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Mandated Sick Pay: Coverage, Utilization, and Welfare Effects

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Mandated Sick Pay: Coverage, Utilization, and Welfare Effects

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Abstract

This paper evaluates how sick pay mandates operate at the job level in the United States. Using the National Compensation Survey and difference-in-differences models, we estimate their impact on coverage rates, sick leave use, labor costs, and non-mandated fringe benefits. Sick pay mandates increase coverage significantly by 18 percentage points from a baseline level of 66% in the first two years. Newly covered employees take two additional sick days per year. We find little evidence that mandating sick pay crowds-out non-mandated fringe benefits. Finally, we develop a model of optimal sick pay provision and illustrate the trade-offs when assessing welfare.

Keywords: sick pay mandates; take-up; social insurance; fringe benefits; moral hazard; unintended consequences; medical leave; National Compensation Survey; optimal social insurance; Baily-Chetty; welfare

JEL classification: I12, I18, J22, J28, J32, J38, J88, H75

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1 Introduction

Optimal design of social insurance systems is a core research field in economics (Chetty and Finkelstein, 2013; Borghans et al., 2014; Powell and Seabury, 2018; Fadlon and Nielsen, 2019; Autor et al., 2019; Johnson, 2020; Fort et al., 2020). A critical question within this field is to what extent governments should mandate the provision of benefits such as health insurance, Workers' Compensation, and paid family leave (Gruber, 1994; Ruhm, 1998; Hendren, 2017; Cabral et al., 2019); or to what extent the government should directly provide benefits, for example, health insurance coverage for low income populations (Goodman-Bacon, 2018; Finkelstein et al., 2019).

Of all countries in the Organization for Economic Cooperation and Development (OECD), three do not provide universal access to paid sick leave for employees: Canada, Japan, and the United States. Traditionally, in the U.S., employers have voluntarily provided paid sick leave, resulting in substantial inequality in coverage across jobs. For instance, 97% of private sector employees in the finance and insurance industry have access to paid sick leave, while 49% of employees in the accommodation and food services industry have access to paid sick leave. Among low-income and part-time employees, coverage rates lie around 50% (Bureau of Labor Statistics, 2021). The only existing federal law, the The Family and Medical Leave Act of 1993 (FMLA), exempts part-time employees and employees in small employers, and mandates unpaid leave only. As of 2012, an estimated 44%—or 49 million—of private sector employees were not covered by FMLA (Jorgensen and Appelbaum, 2014). Currently, Congress discusses intensively whether to pass sick pay mandates similar to those evaluated in this paper at the federal level (along with differently designed paid family and medical leave provisions, see Pichler and Ziebarth (2020b) for a comparison).

University of Hamburg, the University of Mannheim and ZEW, the University of Ottawa, the University of Southern Florida, the University of St. Gallen, the University of Tennessee, and Weill Cornell Medicine for their helpful comments and suggestions. Last but not least we thank Maury Gittleman at the Bureau of Labor Statistics for helping us with numerous data questions. This research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS. Generous funding from the Robert Wood Johnson Foundation's Policies for Action Program (#74921) and the W.E. Upjohn Institute for Employment Research's Early Career Research Awards (ECRA) program #17-155-15 are 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.

This paper is the first to use high-quality government firm-level data to estimate the first-order effects of U.S. state-level sick pay mandates on (a) the probability that jobs provide sick leave coverage, (b) the use of both unpaid and paid sick leave, (c) labor costs as calculated by the Bureau of Labor Statistics (BLS), and (d) a range of non-mandated fringe benefits such as paid vacation or holidays that employers could systematically reduce in response to the mandates.[1] We also assess whether there is evidence that hours worked and hours paid systematically changed in response. Whereas some existing papers also estimate effects on coverage and take-up, with one exception[2], all these existing papers have in common that they rely on employees' self-reports which can suffer from inherent response and recall biases (Ahn and Yelowitz, 2016; Stearns and White, 2018; Callison and Pesko, 2021). However, to calculate take-up elasticities and to derive valid policy recommendations, precisely knowing the number of paid and unpaid sick leave *hours* taken over the course of the entire year is crucial. Moreover, to assess possible unintended consequences of sick leave mandates and their welfare consequences, testing whether mandates crowd-out non-mandated fringe benefits is key. To this end, we use restricted-access data from the National Compensation Survey (NCS) at the firm-job level from 2009 to 2017 coupled with difference-in-differences (DD) models, methods that are robust to heterogeneous treatment effects with staggered policy adoption over states and time, and event studies. The rich NCS government data are specifically designed to measure employee compensation and employer costs—indeed the U.S. government uses the NCS to adjust federal employee compensation. Over the time period covered in this paper, five U.S. states (California, Connecticut, Massachusetts, Oregon, and Vermont) adopted sick pay mandates. Figure 1 illustrates how the share of covered U.S. employees increased over the time period from 2009 to 2017, namely from half a million to more than 21 million, representing about 15% of all U.S. employees in 2017.

[Insert Figure 1 about here]

The paper also develops a model of optimal sick pay building on the Baily-Chetty sufficient statistics framework (Baily, 1978; Chetty, 2006; Chetty and Finkelstein, 2013). Naturally the model

[1]The previous economic literature on U.S. sick leave is scarce. Gilleskie (1998, 2010) represent notable exceptions. The European literature on paid sick leave is much richer because of the long history of this social insurance (see Section 2). However, the design and generosity differs substantially from the recently adopted U.S. versions. Several studies find positive labor supply elasticities (Johansson and Palme, 2005; Ziebarth and Karlsson, 2010, 2014; De Paola et al., 2014; Fevang et al., 2014; Marie and Vall-Castello, 2020). Other papers investigate interaction effects with other social insurance programs (Fevang et al., 2017), the role of probation periods (Ichino and Riphahn, 2005), culture (Ichino and Maggi, 2000), social norms (Bauernschuster et al., 2010), gender (Ichino and Moretti, 2009; Herrmann and Rockoff, 2012), coworkers (Hesselius et al., 2009), income taxes (Dale-Olsen, 2013), union membership (Goerke and Pannenberg, 2015), and unemployment (Nordberg and Røed, 2009).

[2]However, Pichler and Ziebarth (2020a), has an entirely different focus and uses synthetic control groups to assess whether there is macro-level evidence on the extent to which mandates reduce employment or wage growth *in the aggregate, at the county level* using the Quarterly Census of Employment and Wages. The authors do not find that this was the case. In contrast, this paper uses micro-level data at the *firm-job level*.

must simplify some features of real-world labor markets, but illustrates relevant marginal trade-offs to assess the welfare consequences of mandating sick pay. This is, the impact of sick leave on firm production—measured by work productivity when working sick—weighted by the labor supply elasticity and the increase in sick leave use when employees gain access to sick pay. Moreover, as in most models of optimal social insurance, marginal employees must value the benefit more than it costs employers to provide under the standard assumption of rigid wages. (Under fully flexible wages, wages simply adjust downward and mandating a benefit would never be optimal.)

Historically, the first legislative initiative for a federal sick pay mandate—the Healthy Families Act—was spearheaded by Senator Theodore Kennedy. First introduced to the U.S. Congress in 2005, the bill was reintroduced in 2019 after several failed attempts at passage (Senate Bill 840 - Healthy Families Act, 2019). In the meantime, numerous U.S. cities and states have passed similar sick pay mandates within their jurisdictions. San Francisco was the first locality to implement mandate in 2007, increasing coverage rates above 90% among employees (Colla et al., 2014). In the following years, based on widespread voter support—opinion polls suggest that 75% of Americans support sick pay mandates, with majority support across party affiliation (National Paid Sick Days Study, 2010; HuffPost/YouGov, 2013)—a wave of cities and states adopted sick leave legislation. As of writing, 14 states and 20 cities and counties (including Chicago, New York City, Philadelphia, Portland, Seattle, and Washington D.C.) have passed sick pay mandates, see A Better Balance, 2021b. Moreover, in response to the COVID-19 pandemic, in March of 2020, Congress passed a bipartisan Families First Coronavirus Response Act (FFCRA) that contained up to two weeks of temporary emergency sick leave for employees in private firms with up to 500 employees (H.R.6201 - Families First Coronavirus Response Act, 2020). However, this emergency provision has now expired although voters across party affiliations continue to overwhelmingly support permanent paid sick days (National Partnership for Women and Families, 2020).

The canonical economic model of mandated job benefits predicts that employer mandates could be more efficient than direct government provision funded through taxation, if employees value the mandated benefit and accept lower wages from their employers in return (Summers, 1989). Gruber (1994), however, points out that anti-discrimination and minimum wage laws, as well as social norms, may prevent such wage reductions (and may lead to job losses instead). In line with Gruber (1994)'s wage argument, Pichler and Ziebarth (2020a) find no evidence that U.S. sick pay mandates significantly or systematically reduce wage growth. The authors also find no evidence for significant employment losses at the aggregated regional labor market level. Explanations for their findings could include sick pay plausibly reducing presenteeism ('working sick') and disease spread at the workplace, thereby offsetting increased labor costs (Pichler and Ziebarth, 2017). Another reason for the absence of wage and employment effects could be that the U.S. sick pay mandates are relatively mild government interventions (i.e., confer limited benefits to employees), particularly compared to paid family leave entitlements (Dahl et al., 2016).

Specifically, the sick leave mandates studied in this paper stipulate that employees have the right to earn one hour of paid sick leave per 30 to 40 hours worked for the employer. In other words, in a static perspective, this benefits equals 2.5 to 3.3% of employees' wage compensation. Moreover, such individualized sick leave accounts resemble medical savings accounts for health insurance, which are specifically designed to minimize moral hazard and employer financial burden (cf. Schreyögg, 2004). This paper studies the effects of mandating sick pay in the U.S. and thus builds on several important literatures: research on labor market inequalities (cf. Card et al., 2013; Maestas et al., 2018; Song et al., 2019) and parental leave[3] (Ruhm, 1998; Campbell et al., 2019; Bailey et al., 2019), disability insurance and workers' compensation (cf. von Wachter et al., 2011; Maestas et al., 2013; Dahl et al., 2014; Powell and Seabury, 2018; Dahl and Gielen, 2021) as well as the research on optimal social insurance more generally (Chetty and Finkelstein, 2013; Kolsrud et al., 2018) and employer mandates (Summers, 1989; Gruber, 1994).

Our findings suggest that state-level mandates are effective in increasing sick pay coverage rates among U.S. employees. Within the first two years following mandate adoption, the probability that an employee has access to paid sick leave increases by 18 percentage points from a base coverage rate of 66%. The increase in coverage persists for at least four years without rising further, although it should be noted that these medium-term effects are driven by few states. Over all post-mandate periods covered by this paper, we find a 13 percentage point increase in the coverage rate. As a result, employees take more sick days: on average, paid sick leave use increases by almost two hours per year. Scaling this two-hour increase by the 13 percentage points increase in coverage implies that newly covered employees take a little less than two additional sick days per year.[4] Employer sick leave costs also increase, but effect sizes are modest. On average, the increase amounts to 2.7 cents per hour worked, which translates to 21 cents per hour worked for a marginal employer. Further, we find little evidence that sick pay mandates crowd-out non-mandated bene-

[3] Paid sick leave differs from parental leave in both aim and scope (Lalive et al., 2014; Dahl et al., 2016; Baum and Ruhm, 2016; Brenøe et al., 2020). Whereas paid sick leave coverage is an insurance against wage losses due to (unpredictable) sickness, parental leave is typically mandated with the objective of balancing employees' family and work responsibilities and addressing gender inequality in the workplace.

[4] The relatively small increases in paid leave use provide suggestive evidence against wide-spread shirking on the part of employees and are in line with an incentive-compatible design of paid sick leave accounts.

fits such as paid vacation or holidays. Likewise, we find not much evidence that employers curtail the provision of group benefit policies such as health, dental, or disability insurance.

After empirically assessing mandate effects in a reduced-form perspective, we extend the standard Baily-Chetty framework of optimal social insurance and develop an optimal sick pay model (see Baily, 1978; Chetty, 2006; Chetty and Finkelstein, 2013). In our model, in contrast to Baily-Chetty, we study the optimality of *introducing* a new benefit, hence the extensive margin, not the intensive margin. Specifically, when employees gain access to paid sick leave, the social planner weighs the marginally higher consumption utility of employees against the higher employer costs of providing sick pay and also considers negative externalities due to the spread of contagious diseases when employees work sick (Adda, 2016; Pichler and Ziebarth, 2017; Pichler et al., 2020, 2021). Because work productivity decreases in the sickness level, a profit maximizing employer will provide some level of sick pay voluntarily, even in the absence of a social planner. Otherwise, employees will work sick but their reduced work productivity leaves them unprofitable for the employer. Sick pay incentivizes sick employees to call in sick. When we feed the empirically identified parameters into our derived optimality condition, we particularly consider effect heterogeneity by type of job and industry due to the large variation in pre-mandate coverage rates and take-up. This effect heterogeneity naturally carries over to the welfare implications and relevant trade-offs. For example, in the accommodation and food industry—both with low pre-mandate coverage rates—estimated elasticities are relatively low: 0.24. Also because of relatively low wages, this implies a lower welfare break-even threshold than in the construction industry or among large employers where elasticities and pre-mandate coverage rates are much higher.

2 U.S. Sick Pay Mandates

Paid sick leave was an integral part of the first social insurance system in the world. The Sickness Insurance Law of 1883 implemented federally mandated employer-provided health insurance in Germany, which covered up to 13 weeks of paid sick leave along with healthcare. Insurance against wage losses due to health shocks was a crucial element of health insurance at that time. Given the limited availability of expensive healthcare treatments in the 19th century, expenditures for paid sick leave initially accounted for more than half of all health insurance expenditures (Busse and Blümel, 2014). In subsequent years and decades, other European countries also implemented sick leave mandates. Today, although the generosity varies between countries, every European country provides universal access to paid sick leave to employees.

As noted earlier, the U.S. is one of three OECD countries without universal access to paid sick leave. As a result, in 2011, approximately half of U.S. employees did not have access to paid sick leave (Susser and Ziebarth, 2016). Since then, this share has decreased to less than 30% (Bureau of Labor Statistics, 2021). The only existing federal law related to leave is FMLA, which provides *unpaid* leave to employees in case of pregnancy, own sickness, or sickness of a family member to employees who work at least 1,250 hours annually for an employer with 50 or more employees (cf. Waldfogel, 1999). Given the exemptions to this law, Jorgensen and Appelbaum (2014) estimate that 44% of private sector employees are eligible for FMLA. Susser and Ziebarth (2016) also document that many low-wage and service sector employees are either not covered by FMLA or not aware of their rights to the federally mandated benefit.

Given this legislative landscape, although some exemptions exist especially for smaller employers, the state-level mandates analyzed in this paper provide previously not covered employees with the right to take paid and unpaid sick leave. For employees not previously covered by FMLA, this right also entails job protection. That is, although U.S. employment is overwhelmingly at will and employees can be terminated without reason or warning, employers cannot terminate employees for taking sick leave. In practice, we consider this job protection benefit to play a minor role in determining the extent to which state-level paid sick leave mandates impact labor markets.

Table A1 (Appendix) provides a detailed summary of all regular U.S. state-level mandates enacted at the time of writing.[5]. This paper evaluates all state-level mandates adopted between March 2009 and March 2017. While the details of the mandates differ from state to state, all existing mandates are employer mandates. Under these mandates, employees 'earn' a paid sick leave credit; typically one hour per 30 to 40 hours worked with a maximum of seven days per year. If unused, the sick leave credit rolls over to the next calendar year. Because employees must accrue the paid sick leave credit, most mandates explicitly state a 90 day accrual period in addition to waiting periods for new employees. However, several mandates that exempt small employers compel such employers to provide *unpaid* sick days (Massachusetts Attorney General's Office, 2016).

As seen in Table A1 (Appendix), qualifying reasons for sick leave are own sickness or sickness of a dependent child (and sometimes a family member)s)). Note that, in contrast to most European schemes, in the U.S., formal monitoring is deliberately light and employers cannot require doctors' notes, as such formal proof would impose a significant access barrier. Rather, following the

[5]Newly enacted COVID-19 emergency sick leave time in Arizona, California, and Colorado is not included as these policies are temporary for the duration of the pandemic and recovery period, see A Better Balance (2021a). The most recent mandate in New York State is not listed because it became effective only on September 30, 2020.

incentive-compatibility of medical savings accounts, moral hazard is mainly constrained through individual sick leave credit accounts in combination with relatively restrictive accrual rates (at least in an international comparison). Put differently, sick pay mandates require employees and employers to earmark one per 30 to 40 hours of work as sick leave credit, to be taken when needed and rolled over when unused.[6]

Employers are required to post employee right notifications related to minimum wages, harassment, and discrimination protection as well as paid sick leave at the workplace. Figure A1 depicts two examples of such notices. Figure A1a shows an earned sick time notice for Massachusetts that employers could post to comply with that state's workplace poster requirements (Commonwealth of Massachusetts, 2019). Alternatively, employers can post notices as in Figure A1b that include *all* employee right provisions that employers must comply with (Industrial Commission of Arizona, 2019).

Note that *none* of the mandates levies an explicit employer or employee tax to fund the sick days. Instead, these short-term sick leave provisions are funded entirely through work credit as described above. Our empirical analysis will assess their impact on labor costs and test for compensatory employer behavior, for instance, through reduced non-mandated fringe benefits.

Despite minor differences in the laws' specifics, as illustrated in Table A1, all mandates are individual sick time credit accounts. Moreover, all mandates have very similar structures, framed after the *Healthy Families Act*. While accrual rates and waiting periods differ slightly[7], we consider the design homogenous and thus do not differentiate by mandate generosity in our empirical analysis. Our main objective is to empirically assess the consequences of gaining sick leave coverage through state mandates. That is, we study the effects on the extensive, not the intensive, margin.[8]

Connecticut was the first U.S. state to mandate paid sick leave effective January 1, 2012. However, the mandate only applies to service sector employees who work for large employers and covers just 20% of the workforce. Over our study period, more states adopted paid sick leave mandates: California (July 1, 2015), Massachusetts (July 1, 2015), Oregon (January 1, 2016), and

[6]An open question for future research is employee sick leave behavior when changing jobs. The current state legislation does not specify any mandated pay out rate which induces incentives to take the remaining sick time prior to leaving the job.

[7]Ten of the 12 states in Table A1 require at least one hour of sick leave credit per 30 to 40 hours worked. Vermont is an exception with 52 hours and Washington DC requires 43 hours.

[8]Further, we leverage variation offered by five states. Our empirical framework does not carry sufficient statistical power to categorize mandates beyond the extensive margin (i.e., no mandate vs any mandate). We leave a more detailed analysis of mandate provisions for future work.

Vermont (January 1, 2017).[9] Additional states adopted mandates after the close of our study period: Arizona (July 1, 2017), Washington (January 1, 2018), Maryland (February 11, 2018), Rhode Island (July 1, 2018), New Jersey (October 29, 2018), and Michigan (March 29, 2019).

In terms of the political economy of the reforms, as we will also show empirically, they were not reactions to specific trends in our outcome measures. Since 2005, the *Healthy Families Act* has been (re)introduced to Congress several times but has not reached bipartisan support, largely because of other topics such as the Great Recession or the Affordable Care Act, and also because of the increased political polarization. However, interestingly, over many years, paid sick leave has had large bipartisan support among voters (National Paid Sick Days Study, 2008, 2010; HuffPost/YouGov, 2013). In essentially all the large states that adopted paid sick leave mandates over the past years—and that are the main source of our identifying variation—grassroots ballot initiatives were instrumental to the mandates' implementation, e.g., in California and Massachusetts.

As mentioned, in addition to states, dozens of cities passed sick pay mandates over the past years (see A Better Balance, 2021b for an overview). This paper focuses on the state-level mandates and disregards all sub-state mandates in our analyses. We chose to not evaluate sub-state paid sick leave mandates because the geographic information in our data, detailed below, does not map to the city-level mandate boundaries, plus the data suffer from small non-representative sample sizes at the sub-state level.[10] Specifically, whenever state and sub-state mandates coexist, legal complexities arise. When states pass mandates, existing sub-state laws are typically preempted, as with the 13 New Jersey city laws that existed prior to the state law (Title 34. Chapter 11D. (New) Sick Leave §§ 1-11). However, preemption is not always the case, especially not when city laws are passed *after* the state law and/or are more comprehensive. Because we focus on state-level mandates and because most state laws are very recent, we circumvent the legal complexities of this institutional city-state legal interplay. Moreover, in California, Connecticut, Massachusetts, Oregon, and Vermont (states that offer policy variation in this paper), no sub-state laws have been passed after the state mandates became effective.

[9] In our empirical analysis, we treat Washington DC —which enacted a mandate effective Nov 13, 2008, and expanded the scope on Feb 22, 2014—-as a state. We note that this locality is a 'treated control.' However, results are robust if we exclude this locality from the analysis sample, as shown in Appendix Table B2.

[10] In our main specification, we drop counties that adopted sick leave mandates or where cities that are part of the county adopted mandates. Note that all counties or cities adopted mandates prior to the states over our time period. Our findings are broadly robust to including fully or partially treated counties. These results are available upon request. We note that these results are somewhat less precise and attribute it to sub-state mandates having less bite, as has been documented in other contexts. One reason for the reduced precision could be pending lawsuits. Limited statistical power also prevents us from identifying effects for sub-state policies. As noted earlier, we treat Washington DC as a state and retain this locality in our empirical analysis.

One possible unintended consequence of paid sick leave mandates is that employers may increasingly discriminate against employees based on observable factors that employers believe are correlated with sick leave use. However, federal anti-discrimination law may limit such employer discrimination. Moreover, compared to other mandated benefits, e.g., Workers Compensation or health insurance, sick leave mandates are relatively minor mandates and substantially less costly, as we will see later. Thus, mandate-induced discrimination is likely negligible. To the extent that it exists, our data will not allow us to identify such effects, but one could view possible incremental changes in hiring and retention practices as part of the intent-to-treat (ITT) policy effect. Further, we would expect such (illegal) hiring practices in response to paid sick leave mandates to mute mandate effects, thus our coefficient estimates would reflect lower bounds on the true policy impact.

A final institutional point is worth mentioning. In several cases, paid sick leave mandates have been challenged through the court system, mostly by business groups seeking to have the laws overturned. For example, Airlines for America has sued the states of Massachusetts and Washington to seek an exemption from the law, arguing that the law would adversely affect their carrier prices, routes, and services (Bloomberg BNA - Workplace Law Report, 2018). As another example of pending legal questions, the Massachusetts Supreme Judicial Court ruled that sick pay does not constitute wages, which implies that employers are not liable if they do not pay out unused sick days to employees (Kaczmarek, 2018). In the empirical specifications, we do not differentiate by whether a lawsuit is pending anywhere at a given time for a specific jurisdiction.

3 National Compensation Survey (NCS)

We use the restricted access version of the NCS which is collected and maintained by the BLS. These data consist of a rotating panel of establishments, where establishments typically stay in the sample for three to five years. The data include detailed information on geographic location of establishments, which allows us to accurately match state-level mandates to the data.[11] Our data data covers all states.

The NCS is well-suited to our research as it produces official government statistics on a wide range of compensation and labor cost items. The data are also used to officially adjust wages for federal employees. Further, the NCS includes information on access to paid sick leave, paid and unpaid sick leave utilization, and sick leave costs to employers. Moreover, the data allow us to

[11] We use the restricted access version of the NCS, which is accessible in a BLS data research center located in Washington DC.

explore potential spillovers from sick leave mandates to non-mandated benefits that employers could reduce to offset costs; for instance, paid vacation or parental leave. There are no other sources of data within the U.S. that offer these features.

The NCS is nationally representative at the establishment-job level. In the NCS, random sampling is first carried out at the establishment level. The BLS defines establishments as 'a single economic unit that engages in one, or predominantly one, type of economic activity' (Bureau of Labor Statistics, 2020a). Second, within establishments, and depending on establishment size and number of different jobs within the establishment, the NCS collects information on compensation and benefits at the *establishment-job* level (Bureau of Labor Statistics, 2020a).[12]

The BLS selects establishments and jobs within establishment probabilistically. Within a selected establishment, four to eight jobs are probabilistically sampled from a list of employees provided by the establishment. The BLS field economist then selects employees from the overall employee roster; next, if there are other employees with the same job, all employees with the same job as the selected employee are sampled. Thus, in the NCS, a job is an employee or a group of employees within a sampled establishment with the same job. Please see NCS documentation for full details.[13] We refer to establishments as 'employers' or 'firms' in the manuscript.

The NCS is a quarterly survey. The human resource administrators of each firm provide detailed information to the BLS field economists on a range of job benefits, including paid sick leave. Because the information is based on employer-level administrative records, response error due to, for example, employees being unaware of their benefits is minimized. In our main analysis, we leave the microdata at the establishment-job level and restrict the sample to private sector firms.[14] Moreover, we focus on the March responses of the first quarter interview because the BLS only provides information from this interview for the offering of many benefits (including access to paid sick leave). Further, one can distinguish between stock and flow measures in the NCS. The stock measures (such as access to paid sick leave) refer to the status quo at the time of the first quarter interview in March. The flow measures (such as sick leave utilization) generally refer to *the past 12 months*; that is, from April of the previous year to March of the survey year. Finally, note that we only observe the total number of sick hours taken in the past 12 months, but do not see *when* specifically these hours were taken. Therefore, we are unable to see which weekdays are most popular for sick leave as, for instance, in Card and McCall (1996). Throughout our analysis,

[12]Note that within an establishment-job cell, there can be multiple employees. When there are multiple employees, the NCS reports the average value.

[13]Please see https://www.bls.gov/ncs/ for details (last accessed June 8, 2021).

[14]The mandates that we study only apply for the private sector.

we use the BLS survey weights to provide nationally representative estimates.[15] The NCS is a quarterly survey. The human resource administrators of each firm provide detailed information to the BLS field economists on a range of job benefits, including paid sick leave. Because the information is based on employer-level administrative records, response error due to, for example, employees being unaware of their benefits is minimized. In our main analysis, we leave the microdata at the establishment-job level and restrict the sample to private sector firms.[16] Moreover, we focus on the March responses of the first quarter interview because the BLS only provides information from this interview for the offering of many benefits (including access to paid sick leave). Further, one can distinguish between stock and flow measures in the NCS. The stock measures (such as access to paid sick leave) refer to the status quo at the time of the first quarter interview in March. The flow measures (such as sick leave utilization) generally refer to *the past 12 months*; that is, from April of the previous year to March of the survey year. Finally, note that we only observe the total number of sick hours taken in the past 12 months, but do not see *when* specifically these hours were taken. Therefore, we are unable to see which weekdays are most popular for sick leave as, for instance, in Card and McCall (1996). Throughout our analysis, we use the BLS survey weights to provide nationally representative estimates.[17]

[Insert Table 1 about here]

Table 1 reports the summary statistics. In our main sample, we have 399,586 observations at the establishment-job level for the years 2009 to 2017. Using the Consumer Price Index, we convert all monetary values to 2017 U.S. dollars.

3.1 Outcome Variables

The main objective of our study is to carefully assess how sick pay mandates affect employer propensities to offer mandated and non-mandated benefits, employee utilization of paid and unpaid sick leave, and employer costs related to sick leave. Our first outcome variable measures employees' access to paid sick leave through their employer as of March in a given calendar year.

[15]Note that the NCS is not designed to be representative at the state-level, rather the data are representative at the national level. To the best of our knowledge, no state-level representative dataset that allows us to measure the wide-range of margins salient to paid sick leave mandates exists. Hence, we view the benefits of the NCS to outway the costs of this dataset. Further, to the extent that our identification assumptions hold, non-representativeness at the state level is no threat to the internal validity of our estimates.

[16]The mandates that we study only apply for the private sector.

[17]Note that the NCS is not designed to be representative at the state-level, rather the data are representative at the national level. To the best of our knowledge, no state-level representative dataset that allows us to measure the wide-range of margins salient to paid sick leave mandates exists. Hence, we view the benefits of the NCS to outway the costs of this dataset. Further, to the extent that our identification assumptions hold, non-representativeness at the state level is no threat to the internal validity of our estimates.

Sick leave offered is coded one if a job provides paid sick leave and zero otherwise. Over all employers and years, the average coverage rate is 63% in our sample.

Our second outcome variable measures employees' use of paid sick leave. *Paid sick hours taken* indicates the average number of hours of paid sick leave taken by employees in this specific job in the previous 12 months. Again, note that we do not observe the specific weekdays or calendar months of use. The sample average is 15.8 hours, which corresponds to just under two full workdays of paid sick leave.

Our third main outcome variable measures employees' use of unpaid sick leave. *Unpaid sick hours taken* also refers to utilization over the past 12 months before the March interview. We include unpaid sick leave as this benefit may be a substitute for paid sick leave. The average annual number of unpaid sick days taken is 0.65 per employee in our sample.

3.2 Additional Variables

We also assess whether mandated sick pay leave increases labor costs and, as a consequences, crowds-out non-mandated benefits. To meet this objective, we examine how sick pay mandates affect labor costs per hour worked as well as a range of fringe benefits and other forms of non-wage compensation.

The BLS calculates *Sick leave costs per hour worked*, inclusive of fringe benefits, considering the total hours worked per employee on the job. As seen in Table 1, the sample average is 25.1 cents.[18] Regarding other fringe benefits provided, on average jobs offer 70 paid vacation hours and 44 national holiday hours per year.[19] Moreover, 69% of all jobs offer health insurance[20] and 57% offer life insurance.

The remaining rows in Table 1 list control variables, or variables that we use to stratify the sample to investigate effect heterogeneity. In particular, these are measures for full-time work, unionization, occupation, and industry. Approximately three quarters of the jobs in our sample are full-time jobs and just under 9% of jobs are unionized. The three most common occupations are

[18] Again, following the flow measure concept of sick leave utilization, this measure refers to the past 12 months before the first quarter interview. The BLS NCS survey administrators generate this variable and use the employee's own wage in the calculation. The variable assumes that sick hours represent 100% lost labor and does not consider changes in employee on-the-job productivity because of sick pay, or compensatory behavior by employees after returning to work. Moreover, our data do not allow us to calculate the potential employer costs of finding a replacement for employees on sick leave or other workplace disruptions when an employee is away from the job for short spells.

[19] For vacation and national holiday hours, the BLS assumes that all offered hours are fully used.

[20] To be precise, here we use what the BLS labels 'medical insurance.' This variable does not necessarily cover prescription medications.

'office and administrative,' 'sales,' and 'food preparation and serving.' The three most common industries are 'healthcare and social assistance,' 'retail and trade,' and 'manufacturing.'

4 Empirical Approach

4.1 Difference-in-Differences

First, we use the staggered implementation of the sick pay mandates in different states at different points in time to estimate traditional difference-in-differences (DD) models:

$$y_{f,j,t} = \gamma_{f,j} + \delta_t + \phi D_f \times T_{s,t} + \rho X_{f,j,t} + \mu_{f,j,t} \tag{1}$$

where $y_{f,j,t}$ is one of the outcome variables (e.g., *paid sick leave offered*) at firm f in job j and year t. $\gamma_{f,j}$ are firm-job fixed effects (which incorporate state fixed effects) and δ_t are year fixed effects from 2009 to 2017.

D_f is an firm-specific treatment indicator, which is coded one for firms to whom the sick pay mandates before 2017 apply based on mandate-specific size thresholds.[21] These firms are located within states that implemented a sick pay mandate between 2009 and 2017.[22] The interaction of D_f with the vector $T_{s,t}$, where s refers to the state specific treatment timing, yields the binary DD variable of interest. The interaction term is one for firms above the size threshold in states and time periods in which a paid sick leave mandate is in effect (see Table A1, Column (3)).

$X_{f,j,t}$ is a vector of control variables that we include in the saturated specifications, e.g., to control for full or part-time jobs. The standard errors ($\mu_{f,j,t}$) are clustered at the state-level (Bertrand et al., 2004).

Given the identification assumptions hold, Equation (1) estimates ϕ—the causal effect of mandated state sick pay on coverage, utilization, labor costs, and non-mandated benefits.

[21] Note that firms below the threshold are included in the comparison group. In a robustness check, results available on request, we exclude small firms from the comparison group and find slightly larger effects (although 95% confidence intervals generally overlap which makes us reluctant to overstate any heterogeneity).

[22] In particular, Washington DC is treated in all years of the study, and treatment 'turns on' in March 2012 in Connecticut; March 2016 for California, Massachusetts, and Oregon; and March 2017 for Vermont. Treatment status is absorbing, that is no states repeal their paid sick leave mandate. Moreover, as mentioned earlier, in the main specification, we exclude all counties and cities which passed county-level or city-level mandates. However, our findings are robust to including those treated counties in the sample. As mentioned in Section 2, one complication with the city-level mandates is that the city boundaries where the mandate applies rarely coincide with the county boundaries, which is why we elect to exclude the entire county from the analysis.

Moreover, in recent years, methodological advancements in DD models highlight that, with a staggered policy roll-out, the resulting coefficient estimate is a weighted average and may be a biased average treatment effect under effect heterogeneity across regions and over time (Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2020). In particular, bias is attributable to comparing later treated units to earlier treated units (i.e., 'forbidden' comparisons).

Several approaches to account for such heterogeneity and decompose treatment effects have been developed. We follow Goodman-Bacon (2021) and first decompose our overall coefficient estimate—that is, we examine the various two-by-two DD comparisons that contribute to our overall estimate—and then report a re-weighted estimate. Overall, we find that our results are not driven by bias due to the staggered roll-out of the mandates. Rather, our decomposition analysis reveals that our results are driven by contrasting treated states with not treated states ('clean' comparisons). Consequently, our re-weighted estimate is very similar to our the estimate generated in our main DD model. Given our setting, this result is perhaps not entirely surprising as we have a large 'true' control group—only five states adopted the policy by the end of our study period and thus 46 states form our comparison group.

4.2 Event Study

We also estimate and plot event study models. To this end, we decompose the binary $T_{s,t}$ time indicator in Equation (1) into a series of leads and lags around the effective date of each mandate (Schmidheiny and Siegloch, 2019). We use indicators for five or more years through one year in advance of the state-level mandates ('leads', $\sum_{i=-5}^{-2} Lead_{f,i}$), the effective year of the mandate, and one through five or more years following the mandate ('lags', $\sum_{k=0}^{5} Lag_{f,k}$).[23] Doing so, we center the data around the mandate passage date, with the March prior to passage as the reference year. We assign all localities without a mandate a zero for all lead and lag variables. Our event study equation is as follows:

$$y_{f,j,t} = \gamma_{f,j} + \delta_t + \kappa_j \sum_{i=-5}^{-2} Lead_{f,i} + \gamma_k \sum_{k=0}^{5} Lag_{f,k} + \rho X_{f,j,t} + \epsilon_{f,j,t} \quad (2)$$

[23]More specifically, the -5 indicator includes all years five or more years (in event-time) in advance of the effective date and the $+5$ indicator includes all years (in event-time) five or more years after the effective date.

The event study model offers two important extensions to the traditional DD model. First, visual examination of the normalized pre-mandate trends (that is, the coefficient estimates on the lead indicator variables) allows us to test for and assess the plausibility of the common time trends assumption necessary for DD models to recover estimates of causal effects. Second, inclusion of the lag variables allow treatment effects to vary over time in the post-mandate years. For example, if employers are slow to comply with the mandated benefits or if employees require time to learn about their new benefits, allowing for dynamic treatment effects and differentiating between short- and medium-term effects may be crucial. We note that that employees must earn and accrue sick time before they can claim this benefit (i.e., take paid sick leave), this feature of U.S. state paid sick leave mandates suggests that effects may emerge over time.

4.3 Identification

Overall, we evaluate the average impact of the mandates for California, Connecticut, Massachusetts, Oregon, and Vermont; that is, the five mandates adopted at the state-level between March 2009 and March 2017.[24] If mandates are a reaction to pre-existing trends in the outcome variables in the treated states, we would identify such an endogenous implementation via our event study (that is, coefficient estimates on the mandate lead variables that are statistically different from zero). Similarly, event studies have the power to provide evidence for anticipation effects.

The main remaining identification assumption is the absence of other confounding effects that are correlated with the staggered implementation of the sick pay mandates in all states over a decade. Specifically, the implementation of the mandates and the outcome variables must not be correlated with a systematic, third, unobservable driving force. Note that the mandates were implemented at different times of the year, in January as well July (Table A1), which adds to the credibility of the identifying assumption. Because we rely on variation over one decade and across five U.S. states, as compared to the canonical DD setting with just one treatment and one comparison group, other policies (or unobservables) contemporaneous to the treatments in all states inflicting a systematic bias are much less likely to occur.

If the identification assumptions hold, Equations (1) and (2) estimate internally valid causal mandate effects. The extent to which these estimates are externally valid for other U.S. states is difficult to assess. For such predictions, using estimates of regions whose labor markets are most similar to those in the state of interest is a promising approach. Our detailed heterogeneity

[24] Note that Washington D. adopted its mandate in the year prior to our study period (2008) and is therefore a 'treated control' in our analysis. Removing Washington DC from the sample does not appreciably change our main findings. Results are available on request.

analysis by industry, occupation, and both type of employee and employer will provide additional guidance.

5 Results

We begin this section by estimating Equation (2). That is, we estimate event studies to elicit intent-to-treat (ITT) effects of the state-level mandates on a range of outcomes. We then supplement these event studies with average post-reform estimates by estimating traditional DD models as presented in Equation (1). We also decompose our overall estimate into the two-by-two comparisons that comprise the DD estimate, and construct a re-weighted estimate that is robust to bias from heterogeneity across states adopting mandates at different points in time. Next, we assess effect heterogeneity by stratifying the mean effects by type of job, occupation, and industry. Finally, we provide evidence for possible compensatory behavior by employers by estimating the impact of the mandates on non-mandated benefits such as paid vacation days.

5.1 Impact of the Mandates on Coverage Rates and Take-Up

5.1.1 Event Studies

Figure 2 plots events studies as described in Equation (2) for our main outcome variables. The March prior to the mandate's effective date is our reference period. The x-axis of Figure 2 shows the normalized time dimension for all treatment states. The y-axis shows the treatment effect in natural units.

By examining the mandate leads, the event studies allow us to asses the credibility of our main identification assumption (i.e., common trends). As seen, differential trends between the treatment and comparison groups are largely absent; the pre-mandate coefficient estimates are small in magnitude and the gray confidence bands surrounding these estimates entirely cover the zero line on the y-axis. Notice that we show event studies without state-level time trends in the left column. To allow for underlying state-level differences in the adoption of non-mandated sick leave, we also show event studies with state-level time trends in the right column. We take the view that including state-level time trends is appropriate here because of these underlying trends. However, we note that there is a discussion in the literature of whether to include state-level time trends in DD models (Angrist and Pischke, 2009; Pischke, 2019) or not (Goodman-Bacon, 2021). As seen, while the point estimates for the post-mandate years slightly vary across specifications, specifically the results on the pre-mandate years are very robust to the inclusion of state-level time trends.

One common pattern in all event study figures is a large increase in confidence intervals two to three years post treatment ($\gamma = 2$). This occurs because most of the states enacted mandates in the summer of 2015, producing the first post-mandate NCS data point in March 2016. Our data currently end in March 2017.[25] Thus, we are unable to examine longer-term effects for most treated states, leading to an increase in the uncertainty surrounding our estimates.

[Insert Figure 2 about here]

Coverage Rates. Figure 2a documents a substantial increase in sick pay coverage rates in the year of the mandate's adoption; for example, in Oregon, where the law became effective January 1^{st}, 2016, $\gamma = 0$ refers to the survey as of March 2016. In the first post-mandate year, $\gamma = 1$, coverage rates further increase to roughly 18 percentage points and then remain at this level for the next four years, that is, through $\gamma_k \sum_{k=0}^{5} Lag_{s,k}$. This dynamic pattern of mandate effects is important. In particular, this pattern suggests large increases in coverage during the first two years post-mandate, but no further increases in the following years in Figure 2b or even evidence for, non-significant, decreases in Figure 2a. Put differently, the medium-term effects appear to equal the short-term effects.

As the pre-treatment average coverage rate is 66%, a reasonable question to ask is why coverage rates 'only' increase by roughly 18 percentage points to 84% in the first two post-mandate years? Note that our sample only includes private sector firms for whom the mandates *should* be binding. In the following, we offer some explanations for this finding.

First, similar to non-compliance in case of minimum wage laws (Basu et al., 2010) or workplace safety regulations (Johnson, 2020), deliberate non-compliance could limit benefit provision. Second, as discussed in Section 2, in several states lawsuits are pending and thus these mandate may not be strictly enforced by authorities. This unclear legal situation benefits non-compliant employers or those who are willfully ignorant. Third, despite our best efforts, our classification of firms and mandates may include unavoidable imprecision due to the nature of the mandates and our ability to accurately map mandates to data. As an example, in Connecticut, the mandate provides relief to firms that experience seasonal or transitional fluctuations in their workforce. This exemption may lead us to mis-classify firms. Finally, note that our study period extends to 2017 with only a few post-reform years for most laws; thus, coverage rates may further increase over

[25] COVID-19 and the associated shut-down of the BLS data research center prevented us from updating the data.

time when more years become available. For example, in California, Massachusetts, and Oregon our data include just two post-reform years.

Despite these reasons for why we may not find an increase to full labor market coverage, we consider it precisely one contribution of this research to inform policymakers and researchers about the *de facto* increase in coverage as a result of government mandates. Recall that, for this analysis, we use the best available government-provided firm-level data specifically designed to measure employee compensation.

Take-up. Figure 2c to f show the dynamic effects on actual take-up of paid and upaid sick leave. After the mandates' implementation, paid sick leave utilization increases from year one in Figures 2c and d. As for the coverage effects, we observe further increases in sick leave utilization in the second post-mandate year, and then a minor downward trend in the model without state time trends in Figures 2c. However, our statistical power also decreases due to fewer states being observed for more than two post-mandate years (see Table A1). The general increase in paid sick leave utilization over time is plausible as employees earn and accumulate sick leave credit over time.

Figure 2e (without state time trends) and Figure 2f (with state time trends) show the event studies for unpaid sick leave utilization. Again, we observe nonlinear dynamic effects that are in line with our priors. First, in both figures, we do not observe substantial evidence for pre-mandate trends. The pre-mandate coefficient estimates are close to zero in size and the 95% confidence intervals generally overlap with the zero line. After employees in small firms gained the right to take unpaid sick days because of the mandates,[26] we observe increases in use for the first two post-mandate years. After that, the likelihood to take unpaid sick hours starts to decline again and revert back to the zero line in the fourth post-mandate year. This nonlinear effect is plausibly a function of how the sick pay mandates are designed—employees must first earn paid sick leave credit by working for an employer and thus there is an built-in time delay. Hence, initially employees primarily take unpaid sick hours. Once they accrue sufficient paid sick hours over time, employees increasingly take *paid* sick time, and unpaid sick leave use decreases again. This nonlinear pattern suggests that the medium-term effect of the mandates on unpaid sick leave utilization may not be different from zero.

[26] Employees in firms with more than 50 employees have been covered by FMLA.

Labor Costs. Finally, Figure B2a in the Appendix shows the event study for sick leave costs per hour worked. We do not observe substantial trending in pre-treatment years but some evidence for possible (small) anticipation effects in t-1. In any case, once employees begin to take paid sick time, we observe increases in labor costs. This pattern is again in line with our priors as sick leave costs are simply the product of paid sick hours taken and the employee's hourly wage.

In all event studies, the lead and lag coefficient estimates are identified off different states based on when those states adopted a paid sick leave mandate. Note that, due to changes in the panel of firms that are in the NCS in a given year and due to employee turnover, the sample composition changes over time, which could also contribute to changes in the effect sizes over time.[27] However, our models include firm-job fixed effects to net out unobservables at the firm-job level and control for spurious or by chance compositional changes in the included firms. Moreover, below we decompose the treatment effects to assess treatment effect heterogeneity by early vs. late mandates in our sample.

5.1.2 Traditional DD Regression Models

Table 2 reports the results from Equation (1) for our main outcome variables. Each panel shows a separate DD model that controls for an increasingly larger set of covariates. Panel A includes year and employer fixed effects, whereas Panel B adds employee controls, Panel C adds firm-job fixed effects, and Panel D adds state-specific linear time trends. Overall, our results are robust across the various specifications. The stability across specifications with different sets of covariates offers a test of conditional balance. Moreover, results are in line with the event study estimates above.

[Insert Table 2 about here]

The four DD models in Column (1) of Table 2 show that, on average, state-level sick pay mandates increase coverage rates by 13 percentage points over all post-mandate years. Relative to the pre-mandate coverage rate in treated states of 66%, the effects translate into an increase of 20%. Across all three specifications, the coefficient estimates are statistically significant at the 5% significance level.[28]

Columns (2) and (3) of Table 2 show the estimated effects on paid and unpaid sick leave hours taken in the last 12 months (recall that we use the March responses of the NCS, so this period refers to April of the year before until March of the survey year). As seen in Column (2), there is

[27] We do not have information on employee tenure in the NCS.

[28] The BLS imputed values for sick pay coverage for roughly 30,000 observations. After excluding imputed observations the treatment effect on coverage increases by roughly two percentage points. Results are available upon request.

robust evidence that, on average, paid sick leave use increases by approximately two hours per year, which corresponds to an 11% increase relative to the baseline. Scaling this average effect by the 13 percentage point increase in coverage rates (Column (1)) yields 15.4 hours or approximately two additional paid sick days taken per year.

Equivalently, the number of unpaid sick hours taken almost doubles to 0.9 (Column (3)), which yields a scaled effect of 3.5 hours or roughly half of an eight-hour workday. Recall that many employees also gain the right to take unpaid leave as a result of the mandates. Jorgensen and Appelbaum (2014) report that in 2012, almost half the U.S. workforce is not eligible for FMLA (also see Section 2). Thus, the increase in unpaid sick hours taken is expected.

Column (4) of Table 2 shows the estimated effects on associated employer labor costs per hour worked. Labor costs are important to assess in this context because mandate critics commonly cite rising labor costs and depressed labor demand as reasons against government mandated sick pay (Kruth, 2018). However, using the Quarterly Census of Employment and Wages, Pichler and Ziebarth (2020a) neither find evidence that *aggregated county-level wages and employment* decreased significantly, nor find they any evidence for systematic changes. Column (4) provides a possible explanation for this null finding. In the NCS, we find that mandates increase sick leave costs per hour worked by 2.7 cents (Column (4), Panel D). Scaling this hourly cost increase by the 13 percentage point increase in coverage rates, costs increase by 21 cents per hour for the marginal employer.

We note that this sick leave cost estimate is a static calculation. In particular, the calculation does not consider possible changes in work productivity attributable to the mandate. For instance, overall work productivity could increase because employees can recover from their sickness, work moral among employees could increase or employees may (over-) compensate for lost labor after their sick leave. On the other hand, shirking and a lower work morale among employees who are not on sick leave (and therefore must cover for their sick coworkers) could reduce productivity.

However, the labor cost estimate implicitly considers potentially lower flu infection rates at the workplace and thus a reduced need for sick leave (cf. Pichler and Ziebarth, 2017; Stearns and White, 2018; Pichler et al., 2020). If total sick hours taken decrease in some firms or occupations as a result of less presenteeism behavior and fewer infections, our labor cost estimate implicitly considers such an effect. Below, we estimate effects on infections and also consider such externalities in our model of optimal sick pay.

Decomposing Treatment Effects. Finally, we use insight from Goodman-Bacon (2021) in order to learn more about the underlying variation of our main results. In his contribution, Goodman-Bacon (2021) decomposes DD models in which multiple states adopt polices at different times into a weighted average of all possible two-by-two DD comparisons.

[Insert Table 3 about here]

In order to perform the Goodman-Bacon decomposition using our data, we aggregate them to the state level and dichotomize our treatment variable so that it is binary (at the state level).[29] Then, we re-estimate our main models and decompose the treatment effect following Goodman-Bacon et al. (2019).

The results are in Table 3. We find that the estimated treatment effect is comparable but slightly smaller than the results in Panel B of Table B6 in the Appendix, where we also aggregate at the state level. With regard to the decomposition, the two-by-two DD estimate compares workers at employers in states that mandated benefits between 2009 and 2017 with workers that were never treated ('Never_vs_timing'). This element, which captures 'clean' comparisons, receives more than 96% of the total weight. The resulting treatment effect estimate is thus very similar to the overall treatment effect. Furthermore, comparing workers in states that mandated sick pay earlier as compared to later ('Timing_groups') yields a slightly larger treatment effect—however, this element receives only 2% of the weight. Moreover, the table displays some irregularities in terms of effect size when comparing Washington D.C. to the never treated (Always_vs_never). Therefore in a robustness check we drop Washington D.C. in Table B2. The results after dropping D.C. are very similar to our original results (Table 2) and thus confirm that the observed irregularities are not driving our main effects.

Further, while our sample is representative for the U.S., the representativeness for smaller states might be limited. To see whether smaller states drive our results, we solely keep California and all untreated states in another robustness check (Table B3). As seen, although the point estimates increase slightly (for coverage and take-up) when focusing on California, they are not appreciably different from our main results in Table 2.

Finally, the within variation has a very low weight of less than 1% implying that our results are not driven by whether we include controls or not. Overall, we interpret this decomposition

[29] As some state mandates have firm size exemptions, aggregating to the state level leads to a non-binary treatment (for more details on the laws see Table A1). In order to decompose our treatment effect, a binary treatment variable in required.

analysis to suggest that our results are not driven by bias attributable to staggered roll-out of policies across units and time.

5.1.3 Heterogeneity in Mandate Effects

Next, we explore effect heterogeneity by type of job and employer. Mirroring the large inequalities across employers and employees in the pre-mandate era, one would hypothesize that heterogeneity in treatment effects should be large as well. In other words, we expect the mandates to have more bite in part-time and low-wage employers where coverage was particularly low in pre-mandate years.

To this end, we re-estimate an augmented version of Equation (1) using triple difference models. Specifically, we add a triple interaction term $D_s \times T_t \times covariate$ to Equation (1) along with the associated two-way interactions, $T_t \times covariate$ and $D_s \times covariate$. For readability, we report only the triple interaction terms; coefficient estimates for all other terms are available upon request.

[Insert Table 4 about here]

Table 4 reports results from heterogeneity analyses. In particular, we test whether the treatment effects differ by full-time vs. part-time jobs (Panel A), union vs. non-union jobs (Panel B), and large (Panel C) vs. small (Panel D) employers. Focusing on the triple interaction term in Column (1), as hypothesized, the increase in coverage is larger in part-time (vs. full-time) jobs, non-unionized (vs. unionized) jobs, and small (vs. large) employers.

The increase in coverage rates for full-time, unionized and large firm jobs is significantly larger, which is a function of the lower baseline pre-mandate coverage levels. The findings for use of paid and unpaid sick leave largely follow the pattern of the coverage rates, although there are some notable exceptions. For example, not surprisingly, employees working for employers with fewer than 50 employees experience a larger increase in utilization as a result of the mandates (Columns [2] and [3], Panel D). However, for full- vs. part-time employees, we do not find statistically significant differences. We hypothesize that the larger coverage increase for part-time employees is counteracted by fewer opportunities of these employees to take sick days due to, among other factors, fewer work hours. More specifically, we view the similarity across these two types of employees as a mechanical artifact of the these jobs: part-time employees work fewer hours, accumulate fewer hours of paid sick leave, and therefore can take fewer hours of paid time off than full-time employees.

A similar countervailing force likely operates for the labor cost changes in Column (4): because wages in small firms and non-unionized jobs are lower, we find no statistically different labor cost effects between large and small employers as well as unionized and non-unionized jobs—although the former job-types experience much larger coverage rate increases. An alternative explanation is that employees working for small employers and in non-unionized jobs are less likely to be aware of their rights (Hall et al., 2018), or are less likely to take sick days out of concern that it may trigger negative job consequences (Shapiro and Stiglitz, 1974; Ziebarth and Karlsson, 2014).

[Insert Figure 3 about here]

Heterogeneity by industry and occupation. Figure 3 graphically illustrates effect heterogeneity for coverage effects by industry and occupation. The dark dots report the baseline coverage rates, whereas the lighter diamonds show the post-mandate coverage rates (i.e., the summation of the baseline coverage rate and the estimated treatment effect). We find substantial inequality in baseline coverage rates across jobs and also substantial reform effect heterogeneity. For example, pre-mandate coverage rates are particularly low in the accommodation and food services industry (27%, Table B1) as well as in the construction industry (42%, Table B1). These industries also experience the largest increase in coverage rates through the mandates and show post-mandate coverage rates of 45% (accommodation and food) and 62% (construction) respectively.

[Insert Table 5 about here]

All exact effect heterogeneity estimates for all industries and all occupations as well as the estimates for the other outcome measures are in Table 5. The results largely follow the pattern just discussed, see also Table 4 and Figure 3. In conclusion, the state-level sick pay mandates significantly decrease the widespread inequality in sick leave access and take-up in the U.S. labor market.

5.2 Aggregating at Firm, County, and State-Level and Further Robustness

Now we aggregate our data (a) at the firm-level (Table B4, Appendix), (b) at the county-level (Table B5, Appendix) as well as (c) at the state-level (Table B6, Appendix). Note that these aggregations create some imprecision as the sample of firms is changing over time. Therefore, when we find changes over time, it might be due to real treatment effects (changes within a firm over time) or changes in the firm composition. However, our results show that the estimated treatment effects are fairly robust to these different types of aggregation. Hence, we are confident that changing firm composition plays a limited role. Moreover, aggregating to a higher geographic unit allows us to implicitly test for partial or general equilibrium effects that could either enhance or mute the mandate effects at the firm-job-level. If the results across the micro and macro data are equal, such effects are unlikely to play a major role. As observed for all outcomes and model specifications in both tables, the results with aggregate macro data are very similar to our results based on micro data.

Further, in Tables B7 to B9, we conduction additional falsification tests. For example, while we code firms below mandate thresholds as not treated in states that exempt small firm, Table B7 excludes these observations from the sample. Tables B8 and B9, by contrast, replicates the effect heterogeneity tables without including state time trends.

Placebo Estimates. Figure 4 shows placebo estimates based on randomization inference. The estimates correspond to the models in Panels C and D of Table 2. The left column shows estimates without state trends and the right column shows estimates with state trends. (Figure B1 in the Appendix show the graphs for additional outcomes.)

We attempt to mimic the variation in the actual data as closely as possible in our randomization procedure. In particular, in each iteration of the placebo test, we first exclude the six states (including DC) that have adopted a mandate by the end of our study period. Second, we randomly choose five pseudo-treatment states assigning the effective dates of the true mandates (this step holds the pattern of staggered policy adoption constant). Third, we estimate the model in Equation (1). We repeat these three steps 200 times. We then plot the resulting placebo estimates and compare them with our main coefficient estimate in Figure 4.

The dashed lines represent the 5^{th} and 95^{th} percentile of the distribution of treatment effects. The 'true' treatment effect is shown as black line for comparison. As seen, the true treatment effects are always clear outliers outside the 95% confidence interval, providing further evidence that they are not driven by confounding trends.

[Insert Figure 4 about here]

5.3 Impact on Non-Mandated Benefits, Hours Worked, and Type of Sick Plan

Tables 6 and 7 report DD estimates for different components of employee compensation. These benefits are plausibly valuable to employees, but costly to employers and not mandated. Hence they could be curtailed to offset increased sick leave costs due to the mandates. In these auxiliary analyses, we thus test for unintended compensatory and spillover effects of mandating paid sick leave.

5.3.1 Crowding-Out of Non-Mandated Benefits

Columns (1) to (7) of Table 6 test for substitution or crowding-out effects. In particular, the coefficient estimates capture the effect of the mandates on the provision of (1) health insurance, (2) prescription drug insurance, (3) dental insurance, (4) life insurance, (5) short-term disability insurance, (6) long-term disability insurance, and (7) parental leave benefits. Broadly, we observe not much evidence that sick leave mandates significantly affect any of these outcomes. Rather, the coefficient estimates are relatively small in size and the estimates carry alternating signs. An exception to this pattern is health insurance: benefit provision may decline post-mandate, although the coefficients are quite small in size. Importantly, however, the event study shows not much evidence for systematic decreases health insurance provision due to the sick pay mandates (Figures B2b, Appendix).

Columns (1) to (3) of Table 7 test whether mandates affects annual vacation hours, national holiday hours, and overtime hours. Again, all coefficient estimates are small in size relative to the baseline mean. Moreover, none of the eight coefficient estimates in the vacation and overtime hours regressions are statistically different from zero. Only the coefficient estimates for annual national holiday hours are negative and statistically significant, although the estimate in Panel D, Column (2) is only 1.4% of the mean. Moreover, as with health insurance, the event study in Figure B2c (Appendix) shows no evidence of a systematic crowd-out of holiday hours provided by employers.

[Insert Table 6 and 7 about here]

5.3.2 Annual Hours Worked and Paid

Columns (4) to (6) of Table 7 test for mandate-induced changes in hours worked per year (4), hours of overall paid leave per year (5), and hours paid per year (6). Hours of paid leave per year includes all forms of paid leave such as paid sick days, parental leave, elder-care, paid vacation, and paid national holidays.

First, we find no statistically significant evidence that sick pay mandates impact hours worked. The coefficient estimates in Column (4) have alternating signs and are small, relative to the mean. For example, in Panel C, the estimate is positive and equals 0.02% of the mean but is not statistically distinguishable from zero. Likewise, Column (5) provides little evidence that the annual number of hours paid change substantially in post-mandate years. However, the coefficient estimates for the annual number of hours on paid leave are marginally significant and 0.9% of the mean (Column (6)). These positive increases are in line with, and likely the result of, the increased use of sick leave.

5.3.3 Wages and other benefits

Columns (7) to (9) of Table 7 test for changes in wages (7), employer costs for health insurance (8), and non-production costs (9). We find no evidence that paid sick leave mandates reduce wages, indeed our results suggest instead that wages may *increase* following mandate adoption. The coefficient estimates imply wage increases of 0.8% to 1.4%. These increases are not attributable to concurrent policy shifts as, to the best of our knowledge, no policy followed the same staggered roll-out as the paid sick leave mandates we study. Note that we control for the effective minimum wage in this regression (results are very similar, and available on request, if we do not control for minimum wages), thus unmeasured changes in wage policies are not likely to lead to this finding. While the coefficient estimates in the health insurance and non-production costs are generally imprecise (one estimate is marginally statistically significant in our most parsimonious specification), one could interpret them as suggestive evidence for a decrease post-mandate, in line with the hypothesis that employers curtail these benefits. Examination of the largest implied 95% confidence intervals shows that we cannot rule out decreases as large as 4.3% and 1.8% in employer health insurance and non-production costs respectively.

However, again and as above, the event study (Figures B2d, Appendix) does not show much support for systematic wage increases nor health insurance or non-production cost decreases. Similar null wage results are also found in Pichler and Ziebarth (2020a).

5.3.4 Type of Sick Leave Plan

Finally, we investigate whether sick pay mandates alter the type of plan offered to employees. Columns (8) and (9) of Table 6 test for whether the mandates affect the propensity that employers offer 'fixed' sick leave plans (Column (8)) or 'consolidated' sick leave plans (Column (9)).

Table 1 shows that 16% of all jobs come with the benefit of a consolidated leave plan. These are also called consolidated 'Paid-Time-Off' (PTO) plans and have become increasingly popular in the U.S. Under a PTO plan, employers do not provide a *separate* number of days for sick leave, vacation, or parental leave, but instead aggregate or *consolidate* the total number of paid leave days per year, independent of reason for paid time away from work (Lindemann and Miller, 2012). For instance, the BLS reports that the average consolidated PTO plan has accumulated 19 days of available paid leave after five years of service with the employer (Bureau of Labor Statistics, 2018). Paid sick leave mandates are in compliance with such PTO plans as long as they are as least as generous as the sick leave accounts required by the law (ADP, 2016).

However, as a result of paid sick leave mandates, Column (8) shows an increase in the share of jobs with *separate* sick leave plans. The increase is 14 percentage points and nearly identical to the main coverage increase in Column (1) of Table 2. The likelihood that employers offer a PTO plan either decreases slightly by 1.7 percentage points (Column (9), Panels A and B) or does not appreciably change (Column (9), Panel D). In conclusion, Columns (8) and (9) imply that sick pay mandates overwhelmingly induce employers to set up separate paid sick leave plans, as intended, likely to avoid uncertainty whether their consolidated PTO plan would comply with the law (Miller, 2015).

5.3.5 Flu Externalities

Finally, in Table B10 we estimate the flu externalities. Our dependent variable here is the weekly (!) flu rate, which has a mean of one flu diagnose for every 50 outpatient doctor visits. After the sick leave mandates become effective this rate decreases by roughly 30%. This result is in line with Pichler et al. (2021), where several robustness checks are provided. We note that the impact of paid sick leave mandates on disease spread will vary across population density, features of the local labor market (e.g., how closely employees work together), vaccine uptake, and so forth. We suspect that many of these variables are slow-moving and thus captured by our state fixed effects and/or linear trends.

6 Optimal Sick Pay and Welfare Effects

This section develops a model of optimal sick pay to assess the welfare effects of mandating sick pay. Our intention is *not* to explain *why* coverage rates are highly unequal across types of jobs nor *why* private insurance markets for short-term sick leave policies are very limited in the U.S. (cf. Hendren, 2013, 2017, for such analyses for health insurance), despite clear evidence that employees highly value sick pay (Maestas et al., 2018; National Partnership for Women and Families, 2020). Rather, as in the Baily-Chetty framework following Baily (1978) and Chetty (2006), we will use the model to derive sufficient statistics. Unlike Baily-Chetty, however, we do not assess optimal unemployment benefits but instead optimal paid sick leave. In addition, we will not focus on the optimal *level* of paid sick leave but rather, following our empirical estimates, on providing *access* to sick pay—the extensive margin. This setup allows us to use the empirical estimates from Section 5 to assess possible welfare implications (cf. Chetty, 2008). Kleven (2020) provides a detailed discussion of the implicit assumptions of the sufficient statistics approach.

Our goal is to assess under what conditions mandating access to paid sick leave could be welfare improving. Because simplifications and assumptions are a necessary part of such an exercise, we explicitly abstain from strong conclusions and policy recommendations. Rather, our objective is to simplify complex real-world interactions to highlight and focus on what we believe are the most relevant trade-offs to consider. For example, because of the current COVID-19 crisis, the impact of paid sick leave on contagious presenteeism behavior and the spread of infectious diseases has received substantial attention. We agree that this public health aspect is important—authors of this paper have studied this phenomenon (Pichler and Ziebarth, 2017; Pichler et al., 2020; Andersen et al., 2020; Pichler et al., 2021). To keep this paper and model tractable, we consider these externalities by adding a welfare penalty of presenteeism that accounts for the possibility of disease transmissions.

6.1 Model Setup

Our model is a one period model. Since we cannot measure productivity at the individual or firm level, we refrain from a multi-period model where sick leave today could affect productivity in future periods. Instead, our model includes both employee utility and employer profits. The social planner considers employee utility and employer profits and also population health externalities due to the spread of contagious diseases.

To consider effect heterogeneity and to address possible concerns that using a representative employee would be oversimplifying, we will allow for a wide range of effect heterogeneity estimates by type of job and industry. We will discuss how the relevant trade-offs and welfare implications differ across these subpopulations. Explicitly allowing for employee heterogeneity *in the model* would add more layers of unnecessary complexity, in our opinion.

6.1.1 Employees

Assume a population of employees equal to one. Employees maximize their utility u, which is a function of their sickness level σ, their consumption c, and their leisure time τ. Hence their utility function is $u(\sigma, c, \tau)$.

The sickness level σ is continuous and bounded between zero and one. σ is zero when the employee is perfectly healthy. σ is positive when the employee is sick, which occurs with probability p. Sickness has density $f(\sigma)$ and a cumulative distribution $F(\sigma)$.

There are two types of employees. A share of employees α with access to sick pay (L-type employees) and a share of employees $(1-\alpha)$ without access to sick pay (0-type employees). Employees consume their income from work, which is (i) w when they work, (ii) $w-l$ when they are on paid sick leave (only relevant for L-type employees with benefits), and (iii) 0 when they are on unpaid sick leave.[30] Notice that while employees on paid sick leave obtain wage w, we subtract a penalty term l to consider that sick leave is limited to roughly seven days per year in the U.S. and that employees on sick leave are using up their sick days.[31]

With h representing contracted work hours and T total time, leisure time equals $\tau = T - h$ when employees work and $\tau = T$ when they are on sick leave. Moreover, utility decreases in sickness, but increases in consumption and leisure over the whole domain. Given these model parameters, we define the utility differential between work and paid sick leave for L-type employees with benefits as $\triangle_L = u(\sigma, w, T-h) - u(\sigma, w-l, T)$. If $\triangle_L$ is positive, employees will work; otherwise, they will call in sick and take sick leave. Setting $\triangle_L = 0$ gives a unique indifference level of sickness σ_L^* for employees with paid sick leave. Similarly employees without paid sick leave have a utility differential of $\triangle_0 = u(\sigma, w, T-h) - u(\sigma, 0, T)$ and a corresponding indifference

[30] Employees might consume some assets or savings in the latter case. To keep the model tractable, we do not model consumption floors explicitly.

[31] For newly covered employees who have accumulated sufficient sick pay credit, mandates imply an increase in sick pay from zero to $w-l$. For newly covered employees who cannot cover their sick leave needs with the available credit, mandates imply an increase in paid leave from zero to $w-l$ for the sick leave hours they have accumulated, and zero for the remaining hours. In the pandemic, despite federal emergency sick leave, unmet sick leave needs have tripled to 10% of the workforce in a given month (Jelliffe et al., 2021).

level σ_0^*. Finally, note that $\sigma_L^* < \sigma_0^*$, or in words: L-type employees will call in sick with milder sickness because of sick pay ('moral hazard').

Summing up, at the population-level, total employee utility is:

$$
\begin{aligned}
U =& (1-p)u(0,w,T-h) \\
&+ p\alpha \int_0^{\sigma_L^*} f(\sigma)u(\sigma,w,T-h)d\sigma + p(1-\alpha)\int_0^{\sigma_0^*} f(\sigma)u(\sigma,w,T-h)d\sigma \\
&+ p\alpha \int_{\sigma_L^*}^1 f(\sigma)u(\sigma,w-l,T)d\sigma + p(1-\alpha)\int_{\sigma_0^*}^1 f(\sigma)u(\sigma,0,T)d\sigma.
\end{aligned} \tag{3}
$$

The first term represents utility for healthy employees who work with $\sigma = 0$. The second and third terms represent utility for sick employees who work ('presenteeism') with and without sick pay, respectively; and the last two terms represent utility for sick employees on paid and unpaid sick leave, respectively.

Next, Equation (4) shows how a change in the share of employees covered by sick pay α affects total employee utility:

$$
\begin{aligned}
\frac{\partial U}{\partial \alpha} =& -p\int_{\sigma_L^*}^{\sigma_0^*} f(\sigma)u(\sigma,w,T-h)d\sigma \\
&+ p\int_{\sigma_L^*}^1 f(\sigma)u(\sigma,w-l,T)d\sigma \\
&- p\int_{\sigma_0^*}^1 f(\sigma)u(\sigma,0,T)d\sigma > 0.
\end{aligned} \tag{4}
$$

When increasing sick pay coverage, employees will call in sick with milder sickness ('moral hazard'), as shown by the first term. The second term stands for an increase in sick employees on paid sick leave, and the third term stands for a decrease in employees on unpaid sick leave (as they now have access to paid sick leave).[32]

[32] The share of employees on paid sick leave (2nd term: $p\int_{\sigma_L^*}^1 f(\sigma)d\sigma$) equals the overall share of employees who are not working (1st term: $p\int_{\sigma_L^*}^{\sigma_0^*} f(\sigma)d\sigma$) minus the share of employees on unpaid sick leave (3rd term: $p\int_{\sigma_0^*}^1 f(\sigma)d\sigma$). As worker utility while on paid sick leave $u(\sigma,w-l,T)$ is higher than employee utility when working sick ($u(\sigma,w,T-h)$) or being on unpaid sick leave $u(\sigma,0,T)$, the sum of the three terms is positive. Finally notice that both employee utility and profits (and total welfare equal to their sum) below are linear in the share of employees with paid sick leave benefits α. From a theoretical perspective only corner solutions are relevant, and thus either all employees should be covered by paid sick leave ($\alpha = 1$) if utility (or total welfare) is increasing in coverage, or no employees should be covered if utility is decreasing in coverage.

Finally, we denote with A the share of employees who are absent from work, which is equal to the sum of employees who are absent and who have sick leave coverage (A_L) as well as those who are absent and do not (A_0):

$$A = A_L + A_0 = p\alpha \int_{\sigma_L^*}^{1} f(\sigma) d\sigma + p(1-\alpha) \int_{\sigma_0^*}^{1} f(\sigma) d\sigma. \tag{5}$$

As the share of employees with sick pay coverage increases, the share of employees on sick leave will increase as follows ('moral hazard'):

$$\frac{\partial A}{\partial \alpha} = p \int_{\sigma_L^*}^{\sigma_0^*} f(\sigma) d\sigma. \tag{6}$$

We denote these employees as *marginal compliers*, as they are call in sick when covered by paid sick leave benefits, but they will work if leave is unpaid.

6.1.2 The Representative Firm

Assume a representative firm, that cannot observe employee sickness σ.[33] Moreover, we assume that employees with sickness level σ have work productivity $\pi(\sigma)$ with $\pi'(\sigma) \leq 0$, which is also partially unobservable.[34] In other words, sickness makes employees less productive. Given σ_L^* and σ_0^*, the profit function of the employer (Π) is:

$$\begin{aligned} \Pi = &(1-p)(\pi(0) - w) \\ &+ p\alpha \int_0^{\sigma_L^*} f(\sigma) \Big(\pi(\sigma) - w\Big) d\sigma + p(1-\alpha) \int_0^{\sigma_0^*} f(\sigma) \Big(\pi(\sigma) - w\Big) d\sigma \\ &- p\alpha \int_{\sigma_L^*}^{1} f(\sigma) w d\sigma. \end{aligned} \tag{7}$$

The first term represents profits generated by healthy employees who work. The second term and third term represent profits generated by sick employees who work (with and without sick pay coverages, respectively). Because of their sickness, sick employees have lower productivity

[33] We note that, in reality, sickness is partially observable at best. First, sickness may not result in physical and observable symptoms. Second, over-the-counter medications that suppress sickness symptoms, e.g., coughing and nasal congestion, are widely available (Earn et al., 2014).

[34] Below we will analyze how welfare is affected by different levels of work productivity.

than healthy employees, but still earn wage w. The last term represents profits—or rather losses—produced by employees who are on sick leave, $p \int_{\sigma_L^*}^{1} f(\sigma) d\sigma$, and who receive sick pay, w, while not participating in production.

We do not explicitly model the labor market. Instead, following Chetty (2006), approximating reality and the emerging evidence on U.S. labor market monopsony (Dube et al., 2020; Hershbein et al., 2020), we do not allow wages w to flexibly adjust in the short-run. This assumption is justifiable as minimum wage and anti-discrimination laws as well as workplace norms prevent flexible wage adjustments in the short-run (cf. Gruber, 1994). Otherwise, the employer's optimization problem would be trivial: the employer would simply pay employees according to their daily productivity. In our model, the employer can only optimize over the share of jobs or employees who are covered by sick pay α. Equation (8) shows how a change in α affects employer profits:

$$\frac{\partial \Pi}{\partial \alpha} = -p \int_{\sigma_L^*}^{\sigma_0^*} f(\sigma) \pi(\sigma) d\sigma - p \int_{\sigma_0^*}^{1} f(\sigma) w d\sigma. \tag{8}$$

When the representative employer provides sick pay to a larger share of their jobs, two changes occur. First, fewer employees work when sick. As seen in the first term of Equation (8), depending on the profitability of the marginal employee, the effect on profits might be positive (if sick employees have a negative productivity) or negative. Notice that wages have to be paid independent of whether employees are on sick leave or work; hence, the wage itself does not enter this tradeoff. Second, more employees are on paid sick leave (see second term of Equation (8)).

For the employer, sick pay is optimal when it incentivizes unproductive employees to call in sick; that is, employees with $\pi(\sigma) < 0$. Or mathematically, the second term above will always be negative. Hence the employer will only increase sick pay coverage if the first term is positive and sufficiently large. More sick pay will then incentivize those unprofitable employees to call in sick, but require the employer to provide more employees with sick pay.

6.1.3 The Social Planner's Decision

The social planner maximizes total welfare. For simplicity, we assume that utility is quasilinear in consumption and thus total welfare is the sum of total employee utility (Equation (3)) and total employer profits (Equation (7)), minus a penalty term $S = p\alpha \int_0^{\sigma_L^*} f(\sigma) s d\sigma + p(1-\alpha) \int_0^{\sigma_0^*} f(\sigma) s d\sigma$ representing the spread of diseases caused by presenteeism and employees with and without sick pay: $W = U + \Pi - S$. Attaching higher weights to either employees or the representative employer is straightforward.

To maximize total welfare, the social planner optimizes the share of employees with sick pay coverage:[35]

$$\frac{\partial W}{\partial \alpha} = \frac{\partial U}{\partial \alpha} + \frac{\partial \Pi}{\partial \alpha} - \frac{\partial S}{\partial \alpha} \lesseqqgtr 0. \tag{9}$$

Welfare is linear in α and thus only corner solutions with either full coverage ($\alpha = 1$) or no coverage are relevant decisions (see footnote 32). If welfare is increasing in α, full coverage is welfare maximizing; if welfare is instead decreasing in α, no coverage is welfare maximizing. Finally, if the slope of the welfare function with respect to α is equal to zero, all values of α are equally optimal.

When rearranging Equation (9), we obtain the following welfare maximizing optimality condition:

$$\begin{aligned} p\int_{\sigma_0^*}^{1} f(\sigma)\Big(u(\sigma, w-l, T) - u(\sigma, 0, T) - w\Big)d\sigma \\ +p\int_{\sigma_L^*}^{\sigma_0^*} f(\sigma)\Big(u(\sigma, w-l, T) - u(\sigma, w, T-h) - \pi(\sigma) + s\Big)d\sigma \lesseqqgtr 0 \end{aligned} \tag{10}$$

The first term subtracts the costs of sick leave, w, from the higher employee utility while on paid sick leave for seriously sick employees ($\sigma > \sigma_0^*$). The second term does the same for marginal compliers ($\sigma_L^* > \sigma > \sigma_0^*$). Here, the employer costs of sick pay are not wages, but productivity losses under presenteeism $\pi(\sigma)$.[36] Finally, as marginal compliers call in sick when they have sick pay (but work sick when not), the optimality condition considers positive externalities of fewer infections due to reduced presenteeism behavior.

Equation (10) is similar to the standard Baily-Chetty formula (Baily, 1978; Chetty and Finkelstein, 2013), but there are some notable differences. First, in the standard Baily-Chetty framework, employees pay for their own welfare benefits through higher taxes. This funding through taxation results in the balancing of marginal utilities in different states. In our setting, the representative employer provides sick pay and the social planner trades-off the marginal employee utility of

[35] Notice that the social planner varies coverage, α, through sick pay mandates. However, the pass-through is imperfect as already shown: even after the mandate's implementation, coverage does not increase to 100%.

[36] This is because marginal compliers would receive wage w in any case, but work sick without sick pay and call in sick with sick pay.

sick pay coverage against the employer costs of providing sick pay, and also considers disease externalities. Second, sickness is a continuous state and affects work productivity. Hence, for the employer, providing sick pay voluntarily might make sense because sick pay incentivizes sick and unproductive employees to call in sick (as sickness is at least partially unobservable for the employer). Finally, in contrast to Baily-Chetty, our framework does not vary the intensive margin and benefit levels, but the extensive margin and the share of employees who are eligible for the benefit.[37]

6.2 Mandating Sick Pay and Relevant Trade-Offs for Welfare Implications

Whether sick pay mandates increase welfare depends on Equation (10) and the empirical parameter estimates from in the previous section. In what follows, we discuss the different parameters of the equation to illustrate how to estimate them empirically.

First, we consider the benefits and costs of paid sick leave for employees who are seriously sick ($\sigma > \sigma_0^*$) and who obtain paid sick leave w as a result of the mandates (A_0 in Equation (5)).

Second, we denote with λ the benefits of paid sick leave, where $\lambda = \frac{u_L - u_0}{w}$ is employees' increase in utility when receiving sick pay, normalized by the wage (which equals sick pay). In other words, we normalize λ such that $\lambda = 1$ would reflect a situation where the employee values sick pay at its monetary cost to the employer. Below, we outline different approaches to assess λ.

Third, s denotes the health externality due to presenteeism and the spread of contagious diseases. For a state of one million inhabitants, our estimates suggest that state paid sick leave mandates increase sick pay coverage by 13 percentage points. For marginal employees, utilization increases by 14 hours per year and employee (Table 2), resulting in 2,500 additional sick days taken per week per one million population.[38] Table B10 shows a statistically significant reduction in Influenza-like Illness (ILI) rates as a result of sick pay mandates, which translates into about 207 fewer cases per week per one million population. Thus, every sick day reduces ILI cases by 207/2500=0.0828. Assuming average sick leave of 16 hours, for every ILI case, this translates to 0.0828*16, which we will price at the hourly wage w, thus $s = 0.0828 * 16 * w = 1.325w$. Below,

[37] See Kleven (2020) for a complete discussion of adjusting the share of covered individuals as compared to adjusting their benefit level. Also note that, under the assumption that sickness is partially unobservable by the employer, this type of asymmetric information results in an underprovision of sick pay. Once the employer offers sick pay, employees have an incentive to overstate their sickness to obtain paid sick leave. However, as U.S. sick leave implies very limited individual sick leave credit, the potential for such strategic gaming moral is small.

[38] This assumes that 50% of the population work and yields 0.13*500,000*2 days/52 weeks=2,500 additional sick days per week. Further, we assume two doctor visits per year or 38,461 per week. The Influenza-like Illness (ILI) rate per 38,461 patients is 692.

we consider two scenarios: (a) s=0 to get a lower bound; (b) our estimated externality costs of $s = 1.325w$ to get an upper bound on the welfare effects of increasing sick pay coverage.

Finally, we denote with $\delta = \frac{\pi}{w}$ the average productivity of employees who would be working without sick pay, but would stay home with sick pay. Again we normalize by wages. This allows us to rewrite Equation (10) as:

$$A_0(\lambda - 1)w + \frac{\partial A}{\partial \alpha}\Big((\lambda - \delta)w + s\Big) = 0. \tag{11}$$

6.2.1 Average Welfare Implications

Next, we feed our empirical inputs into Equation (11), beginning with the average effect, and then differentiating by industry and occupation.

First, Table 2 yields the share of marginal compliers ($\frac{\partial A}{\partial \alpha}$) measuring take-up as a result of an increase in paid sick leave coverage. Columns (1) and (2), Panel D, show that the mandates increase coverage α by 12.8 percentage points and utilization by 1.816 hours, which we normalize by total yearly hours H: $\frac{\partial A}{\partial \alpha} = \frac{1.816/H}{0.128} = 14.19/H$. That is, when sick pay coverage rates increase by one percentage point at the population level, paid sick hours taken increase by 14 hours. Applying pre-mandate sick pay coverage rates of 0.66 and sick hours of 17.8 (Table 2), we obtain an elasticity of $\frac{1.816}{0.128}\frac{0.66}{17.8} = 0.644$. That is, when coverage rates increase by 1%, sick leave use (as a share of total work time) increases by 0.64%.

Next, a share of seriously sick employees A_0 will always be absent, even without sick pay. Their share is equal to $0.54/H$ (the pre-mandate number of unpaid sick leave, Table 2). Average wages are equal to 21.69 and annual working hours H total to 1,700 on average (Table 1).

Further, we must assess work productivity when sick. Unfortunately, our data do not allow a direct estimate. However, the American Working Conditions Survey (AWCS) asks a nationally representative sample of U.S. adults to estimate their reduced work productivity when working sick (Maestas et al., 2020). The estimate for the average employee is a reduction of 23%, which leads us to use $\delta = 0.77$ as our baseline scenario.

Finally, we need to determine λ. To do so, we offer three approaches. As for our first approach, Figure 5 plots the welfare benefit of increasing sick pay coverage via the following function: $\frac{\partial W}{\partial \alpha} = 0.54 \times (\lambda - 1) \times 21.69 + 14.19 \times \Big((\lambda - \delta) \times 21.69 + s\Big)/1700$, where Panel A shows the results for the lower bound of $s = 0$; and Panel B the results for $s = 1.325w = 1.325 \times 21.69$.

[Insert Figure 5 about here]

Panel B of Figure 5 shows, when taking externalities into account, more sick leave coverage is always welfare improving.[39] Panel A of Figure 5 ignores externalities ($s = 0$). We find that the derivative of welfare with respect to sick leave coverage is positive, as long as the valuation of sick pay for employees exceeds the lost productivity ($\lambda \geq \delta$). For our baseline scenario of $\delta = 0.77$, the welfare model suggests that mandating sick pay is welfare improving as long as the marginal employee utility is at least 78% of the labor costs. As already noted by Summers (1989), this difference between the employee value of a mandated benefit and the employer cost of providing it should be fundamental in the social planner's decision whether or not to mandate benefits. Moreover, the estimated share of marginal compliers determines the size of welfare benefits.

As for our second approach to assess λ, we refer to a recent study by Maestas et al. (2018) who experimentally elicits the willingness-to-pay (WTP) for ten PTO days among a representative sample of U.S. employees. The findings show that the elicited WTP value clearly exceeds even the largest possibly assumed WTP of 100% in Figure 5.[40] Moreover, given our calculations, $\lambda = 1$ together with our baseline productivity of 0.77 would lead to marginal welfare benefits of 0.04.[41] Thus, independent of δ, the marginal welfare benefits will always be positive in Figure 5, and more sick pay coverage will always increase welfare.

Finally, even though survey estimates might overestimate the WTP for PTOs, there is suggestive evidence that employees place substantial value on sick pay. Recall that 82% of American voters support sick pay mandates (HuffPost/YouGov, 2013). Further, a clear majority of Americans considers sick leave a basic employee's right and believes that providing this benefit is more important than existing employees' rights such as the right to join a union (National Paid Sick Days Study, 2010).

In conclusion, if employees' WTP for paid sick days is anywhere close to the elicited WTP in Maestas et al. (2018), welfare is very likely to improve if states mandate sick pay in the U.S. Specifically, based on our model of optimal sick pay and our causal labor supply estimates, this will be the case if marginal employees' valuation of gaining access to sick pay exceeds at least 78%

[39] However, as mentioned, even after the mandate, coverage in our data is not universal 100% which could imply that employer costs of non-compliers considerably exceed employer costs for compliers for whom we elicit labor cost estimates.

[40] The average elicited WTP equals 15% of the hourly wage. Assuming 260 working days per year, the ten PTO days represent a little less than 4% of all working days (10/260=0.0385), but still employees would be willing to give up 15% of their hourly wage. Or, in other words, employees themselves could save 4% of hourly wages to pay their own paid leave, but they would be willing to buy an insurance costing them 15% of their wages.

[41] Or 0.4 in Figure 5 after rescaling welfare benefits by 10 for better representation.

of the employer costs of providing it. Finally, taking into account infection externalities, increasing sick leave coverage will be welfare enhancing for complying employers.

A final note of caution almost always applies in such calculations, but is still worth mentioning. The empirical inputs for these welfare calculations stem from average coefficient estimates for five U.S. states and the first post-reform years. Considering effect heterogeneity, statistical uncertainty, and alternative economic conditions would naturally introduce wider bandwidths.

6.2.2 Welfare Implications by Type of Job and Industry

Therefore, we now consider (welfare) effect heterogeneity by type of job and industry. Specifically, we leverage our effect heterogeneity estimates from Section 5.1.3 for this exercise along with our optimality condition in Equation (11). Table 8 shows the results.

[Insert Table 8 about here]

First we present the results for the overall economy derived in the previous subsection. Next, Panel A shows the job and employer characteristics by which we stratify; for example, full or part-time employment, employer size, and whether jobs are unionized or not. Panel B, by contrast, differentiates by industry, while Panel C shows the results for different occupations. Column (1) lists the calculated share of marginal compliers (not adjusted for total hours worked), whereas column (2) shows the calculated marginal productivity using data from Maestas et al. (2018). Finally we show the results for the two different scenarios, no externality ($s = 0$) in Columns (3) and (4), and considering the externality ($s = 1.325$) in Columns (5) and (6). For each group, we first show the break-even valuation for employees (λ) that would be at least required, according to our model, for a mandate to be welfare improving in (3) and (5), along with the marginal welfare benefits, which is the slope of the welfare function.

Table 8 shows the following: We find substantial heterogeneity in the share of marginal compliers across both types of jobs and industries. The smallest shares are in part-time jobs (6.4) and sales occupations (5.8) whereas shares are slightly larger in all of the selected industries. However, average full-time employees exhibit shares of marginal compliers of 24.7, which are even larger among large employers (58.7).

Naturally, the size of the elasticities are also a function of the much higher baseline rates in full-time jobs and larger employers. As seen in Tables 4 and 5, in industries with lower baseline coverage rates, mandates have more bite and the increase in coverage in percentage terms is much larger. While the increase in utilization is also larger in these industries, its not large enough to make up for the large change in the denominator which explains the substantially smaller slopes.

Moreover, Column (2) shows surprisingly little variation in terms of work productivity when sick in the different jobs, industries and occupations. Employees' break-even valuations λ (Column [3]) are very similar to their productivity with no externalities. On the other hand, the slope of the welfare function in Column (4) strongly depends upon the share of marginal compliers.

Finally, when considering disease externalities, all break-even valuations λ in Column (5) turn negative, and the slope of the welfare function increases significantly (6).

7 Discussion and Conclusion

This paper evaluates the labor market and welfare effects of enacting sick pay mandates at the state level in the United States. We estimate the effects of mandating sick pay on coverage, paid and unpaid sick leave utilization, labor costs, and non-mandated benefits. In particular, we leverage the experiences of five U.S. states with more than 70 million residents (California, Connecticut, Massachusetts, Oregon, and Vermont). For our empirical estimates, we use the National Compensation Survey (NCS) from 2009 to 2017, coupled with difference-in-differences and event study models which exploit the policy-induced variation in the implementation of the mandates across U.S. states and over the past decade. We also show that our results are not driven by bias attributable to staggered policy roll-out designs, as has been raised as a possible empirical concern in the contemporary DD literature (Goodman-Bacon, 2021). The NCS is a rich government dataset at the firm-job level specifically designed to measure and track labor compensation and costs, and is used to officially adjust wages and compensation of federal government employees.

Our findings address important gaps in the economics literature on labor market inequalities and employer mandates more broadly. The U.S. is a country with one of the least generous paid leave systems among all OECD countries (Adema et al., 2016; Raub et al., 2018). Federal minimum standards concerning paid vacation, paid parental leave, paid eldercare, and paid sick leave are largely absent, leading to variation in the voluntary provision of such benefits by employers. In general, better paying jobs for higher educated employees tend to offer paid leave benefits, whereas part-time and low-income jobs for lower educated employees do not. An important and open question is to what extent employer mandates are effective in providing and facilitating the provision and use of such benefits; or whether they have unintended consequences and lead to a reduction, and potentially inefficient reallocation, of non-mandated benefits (that employees may value).

This paper provides quasi-experimental evidence on the overall effectiveness of sick pay mandates along several margins. To this end, we study the important 'first stage' effects of mandating sick pay on actual changes in sick pay coverage. Using government data we also estimate take-up effects, assess the relevance of mandates for labor costs, and estimate the extent to which employers respond to the mandates by curtailing other forms of compensation. In addition, we develop a model of optimal sick pay and use the empirical inputs to assess whether mandating sick pay is welfare improving or not. Our research provides timely evidence on all these questions and contributes to a better understanding of how recent mandates function, which is relevant from both an economic and a policy perspective. Opinion polls show large and bipartisan support for mandating paid sick leave. Moreover, the federal Families First Coronavirus Response Act passed Congress on a bipartisan basis in March 2020. This Act provided up to two weeks of temporary emergency sick leave related to COVID-19 but expired at the end of September of 2021. At the time of writing, there are ongoing efforts in the Biden Administration to establish a permanent federal paid leave policy.

The findings of this paper show a clear and significant increase in sick leave coverage rates of 13 percentage points (or 20% relative to the pre-treatment coverage rate of 66%) in the four years following state-level mandate passage. Interestingly, after an initial increase in coverage rates by 18 percentage points, we find no further increase in subsequent years (driven by the first adopters in our data). Further research should probe this coverage gap, for instance, by adding more years of data and effects from more recent mandates. Non-compliance and lack of awareness are both plausible explanations. Hall et al. (2018) report that, in New York City, only 30% of employees were aware of the new sick pay mandate in the first year after the implementation. However, more data-driven explanations of this finding are an important path for future work.

As expected, we also find a significant two hours increase in paid sick leave use following mandate implementation. Scaling this average increase by the share of marginal jobs that have been covered through the mandates suggests that newly covered employees take, on average, two additional sick days per calendar year. The implied elasticity is 0.64, meaning that the share of total work time spend on sick leave increases by 0.64% for every increase in the coverage rate by 1%. Further, we find that total sick leave costs increase by 10%, which translates to 21 cents per hour for marginal employers and represents 1% of the hourly wage. Moreover, we find very limited evidence that employers curtail non-mandated benefits as a response to the mandates to reduce overall labor costs.

Finally, we develop a model of optimal sick pay and generate several findings. Most importantly, for the social planner to mandate sick pay, (1) the employee utility of gaining sick leave coverage has to exceed the employer costs of providing this benefit, and (2) this differential must be benchmarked with the effects of sick pay on employer production, specifically the changes in productivity and wage payments, weighted by the labor supply elasticity. Moreover, (3) the social planner considers negative externalities due to the spread of contagious diseases when sick workers work without sick pay. We then feed our empirical moments and estimated labor supply elasticities—as well as heterogeneity by industry and type of job—into our derived optimality condition to discuss welfare-relevant trade-offs and their sensitivity with respect to several parameters. For example, we find much larger elasticities in industries and jobs with higher pre-mandate coverage rages. These translate into higher required differentials between marginal employee valuation and firm costs for the mandate to be welfare improving, according to our model. In general, surveys and experimentally validated compensating wage differentials suggest that U.S. employees value the benefit highly (Maestas et al., 2018; National Partnership for Women and Families, 2020) but the evidence on overall firm costs is scant.

As states continue to implement sick pay mandates, more empirical evidence on the indented and unintended consequences of these mandates will become available. We look forward to fruitful discussions among social scientists.

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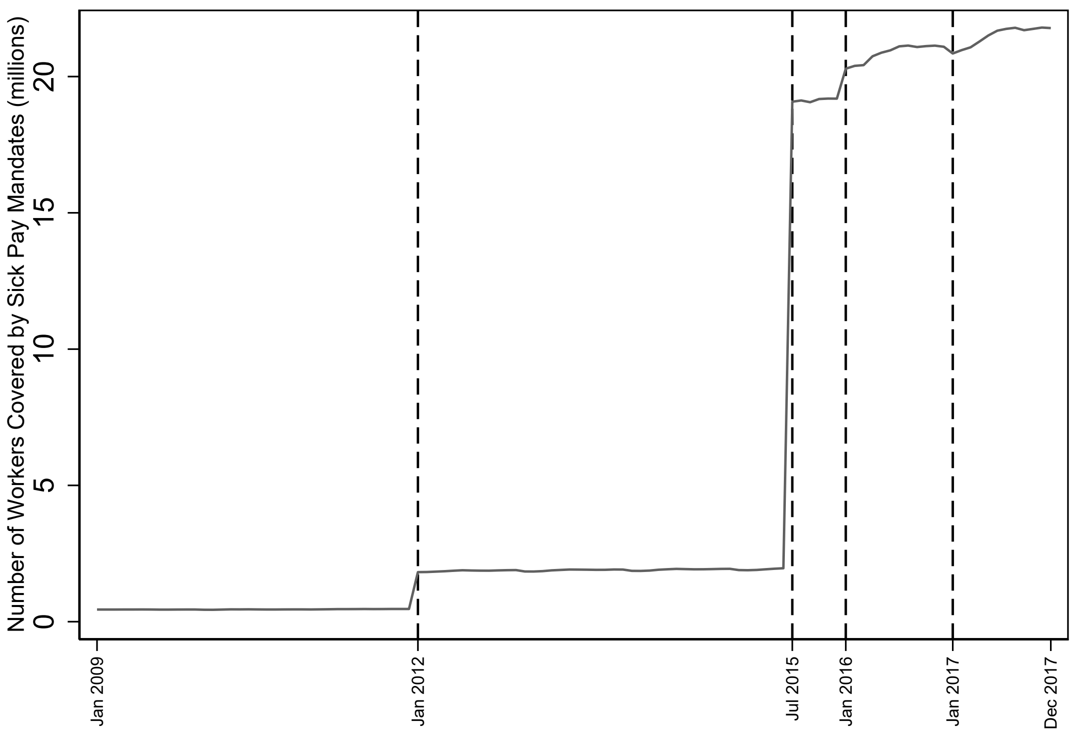
Figures and Tables

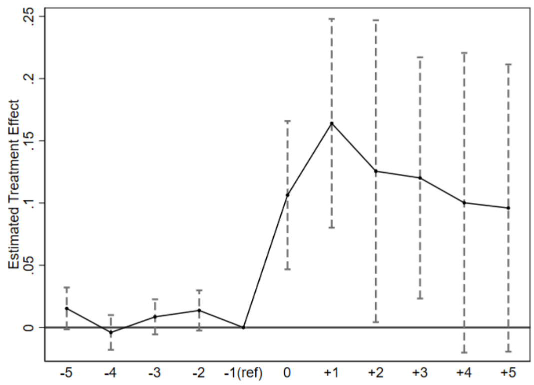
Figure 1: Number of Employees Covered by Sick Pay Mandates



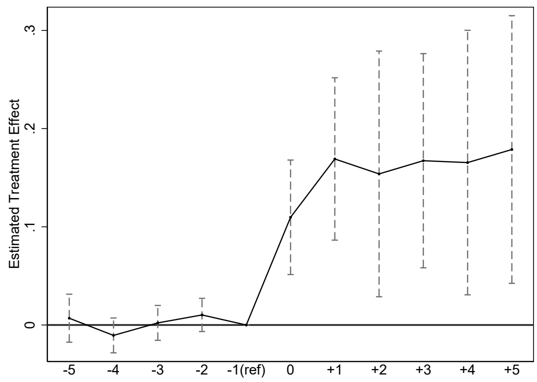
Source: Quarterly Census of Employment and Wages. Own data collection and illustration. The figure shows the number of private sector employees covered by sick pay mandates between 2009 and 2017 in D.C., Connecticut, California and Massachusetts, Oregon and Vermont. Employees in city and county level jurisdictions with mandates are not included, and neither are they in our main models.

Figure 2: Event Studies from Difference-in-Differences Models

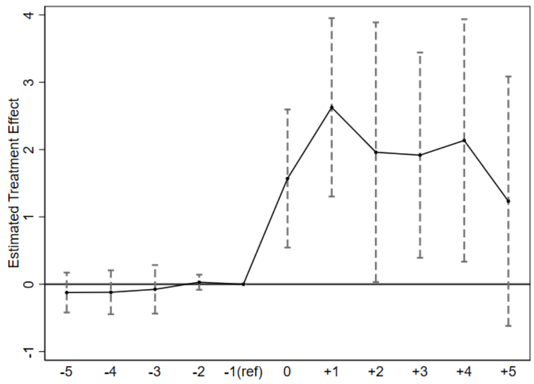
Panel A: No state trends | **Panel B: State trends**



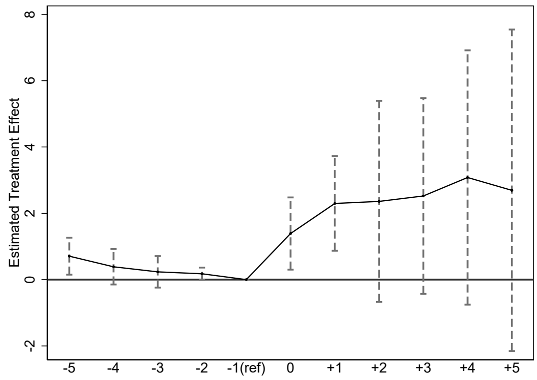
(a) Sick pay coverage



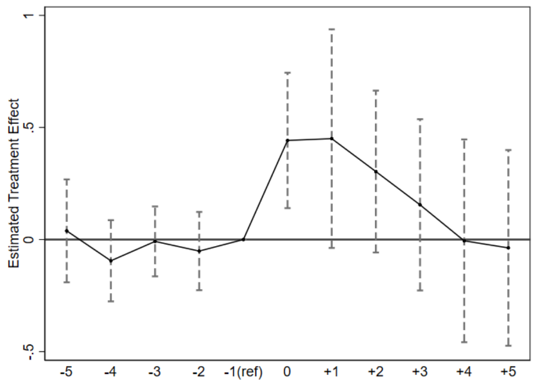
(b) Sick pay coverage



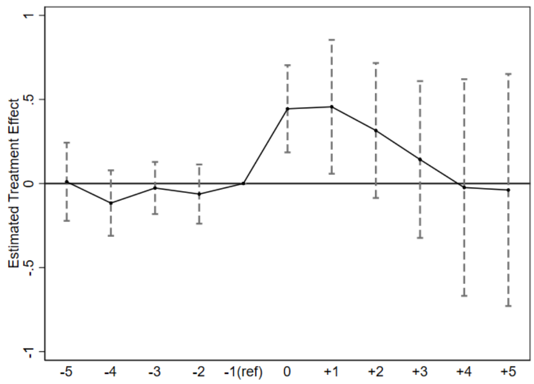
(c) Paid sick hours taken



(d) Paid sick hours taken



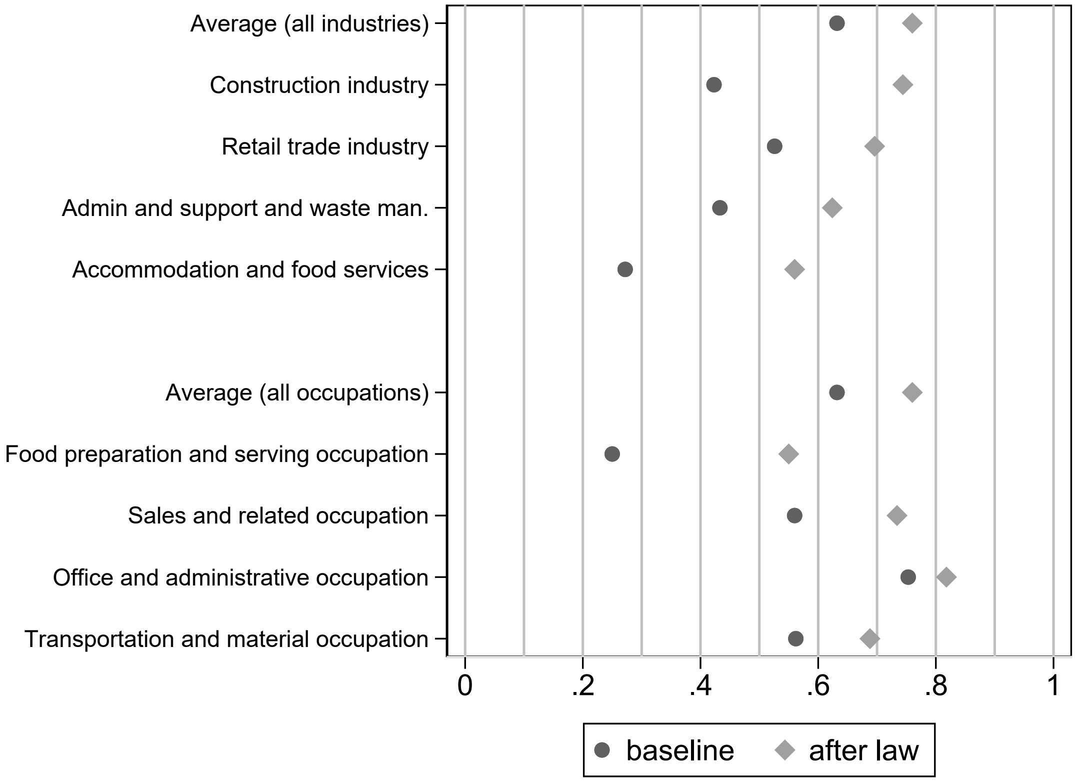
(e) Unpaid sick hours taken



(f) Unpaid sick hours taken

Notes: The graphs show event studies based on DD models as in Equation (2). The standard errors are clustered at the state level and the gray bars depict 95% confidence intervals. For more information about the sick pay reforms, see Table A1.

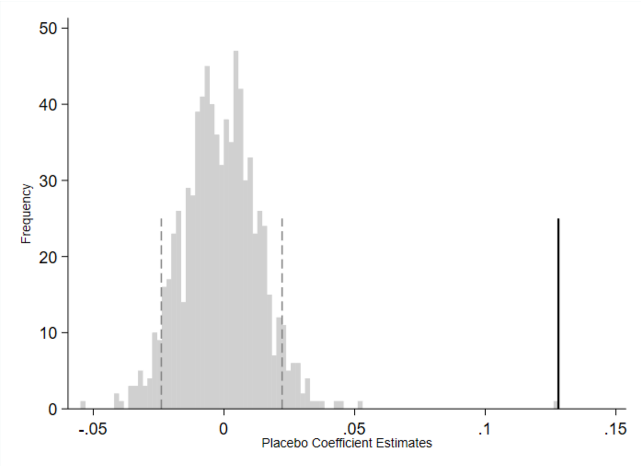
Figure 3: Coverage Effect Heterogeneity by Industry and Occupation



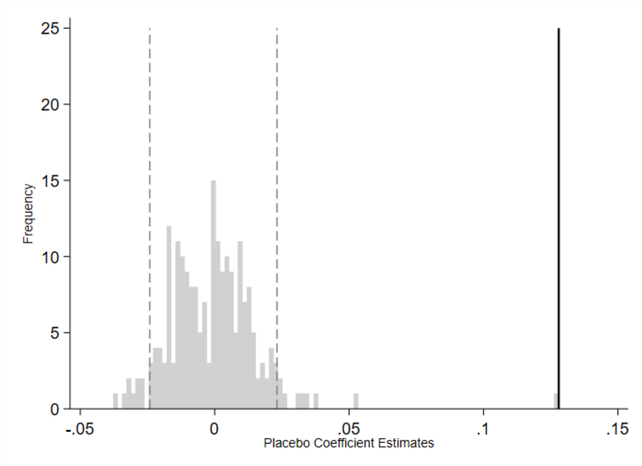
Results are for coverage only. Full results are in Table 5. Industries and occupations are sorted by the weighted frequency of the industries and occupations.

Figure 4: Placebo Regression Results

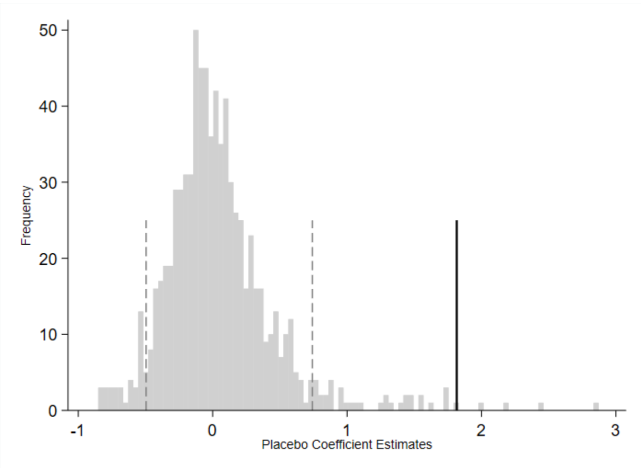
Panel A: No state trends **Panel B: State trends**



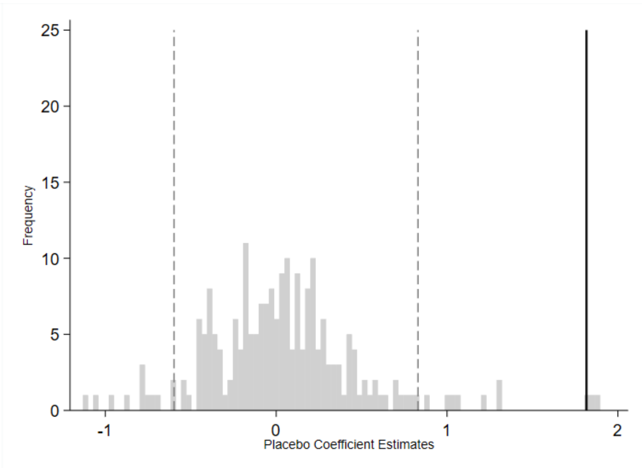
(a) Sick pay coverage (p=0.001)



(b) Sick pay coverage (p=0.01)



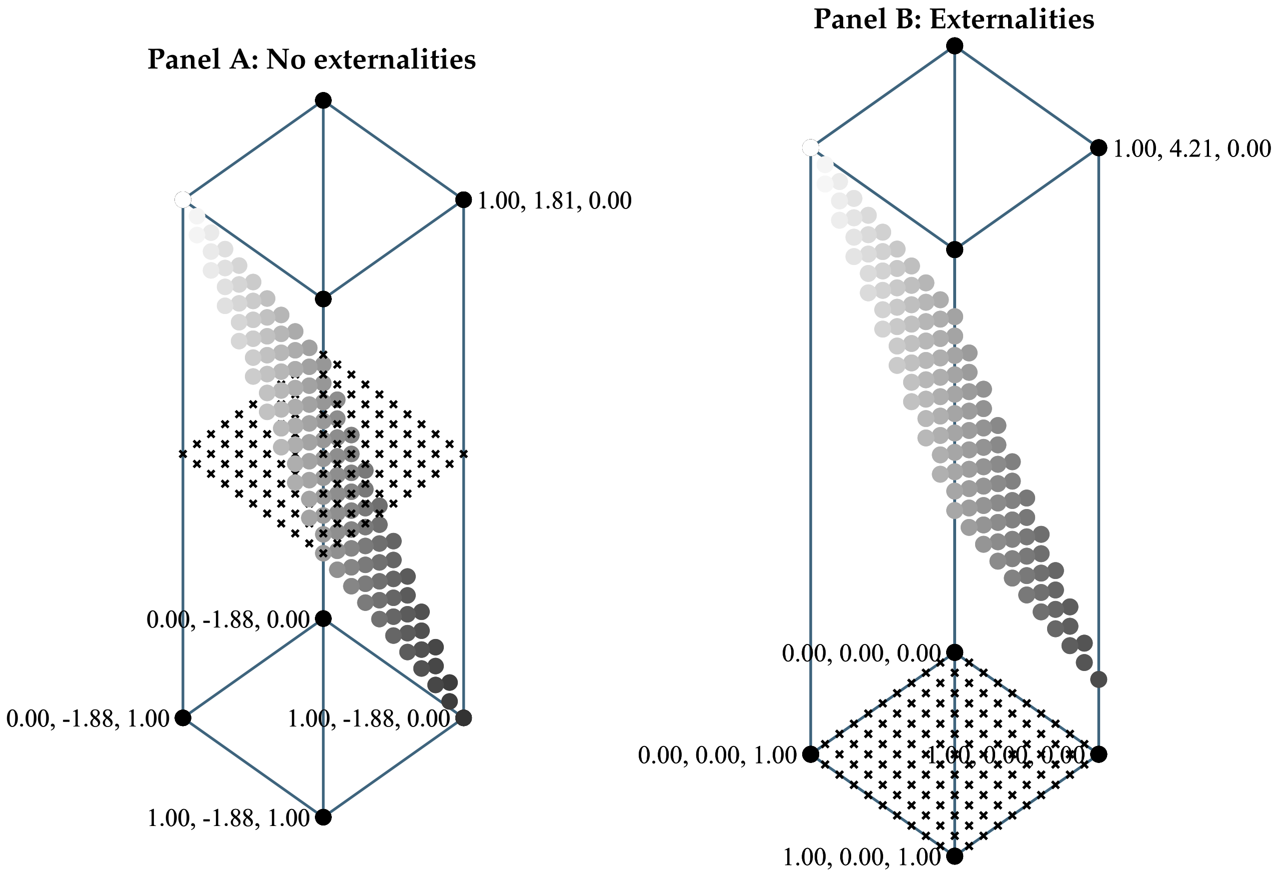
(c) Paid sick leave hours taken (p=0.01)



(d) Paid sick leave hours taken (p=0.01)

Notes: This figure plots the distribution of the estimated placebo regressions (n=200) that excluded treatment states and randomly assigned pseudo treatment states, as compared to the true estimate. All models include employer-job fixed effects and year fixed effects and estimate models as in Equation (1). The vertical black line and corresponding bar denotes the true coefficient estimates. The p-values are displayed next to the variable name. For more information about the sick pay reforms, see Table A1.

Figure 5: Welfare Effects of Sick Pay Mandates



Notes: Coordinates are shown in the following format (δ,$\frac{\partial W}{\partial \alpha}$ $\times$ 10,λ). This figure plots the marginal welfare benefits of increasing sick pay coverage. On the left we assume no externalities ($s = 0$), while on the right externalities are present ($s = 1.325w$). The graph incorporates a zero plane which is indicated with black crosses. δ denotes the productivity when working sick as a share of full productivity. Moreover, $\frac{\partial W}{\partial \alpha}$ denotes the welfare changes. For better representation these welfare changes have been scaled by a factor of 10. Finally, λ denotes the ratio between the marginal increase in employee utility and the marginal employer costs of sick pay. E.g. $\lambda = 0.2$ would suggest that every dollar spent on paid sick leave by the employer has a value of \$0.2 when received by a sick worker. In both graphs welfare is increasing in the worker valuation λ and decreasing in the (lost) productivity δ.

Table 1: Descriptive Statistics, National Compensation Survey (NCS)

	Mean	Std. Dev.
Outcomes		
Sick leave offered (binary)	0.632	0.482
Paid sick hours taken (hours per year)	15.84	18.72
Unpaid sick hours taken (hours per year)	0.647	4.595
Other benefits and characteristics		
Full-time employment (binary)	0.739	0.439
Part-time employment (binary)	0.261	0.439
Unionized (binary)	0.086	0.281
Hourly wage (in 2017 $)	21.69	18.42
Hourly health insurance cost (in 2017 $)	2.403	2.427
Hourly non-production bonus (in 2017 $)	0.656	5.619
Sick leave costs total (in 2017 $)	448.5	792.5
Sick leave cost per hour worked (in 2017 $)	0.251	0.479
Paid vacation hours per year	69.58	57.57
Paid national holiday hours per year	44.11	32.91
Paid overtime hours per year	57.2	106.2
Annual hours worked	1700	469
Annual hours paid leave	138	102
Annual hours paid (=sum of worked and leave)	1838	523
Health insurance offered (binary)	0.688	0.464
Presc. drug insurance offered (binary)	0.673	0.469
Dental insurance offered (binary)	0.436	0.496
Life insurance offered (binary)	0.571	0.533
Short-term disability offered (binary)	0.378	0.485
Long Term disability offered (binary)	0.329	0.470
Family leave offered (binary)	0.111	0.314
Fixed paid sick time (binary)	0.424	0.494
Consolidated sick plan PTO (binary)	0.163	0.369
Main employee occupations (sorted by weighted frequency)		
Office and administrative	0.166	0.372
Sales and related	0.113	0.316
Food preparation and serving	0.104	0.305
Transportation and material	0.086	0.281
Production	0.086	0.28
Health practitioners and technicians	0.061	0.240
Installation, maintenance, and repair	0.045	0.207
Management	0.042	0.200
Main employer industries (sorted by weighted frequency)		
Healthcare and social assistance	0.158	0.365
Retail trade	0.139	0.346
Manufacturing	0.120	0.325
Accommodation and food services	0.113	0.317
Admin, support and waste mgmt., and remed. services	0.072	0.258
Professional, scientific, and technical services	0.068	0.252
Finance and insurance	0.049	0.2180
Construction	0.049	0.2160
Wholesale trade	0.048	0.2140
Transportation and warehousing	0.040	0.1970
Employer size	612	2,127
Observations	399,586	

Source: National Compensation Survey , 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Data are yearly and at the firm-job level; they are weighted by BLS provided weights. Minimum and maximum values not available due to data confidentiality reasons.

Table 2: Effect of Mandates on Coverage, Utilization and Labor Costs

Outcome	Sick leave offered (1)	Paid sick hours taken (2)	Unpaid sick hours taken (3)	Sick leave costs per hour (4)
Pretreatment mean: *(in treated localities)*	0.659	17.8	0.541	0.326
Panel A				
Sick leave mandate	0.128***	1.983***	0.442**	0.031***
($D_c \times T_t$)	(0.035)	(0.610)	(0.199)	(0.007)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Panel B				
Sick leave mandate	0.129***	2.027***	0.441**	0.032***
($D_c \times T_t$)	(0.034)	(0.587)	(0.199)	(0.007)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Employee controls	X	X	X	X
Panel C				
Sick leave mandate	0.130***	2.060***	0.462*	0.033***
($D_c \times T_t$)	(0.041)	(0.704)	(0.243)	(0.009)
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
Panel D				
Sick leave mandate	0.128***	1.816**	0.479**	0.027***
($D_c \times T_t$)	(0.038)	(0.701)	(0.193)	(0.008)
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
State time trend	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. FE=fixed-effects. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Employee controls: unionized job and part-time employment. Standard errors clustered at the state level and reported in parentheses. All models have 399,586 firm-job observations. Employers below the employer size cutoff are coded as zero. See Table B7 for results after dropping these observations.

Table 3: Decomposition of Treatment Effects

Outcome	**Weight**	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Timing_groups	0.020	0.119	0.926	0.318	0.024
Always_vs_timing	0.005	0.045	-1.323	0.908	-0.022
Never_vs_timing	0.969	0.093	0.899	0.323	0.022
Always_vs_never	0.000	-2.618	-141.792	16.436	-5.114
Within	0.006	0.966	34.358	2.741	0.625
Sick leave mandate		0.099***	1.089**	0.342***	0.026**
($D_c \times T_t$)		(0.013)	(0.440)	(0.106)	(0.0101)

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights and estimated with employee controls (unionized job and part-time employment). Standard errors clustered at the state level and reported in parentheses. All models have 399,586 firm-job observations. Employers below the employer size cutoff are coded as zero. See Table B7 for results after dropping these observations.

Table 4: Effect Heterogeneity of Mandates: Coverage, Utilization and Labor Costs

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean: *(in treated localities)*	0.659	17.8	0.541	0.326
Panel A: Full-time vs. part-time				
Sick leave mandate	0.258***	1.656***	0.364**	0.019***
($D_c \times T_t$)	(0.062)	(0.532)	(0.154)	(0.006)
Sick leave mandate	-0.182***	0.223	0.160	0.011**
$\times$full-time	(0.032)	(0.262)	(0.184)	(0.005)
Panel B: Union vs. non-union				
Sick leave mandate	0.145***	1.950**	0.499**	0.024**
($D_c \times T_t$)	(0.044)	(0.798)	(0.197)	(0.010)
Sick leave mandate	-0.168***	-1.310	-0.198***	0.027
$\times$union	(0.044)	(0.895)	(0.064)	(0.018)
Panel C: Large employers (>500 employees)				
Sick leave mandate	0.151***	1.828**	0.648***	0.020**
($D_c \times T_t$)	(0.038)	(0.706)	(0.222)	(0.009)
Sick leave mandate	-0.120***	-0.059	-0.865***	0.036***
$\times$large employers	(0.018)	(0.307)	(0.267)	(0.007)
Panel D: Small employers (<50 employees)				
Sick leave mandate	0.071**	1.640**	0.016	0.024***
($D_c \times T_t$)	(0.028)	(0.749)	(0.110)	(0.008)
Sick leave mandate	0.153***	0.464**	1.243***	0.006***
$\times$small employers	(0.022)	(0.229)	(0.373)	(0.002)

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Each column in each panel stands for one model similar to Equation (1), but augmented with triple interaction terms and all two-way interactions, see main text for details. ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Standard errors clustered at the state level and reported in parentheses. All models have 399,586 firm-job observations. All models in all panels control for year fixed-effects (FE), firm-job FE, and state-specific linear time trends (for estimations without trends see Table B8 in the Appendix). Controls for all other two-way interaction terms are included in all models but not shown (available upon request).

Table 5: Effect Heterogeneity of Mandates: Industries and Occupations

Outcome	Sick leave offered (1)	Paid sick hours taken (2)	Unpaid sick hours taken (3)	Sick leave costs per hour (5)
Pretreatment mean: *(in treated localities)*	0.659	17.8	0.541	0.326
Panel A: Industries				
Panel A1: Construction				
Sick leave mandate	0.119***	1.790**	0.445**	0.028***
$(D_c \times T_t)$	(0.036)	(0.693)	(0.189)	(0.009)
Sick leave mandate	0.202***	0.589***	0.764***	-0.023***
$\times$construction	(0.038)	(0.200)	(0.128)	(0.003)
Panel A2: Retail trade				
Sick leave mandate	0.121***	1.857**	0.561**	0.029***
$(D_c \times T_t)$	(0.040)	(0.729)	(0.216)	(0.009)
Sick leave mandate	0.049***	-0.312	-0.605***	-0.017***
$\times$retail trade	(0.011)	(0.234)	(0.204)	(0.003)
Panel A3: Admin, support, waste mngmt., and remed. services				
Sick leave mandate	0.121***	1.864**	0.450**	0.029***
$(D_c \times T_t)$	(0.039)	(0.738)	(0.219)	(0.009)
Sick leave mandate	0.070***	-0.501	0.307	-0.025***
$\times$admin services	(0.021)	(0.399)	(0.538)	(0.007)
Panel A4: Accommodation and food services				
Sick leave mandate	0.104***	1.676**	0.131	0.028***
$(D_c \times T_t)$	(0.037)	(0.673)	(0.096)	(0.009)
Sick leave mandate	0.184***	1.068***	2.679***	-0.011***
$\times$accommodation	(0.033)	(0.182)	(0.758)	(0.003)
Panel B: Occupations				
Panel B1: Food preparation and serving				
Sick leave mandate	0.105***	1.685**	0.129	0.028***
$(D_c \times T_t)$	(0.037)	(0.651)	(0.095)	(0.009)
Sick leave mandate	0.195***	1.139***	3.034***	-0.015***
$\times$food	(0.035)	(0.288)	(0.820)	(0.003)
Panel B2: Sales and related				
Sick leave mandate	0.122***	1.920**	0.548**	0.029***
$(D_c \times T_t)$	(0.039)	(0.755)	(0.215)	(0.009)
Sick leave mandate	0.052**	-0.942*	-0.630***	-0.024***
$\times$sales	(0.025)	(0.503)	(0.204)	(0.007)
Panel B3: Office and administrative				
Sick leave mandate	0.140***	1.998**	0.550**	0.030***
$(D_c \times T_t)$	(0.041)	(0.766)	(0.213)	(0.009)
Sick leave mandate	-0.075***	-1.145***	-0.447***	-0.019***
$\times$office	(0.014)	(0.390)	(0.116)	(0.006)
Panel B4: Transportation and material				
Sick leave mandate	0.128***	1.776**	0.495**	0.025***
$(D_c \times T_t)$	(0.037)	(0.686)	(0.201)	(0.009)
Sick leave mandate	-0.002	0.417*	-0.175*	0.021**
$\times$transportation	(0.018)	(0.235)	(0.089)	(0.008)

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Each column in each panel stands for one model similar to Equation (1), but augmented with triple interaction terms and all two-way interactions, see main text for details. ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Standard errors clustered at the state level and reported in parentheses. All models have 399,586 firm-job observations. All models in all panels control for year fixed-effects (FE), firm-job FE, and state-specific linear time trends (for estimations without trends see Table B9 in the Appendix). Controls for all other two-way interaction terms are included in all models but not shown (available upon request).

Table 6: Effect of Sick Leave Mandates on Non-Mandated Benefits

	Insurance plans				Disability		Family	Paid sick leave	
	health	presc. medic.	dental	life	short-term	long-term	leave	fixed	consolidated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pretreatment mean: *(in treated localities)*	0.698	0.685	0.515	0.531	0.264	0.311	0.119	0.474	0.135
Panel A									
Sick leave mandate	-0.009**	-0.004	0.003	-0.006	0.002	0.003	0.002	0.142***	-0.017***
($D_c \times T_t$)	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)	(0.004)	(0.002)	(0.034)	(0.006)
Year FE	X	X	X	X	X	X	X	X	X
Employer FE	X	X	X	X	X	X	X	X	X
Panel B									
Sick leave mandate	-0.008*	-0.003	0.003	-0.005	0.002	0.004	0.002	0.142***	-0.017***
($D_c \times T_t$)	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)	(0.004)	(0.002)	(0.034)	(0.006)
Year FE	X	X	X	X	X	X	X	X	X
Employer FE	X	X	X	X	X	X	X	X	X
Employee controls	X	X	X	X	X	X	X	X	X
Panel C									
Sick leave mandate	-0.006	-0.003	0.004	-0.004	0.002	0.005	0.002	0.142***	-0.018**
($D_c \times T_t$)	(0.006)	(0.007)	(0.007)	(0.005)	(0.006)	(0.005)	(0.002)	(0.041)	(0.007)
Year FE	X	X	X	X	X	X	X	X	X
Firm-job FE	X	X	X	X	X	X	X	X	X
Panel D									
Sick leave mandate	-0.012*	-0.009	-0.005	-0.002	0.001	0.004	0.002	0.131***	-0.007
($D_c \times T_t$)	(0.006)	(0.007)	(0.007)	(0.005)	(0.004)	(0.005)	(0.002)	(0.040)	(0.008)
Year FE	X	X	X	X	X	X	X	X	X
Firm-job FE	X	X	X	X	X	X	X	X	X
State-spec. lin. time tr.	X	X	X	X	X	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. FE=fixed-effects. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Employee controls: unionized job and part-time employment. Standard errors clustered at the state level and reported in parentheses. All models have 399,586 observations, except for models (8) and (9) where we observe sick leave plans for 392,225 job year pairs. For an event study on health insurance see Figure B2b.

Table 7: Effect of Sick Pay Mandates on Hours Worked vs. on Paid Leave

	Annual hours			Total annual hours			Costs per hour		
	vacation	holiday	overtime	worked	paid leave	paid	wage	health ins.	non-production
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pretreatment mean: (in treated localities)	69.92	46	48.79	1674.2	140	1814.3	24.67	0.97	3.591
Panel A									
Sick leave mandate	-0.253	-0.818***	0.776	-1.473	0.738	-0.734	0.334**	-0.036	-0.030*
$(D_c \times T_t)$	(0.431)	(0.234)	(0.968)	(1.310)	(0.586)	(1.514)	(0.163)	(0.033)	(0.017)
Year FE	X	X	X	X	X	X	X	X	
Employer FE	X	X	X	X	X	X	X	X	
Panel B									
Sick leave mandate	-0.05	-0.724***	0.71	1.026	1.101**	2.127	0.229***	-0.034	-0.029
$(D_c \times T_t)$	(0.376)	(0.267)	(1.108)	(1.557)	(0.436)	(1.293)	(0.044)	(0.033)	(0.018)
Year FE	X	X	X	X	X	X	X	X	
Employer FE	X	X	X	X	X	X	X	X	
Employee controls	X	X	X	X	X	X	X	X	
Panel C									
Sick leave mandate	0.083	-0.699**	0.881	0.983	1.266**	2.249	0.191***	-0.029	-0.024
$(D_c \times T_t)$	(0.446)	(0.311)	(1.086)	(1.650)	(0.549)	(1.363)	(0.039)	(0.041)	(0.021)
Year FE	X	X	X	X	X	X	X	X	
Firm-job FE	X	X	X	X	X	X	X	X	
Panel D									
Sick leave mandate	0.503	-0.652*	1.31	1.462	1.373***	2.825**	0.203***	0.047	-0.035
$(D_c \times T_t)$	(0.466)	(0.388)	(0.788)	(1.569)	(0.467)	(1.386)	(0.046)	(0.076)	(0.023)
Year FE	X	X	X	X	X	X	X	X	
Firm-job FE	X	X	X	X	X	X	X	X	
State-spec. lin. time tr.	X	X	X	X	X	X	X	X	

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. FE=fixed-effects. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Employee controls: unionized job and part-time employment. The wage regression (7) includes the local minimum wage as additional control. Standard errors clustered at the state level and reported in parentheses. All models have 399,586 observations.

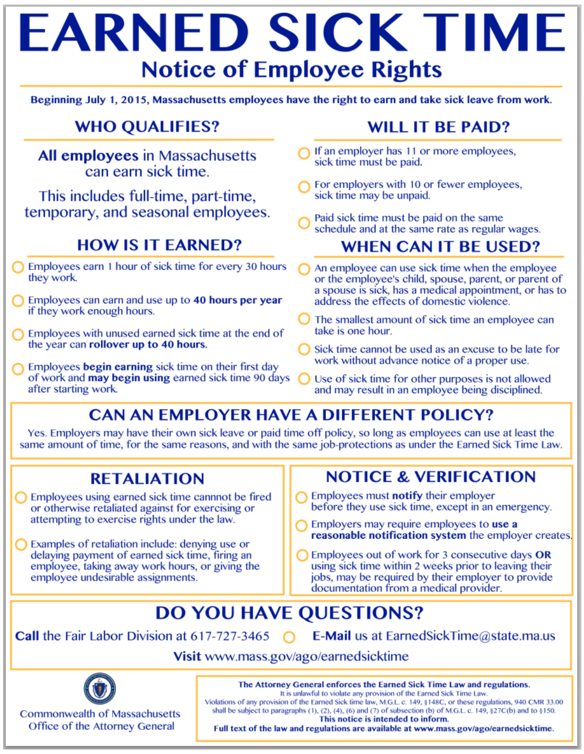
Table 8: Trade-Offs in Welfare Considerations by Type of Job and Industry

			$s = 0$		$s = 1.325w$	
	Marginal Compliers ($\frac{\partial A}{\partial \alpha} \times 10$) **(1)**	**Productivity Losses** (δ) **(2)**	**Break-Even Valuation** (λ) **(3)**	**Welfare Benefits** ($\frac{\partial W}{\partial \alpha} \times 10$) **(4)**	**Break-Even Valuation** (λ) **(5)**	**Welfare Benefits** ($\frac{\partial W}{\partial \alpha} \times 10$) **(6)**
Overall	14.19	0.77	0.78	0.42	-0.50	2.81
Panel A: Job and employer characteristics						
Part-time	6.42	0.73	0.75	0.22	-0.47	1.30
Full-time	24.72	0.77	0.77	0.73	-0.52	4.91
Union	20.00	0.80	0.81	0.50	-0.48	3.88
Non-union	13.45	0.75	0.76	0.42	-0.51	2.70
Large employers $>=$500	58.67	0.77	0.77	1.72	-0.54	11.64
Non-large employers $<$500	12.12	0.76	0.77	0.36	-0.49	2.41
Small employers $<$50	9.55	0.72	0.73	0.34	-0.52	1.96
Non-small employers $>$50	23.10	0.77	0.78	0.67	-0.52	4.57
Panel B: Industries						
Construction	7.44	0.78	0.79	0.21	-0.44	1.47
Retail trade	9.06	0.69	0.71	0.36	-0.55	1.89
Admin services	7.18	0.76	0.78	0.22	-0.46	1.43
Accommodation	9.48	0.71	0.73	0.35	-0.52	1.95
Panel C: Occupations						
Food preparation	14.05	0.72	0.73	0.50	-0.55	2.88
Sales	5.76	0.73	0.75	0.20	-0.46	1.17
Office	13.08	0.77	0.78	0.39	-0.50	2.60
Transportation	17.46	0.78	0.79	0.48	-0.49	3.43

Tables 1, 4, and 5, and Maestas et al. (2017) provide values for calculations, which are based on Equation (11).

Online Appendix (not for publication)

Figure A1: Examples of Legally Required Employee Right Notifications



Left figure shows an earned sick time poster from Massachusetts (Commonwealth of Massachusetts, 2019). Right figure shows a general workplace poster that is compliant with notification requirements in Arizona (Industrial Commission of Arizona, 2019). The Arizona poster includes all labor laws that employers are required to post at the workplace in Arizona.

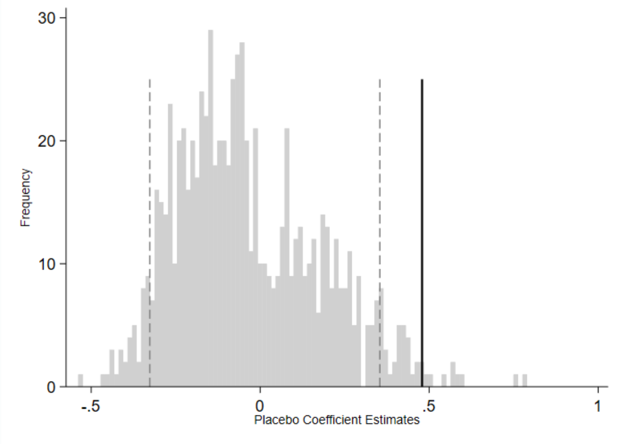
Table A1: Overview of Employer Sick Pay Mandates in the U.S.

Region (1)	County (2)	Law Passed (3)	Law Effective (4)	Content (5)
Washington D.C.	D.C.	May 13, 2008	Nov 13, 2008	'qualified employees'; 1 hour of paid sick leave for every 43 hours, 90 days accrual period; up to 3 to 9 days depend. on employer size; own sickness or family; no health care or restaurant employees
		Dec 18, 2013	Feb 22, 2014	extension to 20,000 temporary and tipped employees (retrosp. in Sep 2014)
Connecticut		July 1, 2011	Jan 1, 2012	full-time service sector employees at employers with >49 employees (20% of workforce); 1 hour for every 40 hours; up to 5 days; own sickness or family member, 680 hours accrual period (4 months)
California		September 19, 2014	July 1, 2015	all employees; 1 hour of paid sick leave for every 30 hours; minimum 24 hours; own sickness or family member; 90 days accrual period
Massachusetts		Nov 4, 2014	July 1, 2015	all employees at employers with >10 employees; 1 hour for every 40 hours; up to 40 hours; own sickness or family member; 90 days accrual period
Oregon		June 22, 2015	Jan 1, 2016	all employees at employers with >9 employees; 1 hour every 30 hours; 90 days accrual period; up to 40 hours; own sickness or family member
Vermont		March 9, 2016	Jan 1, 2017	employees w/ 18 hours/week & >20 weeks/year at employers with > 5 employees; 1 hour every 52 hours; up to 24 hours in 2017, 40 hours thereafter; own sickness or family member; underage employees and employers in first year exempt; some state employees & per diem employees in health care or long-term care facility exempt
Arizona		November 8, 2016	July 1, 2017	all employees; 1 hour for every 30 hours; up to 40 hours at employers with >14 employees, up to 24 hours <15 employees; own sickness or family member; employers can impose 90 day accrual period for new employees
Washington		Nov 8, 2016	Jan 1, 2018	all employees except those who are exempt from minimum wage law; 1 hour for every 40 hours; no cap but no more than 40 hours carry over; own sickness or family member; 90 day accrual for new employees
Maryland		Jan 12, 2018 (override veto by Governor)	Feb 11, 2018	employees w/ 12 hours/week at employers with > 14 employees (<15 employees 40 hours unpaid); 1 hour for every 30 hours; employers can cap at 64 hours accrual and 40 hours carry over; own sickness or family member, also for parental leave; certain groups exempt (e.g. temp. agency employees)
Rhode Island		Sept 28, 2017	July 1, 2018	All employees; 1 hour for every 35 hours; 24 hours in firms >17 (2018, 2019); 40 hours in firms >17 (2020+) own sickness or family member; 90-day accrual period;
New Jersey		May 2, 2018	Oct 28, 2018	all employees; 1 hour for every 30 hours up to 40 hours/year; per diem health care employees exempt own sickness or family member; 120 day accrual for new employees; preempts city laws
Michigan		Dec 13, 2018 (weakened in lame duck session)	March 28, 2019	employees w/ 25 hours/week employed for 25 weeks at employers with > 49 employees; 1 hour for every 35 hours; government employees, certain railway and air carrier employees exempt; own sickness or family member; 90 day accrual for new employees

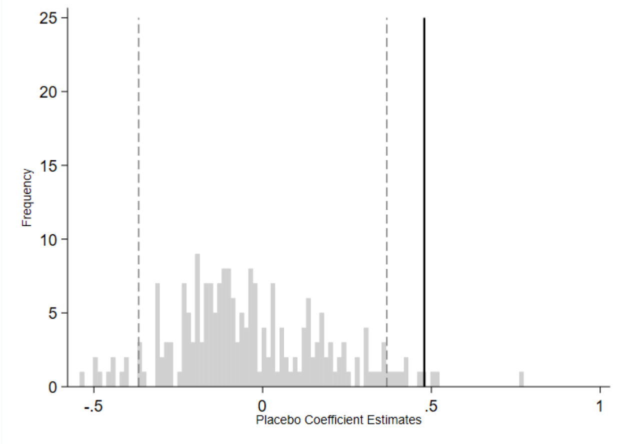
Source: several sources, authors' own collection and illustration. Mandates in gray not used for identification, because our data ends in March 2017.

Figure B1: Placebo Regression Results II

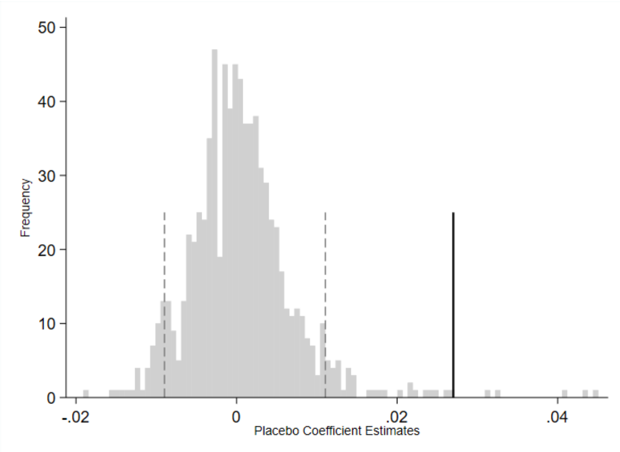
Panel A: No state trends | **Panel B: State trends**



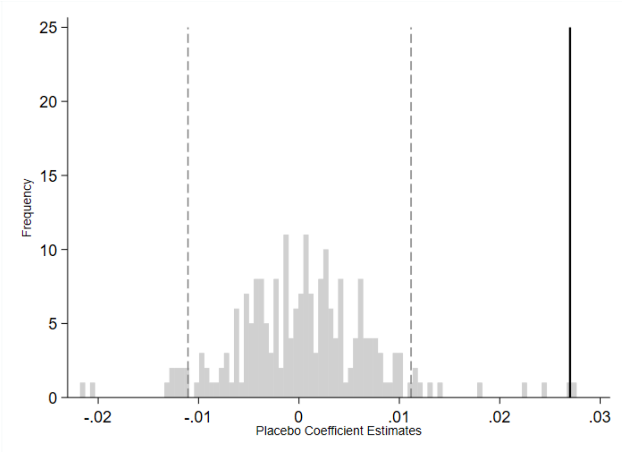
(a) Unpaid sick leave hours taken (p=0.05)



(b) Unpaid sick leave hours taken (p=0.05)



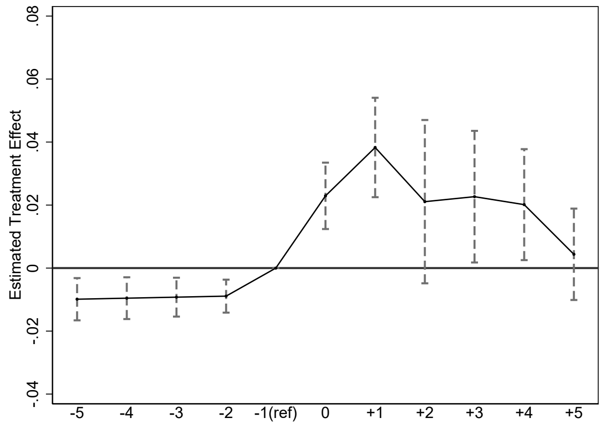
(c) Sick leave costs per hour worked (p=0.01)



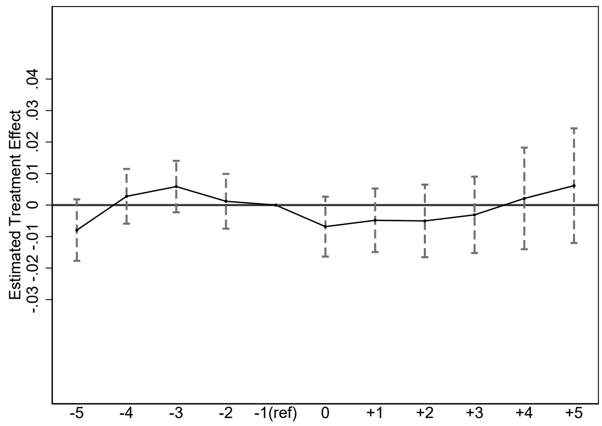
(d) Sick leave costs per hour worked (p=0.01)

Notes: Source: National Compensation Survey (NCS) 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. This figure plots the distribution of the estimated placebo regressions (n=200) that excluded treatment states and randomly assigned pseudo treatment states as compared to the true estimate. All models include firm-job fixed effects and time fixed effects. The vertical black line and corresponding bar denotes the coefficient estimates. The p-values are displayed next to the variable name.

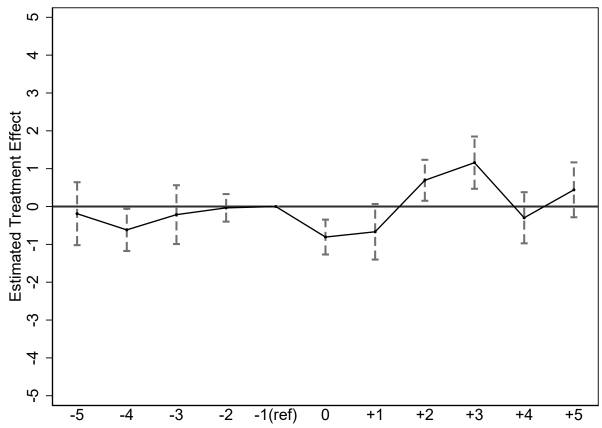
Figure B2: Event Study on Secondary Outcomes



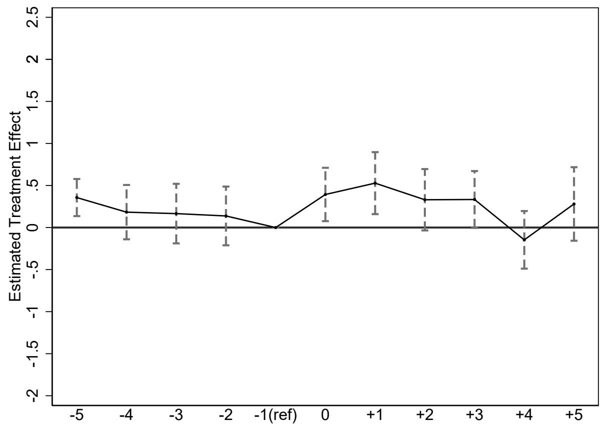
(a) Sick leave costs per hour worked



(b) Health insurance offered



(c) Annual hours holiday



(d) Wages

Notes: Source: National Compensation Survey (NCS) 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. The graphs show event studies based on DD models as in Equation (2). The corresponding point estimates are in column (4) of Table 2 (sick leave costs), column (1), Panel C of Table 6 (health insurance), column (2), Panels C of Table 7 (holiday hours), and in column (7), Panel C of Table 7 (wages). All models include firm-job fixed effects and time fixed effects. The standard errors are clustered at the state level and the gray bars depict 95% confidence intervals. For more information about the sick pay reforms, see Table A1.

Table B1: Share of Jobs with Sick Leave by Type of Job and Industry

	N	Percent with sick leave
Inflation adjusted hourly wages		
Hourly wage $<$ 15$	124,354	0.442
Hourly wage 15-25$	125,710	0.73
Hourly wage 25-35$	68,380	0.821
Hourly wage $\leq$ 35$	81,142	0.885
Employer size		
<10 employees	26,396	0.525
10-50 employees	57,333	0.52
50-100 employees	38,634	0.58
100-500 employees	114,480	0.696
$\leq$ 500 employees	162,743	0.825
Other characteristics		
Full-time employment	334,383	0.761
Part-time employment	65,203	0.268
Non-unionized	354,183	0.625
Unionized	45,403	0.714
Main employee occupations (sorted by weighted frequency)		
Office and administrative	85,343	0.753
Sales and related	36,629	0.56
Food preparation and serving	15,032	0.25
Transportation and material	26,091	0.562
Production	36,979	0.57
Health practitioners and technicians	31,167	0.816
Installation, maintenance, and repair	17,811	0.682
Management	23,356	0.919
Main employer industries (sorted by weighted frequency)		
Healthcare and social assistance	64,973	0.779
Retail trade	48,721	0.526
Manufacturing	64,595	0.659
Accommodation and food services	12,873	0.272
Admin and support and waste man. and remed. services	11,851	0.433
Professional, scientific, and technical services	11,779	0.846
Finance and insurance	59,183	0.933
Construction	17,978	0.423
Wholesale trade	16,718	0.784
Transportation and warehousing	12,494	0.72

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Data are yearly and at the firm-job level; they are weighted by BLS provided weights.

Table B2: Dropping D.C.—Effects on Coverage, Utilization and Labor Costs

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean: *(in treated localities)*	0.659	17.8	0.541	0.326
Panel A				
Sick leave mandate	0.128***	1.972***	0.439**	0.031***
$(D_c \times T_t)$	(0.035)	(0.610)	(0.199)	(0.007)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Panel B				
Sick leave mandate 0.129***	2.016***	0.438**	0.032***	
$(D_c \times T_t)$	(0.034)	(0.587)	(0.199)	(0.007)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Employee controls	X	X	X	X
Panel C				
Sick leave mandate 0.130***	2.047***	0.460*	0.032***	
$(D_c \times T_t)$ (0.041)	(0.703)	(0.243)	(15.708)	(0.009)
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
Panel D				
Sick leave mandate 0.128***	1.809**	0.478**	45.627***	0.026***
$(D_c \times T_t)$ (0.038)	(0.701)	(0.193)	(16.277)	(0.008)
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
State time trend	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. FE=fixed-effects. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Employee controls: unionized job and part-time employment. Standard errors clustered at the state level and reported in parentheses. All models have 397,771 firm-job observations. Employers below the employer size cutoff are coded as zero. See Table B7 for results after dropping these observations.

Table B3: Keeping only California and Untreated States

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean: *(in treated localities)*	0.639	17.32	0.604	0.319
Panel A				
Sick leave mandate	0.170***	2.684***	0.637***	0.040***
($D_c \times T_t$)	(0.005)	(0.095)	(0.075)	(0.002)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Panel B				
Sick leave mandate	0.171***	2.704***	0.636***	0.040***
($D_c \times T_t$)	(0.005)	(0.092)	(0.075)	(0.002)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Employee controls	X	X	X	X
Panel C				
Sick leave mandate	0.171***	2.744***	0.665***	0.041***
(0.006)	(0.103)	(0.091)	(0.002)	
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
Panel D				
Sick leave mandate	0.168***	2.512***	0.640***	0.035***
(0.006)	(0.118)	(0.089)	(0.002)	
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
State time trend	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. FE=fixed-effects. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Employee controls: unionized job and part-time employment. Standard errors clustered at the state level and reported in parentheses. All models have 372,542 firm-job observations.

Table B4: Firm-Level Aggregation—Main Treatment Effects

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean: (in treated localities)	0.588	13.22	0.604	0.203
Panel A				
Sick leave mandate	0.180***	3.552***	0.547**	0.099***
($D_c \times T_t$) (0.019)	(0.843)	(0.252)	(0.024)	
Year FE	X	X	X	X
Employer FE	X	X	X	X
Panel B				
Sick leave mandate	0.181***	3.542***	0.537**	0.098***
($D_c \times T_t$)	(0.017)	(0.668)	(0.252)	(0.022)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Employee controls	X	X	X	X
Panel C				
Sick leave mandate	0.176***	2.427***	0.263	0.057***
($D_c \times T_t$)	(0.024)	(0.923)	(0.276)	(0.018)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Employee controls	X	X	X	X
State time trend	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. FE=fixed-effects. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Employee controls: unionized job and part-time employment. Standard errors are clustered at the state level and reported in parentheses. All models have 80,506 firm-year observations.

Table B5: County-Level Aggregation—Main Treatment Effects

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean:	0.659	17.8	0.541	0.326
(in treated localities)				
Panel A				
Sick leave mandate	0.117***	1.385***	0.465***	0.033***
$(D_s \times T_t)$	(0.028)	(0.478)	(0.143)	(0.011)
Year FE	X	X	X	X
County FE	X	X	X	X
Panel B				
Sick leave mandate	0.119***	1.477***	0.470***	0.035***
$(D_s \times T_t)$	(0.026)	(0.428)	(0.149)	(0.011)
Year FE	X	X	X	X
County FE	X	X	X	X
Job controls	X	X	X	X
Panel C				
Sick leave mandate	0.149***	1.212***	0.255**	0.014
$(D_s \times T_t)$	(0.021)	(0.362)	(0.120)	(0.010)
Year FE	X	X	X	X
County FE	X	X	X	X
State Time Trends	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Yearly data at the county level. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights. Job controls: unionized job and part-time. Standard errors are clustered at the state level and reported in parentheses. All models have 8,100 county-year observations.

Table B6: State-Level Aggregation—Main Treatment Effects

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean: (in treated localities)	0.659	17.8	0.541	0.326
Panel A				
Sick leave mandate	0.107***	1.040**	0.369***	0.024**
($D_s \times T_t$)	(0.023)	(0.394)	(0.119)	(0.010)
Year FE	X	X	X	X
State FE	X	X	X	X
Panel B				
Sick leave mandate	0.110***	1.125**	0.393***	0.025**
($D_s \times T_t$)	(0.020)	(0.440)	(0.128)	(0.010)
Year FE	X	X	X	X
State FE	X	X	X	X
Job controls	X	X	X	X
Panel C				
Sick leave mandate	0.127***	0.532	0.173	0.009
($D_s \times T_t$)	(0.027)	(0.444)	(0.130)	(0.009)
Year FE	X	X	X	X
State FE	X	X	X	X
State Time Trends	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Yearly data at the state level. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Job controls: unionized job and part-time. Standard errors clustered at the state level and reported in parentheses. All models have 451 state-year observations.

Table B7: Dropping Employers below Employee Size Mandate Threshold

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean: *(in treated localities)*	0.657	17.69	0.544	0.324
Panel A				
Sick leave mandate	0.137***	2.146***	0.436**	0.034***
$(D_s \times T_t)$	(0.032)	(0.557)	(0.216)	(0.006)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Panel B				
Sick leave mandate	0.138***	2.190***	0.435**	0.035***
$(D_s \times T_t)$	(0.031)	(0.531)	(0.216)	(0.006)
Year FE	X	X	X	X
Employer FE	X	X	X	X
Employee controls	X	X	X	X
Panel C				
Sick leave mandate	0.139***	2.228***	0.458*	0.035***
$(D_s \times T_t)$	(0.037)	(0.632)	(0.263)	(0.007)
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
Panel D				
Sick leave mandate	0.136***	1.968***	0.464**	0.028***
$(D_s \times T_t)$	(0.037)	(0.659)	(0.228)	(0.008)
Year FE	X	X	X	X
Firm-job FE	X	X	X	X
State Time Trends	X	X	X	X

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Yearly data are at the firm-job level. Each column in each panel stands for one DD model as in Equation (1). ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Employee controls: unionized job and part-time employment. Standard errors clustered at the state level and reported in parentheses. All models have 393,609 firm-job observations.

Table B8: Effect Heterogeneity: Coverage, Utilization and Labor Costs

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean:	0.659	17.8	0.541	0.326
Panel A: Full-time vs. part-time				
Sick leave mandate	0.259***	1.893***	0.341*	0.025***
($D_s \times T_t$)	(0.066)	(0.536)	(0.194)	(0.006)
Sick leave mandate	-0.180***	0.233	0.168	0.011**
$\times$full-time	(0.033)	(0.265)	(0.181)	(0.005)
Panel B: Union vs. non-union				
Sick leave mandate	0.147***	2.189***	0.480*	0.030***
($D_s \times T_t$)	(0.047)	(0.805)	(0.247)	(0.010)
Sick leave mandate	-0.165***	-1.258	-0.177**	0.027
$\times$union	(0.046)	(0.921)	(0.080)	(0.018)
Panel C: Large employers (>500 employees)				
Sick leave mandate	0.154***	2.089***	0.634**	0.026***
($D_s \times T_t$)	(0.040)	(0.696)	(0.269)	(0.009)
Sick leave mandate	-0.124***	-0.154	-0.884***	0.033***
$\times$large employers	(0.017)	(0.293)	(0.256)	(0.008)
Panel D: Small employers (<50 employees)				
Sick leave mandate	0.071**	1.871**	-0.004	0.030***
($D_s \times T_t$)	(0.031)	(0.763)	(0.126)	(0.008)
Sick leave mandate	0.157***	0.503*	1.249***	0.007**
$\times$small employers	(0.022)	(0.286)	(0.372)	(0.003)

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Each column in each panel stands for one model similar to Equation (1), but augmented with triple interaction terms and all two-way interactions, see main text for details. ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Standard errors clustered at the state level and reported in parentheses. All models have 399,586 firm-job observations. All models in all panels control for year FE and firm-job FE. Controls for all other two-way interaction terms are included in all models but not shown (available upon request).

Table B9: Effect Heterogeneity: Industries and Occupations

Outcome	**Sick leave offered (1)**	**Paid sick hours taken (2)**	**Unpaid sick hours taken (3)**	**Sick leave costs per hour (4)**
Pretreatment mean: *(in treated localities)*	0.659	17.8	0.541	0.326
Panel A: Industries				
Panel A1: Construction				
Sick leave mandate	0.121***	2.030***	0.427*	0.034***
$(D_c \times T_t)$	(0.039)	(0.696)	(0.238)	(0.009)
Sick leave mandate	0.207***	0.662***	0.789***	-0.022***
$\times$construction	(0.037)	(0.211)	(0.126)	(0.004)
Panel A2: Retail trade				
Sick leave mandate	0.123***	2.103***	0.545**	0.035***
$(D_s \times T_t)$	(0.042)	(0.732)	(0.265)	(0.009)
Sick leave mandate	0.049***	-0.319	-0.613***	-0.017***
$\times$retail trade	(0.010)	(0.226)	(0.198)	(0.003)
Panel A3: Admin, support, waste mngmt., and remed. services				
Sick leave mandate	0.123***	2.110***	0.432	0.035***
$(D_c \times T_t)$	(0.042)	(0.737)	(0.270)	(0.009)
Sick leave mandate	0.069***	-0.530	0.313	-0.026***
$\times$admin services	(0.022)	(0.399)	(0.542)	(0.007)
Panel A4: Accommodation and food services				
Sick leave mandate	0.105**	1.911***	0.111	0.034***
$(D_s \times T_t)$	(0.040)	(0.685)	(0.127)	(0.009)
Sick leave mandate	0.186***	1.138***	2.682***	-0.010**
$\times$accommodation	(0.035)	(0.211)	(0.757)	(0.004)
Panel B: Occupations				
Panel B1: Food preparation and serving				
Sick leave mandate	0.107**	1.921***	0.107	0.034***
$(D_s \times T_t)$	(0.040)	(0.660)	(0.126)	(0.009)
Sick leave mandate	0.199***	1.195***	3.049***	-0.014***
$\times$food	(0.037)	(0.267)	(0.810)	(0.004)
Panel B2: Sales and related				
Sick leave mandate	0.124***	2.165***	0.531*	0.035***
$(D_s \times T_t)$	(0.042)	(0.755)	(0.265)	(0.009)
Sick leave mandate	0.053**	-0.958*	-0.627***	-0.024***
$\times$sales	(0.026)	(0.495)	(0.211)	(0.007)
Panel B3: Office and administrative				
Sick leave mandate	0.142***	2.242***	0.530*	0.036***
$(D_s \times T_t)$	(0.043)	(0.767)	(0.264)	(0.009)
Sick leave mandate	-0.075***	-1.151***	-0.433***	-0.020***
$\times$office	(0.014)	(0.387)	(0.129)	(0.005)
Panel B4: Transportation and material				
Sick leave mandate	0.130***	2.016***	0.478*	0.031***
$(D_s \times T_t)$	(0.040)	(0.692)	(0.250)	(0.009)
Sick leave mandate	-0.001	0.468*	-0.172*	0.022***
$\times$transportation	(0.019)	(0.244)	(0.088)	(0.008)

Source: NCS 2009-2017 (Bureau of Labor Statistics, 2020b), authors' own calculation and illustration. Each column in each panel stands for one model similar to Equation (1), but augmented with triple interaction terms and all two-way interactions, see main text for details. ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All models are weighted using NCS weights provided by the BLS. Standard errors clustered at the state level and reported in parentheses. All models have 399,586 firm-job observations. All models in all panels control for year FE and firm-job FE. Controls for all other two-way interaction terms are included in all models but not shown (available upon request).

Table B10: Treatment Effect on Flu Rate

Outcome	**ILI rate (1)**	**ILI rate (2)**	**ILI rate (3)**	**ILI rate (4)**
Pretreatment mean:	0.018	0.018	0.018	0.018
Sick leave mandate	-0.0054***	-0.0049***	-0.0045***	-0.0059***
($D_s \times T_t$)	(0.0019)	(0.0016)	(0.0014)	(0.0017)
Unemployment		0.0005	-0.0002	
		(0.0007)	(0.0006)	
Other Controls	No	No	Yes	No
Observations	16,820	16,820	16,820	16,493

Sources: Centers for Disease Control and Prevention; Weekly U.S. Influenza Surveillance Report; National Center for Health Statistics Mortality Surveillance Data; U.S. Census Bureau; U.S. Bureau of Labor Statistics; Kaiser Family Foundation; Child Care Influenza Immunization Action Coalition; NOAA; own calculations. Each column in each panel is one difference-in-differences model. The dependent variable is the weekly rate of influenza like illnesses (ILI) as a share of outpatient doctor visits between October 2010 and March 2017. The estimation equation differs from Equation (1), because this data is at state level and not at employer level. ***, **, and * = statistically different from zero at the 1%, 5%, and 10% level. All regressions are weighted by the state populations. Standard errors clustered at the state level and reported in parentheses. Columns (2) and (3) control for the state unemployment rate, additionally Column (3) includes as "other controls" the state population share with health insurance coverage, whether the state expanded Medicaid at the time, the population share above the age of 65, and whether the state had an influenza vaccination mandate for children, as well as the precipitation level. Column (4) drops Washington DC from the sample.

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