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

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April 22, 2017

Abstract

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

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

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

In 2011, a third of US full-time employees lacked sick leave coverage; among low-income and service sector employees, the uninsurance rates exceeded 80% (Susser and Ziebarth 2016). However, support for sick leave mandates has grown substantially in the last decade. At the federal level, the *Healthy Families Act* (reintroduced in Congress in 2015) proposes the legal right for employees to earn one hour of paid sick leave per week, up to seven days per year. The

‡We would like to thank David Bradford, Raj Chetty, Randall Cragun, Harald Dale-Olsen, Davide Dragone, Isaac Ehrlich, Martin Feldstein, Donna Gilleskie, Laszlo Goerke, John Gruber, Michael Hanselmann, Carsten Helm, Nathaniel Hendren, Barry Hirsch, Jason Hockenberry, Kevin M. Hollenbeck, Caroline Hoxby, John Kolstad, Peter Kuhn, Martin Karlsson, Don Kenkel, Bryant Kim, Wojciech Kopczuk, Emily Lawler, Dean Lillard, Neale Mahoney, Wanda Mimra, Tom Mroz, Sean Nicholson, Zhuan Pei, Jim Poterba, Sarah Prenovitz, Neel Rao, Andreas Richter, Chris Ruhm, Daniel Sacks, Nick Sanders, Jörg Schiller, Joseph Shapiro, Justin Sydnor, Rainer Winkelmann, Joachim Winter, and Peter Zweifel for excellent comments and suggestions. In particular we would like to thank Itzik Fadlon, Dan Kreisman, and Andreas Peichl for outstanding discussions of this paper. We also thank seminars participants at the 2016 NBER Summer Institute Health Care Meeting in Boston, the 2016 NBER TAPES Conference in Mannheim, the 2016 NBER Public Economics Spring Meeting in Boston, the 2016 EuHEA Conference in Hamburg, the 13^{th}Southeastern Health Economics Study Group in Atlanta, the 29th Annual Conference of the European Society for Population Economics (ESPE) in Izmir, the 2015 World Risk and Insurance Economics Congress in Munich, the Nordic Health Economics Study Group meeting (NHESG) in Uppsala, the Verein for Socialpolitik (VfS) in Münster, the Centre for Research in Active Labour Market Policy Effects (CAFE) Workshop at Aarhus University, the Risk & Microeconomics Seminar at LMU Munich, the 2015 Young Microeconometricians Workshop in Mannheim, the University of Linz (Economics Department), the Bavarian Graduate Program in Economics (BGPE) in Nuremberg, the Berlin Network of Labor Market Researchers (BeNA), and the Institute for Health Economics, Health Behaviors and Disparities at Cornell University (PAM) for their helpful comments and suggestions. We also thank Eric Maroney and Allison Hewitt Colosky for editing this paper and Sarah Prenovitz and Philip Susser for excellent research assistance. Generous funding from the W.E. Upjohn Institute for Employment Research in the form of an Early Career Research Awards (ECRA) #15-150-15, the Cornell Population Center (Rapid Response Grant), the Cornell Institute for the Social Sciences (Small Grant) and the Mario Einaudi Center for International Studies (Seed Grant) is gratefully acknowledged. Neither we nor our employers have relevant or material financial interests that relate to the research described in this paper. We take responsibility for all remaining errors in and shortcomings of the paper.

epigraphs above suggest potential bipartisan support, but a decade of discussions in Congress has failed to deliver any concrete results. Instead, cities and states have taken steps and implemented mandates at the lower administrative level. On the city level, mandates were implemented in San Francisco (2007), Washington D.C. (2008), Seattle (2012), Portland (2014), New York City (2014), Philadelphia (2015), and meanwhile in several dozen other cities. On the state level, Connecticut was first to implement a sick pay mandate for service sector workers in non-small businesses in 2012. California, Massachusetts, and Oregon followed with more comprehensive mandates in 2015 and 2016.

One objective of this paper is to provide an analytical framework of the underlying labor supply reactions of employees when sick pay changes. Traditionally, behavioral adjustments to varying levels of insurance generosity have been labeled "moral hazard" in economics (Pauly 1974; Nyman 1999). We develop a simple model that decomposes overall employee labor supply adjustments into what we call "contagious presenteeism" and "noncontagious absenteeism." Contagious presenteeism is when employees with a contagious disease (e.g., a common cold) go to work sick and spread the disease to co-workers, customers, and the general population. Such behavior induces negative externalities. Noncontagious absenteeism is when employees without a contagious disease (e.g., back pain) call in sick. When sick pay changes, both behavioral responses work in opposite directions. For example, when employees obtain sick leave coverage, some of those with a common cold will call in sick and not spread the disease because of sick pay. Contagious presenteeism decreases. On the other hand, marginal employees with back pain will call in sick because of sick pay. Noncontagious absenteeism increases.

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

While the traditional social insurance literature has considered behavioral adjustments to government regulation as generally undesirable, more recent papers also identify and emphasize wel-

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

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

The first identifying test exploits high-frequency Google Flu data to estimate the impact of city-level sick pay mandates on influenza-like illness (ILI) rates in the U.S. The staggered implementation of sick pay mandates across metropolitan areas over time naturally leads to the estimation of standard difference-in-differences (DD) models. We make use of publicly available Google Flu data as they are available at the weekly level for 97 metropolitan areas over a long time period. Using Google search queries and a specific algorithm, Google Flu retrospectively replicates the official ILI rate (which is not available to researchers at the city level). We first demonstrate the relevance of measurement error in the Google Flu data and verify that it is not correlated with the reforms. Then we show the following: After U.S. employees gained access to paid sick leave because of sick pay mandates, the ILI rate in the population decreases significantly. This finding yields strong reduced-form evidence for the existence of contagious presenteeism, and that such behavior decreases when employees are able to take sick days. The finding that sick pay mandates can result in fewer infections and lower influenza activity at the population level is highly policy relevant. It also has implications for specific workplace settings, such as schools or hospitals, where particularly vulnerable populations can be affected by the spread of diseases.

The second identifying test is closely linked to the analytical framework in the middle part of the paper. The identification hinges on one main (strong) assumption and the data that we exploit

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

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

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

2 Paid Sick Leave History and Literature

Historically, paid sick leave was part of the first social insurance system in the world. The first public health insurance legislation included sick leave benefits as part of the benefit package. Under Otto 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. At the time, the costs for paid sick leave exceeded half of all health care costs due to the limited availability of (expensive) medical treatments in the nineteenth century (Busse and Riesberg 2004). Today, every European country has some form of universal access to paid sick leave.

Opponents of universal paid sick leave argue that such a social insurance benefit 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. However, using synthetic control group methods and similar variation than this paper, Pichler and Ziebarth (2016a) find no evidence that the early US sick pay mandates had an economically relevant effect

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

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

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

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

absence determinants as well as transmission rates of contagious diseases, linking the estimation approach to an economic model of absence behavior.

This paper also adds to the literature on the determinants and consequences of infectious diseases, epidemics and vaccinations (Mullahy 1999; Bruine de Bruin et al. 2011; Uscher-Pines et al. 2011; Ahn and Trogdon 2015). For example, Maurer (2009) models supply and demand side factors of influenza 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 large events are the underlying mechanism. Adda (2016) shows that reductions in inter-personal contacts, e.g., through school closures or the shut-down of public transportation, reduce transmission rates.

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

3 Evidence from U.S. Sick Leave Reforms

Whereas Germany has one of the most generous sick leave systems worldwide, the U.S. has 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 city-level sick pay mandates on influenza-like disease rates in the U.S.

3.1 The U.S. Sick Leave Landscape

About half of the U.S. workforce lacks access to paid sick leave, particularly low-income employees in the service sector (Heymann et al. 2009; Susser and Ziebarth 2016). Appendix Table A1 provides a summary of recent sick pay reforms at the city level. The details of the bills differ from city to city but all sick pay schemes are employer mandates. Small firms are sometimes exempt. Employees "earn" paid sick leave credit (typically one hour per 30-40 hours worked) up to nine days per year; this credit rolls over to the next calendar year if unused. Because employees need to accrue sick days, most sick pay schemes explicitly state a 90 day accrual period. However, the right to take *unpaid* sick leave is part of most sick pay mandates. Note that gaining the right to

take unpaid leave can be seen as a normalization and represents an increase in sick leave benefits because the right to take unpaid leave decreases the likelihood of being dismissed when calling in sick. Also note that these sick pay mandates do not require (or allow) employers to require doctors' notes as a precondition for taking sick leave (Polsky 2016; New York City Consumer Affairs 2016).

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

3.2 Using Google Flu Data to Test for Changes in Infections

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

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

3.2.1 Outcome Variable

As mentioned, Google Flu replicates the official CDC ILI rate per 100,000 doctor visits by the via an (unknown) algorithm. The mean for the city sample is 1,913 ILI per 100,000 doctor visits. We take the natural logarithm of this measure as dependent variable.

The reason for the normalization of the ILI rate by doctor visits is its CDC benchmark measure which has the same normalization. Consequently, the ILI rate can be interpreted as "diagnosed" ILI rate. As U.S. sick pay mandates do not require (or allow) a doctor's note, doctor visits should

[1] We omit New Orleans, which has missing information due to Hurricane Katrina. We also omit the cities which were not treated through a city mandate but a state mandate, and we omit cities that are in close proximity of the treated cities due to potential spillovers.

not increase due to the mandates. If that was nevertheless the case, our estimates would represent lower bounds.

3.2.2 Treatment and Control Groups

Table A1 (Appendix) lists all cities that implemented sick pay mandates between 2006 and 2015. All seven cities listed and Washington D.C. are treated units and the remaining 74 metropolitan areas in Appendix A2 are control units.

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

3.2.3 Assessing Google Flu Measurement Error

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

First of all, even if systematic over- or underestimation occurred, it should not be a threat to our estimates as long as this bias is not correlated with the introduction of sick pay mandates at the city level. Our main model is a rich fixed effects specifications with 81 city and 617 week-year fixed effects that net out time-variant seasonal trends in influenza activity as well as time-invariant regional effects.

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

Third, we formally test whether there is evidence that Google Flu over- or underreports are correlated with the reforms. For this test, we acquired official CDC data on ILI cases per 100,000

doctor visits. These data are available on the weekly level and the level of the 10 HHS regions (but not at the city level). We aggregate our Google Flu data and construct an equivalent dataset. Figure 1a plots both time series. The vertical lines represent the implementation of *all* sick pay mandates during this time, both city and state mandates (e.g. Connecticut (Jan 1 2012), California and Massachusetts (July 1 2015)). As seen, one does *not* observe any trend in the measurement error but single spikes here and there, some of which represent an overestimation of the true flu rate. Particular striking is the huge spike in the second half of 2012 that triggered the media debates about the "Epic Failure of Google Flu " (Lazer et al. 2014). However, as seen, this seems to have been a single outlier that is not particularly worrisome in our model with week fixed effects.

[Insert Figure 1 about here]

Figure 1b plots the difference in residuals between both datasets (CDC vs. Google Flu) after regressing each flu rate on 617 week and 9 HHS region fixed effects. In other words, Figure 1b provides a visual assessment of the differences in the remaining variation by week and region after netting out seasonal and regional effects. The thin sold black line represents HHS Region 1 which includes the treatment states Connecticut and Massachusetts. The corresponding dashed vertical line represents the date when the sick pay mandates were implemented in both states. Equally constructed are the thick black and gray colored lines and dots. As seen, there is no visual evidence of any systematic correlation between week-region measurement errors and the implementation of sick pay mandates. This visual assessment is confirmed when we regress the differences in residuals on a treatment-time indicator: With 6,191 region-week observations, the point estimate is 0.0247, positive and not statistically significant (standard deviation: 0.0697).

As final robustness checks, we scrap Google Trends (2017) data at the week-city level using the search terms (i) flu shot, (ii) back pain, and (iii) gun shows. Using the search frequencies for these search terms as outcome variable, we re-run our standard model. The (non-significant) findings are discussed in the results section. We also estimate placebo models pretending other major cities in the same state were treated instead of the real ones.

3.3 Changes in Influenza Activity When Employees Gain Sick Pay Coverage

3.3.1 Parametric Difference-in-Differences Model

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

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

where $log(y_{it})$ is the logarithm of the Google Flu (2015) rate in city i in week of the year t. γ_i are 80 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 mandate between 2003 and 2015, see Table A1. The interaction with the vector $LawEffective_t$ yields the binary variable of interest. The interaction term is one for cities and time periods where a sick pay scheme was legally implemented (see Table A1, column (3)). In addition to the rich set of city and time fixed effects, we control for the monthly BLS provided unemployment rate at the city level, $Unemp_{ci}$. The standard errors are routinely clustered at the city level (Bertrand et al. 2004) but the standard errors obtained by bootstrapping are very similar (available upon request). Thus this empirical specification allows us to estimate ϕ_t—the effect of mandated sick pay on the population ILI rate.

Event study. 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 T=-24 months to T=0 and T=+24 months. This allows us to net out, normalize and graphically plot changes in flu rates, relative to when the laws were implemented. Event studies also help assessing whether there is any evidence for confounding factors or an endogenous implementation of the laws, for example, as a reaction to pre-existing trends.

3.3.2 Empirical Results

Event Study Graphs. We start with the event study graphs and Figure 2. In Panel A we plot the coefficient estimates of the continuous time indicators counting the weeks before and after the laws became effective in each city. Recall that the coefficient estimates are net of city fixed effects and week-year fixed effects, i.e., correct for common influenza seasonalities across the major U.S. metropolitan areas. Panel A of Figure 2 shows the result for the unbalanced panel. There is very little trending in the two years before the mandates became effective. The large majority of the coefficient estimates are not statistically different from zero and fluctuate only slightly around the zero line. There is also not much evidence for anticipation effects.

[Insert Figure 2 about here]

Immediately after all employees gained the right to take paid and unpaid sick leave, the ILI rate decreases significantly. The estimates for T=+18 months and beyond lack precision because

they are solely based on San Francisco (2007), D.C. (2008), and Seattle (2012). New York City's comprehensive bill became effective April 1, 2014—about one year and 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 observed rebound of infection rates to the zero line is determined by a lack of precision and the early experiences in these three cities. More important, the rebound may be driven by the confounding effect of the Great Recession for San Francisco (it is well documented that fear of unemployment increases presenteeism (cf. Pichler 2015; Schön 2015). We test this hypothesis by excluding San Francisco from the sample and re-running the model. Panel A of Appendix Figure A1 shows that, indeed, the rebound effect was at least partially driven by the Great Recession that kicked in 2008.

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

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

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

[Insert Table 1 about here]

Main DD Results. Panel A of Table 1 shows the main regression results of the DD model in Equation (1). Every column represents one model where the first two columns represent the standard model. The only difference between even and uneven columns is that the former additionally control for the monthly BLS unemployment rate. We find that controlling for the monthly

unemployment rate barely alters the results (the same is true when we generate and control for additional covariates scraping Google Trends (2017)).

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

[Insert Table 1 about here]

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

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

[2] 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.

Placebo Estimates and Robustness Checks. Panel B of Table 1 provides placebo estimates. We run almost exactly the same models as in Panel A. However, instead of the treated cities we assign a placebo treatment to "neighboring" cities in the same state. To avoid the possibility of confounding spillovers, we require a distance of at least 100 miles between the treated and placebo city, which results in 13 placebo cities for the 7 treatment cities.[3] Panel B of Table 1 shows that none of the six placebo model estimates are statistically significant.

[Insert Table 2 about here]

Next, we scrap three search terms using Google Trends (2017) data for the same set of US cities and the same time horizon on the weekly level. Unfortunately, not all 81 cities in Table A2 were available on Google Trends on a weekly basis for the entire time period. Consequently, the eight models in Table 2 have slightly smaller sample sizes (but still enough statistical power). Moreover, the raw search data contains many zeros which is why we cannot run exactly re-run our main model. Instead we run a generalized linear model with a log link as suggested in Silva and Tenreyro (2006) and show the estimates' robustness to the functional form.

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

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

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

Summing up, all results together provide suggestive evidence that sick pay reduces contagious presenteeism which reduces ILI rates. For non-contagious diseases such as back pain, on the other hand, we do no find evidence for a reduction in the incidence rate.

[3]Specifically, we assign placebo treatments to Los Angeles, San Diego, San Jose, Irvine, Fresno and Sacramento, CA (instead of San Francisco and Oakland, CA); Spokane, WA instead of Seattle, WA; Albany, Buffalo, and Rochester, NY (instead of New York City); Eugene, OR instead of Portland, OR; and Pittsburgh and State College, PA (instead of Philadelphia, PA).

Discussion of Effect Sizes. Our main models in the first two columns of Table 1 suggest reductions in population-level ILI rates by between 6 and 7 percent after sick leave mandates were implemented at the city level. Our model in the next section will provide theoretical evidence of the underlying labor supply mechanisms: marginal employees with contagious diseases will call in sick and stay at home when the costs of taking sick leave decrease. Consequently, social interactions of contagious employees decrease and hence the infection rate in the population.

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

Per week and over the time period considered in this paper, the CDC counted an average of 1,655 confirmed ILI cases per 100,000 doctor visits (Centers for Disease Control and Prevention 2016). Because, in the US, the average number of doctor visits is about three (Centers for Disease Control and Prevention 2017), this implies that a county with a population of 100,000 has 4965 ILI cases per year, or close to 100 per week.

Thus, our back-of-the envelope calculation suggests that implementing a sick pay mandate in a metropolitan area with 1 million inhabitants *(i)* provides sick leave coverage for 200,000 employees, and *(ii)* prevents the transmission of around 3000 ILI cases per year ($4965 \times 10 \times 0.06 = 2979$).

4 Identifying Contagious Presenteeism and Negative Externalities

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

4.1 Modeling Contagious Presenteeism and Noncontagious Absenteeism Behavior

We extend and build upon a mix of standard work-leisure models to theoretically study the absence behavior of workers (Allen 1981; Brown 1994; Barmby et al. 1994; Brown and Sessions 1996; Gilleskie 1998). Our model focuses on the trade-off between employee absenteeism and presenteeism behavior. In particular, it focuses on negative externalities due to contagious diseases. These externalities stem from incomplete employer information about the existence and the de-

gree of contagiousness of employees' diseases. Because contagiousness is not perfectly observable by employers (and customers), employers cannot prevent contagious employees from working and thus cannot fully internalize the externalities. However, we abstain from explicitly modeling the employer side and the firm level below.[4] We also abstain from analyzing general equilibrium labor market effects.

To simplify notation, we omit the subscript i. The employee's utility function is

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

where u_t represents the worker's utility at time t, $c_t \geq 0$ represents consumption, and $l_t \geq 0$ represents leisure. Utility increases in consumption and leisure over the whole domain. The sickness level is continuous but bounded, $\sigma_t \in [0, 1]$. For $\sigma_t = 0$ workers are in perfect health and their maximum sickness level is $\sigma_t = 1$. Utility decreases over the whole domain of sickness.

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

Hours of work are defined as $h > 0$ and T is the total amount of time available. Workers consume their entire income from work w_t or sick pay $s_t = \alpha_t w_t$, $\alpha_t \in [0, 1)$.[5] If contagiousness and sickness were perfectly observable, we would obtain the first best solution. However, due to information frictions, the employer offers a contract with payments w_t and s_t depending on whether workers are giving up their leisure $T - h$ or not.[6]

The utility differential between work and work absence can be written as $\Delta = u_t(\sigma_t, w_t, T - h) - u_t(\sigma_t, s_t, T)$. To ensure that the worker works when in perfect health, we assume $\Delta > 0$ for $\sigma_t = 0$.

For a given replacement rate α_t, the sickness reservation level $\sigma^*_{\alpha_t}$ can then be derived from [7]

[4] This could include employer signaling, peer effects at the firm level, or discrimination against unhealthy workers.

[5] Note that w also includes career opportunity costs. Scoppa and Vuri (2014) find that absent workers are at a higher risk of getting laid off. Also, workers who call in sick a lot may not get promoted or receive bonus payments. Thus, even a nominal replacement rate of 100% (like in Germany or the US) implies $\alpha_t < 1$. Wages equal average work productivity and are rigid and time-invariant.

[6] For a more parsimonious model we focus on a myopic worker in a one period model framework. Potential effects of turning this model into a multiple period framework are discussed in Appendix B.

[7] To avoid unnecessary brackets for function values $\sigma^*(\alpha)$, we denote α as subscript σ^*_α. There exists a unique $\sigma^*_{\alpha_t}$ because of equation (3) and as utility increases in consumption and leisure and decreases in sickness.

$$u_t(\sigma_t^*, w, T-h) - u_t(\sigma_t^*, \alpha_t w, T) = 0. \tag{4}$$

If $\sigma_t > \sigma_{\alpha_t}^*$ workers call in sick. They work if $\sigma_t < \sigma_{\alpha_t}^*$.

4.1.1 Changes in Sick Pay

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

How do changes in sick pay affect the sickness reservation level $\sigma_{\alpha_t}^*$? Applying the implicit function theorem to equation (4), we obtain (see Appendix B):

$$\frac{\partial \sigma_\alpha^*}{\partial \alpha} < 0 \tag{5}$$

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

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

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

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

4.1.2 Two Types of Diseases and Negative Externalities Due to "Contagious Presenteeism"

Next, let us assume that two types of (mutually exclusive) diseases exist: *(1)* contagious diseases denoted by subscript c (e.g., flu) and *(2)* noncontagious diseases denoted by subscript n (e.g., back

pain). The share of sick workers equals the sum of workers with contagious and noncontagious diseases $p_t = p_n + p_{ct}$.[8]

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

As for contagious diseases, p_{ct} is time-varying and depends on the share of contagious workers who work. Henceforth, we define as "contagious presenteeism" when workers with contagious diseases work. Here, the first term of equation (6), becomes relevant as well; however, it is outside the scope of this paper to model the transmission rate of contagious diseases explicitly (Philipson 2000; Pichler 2015). However, more sick pay reduces contagious presenteeism which reduces infections and the associated negative externalities when contagious workers work and infect coworkers or customers. It holds $\frac{\partial p_c}{\partial\alpha} < 0$ for the first term of equation (6) (see Appendix B).

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

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

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

4.1.3 Changes in Sick Pay and Sick Leave: Graphical Representation

To simulate the German sick pay reform of 1996 in the next section, Figure 3 illustrates the effects of a cut in sick pay. Panel A shows the case for noncontagious diseases. Initially, the share of workers who engage in noncontagious absenteeism behavior—indicated by the sum of the two dark gray areas—is quite large. As sick pay decreases, more workers with noncontagious diseases come to work and the noncontagious absenteeism rate decreases.

[Insert Figure 3 about here]

Panel B shows the case for contagious diseases. As sick pay decreases, more workers with contagious diseases come to work and contagious presenteeism increases. Because of additional in-

[8] In principle, noncontagious diseases represent a special case of contagious diseases, where infections are equal to zero.

fections, the share of individuals with a contagious disease, p_t, increases, as represented by the outward shift of $p_t f(\sigma)$.

4.1.4 Changes in Sick Pay and Moral Hazard: Analytical Derivation

$\beta_{nt} = \frac{A_{nt} - A_{n0}}{A_{n0}}$ denotes the percentage change in noncontagious absenteeism when sick pay decreases. β_{nt} represents the cumulative reform effect at t or formally

$$\beta_{nt} = \frac{1}{A_{n0}} p_n \left(F_n(\sigma^*_{\alpha_0}) - F_n(\sigma^*_{\alpha_t}) \right) < 0. \tag{8}$$

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

$$\beta_{ct} = \frac{1}{A_{c0}} \left(\underbrace{p_{c0}(F_c(\sigma^*_{\alpha_0}) - F_c(\sigma^*_{\alpha_t}))}_{<0} + \underbrace{(p_{ct} - p_{c0})(1 - F(\sigma^*_{\alpha_t}))}_{>0} \right) \tag{9}$$

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

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

$$\frac{F_n(\sigma^*_{\alpha_t}) - F(\sigma^*_{\alpha_0})}{1 - F_n(\sigma^*_{\alpha_0})} = \frac{F_c(\sigma^*_{\alpha_t}) - F(\sigma^*_{\alpha_0})}{1 - F_c(\sigma^*_{\alpha_0})}. \tag{10}$$

This means that the share of individuals between the two indifference points is the same for contagious and noncontagious diseases. In other words: if one is willing to assume that the share of marginal compliers is identical across the two disease types, then new infections are identified by comparing the labor supply elasticities for contagious and noncontagious diseases. This assumption would be violated if the disutilities differed systematically between contagious and

noncontagious diseases. If, for example, all contagious diseases were mild (and the labor supply elasticites for those diseases large) and all non-contagious diseases severe (and the labor supply elasticity small), the assumption would be violated.

Using (10), we can rewrite β_{ct} as:

$$\beta_{ct} = \beta_{nt} + \frac{1}{A_{c0}} \left((p_{ct} - p_{c0})(1 - F(\sigma^*_{\alpha_t})) \right) \tag{11}$$

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

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

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

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

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

4.2 Identifying Contagious Presenteeism and Negative Externalities Empirically

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

4.2.1 Using Population-Level Influenza Rates to Identify Contagious Presenteeism

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

Furthermore, our model predicts that access to sick pay reduces contagious presenteeism, which reduces infections. If no sick pay exists at $t = 0$ and is introduced at $t = 1$, the reduction in contagious diseases, ϕ_t, is:

$$\phi_t = p_{ct} - p_{c0}. \tag{13}$$

In Section 3 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.

4.2.2 Using Disease-Specific Sick Leave Rates to Identify Contagious Presenteeism

In the next section, we will directly implement the model and its identifying test. To do so, we need data on sick leave behavior and variation in sick pay that affects different groups of workers. Then we can estimate the causal effect of changes in sick pay on workplace absenteeism and empirically identify 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 test whether $\beta_{nt} < \beta_{ct}$. Under the identifying assumption in equation (10), the differential $\beta_{ct} - \beta_{nt}$ would then identify additional infections due to contagious presenteeism. These represent negative externalities under lower sick pay.

5 Evidence from German Sick Leave Reforms

Our first results section, Section 3, provided reduced from evidence from the U.S., a country where many workers have no sick pay coverage. We found that more sick pay coverage reduces the ILI rate at the population level. Then, Section 4 developed a simple theoretical illustration of the potential underlying worker reactions to changes in sick pay, and how these relate to changes in infections. The model section also proposed an identifying test of changes in infections and contagious presenteeism, given a (rather strong) main identifying assumption. The main purpose of this final result section is to illustrate *how* to implement the proposed test of Section 4 in practice.

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

5.1 The German Employer Sick Pay Mandate

Germany has one of the most generous universal sick leave systems in the world. The system is predominantly based on employer mandates. In Germany, employers are mandated to continue wage payments for up to six weeks per sickness episode. [9] When employees fall sick and want to take sick leave, they have to inform their employer immediately about their sickness and the expected duration. From the fourth day of a sickness episode, a doctor's note is legally required, but employers can ask for a note from day one. Note that the sickness itself remains confidential. Employees just have to inform their employer *that* they are sick, not why; the standardized public insurance form for doctors' notes does not indicate the type of disease, which is only confidentially transmitted to the sickness fund. This is important because the model assumes that the type of disease is unobservable to the employer.

5.2 The Sick Pay Cut at the End of 1996

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

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

Group I includes the construction sector whose collective agreement covered about 1.1 million private sector workers. When the law became effective at the end of 1996, the existing collective agreement did not include any explicit provision on sick pay, which is why the entire federal regulations applied at the time of the bill's implementation. A negotiated compromise between unions and employers validated the cut but only for the first three days of a sickness episode. The new agreement which became effective July 1, 1997.

Group II counts at least 4.4 million covered employees and is quantitatively the largest group. It includes 11 industries as specified in Table C2, among them the steel, textile and automobile industry. Union leaders in these industries managed to maintain the symbolically important 100

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

percent sick pay level. However, in return, they agreed to exclude paid overtime from the basis of calculation for sick pay. Hence employees with many overtime hours experienced sick pay cuts.

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

5.3 Using Data on Disease-Specific Sickness Absence to Test for Elasticity Differences

In Germany, information on certified sickness absence—including diagnoses—are collected by the nonprofit SHI sickness funds. In 1995, a total of 960 SHI sickness funds existed; 72 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 (Schmitz and Ziebarth 2017; Pilny et al. 2017).

The Federal Association of Company-Based Sickness Funds (*BKK Dachverband*) annually publishes sick leave statistics—the *Krankheitsartenstatistik*—of their 4.8 million enrollees who are gainfully employed and mandatorily insured under SHI (Bundesverband der Betriebskrankenkassen (BKK) 2004).[10] The *Krankheitsartenstatistik* reports aggregated sick leave episodes separately by gender, age group, industry, and main diagnosis according to the *International Classification of Diseases (ICD)*. We collected and digitized information from annual reports between 1994 and 1998 to study the effects of the sick pay cut.[11] Note that the data are only available at the industry level for 12 main disease categories as defined by the ICD; unfortunately, the data are *not* available at the individual level and we have no discretion over how to categorize the disease groups. The descriptives of the disease groups used are in Table C1 (Appendix).

As seen, in total, we count 1,080 observations. Each observation represents one industry, year and sickness category. We make use of 5 years and 18 industries, which adds up to 90 industry-year observations per sickness category.

Sick leave variables. Our outcome variable is the sick leave rate which measures *sick cases per 100 enrollees*. We take the logarithm of each variable, mainly because β_{nt} and β_{ct} (equations (8) and (9)) are expressed in percent and we would like to link the model to the empirical part as

[10] BKKs are not obliged to contribute to the *Krankheitsartenstatistik*. However, the overwhelming majority does, probably out of tradition to contribute to this important statistic that has been existing since 1976. In 2013, more than 90 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.

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

closely as possible. Figure 4 shows this dependent variable and a relatively symmetric, close to normal, distribution. The untransformed *sick cases per 100 enrollees* variable has a mean of 126, implying 1.26 sick leave cases per year and worker across all industries and years. However, the variation ranges from 90 to 163 (Table C1).

[Insert Figure 4 about here]

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

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

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

5.4 Nonparametric Graphical Evidence

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

[Insert Figure 5 about here]

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

Figure 5 shows: First, in general, the data support the common time trend assumption. Despite some minor spikes here and there, all three groups in the four graphs develop in a relatively parallel manner over the pre-reform years.

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

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

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

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

Summary. (i) There is clear evidence that the sick leave rate decreased significantly following the sick pay cut in Germany, $\beta_t < 0$. In addition, we (ii) find a large decrease in back pain cases, suggesting that shirking may have decreased, $\beta_{nt} < 0$. (iii) The annual aggregated labor supply elasticity for contagious diseases appears to be smaller (close to zero) than the elasticity for noncontagious diseases. Thus, there is evidence that $\beta_{nt} < \beta_{ct}$ holds up. (iv) The increase in presenteeism appears to slightly outweigh additional infections $\beta_{ct} < 0$. Finally, because *(v)* $\beta_{nt} - \beta_{ct} < 0$, there is evidence that the sick pay cut may have increased infections through more contagious presenteeism behavior.

5.5 Parametric Difference-in-Differences Model

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

$$log(y_{it}) = \beta_0 + \beta_1 GroupI_i \times post + \beta_2 GroupII_i \times post + \\ + \delta_t + \gamma_i + \mu_{it} \tag{14}$$

where $log(y_{it})$ is the sick leave rate for industry i in year t. γ_i are 16 industry fixed effects and δ_t 4 year fixed effects. The standard errors are clustered at the industry level. The reference period is 1994 to 1996.

$GroupI_i$ and $GroupII_i$ are binary treatment indicators which are one for industries that were affected by the sick pay reform (Table C2). Group I experienced a sick pay cut from 100 percent to 80 percent, while Group II experienced a "soft cut"—with paid overtime excluded from the basis of calculation. Group III was not affected, serving as the control group. Thus 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 1994-1996. Moreover, β_2 identifies the effect of excluding paid overtime for Group II in 1997/1998 relative to Group III and the pre-reform period.

5.5.1 Decomposing Total Labor Supply Adjustments via Disease-Specific Elasticities

Estimating $\hat{\beta}_t$, $\hat{\beta}_{nt}$, and $\hat{\beta}_{ct}$. Table 3 shows the results of the DD model using the four different outcome variables in Figure 5, plus sick leave due to *injuries and poisoning*. Each column is one model as in Equation (14). For illustrative purposes, we only display β_1 and β_2. Note that the empirical models are closely linked to the theoretical model in Section 4. For example, β_1 in the first row of the first column of Table 3 estimates β_t in Equation (12).

[Insert Table 3 about here]

We summarize Table 3: First, the overall sick leave rate decreased by about 18 percent when sick pay was cut from 100 to 80% (column (1)). This is $\hat{\beta}_t$ in Equation (12) and represents the total labor supply effect, traditionally called "moral hazard." As seen, β_1 is highly significant and clearly smaller than zero. Related to the decrease in sick pay of about 20 percent, the sickness rate elasticity with respect to the replacement rate would be about 1.

Second, as already inferred from Figure 5, all β_2 interaction effects are imprecise and relatively small in size. Hence, in a regression framework with industry and year fixed effects we cannot identify any significant changes in the sick leave rate for Group II. This is at least partly due to the coarse industry-year data that masks the subset of treated workers.

Third, musculoskeletal diseases, representing the noncontagious disease category n in our model, decreased overproportionally by 30 percent (column (4)). This fits the common perception that the labor supply of this category is particularly elastic and prone to shirking behavior. Equation (8) of our model illustrates the analytical derivation of β_{nt}, which is represented by β_1 in column (4) of Table 3. It is the decrease in noncontagious absenteeism when sick pay decreases.

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

Further Results and Robustness Checks. Column (5) is a robustness test because 50% of all *injury and poisoning* absences are due to workplace accidents (BKK 2004). The bill that cut sick pay, however, exempted sick leave due to workplace accidents from the cuts. Indeed, as see by β_1 in column (5), the *injury and poisoning* absence rate decreased underproportionally by 11 percent.

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

5.5.2 Directly Testing $\beta_{nt} = \beta_{ct}$

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

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

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

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

6 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 coworkers and customers. Such behavior is a public health issue and one driving force of the spread of contagious diseases. If contagion is unobservable, which is usually the case at the beginning of sickness 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 level in the U.S.—a country without generous sick pay coverage. Using Google Flu Trends data, we show that influenza-like disease rates decreased significantly when employees gained 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 coverage for themselves and their children. Our estimates suggest that

the relatively comprehensive laws at the level of eight major U.S. cities helped preventing about 3000 influenza-like disease cases per year and 1 million population. Infections rates may further decrease in the medium to long-run when employees have accrued larger amounts of paid sick days.

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

Finally, we 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 main, relatively strong, identifying assumption that the first order labor supply reactions are similar for contagious and noncontagious diseases, we also find evidence for the existence of contagious presenteeism in Germany. However, in case of Germany with one of the most generous sick leave systems worldwide (and conditional on the identifying assumption above), the reduction in noncontagious absenteeism was clearly larger than the increase in the infectious sick leave rate due to contagious presenteeism when sick pay was cut from a baseline level of 100 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 to changes in sick pay. Important fields of applications include contagious presenteeism by teachers or school kids, for example, 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 and minimized by optimized sick pay schemes. Note that our test can be carried out using many different types of data, including school-level, firm-level data, or hospital-level data. Ideally, one would want to exogenously vary different types of sick pay schemes (e.g. in field experiments) and then measure changes in noncontagious absenteeism and contagious presenteeism behavior.

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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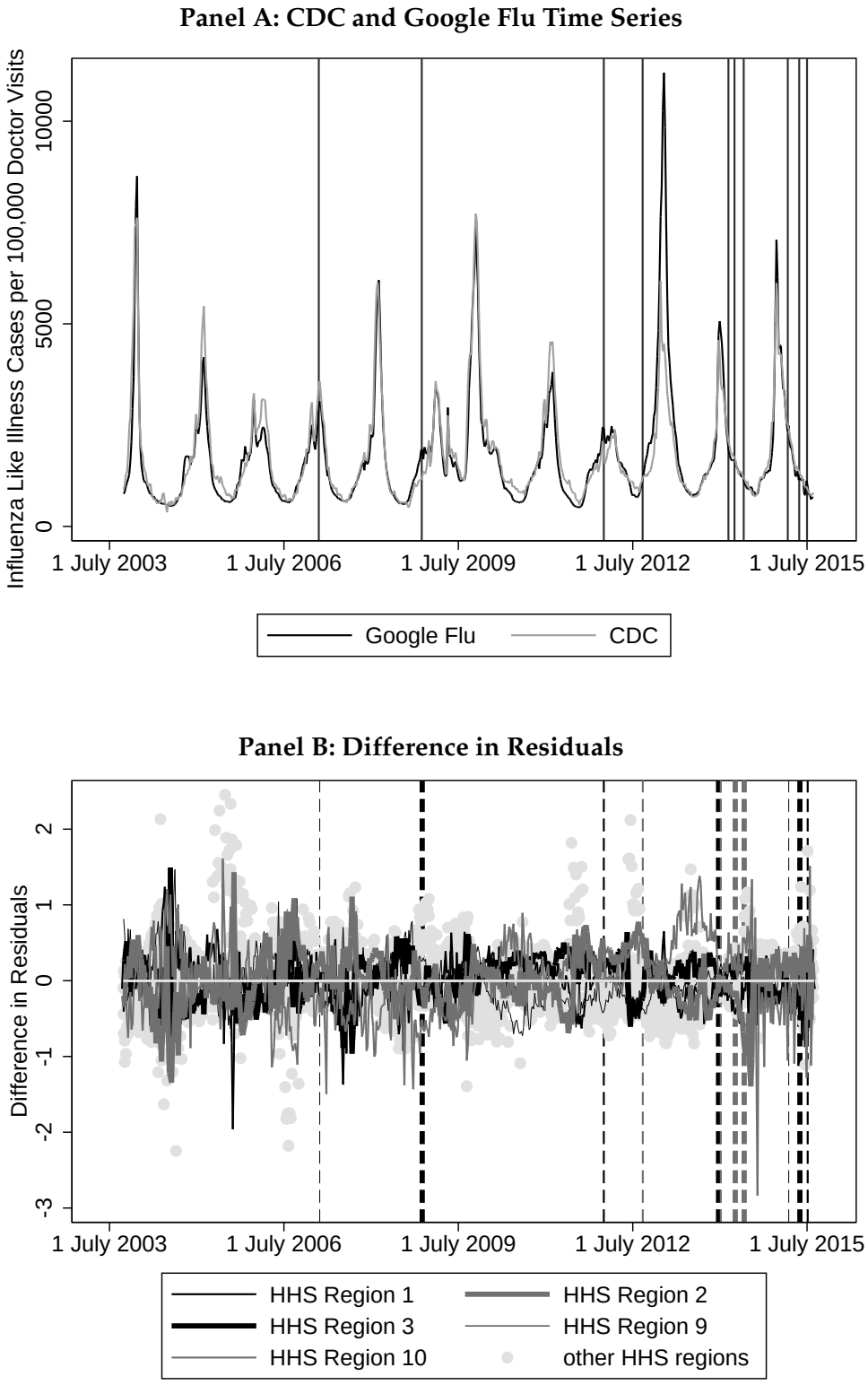
Figures and Tables

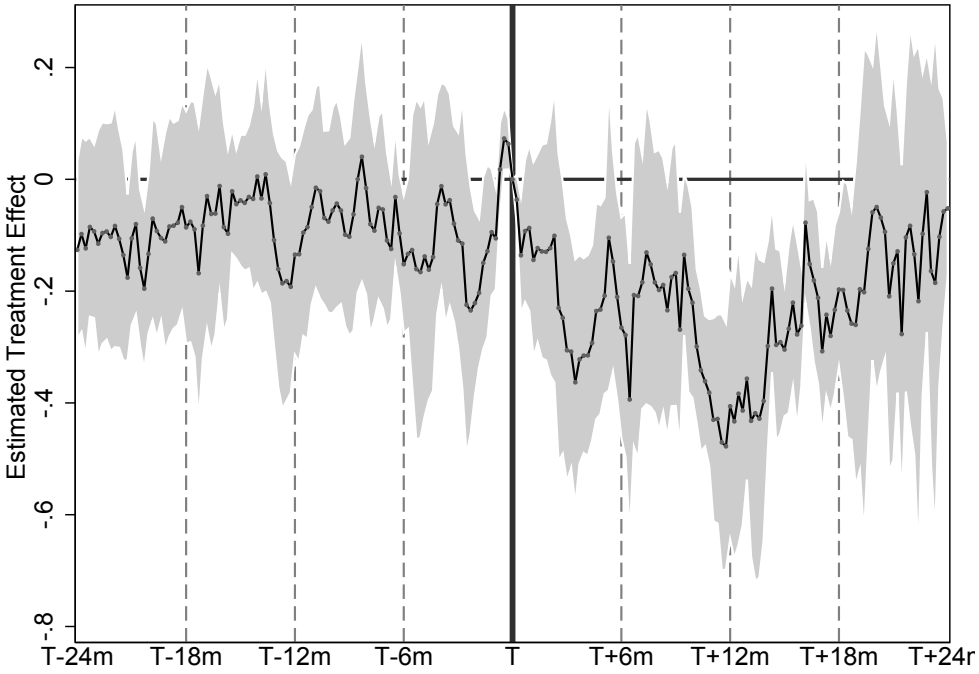
Figure 1 Google Flu Measurement Error



Panel A shows official influenza-like illnesses per 100,000 doctor visits by the CDC and the estimates by Google Flu. CDC data are available at the weekly level for 10 HHS Regions. Panel B plots the difference in residuals between the two datasets. 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 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.

Figure 2 Event Study—Effect of City-Level Sick Pay Mandates

Panel A: Unbalanced Weekly



Panel B: Balanced Monthly

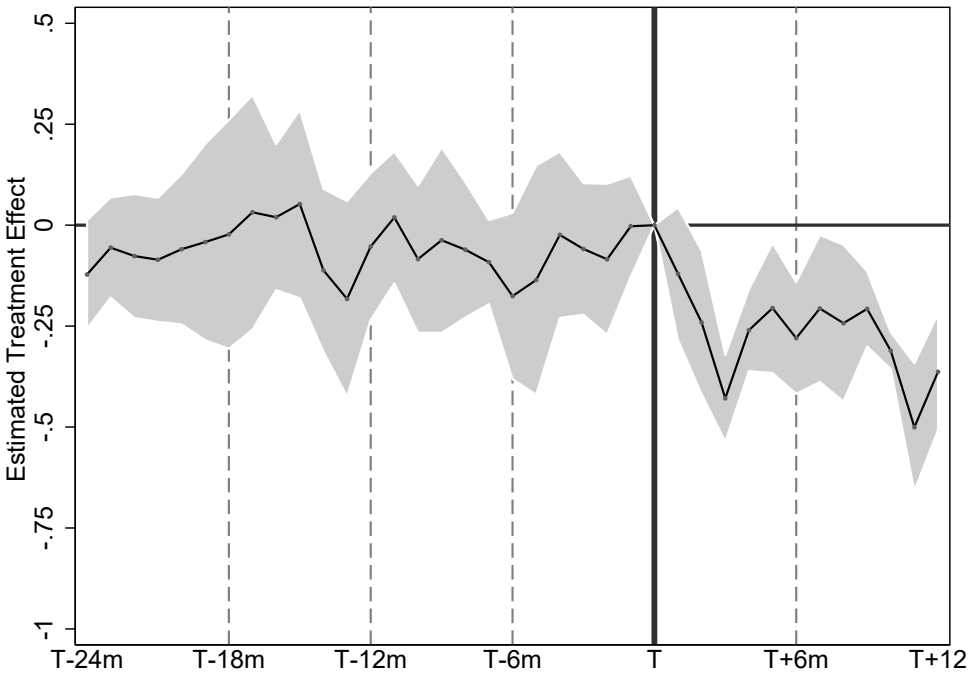
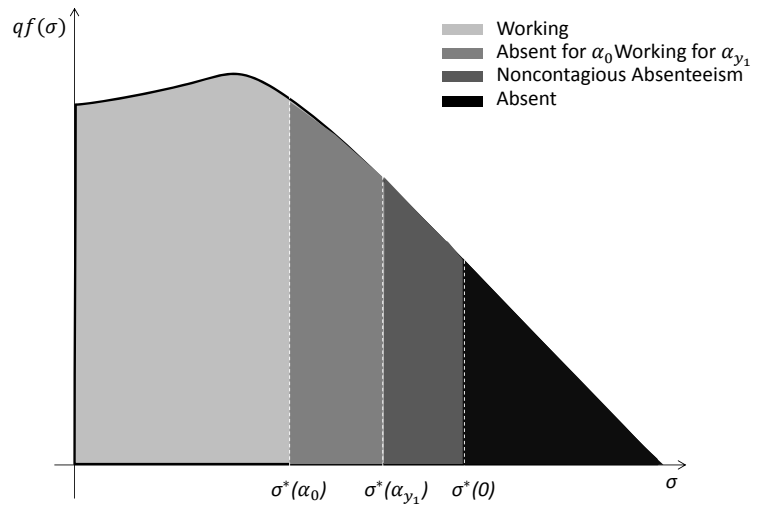
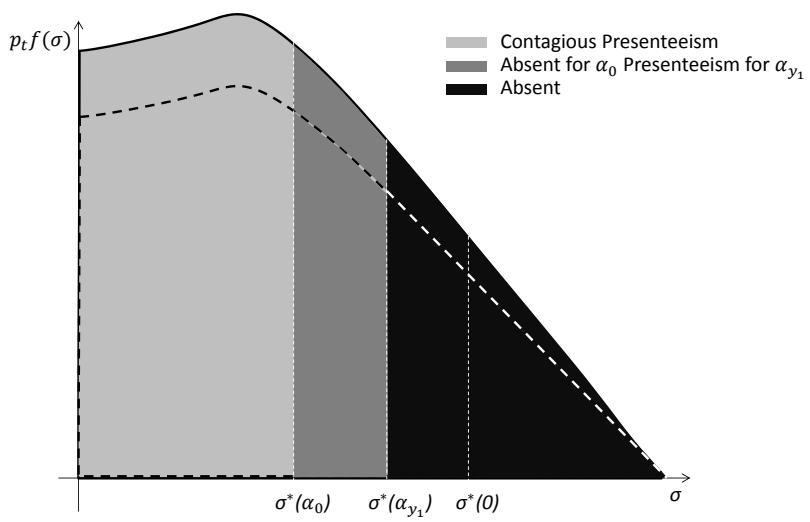


Figure 3 Graphical Representation and Classification of Working and Absent Employees

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, noncontagious absenteeism decreases. Panel B shows the case for contagious diseases. A sick pay cut increases contagious presenteeism and p_t, represented by the outward shift of the curve.

Figure 4 Distribution of Logarithm of Sick Leave Cases

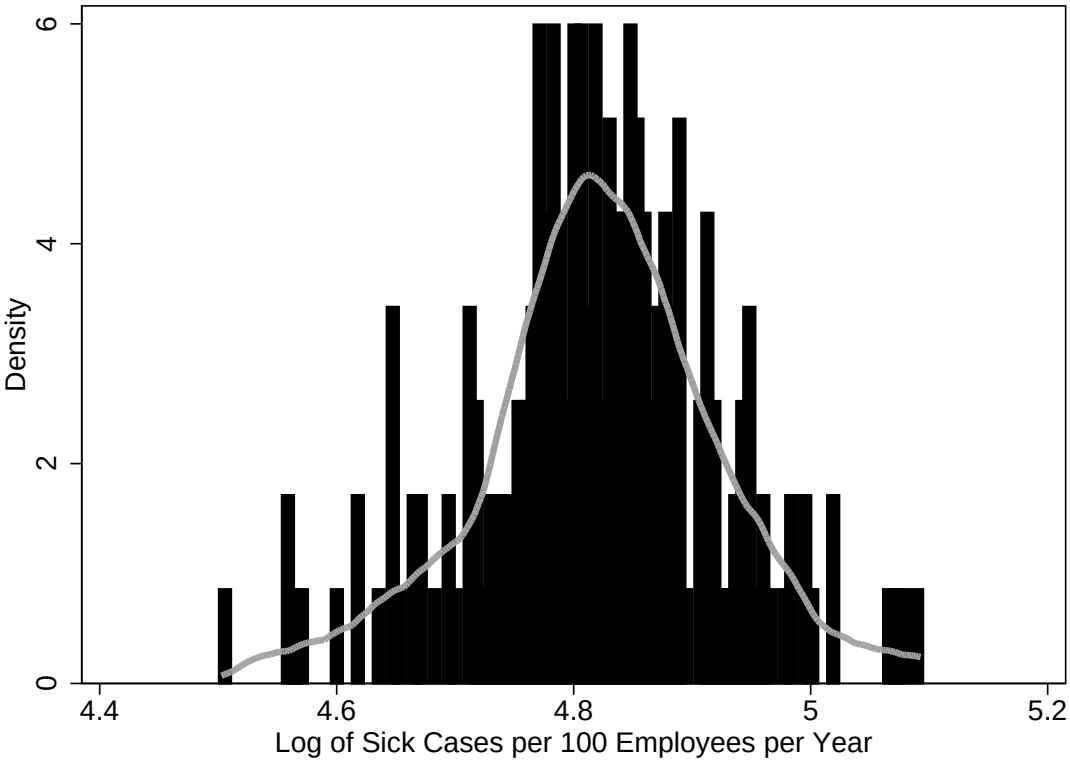
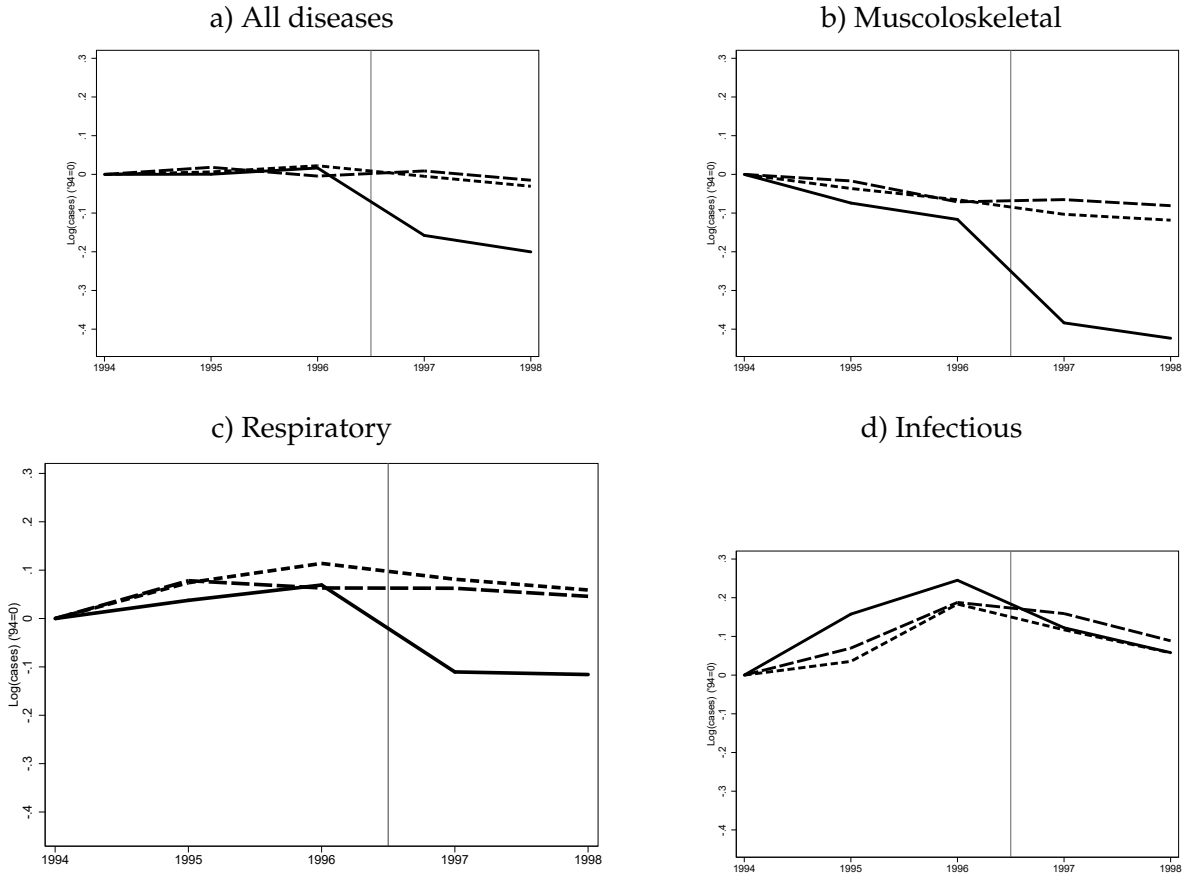


Figure 5 Development of Sick Leave Rates by Treatment Groups over Time



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

Table 1 Effect of City-Level Sick Pay Mandates on ILI Rates 2003-2015

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

* 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 ILI rate per 100,000 doctor visits as reported by Google Flu (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. N=48,333 for Panel A and 48,505 for Panel B. Even numbered columns additionally control for the local monthly unemployment rate (BLS 2015). Treated cities in Panel A are listed in Table A1 together with the dates for *law effective*, *law passed* and *probation over*. The placebo cities in Panel B are cities from the same state but which are at least 100 miles distant. Specifically, we assign placebo treatments to Los Angeles, San Diego, San Jose, Irvine, Fresno and Sacramento, CA (instead of San Francisco and Oakland, CA); Spokane, WA instead of Seattle, WA; Albany, Buffalo, and Rochester, NY (instead of New York City); Eugene, OR instead of Portland, OR; and Pittsburgh and State College, PA (instead of Philadelphia, PA). The entire sample of cities considered is in columns one and two of Table A2.

Source: Google Flu (2015), own calculation and illustration.

Table 2 Effect of City-Level Sick Pay Mandates on ILI Rates and Alternative Outcomes 2003-2015

	ILI Rate		Flu Shots		Back Pain		Gun Shows	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TreatedCity×LawEffective	-0.061***	-0.060***	-0.135	-0.134	0.100	0.101	-0.134	-0.136
	(0.023)	(0.022)	(0.083)	(0.083)	(0.085)	(0.085)	(0.155)	(0.156)
N	39,510	39,510	39,510	39,510	39,510	39,510	39,510	39,510

* p<0.1, ** p<0.05, *** p<0.01; standard errors in parentheses are clustered at the city level. The dependent variable is shown in the column header. In the first two columns, it is the ILI rate as reported by Google Flu (2015). All regressions contain week-of-year fixed effects and city fixed effects as in equation (1). Each column represents one generalized linear model with a log link. Even numbered columns additionally control for the local monthly unemployment rate (BLS 2015). *TreatedCity* is a treatment indicator which is one for all cities listed in Table A1. The sample of cities considered is in Table A2.
Source: Google Flu (2015) and Google Trends (2017), own calculation and illustration.

Table 3 Effect of Sick Pay Cut on Sick Cases by Disease Groups

	All diseases	Infectious	Respiratory	Musculosk.	Inj. & Pois.
	(1)	(2)	(3)	(4)	(5)
Group I×post	-0.177***	-0.082**	-0.156***	-0.296***	-0.109***
	(0.042)	(0.038)	(0.046)	(0.063)	(0.032)
Group II×post	-0.020	-0.024	0.000	-0.033	-0.027
	(0.056)	(0.059)	(0.058)	(0.078)	(0.059)
Number of industries	18	18	18	18	18
R2	0.571	0.729	0.598	0.892	0.875

* p<0.1, ** p<0.05, *** p<0.01; standard errors in parentheses are clustered at the industry level. The descriptive statistics are in the Appendix (Table C1). Each column represents one model as in equation (14), estimated by OLS, i.e., all models include industry and year fixed effects. The dependent variables are logarithms of the normalized sick cases per 100 employees. Column (1) uses the total number of sick cases as dependent variable, column (2) solely uses infectious sick cases and so on. For more data information, see section 3.2. Group I's sick pay was cut from 100 to 80% at the end of 1996. Group II's sick pay was also cut but the cut was smaller. For more information about the sick pay reforms, see Section 5.2.
Source: BKK (2004), own calculation and illustration.

Appendix A: US

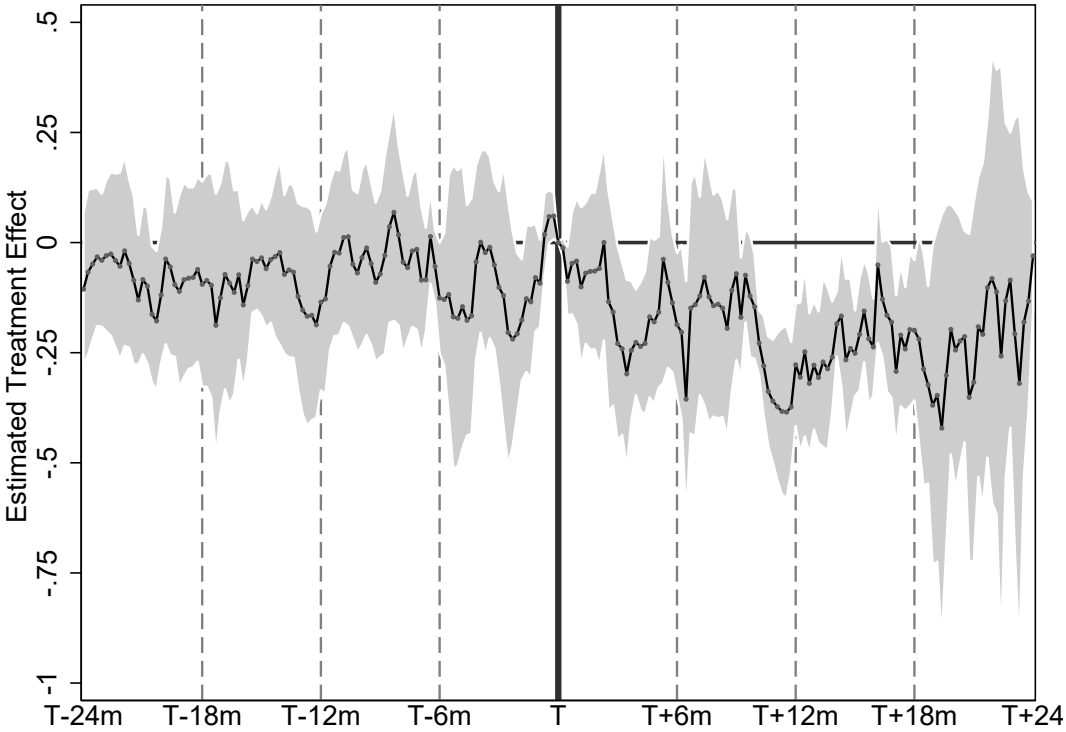
Table A1 Overview of Employer Sick Pay Mandates at the City Level in the U.S.

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 Dec 18, 2013 (extension pending funding)	Nov 13, 2008 Feb 22, 2014 (retrospective in Sep 2014)	'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 extension to 20,000 temporary workers and tipped employees
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
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

Source: several sources, own collection, own illustration.

Figure A1 Event Studies Robustness Checks

Panel A: Unbalanced Weekly without San Francisco



Panel B: Unbalanced Monthly

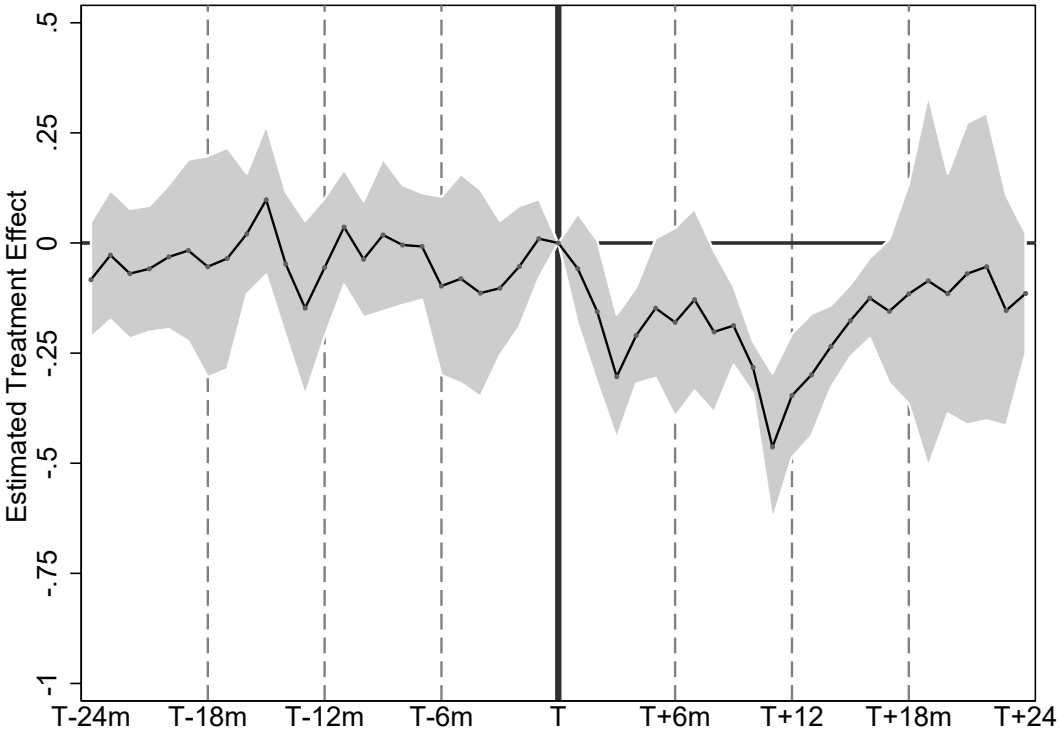


Table A2 Cities Included in Weekly Google Flu Data

City	Month	Day	Year	City	Month	Day	Year
Albany, NY	9	28	2003	Mesa, AZ	11	7	2004
Albuquerque, NM	10	12	2003	Miami, FL	9	28	2003
Anchorage, AK	10	17	2004	Milwaukee, WI	9	28	2003
Arlington, VA	9	28	2003	Nashville, TN	9	28	2003
Atlanta, GA	9	28	2003	**New York, NY**	9	28	2003
Austin, TX	9	28	2003	**Newark, NJ**	9	28	2003
Baltimore, MD	9	28	2003	Norfolk, VA	9	28	2003
Baton Rouge, LA	9	26	2004	**Oakland, CA**	9	28	2003
Beaverton, OR	12	14	2003	Oklahoma City, OK	9	28	2003
Bellevue, WA	11	30	2003	Omaha, NE	9	28	2003
Berkeley, CA	9	19	2004	Orlando, FL	9	28	2003
Birmingham, AL	9	28	2003	**Philadelphia, PA**	9	28	2003
Boise, ID	10	3	2004	Phoenix, AZ	9	28	2003
Boston, MA	9	28	2003	Pittsburgh, PA	9	28	2003
Buffalo, NY	10	19	2003	Plano, TX	10	16	2005
Cary, NC	9	26	2004	**Portland, OR**	9	28	2003
Charlotte, NC	9	28	2003	Providence, RI	10	17	2004
Chicago, IL	9	28	2003	Raleigh, NC	9	28	2003
Cleveland, OH	9	28	2003	Reno, NV	10	24	2004
Colorado Springs, CO	9	19	2004	Reston, VA	11	28	2004
Columbia, SC	10	10	2004	Richmond, VA	9	28	2003
Columbus, OH	9	28	2003	Rochester, NY	9	28	2003
Dallas, TX	9	28	2003	Roswell, GA	11	23	2003
Dayton, OH	11	23	2003	Sacramento, CA	9	28	2003
Denver, CO	9	28	2003	Salt Lake City, UT	9	28	2003
Des Moines, IA	10	17	2004	San Antonio, TX	9	28	2003
Durham, NC	9	28	2003	San Diego, CA	9	28	2003
Eugene, OR	10	17	2004	**San Francisco, CA**	9	28	2003
Fresno, CA	12	7	2003	San Jose, CA	9	28	2003
Ft Worth, TX	10	3	2004	Santa Clara, CA	9	28	2003
Gainesville, FL	10	12	2003	Scottsdale, AZ	10	24	2004
Grand Rapids, MI	10	3	2004	**Seattle, WA**	9	28	2003
Greensboro, NC	11	14	2004	Somerville, MA	9	28	2003
Greenville, SC	10	24	2004	Spokane, WA	1	16	2005
Honolulu, HI	9	28	2003	Springfield, MO	10	30	2005
Houston, TX	9	28	2003	St Louis, MO	9	28	2003
Indianapolis, IN	9	28	2003	St Paul, MN	9	28	2003
Irvine, CA	10	3	2004	State College, PA	9	5	2004
Irving, TX	9	28	2003	Sunnyvale, CA	9	28	2003
Jackson, MS	11	14	2004	Tampa, FL	9	28	2003
Jacksonville, FL	10	3	2004	Tempe, AZ	9	28	2003
Kansas City, MO	9	28	2003	Tucson, AZ	9	28	2003
Knoxville, TN	10	3	2004	Tulsa, OK	9	28	2003
Las Vegas, NV	9	28	2003	**Washington, DC**	9	28	2003
Lexington, KY	9	26	2004	Wichita, KS	9	26	2004
Lincoln, NE	10	31	2004				
Little Rock, AR	10	3	2004				
Los Angeles, CA	9	28	2003				
Lubbock, TX	10	17	2004				
Madison, WI	9	28	2003				
Memphis, TN	10	24	2004				

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

Appendix B: Model

B1: Derivation of (5)

With the implicit function theorem applied to equation (4), one obtains:

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

$$= \frac{\frac{\partial u(\sigma,\alpha w,T)}{\partial c}\frac{\partial c}{\partial \alpha}}{-\left(\underbrace{\frac{\partial u(\sigma,\alpha w,T)}{\partial \sigma} - \frac{\partial u(\sigma,\alpha w,T-h)}{\partial \sigma}}_{>0(3)}\right) + \left(\underbrace{\frac{\partial u(\sigma,w,T-h)}{\partial \sigma} - \frac{\partial u(\sigma,\alpha w,T-h)}{\partial \sigma}}_{\leq 0(3)}\right)} < 0,$$

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

B2: Discussion of Assumptions and Possible Model Extensions

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

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

Second, we discussed the possibility that sick leave could affect workers' future wages, including a higher likelihood of dismissal (footnote 4). However, we do not model this mechanism explicitly. The benefits of doing it are limited and it would make the model unnecessarily complex. Similarly, sick leave could directly affect future health and productivity. The effects could go in both directions (fully recovering from a disease could increase future health and productivity;

however, workers might also lose firm-specific human capital). In addition, these mechanisms likely differ by disease and occupation and would require to model complex effect heterogeneity.

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

$$u_t(\sigma_t^*, w, T-h) + \beta E[u_{t+1}|present_t] - u_t(\sigma_t^*, \alpha_t w, T) - \beta E[u_{t+1}|absent_t] = 0 \tag{B13}$$

where β is the discount rate.

Next we derive how the reservation sickness level responds to changes in sick pay (similar to equation (6)). First, the numerator is equal to the derivative of equation (6) with respect to α_t. Multiplied by (-1), it reads:

$$\frac{\partial u(\sigma, \alpha w, T)}{\partial c}\frac{\partial c}{\partial \alpha} + \frac{\partial \beta E[U_{t+1}|absent_t]}{\partial \alpha} - \frac{\partial \beta E[U_{t+1}|present_t]}{\partial \alpha} > 0 \tag{B13}$$

As before, $\frac{\partial u(\sigma,\alpha w,T)}{\partial c}\frac{\partial c}{\partial \alpha}$ is positive. In addition, the effects of sick pay on $E[U_{t+1}|absent_t]$ are negligible because slightly more sick pay in t is unlikely to affect u_{t+1}.[12] Finally, $\frac{\partial \beta E[u_{t+1}|present_t]}{\partial \alpha} < 0$ because, *ceteris paribus*, increasing sick pay decreases contagious presenteeism and thus the likelihood of future infections decreases. Therefore the numerator of equation (6) becomes smaller.

In words: if workers were fully rational and expected to meet many sick co-workers at work after a sick pay cut, they might prefer to stay at home in order to avoid infections. However, as it is very difficult to predict infections, abstracting away from this mechanism appears to be a realistic assumption. Moreover, as the flu season extends over several months, it is unrealistic that workers call in sick over a longer period of time to avoid infections. Thus, $\frac{\partial \beta E[u_{t+1}|present_t]}{\partial \alpha}$ is small and smaller than $\frac{\partial u(\sigma,\alpha w,T)}{\partial c}\frac{\partial c}{\partial \alpha}$ which means that the numerator represented by equation (6) remains positive. Overall, it implies that workers' direct/first order response (more sick pay leads to more consumption when sick) is stronger than indirect/second order response (more sick pay increases the likelihood of being infected).

Second, the denominator of the derivative of equation (6) with respect to σ becomes:

[12] An example for a large derivative could be: medicine only becomes affordable once sick pay exceeds a certain threshold and this medicine would cure the disease only in combination with sickness absence.

$$\frac{\partial u(\sigma, w, T-h)}{\partial \sigma} - \frac{\partial u(\sigma, \alpha w, T)}{\partial \sigma} + \frac{\partial \beta E[U_{t+1}|present_t]}{\partial \sigma_t} - \frac{\partial \beta E[U_{t+1}|absent_t]}{\partial \sigma_t} < 0 \tag{B13}$$

The assumption of myopic workers implies $\frac{\partial \beta E[U_{t+1}|present_t]}{\partial \sigma_t}$ - $\frac{\partial \beta E[U_{t+1}|absent_t]}{\partial \sigma_t} = 0$. However, a higher sickness level today will likely decrease expected utility tomorrow as individual health is correlated over time. This decrease in expected utility is most likely even larger if workers work sick instead of recovering at home. Thus the overall denominator in equation (6) becomes even more negative when we relax the myopia assumption.

In sum, relaxing the assumption of worker myopia results in a positive numerator (equation (6)) and a negative denominator (equation (6)); the extended version of equation (6) remains negative and more workers call in sick when sick leave increases.

Appendix C: Germany

Table C1 Descriptive Statistics of Sick Leave Measures

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

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

Table C2 Number of Enrollees per Industry and Treatment Group

Industry and Classification	Mean	Std. Dev.
Group I		
Construction	31,693	13,913
Group II		
Steel	114,236	8,831
Textile	33,034	11,579
Mechanical Engineering	162,364	35,168
Automobile	311,173	42,239
Ship and Aerospace	26,090	7,698
Electrical engineering, optics	251,284	27,410
Wood and Paper	33,375	6,146
Printing	21,111	3,080
Food and Hospitality	27,074	4,518
Trade	135,812	35,417
Banking and Insurance	85,851	15,263
Group III		
Chemical	233,675	63,750
Oil	11,572	2,218
Glass	36,484	7,417
Energy and Water	39,670	8,399
Postal and Transportation	513,328	138,192
Public Administration	317,137	63,066

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

Table C3 Effect of Sick Pay on Sick Leave—Pooled Regressions

	(1) Musculoskeletal	(2) Musculoskeletal, Infectious	(3) Muscul., Infect. Respiratory
Group I×post	-0.296***	-0.296***	-0.296***
	(0.063)	(0.062)	(0.062)
Group II×post	-0.033	-0.033	-0.033
	(0.078)	(0.078)	(0.077)
Group I×post×Infectious		0.214***	0.214***
		(0.073)	(0.072)
Group II×post×Infectious		0.009	0.009
		(0.097)	(0.096)
Group I×post×Respiratory			0.141*
			(0.077)
Group II×post×Respiratory			0.033
			(0.096)
R2	0.892	0.988	0.991
Observations	90	180	270

* p<0.1, ** p<0.05, *** p<0.01; standard errors in parentheses are clustered at the industry-disease-level. The descriptive statistics are in Table C1. 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 sick leave, where musculoskeletal sick leave is the reference group. The third column additionally adds respiratory sick leave. All regressions are estimated by OLS and include industry, disease and year fixed effects. The dependent variables are logarithms of the normalized sick leave cases per 100 employees. For more information on how the variables were generated, see Section 5.3.
Source: Bundesverband der Betriebskrankenkassen (BKK) (2004), own calculation and illustration;