After the sick pay cut, shirking decreases. Panel B depicts the same situation for contagious diseases. A sick pay cut increases contagious presenteeism and p_t, represented by the outward shift of the curve.

Figure 3 Distribution of (a) Sick Leave Cases and (b) Logarithm of Sick Leave Cases per 100 Employees

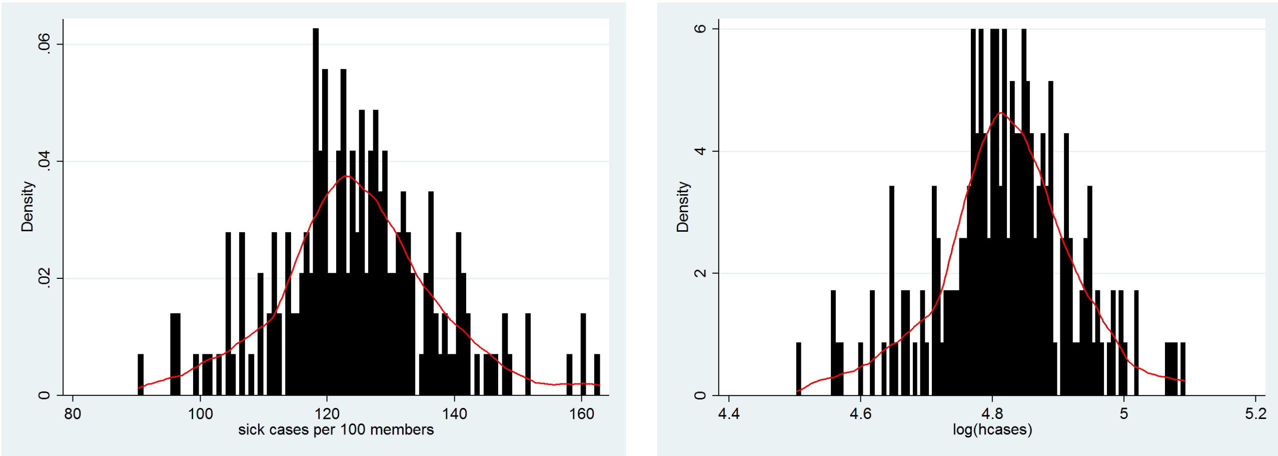


Figure 4 Development of Sick Leave Rates by Treatment Groups Over Time

The solid line shows the development of industries in **Group I**. This group experienced a sick pay cut from 100% to 80% in 1997 and the reverse of this cut in 1999. The short dashed line represents **Group II**. This group witnessed a "soft cut" in 1997 through the exclusion of overtime. Finally, the long dashed line depicts **Group III**, which had a soft cut in 1999. For more information about the sick pay reforms, see Table C1.

Table 1 Effect of Introduction of Sick Pay Mandates on Influenza Rate (Sample I: U.S. Cities 2003-2015)

	(1)	(2)	(3)	(4)	(5)	(6)
TreatedCity×LawEffective	-0.0569**	-0.0545**				
	(0.0238)	(0.0229)				
TreatedCity×LawPassed			-0.0244	-0.0216		
			(0.0252)	(0.0256)		
TreatedCity×ProbationOver					-0.0644**	-0.0623**
					(0.0293)	(0.0282)
N	49,560	49,560	49,560	49,560	49,560	49,560

NOTE: * $p<0.1$, ** $p<0.05$, *** $p<0.01$; standard errors in parentheses are clustered at the city level. The dependent variable is always the logarithm of the number of influenza-like illnesses (ILI) per 100,000 doctor visits as reported by Google (2015). All regressions contain week-of-year fixed effects and city fixed effects as in equation (1). Each column represents one model, estimated by OLS. 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 entire sample of cities considered is in columns one and two of Table A2.

SOURCE: Google (2015), own calculation and illustration.

Table 2 Effect of Introduction of Sick Pay Mandates on Influenza Rate (Sample II: U.S. States 2003-2015)

	(1)	(2)	(3)	(4)	(5)	(6)
TreatedState×LawEffective	-0.0223*	-0.0264*				
	(0.0131)	(0.0147)				
TreatedState×LawPassed			-0.00889	-0.0113		
			(0.0179)	(0.0198)		
TreatedState×ProbationOver					-0.0139	-0.0185
					(0.0104)	(0.0112)
N	30,141	30,141	30,141	30,141	30,141	30,141

NOTE: * $p<0.1$, ** $p<0.05$, *** $p<0.01$; standard errors in parentheses are clustered at the state level. The dependent variable is always the logarithm of the number of influenza-like illnesses (ILI) per 100,000 doctor visits as reported by Google (2015). All regressions contain week-of-year fixed effects and state fixed effects as in equation (1). Each column represents one model, estimated by OLS. Even numbered columns additionally control for the monthly unemployment rate in the state (BLS 2015). *TreatedState* is a treatment indicator which is one for all states listed in Table A1. The entire sample of states considered is in column three of Table A2.

SOURCE: Google (2015), own calculation and illustration;

Table 3 Effect of Changes in Sick Pay on Normalized Cases of Sick Leave by Disease Groups

	All diseases (1)	Infectious (2)	Respiratory (3)	Musculosk. (4)	Inj. & Pois. (5)
Group I×'97-'98	-0.220***	-0.148***	-0.208***	-0.341***	-0.112**
(Effect of Cut '97)	(0.057)	(0.047)	(0.054)	(0.076)	(0.045)
Group I×'99-'04	-0.135*	-0.075	-0.131***	-0.150	0.030
(Level post-'99 vs. pre-'97)	(0.070)	(0.053)	(0.044)	(0.157)	(0.087)
Group II×'97-'98	-0.029	-0.041	-0.022	-0.038	-0.006
(Effect of Soft Cut '97)	(0.065)	(0.073)	(0.061)	(0.086)	(0.065)
Group II×'99-'04	0.053	0.053	0.017	0.131	0.107
(Level post-'99 vs. pre-'97)	(0.078)	(0.070)	(0.055)	(0.164)	(0.095)
[Group I×'99-'04] - [Group I×'97-'98]	0.084***	0.073	0.077***	0.191**	0.142***
pvalue	0.000	0.121	0.000	0.032	0.004
(Effect of Increase '99)					
R2	0.659	0.949	0.816	0.858	0.918
Observations	198	198	198	198	198
Number of industries	18	18	18	18	18

NOTE: * $p<0.1$, ** $p<0.05$, *** $p<0.01$; standard errors in parentheses are clustered at the industry-level. All regressions are weighted by the annual number of industry-specific sickness fund enrollees. The descriptive statistics are in the Appendix (Table C2). Each column represents one model as in equation (18), estimated by OLS, i.e., all models include industry and year fixed effects. The dependent variables are logarithms of the normalized sick leave cases per 100 employees. Column (1) employs the total number of sick leave cases as dependent variable, column (2) solely uses certified infectious sick leave cases and so on. For more information on how the variables were generated, see section 2.2. *Treated* is a treatment indicator with one for **Group I** and zero for **Group III**, whereas *PartlyTreated* is one for **Group II** and zero for **Group III**. **Group I** experienced a sick pay cut from 100 to 80% in 1997 and a reversal in 1999. **Group II** experienced a soft cut in 1997 and **Group III** experienced a soft cut in 1999. For more information about the sick pay reforms, see Table C1.

SOURCE: BKK (2004), own calculation and illustration;

Appendix A

Table A1 Overview of Employer Sick Pay Mandates in the U.S.

Region (1)	Law Passed (2)	Law Effective (3)	Content (4)
San Francisco, CA	Nov 7, 2006	Feb 5, 2007	all employees including part-time and temporary; 1 hour of paid sick leave for every 30 hours 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 hour of paid sick leave for every 43 hours, 90 days accrual period; up to 3 to 9 days depend. on firm size; own sickness or family; no health care or restaurant workers
	Dec 18, 2013 (extension pending funding)	Feb 22, 2014 (retrospective in Sep 2014)	extension to 20,000 temporary workers and tipped employees
Connecticut	July 1, 2011	Jan 1, 2012	full-time service sector employees in firms>49 employees (20% of workforce); 1 hour for every 40 hours; up to 5 days; own sickness or family member, 680 hours accrual period (4 months)
Seattle, WA	Sep 12, 2011	Sep 1, 2012	all employees in firms with >4 full-time employees; 1 hour for every 30 or 40 hours 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 extended	April 1, 2014 (pending economy)	employees w >80 hours p.a in firms >4 employees or 1 domestic worker; 1 hour for every 30 hours; up to 40 hours; own sickness or family member; 120 days accrual period
Portland, OR	March 13, 2013	Jan 1 2014	employees w >250 hours p.a. in firms >5 employees; 1 hour for every 30 hours; up to 40 hours; own sickness or family member
Newark, NJ	Jan 29, 2014	May 29, 2014	all employees in private companies; 1 hour of for every 30 hours; 90 days accrual period; up to 24 to 40 hours depending on size; own sickness or family
Philadelphia, PA	Feb 12, 2015	May 13, 2015	employees in firms >9 employees; 1 hour of paid sick leave for every 40 hours; 90 days accrual period; up to 40 hours; own sickness or family member
California	September 19, 2014	July 1, 2015	all employees; 1 hour of paid sick leave for every 30 hours; 90 days accrual period; minimum 24 hours; own sickness or family member
Massachusetts	Nov 4, 2014	July 1, 2015	employees in firms >10 employees; 1 hour of paid sick leave for every 40 hours; 90 days accrual period; up to 40 hours; own sickness or family member
Oakland, CA	Nov 4, 2014	March 2, 2015	employees in firms >9 employees; 1 hour of paid sick leave for every 30 hours; 90 days accrual period; up to 40 to 72 hours depending on firm size; own sickness or family member
Oregon	June 22, 2015	Jan 1, 2016	employees in firms >9 employees; 1 hour of paid sick leave for every 30 hours; 90 days accrual period; up to 40 hours; own sickness or family member

SOURCE: several sources, own collection, own illustration.

Figure A5 City Event Study without San Francisco

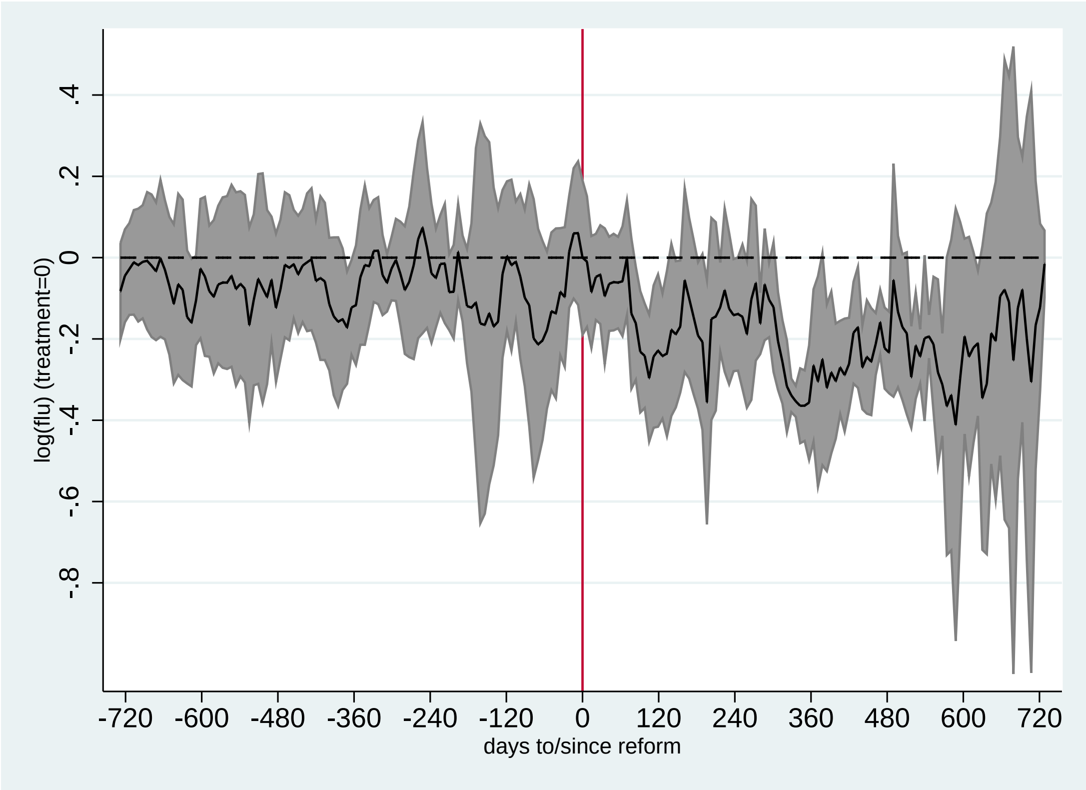


Table A2 U.S. Cities and States (in alphabetical order) with Weekly Google Flu Data As Of

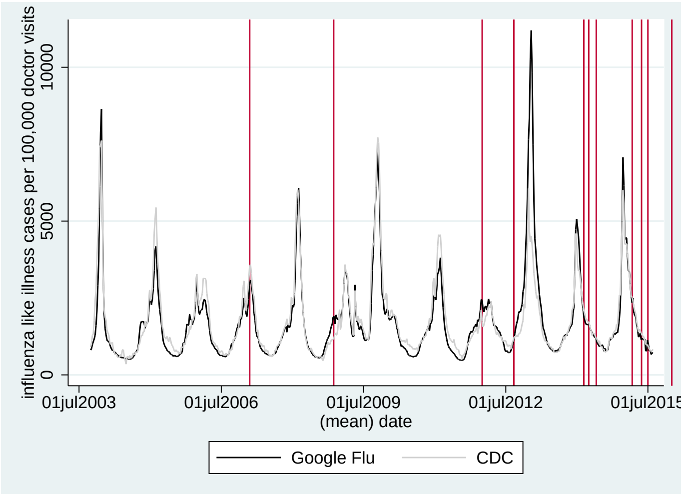
City	Month	Day	Year	City	Month	Day	Year	State	Month	Day	Year
Albany, NY	9	28	2003	Mesa, AZ	11	7	2004	Alabama	28	9	2003
Albuquerque, NM	10	12	2003	Miami, FL	9	28	2003	Alaska	12	12	2004
Anchorage, AK	10	17	2004	Milwaukee, WI	9	28	2003	Arizona	28	9	2003
Arlington, VA	9	28	2003	Nashville, TN	9	28	2003	Arkansas	7	11	2004
Atlanta, GA	9	28	2003	**New York, NY**	9	28	2003	**California**	28	9	2003
Austin, TX	9	28	2003	**Newark, NJ**	9	28	2003	Colorado	28	9	2003
Baltimore, MD	9	28	2003	Norfolk, VA	9	28	2003	**Connecticut**	28	9	2003
Baton Rouge, LA	9	26	2004	**Oakland, CA**	9	28	2003	Delaware	30	10	2005
Beaverton, OR	12	14	2003	Oklahoma City, OK	9	28	2003	**District of Columbia**	28	9	2003
Bellevue, WA	11	30	2003	Omaha, NE	9	28	2003	Florida	28	9	2003
Berkeley, CA	9	19	2004	Orlando, FL	9	28	2003	Georgia	28	9	2003
Birmingham, AL	9	28	2003	**Philadelphia, PA**	9	28	2003	Hawaii	2	11	2003
Boise, ID	10	3	2004	Phoenix, AZ	9	28	2003	Idaho	14	11	2004
Boston, MA	9	28	2003	Pittsburgh, PA	9	28	2003	Illinois	28	9	2003
Buffalo, NY	10	19	2003	Plano, TX	10	16	2005	Indiana	28	9	2003
Cary, NC	9	26	2004	**Portland, OR**	9	28	2003	Iowa	28	9	2003
Charlotte, NC	9	28	2003	Providence, RI	10	17	2004	Kansas	28	9	2003
Chicago, IL	9	28	2003	Raleigh, NC	9	28	2003	Kentucky	28	9	2003
Cleveland, OH	9	28	2003	Reno, NV	10	24	2004	Louisiana	28	9	2003
Colorado Springs, CO	9	19	2004	Reston, VA	11	28	2004	Maine	31	10	2004
Columbia, SC	10	10	2004	Richmond, VA	9	28	2003	Maryland	28	9	2003
Columbus, OH	9	28	2003	Rochester, NY	9	28	2003	**Massachusetts**	28	9	2003
Dallas, TX	9	28	2003	Roswell, GA	11	23	2003	Michigan	28	9	2003
Dayton, OH	11	23	2003	Sacramento, CA	9	28	2003	Minnesota	28	9	2003
Denver, CO	9	28	2003	Salt Lake City, UT	9	28	2003	Mississippi	28	11	2004
Des Moines, IA	10	17	2004	San Antonio, TX	9	28	2003	Missouri	28	9	2003
Durham, NC	9	28	2003	San Diego, CA	9	28	2003	Montana	27	11	2005
Eugene, OR	10	17	2004	**San Francisco, CA**	9	28	2003	Nebraska	9	11	2003
Fresno, CA	12	7	2003	San Jose, CA	9	28	2003	Nevada	23	11	2003
Ft Worth, TX	10	3	2004	Santa Clara, CA	9	28	2003	New Hampshire	30	11	2003
Gainesville, FL	10	12	2003	Scottsdale, AZ	10	24	2004	New Jersey	28	9	2003
Grand Rapids, MI	10	3	2004	**Seattle, WA**	9	28	2003	New Mexico	17	10	2004
Greensboro, NC	11	14	2004	Somerville, MA	9	28	2003	New York	28	9	2003
Greenville, SC	10	24	2004	Spokane, WA	1	16	2005	North Carolina	28	9	2003
Honolulu, HI	9	28	2003	Springfield, MO	10	30	2005	North Dakota	12	11	2006
Houston, TX	9	28	2003	St Louis, MO	9	28	2003	Ohio	28	9	2003
Indianapolis, IN	9	28	2003	St Paul, MN	9	28	2003	Oklahoma	28	9	2003
Irvine, CA	10	3	2004	State College, PA	9	5	2004	**Oregon**	28	9	2003
Irving, TX	9	28	2003	Sunnyvale, CA	9	28	2003	Pennsylvania	28	9	2003
Jackson, MS	11	14	2004	Tampa, FL	9	28	2003	Rhode Island	24	10	2004
Jacksonville, FL	10	3	2004	Tempe, AZ	9	28	2003	South Carolina	28	9	2003
Kansas City, MO	9	28	2003	Tucson, AZ	9	28	2003	South Dakota	5	11	2006
Knoxville, TN	10	3	2004	Tulsa, OK	9	28	2003	Tennessee	28	9	2003
Las Vegas, NV	9	28	2003	**Washington, DC**	9	28	2003	Texas	28	9	2003
Lexington, KY	9	26	2004	Wichita, KS	9	26	2004	Utah	9	11	2003
Lincoln, NE	10	31	2004					Vermont	30	10	2005
Little Rock, AR	10	3	2004					Virginia	28	9	2003
Los Angeles, CA	9	28	2003					Washington	28	9	2003
Lubbock, TX	10	17	2004					West Virginia	21	11	2004
Madison, WI	9	28	2003					Wisconsin	28	9	2003
Memphis, TN	10	24	2004					Wyoming	2	12	2007

NOTE: The table indicates the first observation period and all cities (Sample I) and states (Sample II) included. The last observation period is July 26, 2015 for the whole sample. Treated cities and states are in bold. Cities in gray are not included in Sample I because they were covered via a state, not a city, mandate which are evaluated using Sample II.
SOURCE: Google (2015), own collection, own illustration.

Appendix B

Figure B6 Google Flu Measurement Error Over Time

a) Raw Deviation CDC-Google Flu



b) Difference in Residuals

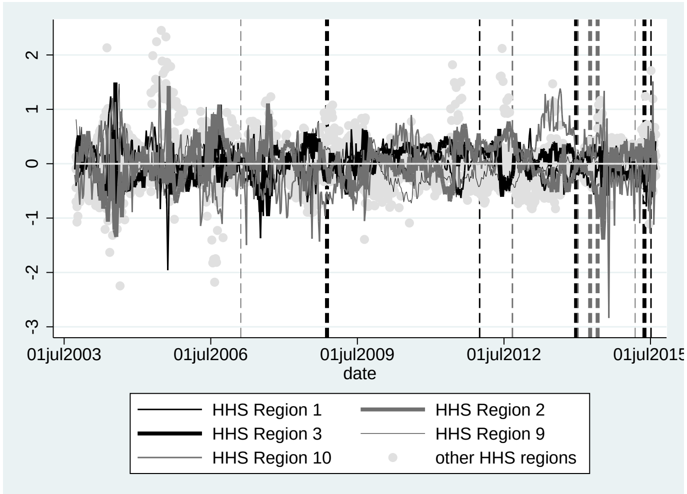


Figure B6a shows officially reported influenza-like illnesses per 100,000 doctor visits by the CDC as well as those reported by Google Flu. CDC Data are available at the weekly level for 10 HHS Regions. Figure B6b plots the difference in residuals between the two datasets. Residuals are calculated for both datasets separately by regressing the flu rate on a set of 617 week fixed effects and 9 HHS region fixed effects. The differently colored lines and dots represent different HHS regions that include treatment regions. The vertical lines represent the implementation of the sick pay mandates. HHS1 includes Connecticut and Massachusetts, HHS2 New York City and Newark City, HHS3 Philadelphia and DC, HHS9 California and HHS10 Oregon and Seattle.

Appendix C

Table C1 Detailed Overview of Reductions and Increases in German Federal Employer Sick Pay Mandates and Industry-Specific Collective Agreements

	Before 10/1996 (1)	10/1996–12/1998 (2)	Since 1/1999 (3)
Panel A: Federal Employer Mandate Regulations			
	100% sick pay No waiting period for new employees Paid overtime included in basis of calculation Extra payments included in basis of calculation	**80% sick pay** **Waiting period 4 weeks** Paid overtime included in basis of calculation **Extra payments can be contractually excluded** **No cut if 1 day of paid vacation traded for 5 sick days**	**100% sick pay** Waiting period 4 weeks **Paid overtime excluded in basis of calculation** Extra payments can be contractually excluded
Panel B: Industry-Specific Collective Bargaining Regulations			
Group I		80% sick pay during first 3 days (eff. July 1, 1997)	
Group II		100% sick pay Paid overtime excluded in basis of calculation	
Group III		100% sick pay	
Panel C: Combined Effect for Different Industries			
Group I	as in Panel A	**80% sick pay, since 07/'97 during first 3 days** **Waiting period 4 weeks**	**100% sick pay** Waiting period 4 weeks **Paid overtime excluded in basis of calculation**
Group II	as in Panel A	100% sick pay **Waiting period 4 weeks** **Paid overtime excluded in basis of calculation**	100% sick pay Waiting period 4 weeks Paid overtime excluded in basis of calculation
Group III	as in Panel A	100% sick pay **Waiting period 4 weeks**	100% sick pay Waiting period 4 weeks **Paid overtime excluded in basis of calculation**

NOTE: *Group I* is composed of the construction sector. *Group II* contains the following industries: steel, textile, mechanical engineering, automobile, ship and aerospace, electrical engineering and optics, wood and paper, printing, food and hospitality, trade, banking and insurance. *Group III* represents the chemical, oil, glass, energy and water, postal and transportation as well as public administration sector. Changes in regulation between time periods are in bold. The negotiated agreements cover 1.1M employees in *Group I* and at least 4.5M in *Group II* and 4M in *Group III* (Jahn 1998; Hans Böckler Stiftung 2014).
SOURCE: Hans Böckler Stiftung (2014), own illustration.

Table C2 Descriptive Statistics of Sick Leave Measures

Variable	Mean	Std. Dev.	Min.	Max.	N
Total sick cases per 100 enrollees	122.3	11.5	90.3	162.8	198
Total log(cases)	4.80	0.1	4.50	5.09	198
Infectious sick cases per 100 enrollees	8.2	2.2	3.9	14.9	198
Infectious log(cases)	2.07	0.29	1.36	2.70	198
Respiratory sick cases per 100 enrollees	35.4	4.3	25.2	50.0	198
Respiratory log(cases)	3.56	0.12	3.23	3.91	198
Digestive sick cases per 100 enrollees	16.3	2.0	12.8	24.0	198
Digestive log(cases)	2.79	0.12	2.55	3.18	198
Musculoskeletal sick cases per 100 enrollees	22.7	4.9	9.8	34.4	198
Musculoskeletal log(cases)	3.10	0.24	2.28	3.54	198
Injury sick cases per 100 enrollees	12.7	3.2	6.8	23.5	198
Injury log(cases)	2.51	0.25	1.92	3.16	198

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

Table C3 Number of Enrollees per Industry and Treatment Group

Industry and Classification	Mean	Std. Dev.
Group I		
Construction	127,642	104,205
Group II		
Steel	109,397	7,405
Textile	32,367	7,854
Mechanical Engineering	191,391	44,035
Automobile	301,725	43,313
Ship and Aerospace	33,626	9,323
Electrical engineering, optics	306,296	71,383
Wood and Paper	57,070	27,307
Printing	38,477	19,605
Food and Hospitality	55,045	33,748
Trade	341,566	227,279
Banking and Insurance	149,188	74,095
Group III		
Chemical	230,382	46,215
Oil	15,586	5,074
Glass	34,097	5,480
Energy and Water	50,702	13,149
Postal and Transportation	478,490	104,031
Public Administration	732,958	476,804

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

Table C4 Effect of Changes in Sick Pay on Normalized Cases of Sick Leave—Pooled Regressions

	(1) Musculoskeletal	(2) Musculoskeletal, Infectious	(3) Muscul., Infect. Respiratory
Group I×'97-'98	-0.341***	-0.341***	-0.341***
	(0.076)	(0.075)	(0.075)
Group I×'99-'04	-0.150	-0.150	-0.150
	(0.157)	(0.155)	(0.154)
Group II×'97-'98	-0.038	-0.038	-0.038
	(0.086)	(0.085)	(0.085)
Group II×'99-'04	0.131	0.131	0.131
	(0.164)	(0.161)	(0.161)
Group I×'97-'98×Infectious		0.193**	0.193**
		(0.088)	(0.088)
Group I×'99-'04×Infectious		0.075	0.075
		(0.164)	(0.163)
Group II×'97-'98×Infectious		-0.003	-0.003
		(0.112)	(0.111)
Group II×'99-'04×Infectious		-0.079	-0.079
		(0.176)	(0.175)
Group I×'97-'98×Respiratory			0.133
			(0.092)
Group I×'99-'04×Respiratory			0.019
			(0.160)
Group II×'97-'98×Respiratory			0.016
			(0.104)
Group II×'99-'04×Respiratory			-0.115
			(0.170)
Observations	198	396	594
R2	0.858	0.982	0.989

NOTE: * $p<0.1$, ** $p<0.05$, *** $p<0.01$; standard errors in parentheses are clustered at the industry-disease-level. All regressions are weighted by the annual number of industry-specific sickness fund enrollees. The descriptive statistics are in the Appendix (Table C2). The regressions are based on equation 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, where musculoskeletal form the reference group. The third column additionally adds respiratory diseases. The fourth column adds all other diseases as a separate category. 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 employees. For more information on how the variables were generated, see Section 4.3. *Treated* is a treatment indicator with one for **Group I** and zero for **Group III**, whereas *PartlyTreated* is one for **Group II** and zero for **Group III**. **Group I** experienced a sick pay cut from 100 to 80% in 1997 and a reversal in 1999. **Group II** experienced a soft cut in 1997 and **Group III** experienced a soft cut in 1999. For more information about the sick pay reforms, see Table C1.

SOURCE: Bundesverband der Betriebskrankenkassen (BKK) (2004), own calculation and illustration;