Labor Market Effects of US Sick Pay Mandates

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Abstract

This paper exploits temporal and spatial variation in the implementation of US sick pay mandates to assess their labor market consequences. We use the Synthetic Control Group Method (SCGM) and the Quarterly Census of Employment and Wages (QCEW) to estimate the causal effect of mandated sick leave on employment and wages. We do not find much evidence that employment or wages were significantly affected by the mandates which typically allow employees to earn one hour of paid sick leave per work week, up to seven days per year. Joint tests for all treatment regions let us exclude, with 99% statistical probability, that wages decreased by more than 1% as a result of the mandates. With 97% probability, we can exclude that employment decreased by more than 1%.

Keywords: Sick Pay Mandates, Sick Leave, Medical Leave, Employer Mandates, Employment, Wages, Synthetic Control Group, Quarterly Census of Employment and Wages (QCEW), United States

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

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1 Introduction

As part of the first federal health insurance legislation, paid sick leave was one of the first social insurance pillars worldwide. 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 as well as medical care. Insurance against wage losses due to health shocks was a crucial element of health insurance at that time, and valued by employees and unions alike. Given the limited availability of expensive medical treatments in the 19^{th} century, expenditures for paid sick leave initially accounted for more than half of all health insurance expenditures (Busse and Riesberg, 2004). Increasingly more European countries implemented paid sick leave and today, virtually every European country has some form of universal access to paid sick leave—with varying degrees of generosity.

The US, Canada and Japan are the only industrialized countries that do not provide universal access to paid sick leave, which is largely provided as a fringe benefit by employers on a voluntary basis (Heymann et al., 2009). Coverage rates among full-time workers are around 65%; low-income, part-time and service sector workers have coverage rates of less than 20% (Lovell, 2003; Boots et al., 2009; Susser and Ziebarth, 2016). Susser and Ziebarth (2016) estimate that, in a given week of the year, the total demand for paid sick leave sums to ten percent of the workforce in the US. In addition to concerns concerning inequality, worker well-being, and productivity, a lack of sick leave coverage can induce contagious employees to work sick and spread diseases (Pichler and Ziebarth, 2015).

In the last decade, support for sick leave mandates has grown substantially in the US. On the city level, sick leave mandates were passed and implemented in San Francisco (2007), Washington D.C. (2008), Seattle (2012), New York City (2014), Portland (2014), Newark (2014), Philadelphia (2015), and Oakland (2015). More cities and counties have followed more recently.[1]

[1] For example, Montgomery County, MD (effective 10/1/2016), Cook County and Chicago, IL (effective 7/1/2017), or Minneapolis and Saint Paul, MN (effective 7/1/2017) have passed legislation recently, among others.

On the state level, Connecticut was first to mandate paid sick leave in 2012. However, the bill excludes businesses with less than 50 full time employees and only applies to the service sector. Consequently, it only covers about 20% of the workforce (Miller and Williams, 2015; Connecticut Department of Labor, 2015). In contrast, California passed a much more comprehensive bill—covering all employees—effective July 1, 2015. Massachusetts and Oregon also passed relatively comprehensive sick leave mandates, effective July 2015 and January 2016, but exempt small businesses. In addition, Vermont (1/1/2017), Arizona (7/1/2017), and Washington State (1/1/2018) passed sick leave legislation very recently. Appendix B1 lists all city-wide (9) and state-wide (4) sick pay mandates that this paper will evaluate.

On the federal level, reintroduced in Congress in 2015, the *Healthy Families Act* proposes a federal sick leave mandate that would cover employees in businesses with more than 15 employees (US Congress, 2015). Similar to the mandates already in place at the state or city level, the *Healthy Families Act* proposes that employees 'earn' one hour of paid sick leave per 30 hours worked, up to 56 hours (or 7 days) per year. Paid sick leave—at the standard wage rate of 100%—can then be taken in case of own sickness or sickness of a relative, in most cases sickness of children.

The main source of controversy related to government mandated sick leave is the possibility that such policy could hurt employment or wage growth. The standard economics textbook example of mandated benefits argues (Summers, 1989): Employer mandates may be more efficient than a direct provision of benefits by the government (funded by higher taxes), as long as employees value the benefit and would accept lower wages in return. Gruber (1994) studies the impact of maternity leave mandates on employment and wages in the US. He argues that the case for a group-specific mandate may be different because anti-discrimination laws or social norms may prohibit the free downward-adjustment of wages for a specific identifiable group. However, using the CPS, he does find significant wage decreases for women of childbearing age, but no significant impact on labor supply.

The case of mandated sick leave benefits may also deviate from the textbook example. When employees earn one hour of paid sick leave per 30 hours worked—assuming that wages could freely adjust and ignoring administrative costs—this would equal a wage increase of 1/30 or 3.3% per week for full-time employees. However, this static calculation assumes that all employees would fully exhaust their annual sick leave credit and would have worked sick with full productivity (or taken unpaid leave) in the counterfactual scenario. Empirically assessing and directly measuring labor productivity under the two scenarios is extremely challenging (if not impossible). To our knowledge, there exists no credible empirical causal evidence on how work productivity changes when employees gain access to paid sick leave. It seems likely that sick employees cannot maintain full work productivity when working sick and that employees on sick leave will compensate for lost productivity after recovery. Hence, the calculated static wage increase of 3.3% appears to be an upper bound for marginal firms.

When abstaining from administrative costs, changes in work productivity, psychological costs or benefits, the textbook example predicts that sick pay mandates would reduce wage growth. However, if wages cannot flexibly adjust because of social norms, anti-discrimination laws, or because employees do not value sick leave, marginal employees might not get hired or even be laid off. In addition, when small businesses are exempt from the mandate, marginal employers may want to become exempt by reducing their workforce or by splitting up big firms into smaller ones. In sum, under several plausible scenarios, the standard textbook example may not hold up in reality. Then, it becomes essentially an empirical question whether wages and employment would significantly be affected by sick pay mandates.

This paper empirically assesses how wages and employment have been affected by the implementation of nine city-wide and four state-wide sick pay mandates (listed in Table B1). We generate two datasets from the Quarterly Census of Employment and Wages (QCEW) which is provided by the Bureau of Labor Statistics (BLS). The QCEW covers 97% of non-farm employment in the US. It is a census of all establishments that are covered by Unemployment Insurance (UI) and is also used by the BLS as an anchoring benchmark for other establishment surveys such as the Current Employment Statistic (Bowler and Morisi, 2006). The data are provided by establishment size and industry. Our first QCEW dataset records total monthly employment and quarterly wages at the county–industry level from January 2001 to June 2016. The second dataset records total monthly employment and quarterly wages at the state–industry–firm-size level from

January 2001 to June 2016. Econometrically, we exploit the quasi-random nature of the implementation of the sick pay mandates across US regions and over time. To mimic pre-treatment trends as closely as possible, we follow Abadie and Gardeazabal (2003) and Abadie et al. (2010) and build synthetic control groups using the rich set of untreated regional units available. In a recent review of the state of applied econometrics, Athey and Imbens (2016) call the Synthetic Control Group Method (SCGM) the most important innovation in program evaluation in the last fifteen years.[2] Finally, we use the approach suggested in Dube and Zipperer (2015) for hypothesis testing with single and multiple events.

The setting exploited in this paper carries additional advantages over the standard example of one treated region and a limited number of potential 'donor' control regions to choose from. First, because we evaluate reforms at the county level, we can build synthetic controls out of a pool of more than 3,000 US counties. This allows us to replicate the labor market dynamics of the treated counties very closely. Second, because the treated units are rather small, the assumption of no general equilibrium or spillover effects to neighboring regions seems to be justified. Third, we can build synthetic controls that match the labor market dynamics of the treated units for a long pre-reform time period, thereby increasing their validity. Fourth, we evaluate sick pay mandates in a dozen different counties and four states. All these US regions were treated with similar reforms. The treatments were implemented subsequently over a decade and the counties and states are very heterogeneous in terms of their size and labor markets. As such, the findings are based on broad regions of common support with a high degree of external validity for other US counties. Also, the validity of the main identification assumption of no systemic unobserved post-reform labor market shocks is high when evaluating thirteen different mandates which were implemented over a decade and many diverse regions.

Our findings do not provide much evidence that either wages or employment significantly and systematically increased or decreased post-reform. The point estimates for private sector employment as a share of the total county population have ambiguous signs and are relatively small in size. Joint tests for all nine treated cities let us exclude

[2] Other papers that apply SCGM are Billmeier and Nannicini (2013); Bohn et al. (2014); Bauhoff (2014); Bassok et al. (2014); Karlsson and Pichler (2015); Restrepo and Rieger (2016).

with 92% statistical probability that employment decreased by more than 1%. The city joint tests for wages let us exclude with 99% statistical probability that wages decreased by more than 1%. Moreover, when evaluating the four states, joint tests yield tiny non-significant employment estimates of 0.1%. The non-significant point estimates for wage dynamics are even positive and we can conclude with more than 99% statistical accuracy that wages did not decrease by more than 1% (if at all).

However, in case of Connecticut (the first state law), we do find some evidence that employment dynamics for affected industries (private service sector firms with more than 49 employees) have developed slightly weaker after the sick pay mandate became effective. However, the negative 2% point estimate is only statistically significant at the 12% level. We interpret this as suggestive evidence that some large companies may have reduced employment or split up firms to avoid the regulation when sick pay mandates are not comprehensive and allow for exceptions in the same industry and state.

The next section summarizes the existing literature. Section 3 discusses the US sick pay mandates in more detail, and Section 4 provides details on the data. The empirical approach and identifying assumptions are in Section 5. Section 6 discusses the empirical findings, and Section 7 concludes.

2 Existing Research on Sick Leave

Existing economic research on sick leave almost exclusively focuses on countries outside the US. In the past, the reason has been simply a lack of government sick leave mandates and a lack of appropriate data. Whereas high-quality administrative sick leave data exist in most Scandinavian countries (Andrèn, 2007; Markussen et al., 2011; Dale-Olsen, 2014), actual sick leave behavior in the US is largely unobservable. There are a few exceptions. One exception is Gilleskie (1998) who exploits 1987 MEPS data both on work absence behavior and demand for medical care to structurally model work absence behavior and simulate the effects of alternative policies. According to Gilleskie (1998), about a quarter of all male employees would not take sick leave when ill. Susser and Ziebarth (2016) use the representative 2011 ATUS Leave Supplement to estimate that, in a given week of the year, two percent of US employees—mostly low-income female employees—would go to work sick. In almost half of all cases, the reasons indicated for such presenteeism behavior were directly related to a lack of paid sick leave coverage. Ahn and Yelowitz (2016) confirm that US employees take more sick leave when they have paid sick leave coverage. Colla et al. (2014) find that 73% of all firms in San Francisco offered paid sick leave before the sick pay mandate in 2006, and that this share increased to 91% in 2009. Some reports suggest that the early mandates in San Francisco and DC did not have negative employment effects (Boots et al., 2009; Appelbaum and Milkman, 2011; van Kammen, 2013). Using 2009 to 2012 data from the American Community Survey (ACS) Ahn and Yelowitz (2015) come to a similar conclusion for Connecticut.

Outside the US, several empirical papers estimate the causal effects of variation in sick pay, and find that employees adjust their intensive labor supply in response (Johansson and Palme, 1996, 2005; Ziebarth and Karlsson, 2010, 2014; De Paola et al., 2014; Dale-Olsen, 2014; Fevang et al., 2014). The focus of these papers naturally differs from others that study extensive labor supply effects of disability insurance (Autor and Duggan, 2006; Burkhauser and Daly, 2012; Kostol and Mogstad, 2014; Borghans et al., 2014; Burkhauser et al., 2015). It is closer to US studies on work-related accidents and diseases covered by Workers' Compensation (Meyer et al., 1995; Campolieti and Hyatt, 2006; McInerney and Bronchetti, 2012; Hansen, 2016).

Other papers on sickness absence investigate general determinants (Barmby et al., 1994; Markussen et al., 2011; Dale-Olsen, 2014), probation periods, known to reduce absenteeism (Riphahn, 2004; Ichino and Riphahn, 2005), culture (Ichino and Maggi, 2000), gender (Ichino and Moretti, 2009; Gilleskie, 2010; Herrmann and Rockoff, 2012), income taxes (Dale-Olsen, 2013), union membership (Goerke and Pannenberg, 2015), and unemployment (Askildsen et al., 2005; Nordberg and Røed, 2009; Pichler, 2015). There is also research on the impact of sickness absence on earnings (Sandy and Elliott, 2005; Markussen, 2012). Finally, a few papers study the phenomenon of presenteeism explicitly (Aronsson et al., 2000; Chatterji and Tilley, 2002; Brown and Sessions, 2004; Pauly et al., 2008; Barmby and Larguem, 2009; Markussen et al., 2012; Pichler, 2015; Pichler and

Ziebarth, 2015).[3] For example, Pauly et al. (2008) ask 800 US managers about their views on employee presenteeism with *(a)* chronic and *(b)* acute diseases.

Finally, note that paid sick leave differs from paid vacation or paid maternity leave in both scope and aim (Gruber, 1994; Ruhm, 1998; Waldfogel, 1998; Rossin-Slater et al., 2013; Lalive et al., 2014; Thomas, 2015; Baum and Ruhm, 2016; Dahl et al., 2016). Whereas paid sick leave is an insurance against wage losses due to health shocks, paid vacation and maternity leave mostly aim at balancing family and work and address gender inequality in the workplace. Sick leave mandates, on the other hand, can also be justified from a public health perspective—because access to paid sick leave reduces contagious presenteeism and the negative externalities associated with the spread of contagious diseases (Pichler and Ziebarth, 2015; Stearns and White, 2016).

3 US Sick Pay Mandates

The US is one of the very few industrialized countries without universal access to paid sick leave. About half of the workforce lacks access to paid sick leave, particularly low-income employees in the service sector (Heymann et al., 2009; Susser and Ziebarth, 2016).

Table B1 in the Appendix provides a summary of all US sick pay reforms evaluated in this paper. The details of the bills differ from city to city and state to state, but basically all sick pay schemes represent employer mandates. Several mandates exclude small firms or offer exemptions. Employees "earn" paid sick leave credit (typically one hour per 30-40 hours worked) up to one week per year and, if unused, the credit rolls over to the next calendar year. Because employees need to accrue sick pay credit, most sick pay schemes explicitly state a 90 day accrual period. However, the right to take *unpaid* sick leave is part of most sick pay schemes.

As Table B1 shows, San Francisco was the first city to mandate paid sick leave effective February 5, 2007. Washington DC enacted its mandate effective November 13, 2008 and expanded the mandate on Feb 22, 2014 to include temporary workers and tipped

[3] Outside of economics, the literature on 'presenteeism' is richer (Dew et al., 2005; Schultz and Edington, 2007; Hansen and Andersen, 2008; Johns, 2010; Böckerman and Laukkanen, 2010; Peipins et al., 2012)

employees. Seattle (September 1, 2012), Portland (Jan 1, 2014), New York City (April 1, 2014), and Philadelphia (May 13, 2015) followed subsequently.

Connecticut was the first state to mandate paid sick leave on January 1, 2012. However, the law only applies to service sector employees in non-small businesses and covers only about 20% of the workforce. The recently introduced bills of California (July 1, 2015), Massachusetts (July 1, 2015), and Oregon (Jan 1, 2016) are much more comprehensive (see Table B1).

4 Quarterly Census of Employment and Wages (QCEW)

The paper makes use of publicly available data from the QCEW provided by the Bureau of Labor Statistics (BLS) (2016). The QCEW comes from an establishment census. All establishments covered by US Unemployment Insurance (UI)—97% of all US civilian employment—are included.[4] Using the quarterly UI contribution reports filed by the establishments, the BLS calculates the number of actually filled jobs per month as well as the average weekly wage per quarter.

The BLS reports the data in different levels of spatial and timely disaggregation. To evaluate reforms at the *(a)* county, and *(b)* state level (see Table B1) we generate two datasets, one at the *(a)* county level and one at the *(b)* state level. The raw data are reported by industry. Because the sick pay mandates mostly apply to private sector jobs, we generate variables that measure private sector employment and private sector wages. The county level data are provided for the time period from January 2001 to June 2016, and the state level data are provided for the time period from January 2001 to June 2016.

County Level Data. Table 1 provides the summary statistic for the *(a)* county level data and all variables generated and employed in the analysis. The table shows summary statistics for the universe of 3,069 counties between 2001 and June 2016. In total, the United States has 3,143 counties or county-equivalents. The 74 missing counties in our data are counties without any official establishment location, e.g., in very rural counties in Alaska (United States Census Bureau, 2016a). As for employment, we have monthly

[4] Not included are self-employed, army members, railroad employees, most elected officials, and most farm workers.

data points for each county over a total of 186 months, yielding 569,004 county-month observations. As for the quarterly wage data, we have 186,720 county-quarter observations. Population counts vary at the annual level and yield 45,888 total county-year observations.

Employment and Wage Measures. We generate two main outcome variables of interest for the county level analysis. The first is *Private Sector Employment.* We use the total number of filled jobs at the monthly county level—and as reported by the QCEW—and divide by the county level population as reported by the United States Census Bureau (2016b). Hence, we obtain county-specific *Private Sector Employment* for each US county on a monthly basis from 2001 to June 2016. Table 1 shows that the average private sector employment share is 27.2%, and the average public sector employment share is 7.7%. This means that, on average, for every 100 residents in a county in the US, 27 private sector jobs paying UI contributions are officially reported.

Note that individuals who hold multiple jobs are counted for every job they hold. In addition, filled jobs are assigned to counties by the physical address of the establishment, not by the county of residence of the jobholder. These are the two reasons (in addition to economic prosperity), why some counties have significantly higher employment ratios than others, and even employment ratios above 100%. Whereas the minimum value for the private sector employment share is a mere 1.1%, the county with the highest employment share reaches a value of 404% (Table 1).

The second variable of interest is *Weekly Wages.* Specifically, employers paying UI contributions report total quarterly gross compensation, including bonus payments and stock options. Wages are then calculated by dividing the total quarterly compensation by the total quarterly employment. Dividing by the number of weeks in a quarter yields the weekly wages displayed in Table 1. Because wages are only reported on a quarterly basis, the number of unique observations decreases to 189,720. The average weekly wage is \$602 (or \$31.4K per year), but the variation ranges from \$155 to \$4,542. Because quarterly Consumer Price Indices are not available at a lower regional level, we use nominal wages as reported by the QCEW. We net out seasonal fluctuation in wages by regressing each time series of each region on a full set of quarter-year fixed effects.

[Insert Table 1 about here]

Finally, Table 1 shows that the average county population is 97.6K. However, the standard deviation is large and 339K. Los Angeles County is the largest county with 10.1M population.

State Level Data. Table 2 provides the summary statistic for the *(b)* state level data. When considering all 51 states, we obtain 51(states)$\times$186(months)=9,486 state-month observations for employment and 51(states)$\times$62(quarters)=3,162 state-quarter observations for wages. Using the state level data, this paper evaluates the sick pay mandates in Connecticut, California, Massachusetts and Oregon. The Connecticut mandate only applies to firms with more than 49 employees in the service sector and the mandates in Massachusetts and Oregon only apply to firms with more than 9 employees (Table B1). Because the QCEW data are broken down by industry, we can carve out employment and wage dynamics for the service sector in Connecticut. In addition, the QCEW state level data are also provided by establishment size which helps us to define the treatment groups in a very precise manner. For Connecticut, we generate variables for the firm size *>49 employees*. For Oregon and Massachusetts, we generate all variables for the category *>9 employees*.[5]

Employment and Wage Measures. Analogous to the county level approach, we assess the employment and wage dynamics for four US states. The upper panel of Table 2 shows that, overall, private sector employment was 37.6 and public sector employment 8.1 per 100 residents. The average weekly wage was $813 and the state-wide population 5.9 million.

The lower panel of Table 2 lists, for Connecticut, *Private Service Sector Employment, >49 employees* as one main outcome variable. Across all US states and between 2001 and June 2016, for every 100 residents of a state, 15.4 people worked in the service sector

[5]Because the data by industry and establishment size are only reported for the first quarter of each year, we impute values for the rest of the year. To do this, we need to make one (reasonable) assumption: That the ratios of, e.g., *<50 employees* vs. *>49 employees* in the first quarter remain stable in the other three quarters. For two firm size categories in Delaware, "fewer than 5 employees per establishment" and "500 to 999 employees per establishment" we have missing data in 2014. We impute the missing values by taking the shares of large and small firms in 2013 along with the monthly employment and quarterly wage data.

and in establishments with more than 49 employees. For Oregon and Massachusetts, following the specifics of the law, we use *Private Sector Employment, >9 employees* as the employment outcome measure (20.3 per 100 pop.). For California, where the law does not make exemptions for small businesses, we accordingly use *Private Sector Employment (37.6 per 100 pop.).*

[Insert Table 2 about here]

Treatment Regions. Table B1 in the Appendix provides the list of cities and states that we evaluate in this paper. However, we only evaluate the effects in Washington DC and Hudson County (Jersey City, NJ) as illustrative examples of cases where SCGM cannot be used due to a poor fit. In case of DC, the fit is poor because *(a)* DC has a very unique employment structure with many non-profit, public sector, and lobbying jobs. Thus, finding appropriate control counties for DC is very challenging. *(b)* DC's original mandate had many exemptions that are difficult to model with our data (e.g. no health care or restaurant workers). Moreover, DC extended the mandate in September 2014, but retrospectively effective February 2014. *(c)* The first DC mandate became effective shortly after the Great Recession hit in October 2008 which makes it very difficult to disentangle labor market effects due to the mandate from the confounding effect of the recession. Because of *(a)*, the recession also affected DC differently than most other US counties.

As for the counties, the second column in Table B1 lists all those that we will formally evaluate. The case for San Francisco (SF) is clear given that the city boundaries equal the county boundaries. However, in the case of Seattle, Portland, Newark, Jersey City, and New York City the county boundaries are not identical to the city boundaries where the mandate formally applied. Portland almost entirely lies within Multnomah County, but small portions fall into Clackamas and Washington County which also include large(r) parts that do not belong to Portland. As for Seattle, Newark and Jersey City: they all lie *within* the county that we use as treatment unit. For example, in 2014, King County had 2,079,967 residents but Seattle only 668,342. Essex County had 795,723 residents but Newark only 280,579. And Hudson County had 669,115 residents in 2014, but Jersey City only 262,146 (United States Census Bureau, 2016c). The fact that these three cities formally only count a third of the total county population simply means that we evaluate the intend-to-treat (ITT) effect for the entire county, not just the core city as in case of SF, NYC, or Portland. Comparing the results for the two sets of treatment groups allows us to indirectly test whether firms re-located just outside the city boundaries to circumvent the mandate. This hypothesis would be reinforced, for example, if we found negative employment effects for the core cities but no impact when assessing employment in the entire county that surrounds the city.

Lastly, we do not separately evaluate the five counties of New York City (NYC) but aggregate them to one regional NYC unit for two reasons: The five regions together represent the entire area where the law formally applied. Employment ratios and wages in Manhattan are extremely high and they are relatively low in the other NYC counties. Most people who work in NYC live in one of the four surrounding counties and commute to Manhattan. NYC can be seen as one integrated labor market and not four separate ones. For these reasons, we treat NYC as one statistical unit.

Control Regions. We employ the SCGM to model an ideal hypothetical control unit for each treatment unit. For example, as for the county level evaluation, Table 1 lists the variables *county population*, *public sector employment*, *non-service* and *service sector employment*, and *private sector wages* which we use to find suitable control "donor" counties. In other words, in addition to having identical pre-reform outcome dynamics, we seek control counties with similar employment and population structures as the treatment counties. Tables A1 to A4 list all donor counties and states used to model the pre-treatment employment and wage dynamics of each treatment unit as closely as possible.

Sample Selection. The county level dataset contains information on 3,069 individual counties. However, in order to proceed with the SCGM as described in the next section, we pre-select the total pool of counties (which we do not do for the state analyses). The main reason for this pre-selection is that running the SCGM with 3,069 donor counties would not technically be feasible due to multiple equilibria and too many degrees of freedom. We pre-select potential donor counties based on similarities in the dimensions: *county population*, *private sector employment* and *private sector wages*. To be specific, we *(a)* separately rank all 3,069 available counties along all three dimensions. Then, we *(b)* select all counties ranked above and below the treated county using a bandwidth of

500 ranks for the first dimension *county population*. Next, we *(c)* proceed with the same procedure on dimension two and three. Finally, we *(d)* use the counties that overlap on all three dimensions and fall within a ranking bandwidth of +/- 500 ranks on each dimension. This pre-selection procedure results in about 200 potential control counties for each treatment county (see the denominator in column (5) of Table 3 for the exact number), which are similar in terms of population and labor market structure.

To harmonize the analysis, we additionally restrict both datasets in Tables 1 and 2 as follows: *(i)* For each treatment region, we focus on 4 pre-treatment years (48 months or 16 quarters). *(ii)* We evaluate up to 3 post-treatment years (36 months or 12 quarters) but, depending on when the mandate was enacted, the length of the post-reform periods differ by treatment region.

5 Empirical Approach: The Method of Synthetic Control Groups

To evaluate the causal effect of sick pay mandates on employment and wage dynamics, we make use of Abadie and Gardeazabal (2003)'s Synthetic Control Group Method (SCGM). The basic idea is to use fractions of several control units to build an ideal—synthetic—control group whose pre-reform outcome dynamics are very similar to those of the treatment group (Abadie et al., 2010). The treated-control difference in post-reform outcome dynamics is then taken to assess the causal effect of the reform.

In our context, following Table B1, the treatment units are counties or states that implemented sick leave mandates, and all potential control units consist of the remaining US counties or states. We analyze the effects for each treatment unit separately. Thus, the notation below refers to one single treatment unit and J control units.

y_{it}^0 denotes the natural logarithm of the outcome ($y_{it}^0 = ln(Y_{it}^0)$ that would have been observed in country i at time t in the absence of the sick pay mandate. Moreover, y_{it}^1 denotes the natural logarithm of the outcome for the treated county i at time t, where sick pay mandates were implemented at time $T_0 + 1$. We assume $y_{it}^1 = y_{it}^0 \forall t = 1, \ldots, T_0, \forall i = 1, \ldots, J + 1$.

Abadie et al. (2010) suggest that the counterfactual y_{it}^0 can then be represented by a factor model:

$$y_{it}^{0} = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \epsilon_{it} \tag{1}$$

where δ_t is a common time effect, θ_t is a vector of possibly time-dependent coefficients, λ_t is a vector of unobserved common factors, and μ_i is a vector of unknown factor loadings.

The SCGM allows for some degree of endogeneity in the treatment indicator—the treatment can be correlated with unobservables. First, applied to our case, one necessary assumption is that employment rates and wages of the control counties are not affected by the treatment, i.e., sick pay mandates. This implies the absence of spatial employment spillovers. Note that, in our case, the treated counties are rather small and thus very unlikely to trigger large labor market spillover effects. Tables A1 to A4 list all single counties and states used by SCGM to build the synthetic control units for each treatment unit. For example, the synthetic control counties to evaluate King County (WA) are—making the 'no spatial labor market spillover' assumption very reasonable: Fulton (GE), Denver (CO), San Mateo (CA), Santa Klara (CA), Durham (NC), Richmond City (VA), and Midland (TX).

Second, as in traditional parametric difference-in-differences (DiD) models, one also assumes away shocks affecting the outcome differently for treatment and control groups in post-reform years. In our case, shocks violating this assumption would be employment policies that are correlated with sick pay mandates. However, one could argue that the SCGM controls for such shocks (better than traditional methods) because the control units—the synthetic control groups—are by construction generated to produce identical outcomes to the treated unit, including unexpected exogenous shocks.

Third, and again similar to traditional DiD methods, treatment-induced geographic migration would lead to biases. When employment prospects worsen due to sick pay mandates and employees lose their jobs (or experience stagnant wages), they might migrate to more prosperous counties. Also, firms could leave the cities or states in response to the laws. For several reasons, economic migration is not a severe issue in our context. First, our data and outcome measures allow to directly test for such migration pattern. In fact it is precisely one objective of this paper to test for changes in *normalized* employment. Recall that we use official population and normalized employment data. In addition, we stratify the effects by the time since implementation and would thus identify negative wage or employment effects in the short-run. As above, it is extremely unlikely that the few control counties—chosen out of a total of 3,069 US counties—are contaminated by worker or firm migration from the treatment counties.

Lastly, in most SCGM settings, only one single treatment unit is evaluated. In our setting, we rely on a total of 13 different treatment units, counties and states of different sizes. There is some probability that single unobserved shocks to single treatment units may confound the evaluation of one county or state. But the probability that all 13 treatment units will be all coincidentally affected by random unobserved labor market shocks goes to zero.

5.1 Implementation

SCGM requires the estimation of two matrices: V is the weighting matrix determining the relative predictive power of Z_i and of y_{it}^0. The vector W is a vector of non-negative weights given to the J control countries. The criterion to be minimized is:

$$\|\bar{X}_1 - \bar{X}_0 W\|_V = \sqrt{(\bar{X}_1 - \bar{X}_0 W)' \, V \, (\bar{X}_1 - \bar{X}_0 W)}, \tag{2}$$

where $\bar{X}_j$ is a vector of averages over the pre-treatment elements of Z_i and y_i, for both treated and control units. In our case, $\bar{X}_j$ includes the variables listed in Tables 1 and 2 (except for population). This means, for the county level analysis, $\bar{X}_j$ includes *private sector employment*, *private sector wages*, *public sector employment*, *service* and *non-service sector employment*. For the state level analysis, $\bar{X}_j$ includes *service sector employment* in large and small firms, *non-service sector employment* and *service sector wages* in large firms, as well as *public sector employment*.

As such, depending on the variable weight ($W^*(V)$), we obtain an optimal weight matrix among all diagonal positive definite matrices. The elements of V are chosen to minimize the distance to the outcome variable. In other words, an optimal weight matrix minimizes the root of the mean squared prediction error (RMSPE) for all pre-reform periods:

$$RMSPE = \sqrt{\frac{\sum_t (y_t^1 - y_t^0 W^*(V))^2}{T_0}}, \tag{3}$$

where T_0 represents the number of pre-reform time periods, i.e., in our case 48 months or 16 quarters.

5.2 Treatment Effects and Inference

In addition to calculating the RMSPE for the pre-treatment period, we also calculate the RMSPE for the post-reform period as well as the ratio of the two, as suggested by Abadie et al. (2010). Whereas the RMSPE for the pre-reform years can be used as an indicator to assess the fit of the synthetic control group, the ratio between post and pre RMSPE indicates the size of a possible treatment effect. Assuming that model fit is stable over time, a ratio larger than 1 indicates that the average differences between treated and synthetic control group is larger (in absolute terms) post as compared to pre-reform, indicating a potential treatment effect.

One disadvantage of this *RMSPE Ratio* (=RMSPE post/RMSPE pre) is, however, that it only yields a relative measure of the treatment effect. Moreover, the sign of the treatment effect remains ambiguous. Hence, we calculate the *Percent Treatment Effect (PTE)* as

$$PTE = \frac{\sum_{T_0+1}^{T} (y_t^1 - y_t^0 W^*(V))}{T - T_0}, \tag{4}$$

and the *Level Treatment Effect (LTE)* as

$$LTE = \frac{\sum_{T_0+1}^{T} (Y_t^1 - Y_t^0 W^*(V))}{T - T_0}. \tag{5}$$

Note that, theoretically, the sign of the treatment effect could change over time. Then positive and negative effects would cancel each other out and bias the *PTE* and *LTE*. Still, in this case, both indicators would provide evidence on the cumulative sign and size of the long-run effect over all post-reform periods.

In terms of inference, we follow Abadie et al. (2010) and run placebo estimates. Because we assess multiple treatments at different points in time, we first construct placebo estimates for each individual treatment unit. Then we rank the treated and all placebo estimates by their *RMSPE Ratio*. Following Abadie et al. (2010), the rank of the true treatment unit relative to the N placebo estimates then determines the p-value of the H_0 hypothesis of no treatment effect. As for the *RMSPE Ratio*, this means that the *RMSPE Ratio* of the treated unit is smaller or equal to the *RMPSE Ratio* of the placebo counties (H_0 : *RMPSE Ratio*$_{Treat}$ $\leq$ *RMPSE Ratio*$_{Placebo}$). Formally, we calculate the percentile rank $p = \hat{F}(RMSPERatio_e)$, where $\hat{F}$ stands for the empirical cumulative distribution of all *RMSPE Ratios*, as obtained from the placebo estimates. For instance, if the true treatment county had the highest rank among 99 + 1 (placebo + treatment) counties, the p-value would be 1/100 = 0.01, one would reject the H_0, and the treatment effect would be highly significant. In the results section, we carry out this testing procedure for the *RMPSE Ratio*, the *LTE* and the *PTE*. Finally, we follow Dube and Zipperer (2015) and calculate joint p-values based on the sum of the previously obtained p-values using the Irwin-Hall distribution.

As in the standard parametric case, p-values could be statistically insignificant for two reasons: either there is no effect, or we do not have enough statistical power to identify an effect. To assess the statistical power of our estimates, we test the p-value of alternative hypotheses, thereby analyzing how broad or narrow the confidence intervals are—following Dube and Zipperer (2015): Calculating the *PTE* and *LTE*, we carry out all N placebo estimates as above, but now assume that the true effect was x percent or q changes of natural units. Then we assess the probability with which our treated unit originates from that distribution, thereby calculating p-values. Using the notation above this means that we calculate $p = \hat{F}(PTE - x)$ and $p = \hat{F}(LTE - q)$. To provide additional intuition: In the SCGM setting, placebos are usually run to see how the treated unit differs from the placebos. The placebos are, by definition, non-treated units and should thus have a treatment effect of zero. Using the distribution of placebo treatment effects, one can then derive the likelihood that the treated unit stems from this (non-treated) distribution. Here, we just slightly modify this basic idea and impose an artificial treatment effect of x (percent) or q (changes) on the placebos. Then, as in the standard case, we assess the likelihood that the treated unit comes from this modified distribution of placebos.

6 Results

We begin by evaluating the employment effects of US sick leave mandates. Then, we evaluate wages effects for the city level mandates as well as Connecticut, Massachusetts, California and Oregon.

6.1 Evaluating Employment Effects of US Sick Pay Mandates

Figure 1 shows the development of normalized county level employment in seven treatment counties and four states (see Table B1). The composition of each synthetic control county—the weights W of the J control counties—-are displayed in Tables A1 and A2. The solid lines represent the treatment counties and the dashed lines represent the synthetic control counties. The solid vertical lines at point zero on the x-axes represent the month when the sick pay mandates became officially law of the city, i.e., became effective and were enforced. The dotted lines to the left indicate the months when the bills were passed; they facilitate an assessment of whether there is evidence of anticipation effects. The dotted lines to the right of the vertical law effectiveness lines indicate when the accrual periods were over. Recall that most bills stipulate a three month accrual period during which sick days could be earned, but not taken. To be specific, during the three month accrual period, *paid* sick leave could not be taken but many employees gained legally guaranteed access to unpaid sick leave (Section 3).

[Insert Figure 1 about here]

We learn the following from the left column of Figure 1: First, the counties exhibit different employment levels that can be substantial. Whereas San Francisco and King County have employment levels of around or above 50%, the levels for NYC and Essex County are below 40%. Similarly, Massachusetts has substantially higher employment levels than California and Oregon.

Second, for all treatment units, the employment dynamics of treated and synthetic controls are basically identical in the pre-reform period, suggesting that the synthetic control counties represent a valid counterfactual.

Third, one cannot visually identify sizable and systematic reform-related employment effects. In post-reform years, control and treatment units' employment dynamics are literally identical for basically all cities or states displayed.

Fourth, to quantitatively evaluate the SCGM fit between treated and controls and assess potential employment effects and conduct inference, we follow Abadie and Gardeazabal (2003) and Abadie et al. (2010) as discussed in Section 5. All relevant statistics are shown in Table 3.

[Insert Table 3 about here]

The first column shows the level of the outcome measure, Y_{it}^1, the *Employment Ratio*—defined as private sector employment as a share of the total county or state population—averaged over the entire pre-reform period. The region of support is broad and ranges between 30% (Hudson County) and 77% (for DC) for the counties. As for the states, employment levels vary between 17% (Connecticut) and 36% (Massachusetts).

Column (2) of Table 3 shows the RMSPE for pre-reform years as defined by equation (3). Note that we take the logarithm of the outcome variable before minimizing. Thus the values in column (2) can be interpreted as percentages of the outcome variable. With the exception of DC which we disregard due to a poor fit but show for completeness in the Appendix (see discussion in Section 4), all pre-reform RMSPE's are very low—around 1% of the outcome measure. This implies that we could very successfully replicate the employment dynamics of the treatment counties and states by building synthetic control groups. As a comparison, evaluating the effects of a tobacco control program in California on cigarette consumption, Abadie et al. (2010) have a pre-reform RMSPE of 3 relative to a mean of about 100.

Column (3) shows the post-RMSPEs for each treated county and state. They appear to be slightly larger than the pre-reform RMSPEs; the *RMPSE Ratio* [column (3)/column(2)] as shown by column (4) lies between 1 for Alameda and 4 for SF. Next, we conduct inference using placebo methods as proposed by Abadie et al. (2010) and described in Section

5. Specifically, for each treatment county, we apply the SCGM to non-treated placebo counties with similar labor markets and demographics. Then we replicate the standard SCGM procedure with each placebo region pretending it had been treated at the same time as the real treatment region.

Column (5) of Table 3 shows how we calculate the p-value for the hypothesis H_0 : $\mathit{RMSPE\ Ratio}_{Treat} \leq \mathit{RMSPE\ Ratio}_{Placebo}$, as [Rank *RMSPE Ratio* Treated County/#Total Counties Assessed]. In other words, after calculating the *RMPSE Ratio* for each placebo evaluation and ranking all of them, we can assess the position of the *RMPSE Ratio* for the treated region in the test statistic distribution (Abadie et al., 2010). As seen in column (4), while we run the SCGM 47 times for each treatment state, the total number of counties [placebo + 1] evaluated for each treatment county varies between 83 and 199. As seen, when ranked, the rank of the true treatment county lies between 23 (NYC) and 139 (Alameda) and those for the states between 9 (Connecticut) and 36 (Massachusetts). Accordingly, except for NYC (p=0.13) and Connecticut (p=0.19), none of the p-values is even close to being considered statistically significant by conventional levels.

We also calculate the sum of all p-values, separately for the city and state mandates (abstaining from DC due to a poor fit) and then evaluate the joint p-value—based on the Irwin-Hall distribution (Dube and Zipperer, 2015). We obtain an overall p-value of 0.25 for the counties and 0.51 for the states.

The right column of Figure 1 shows the placebo analyses and permutation inference graphically. Following the convention in the literature, the graphs for the treatment counties plot the difference in the logarithm of employment ratios (solid black) along with the differences of all placebo evaluations (gray) with good fit ($\text{RMSPE}_{Placebo} \leq \text{RMSPE}_{Treat} \cdot 2$). As seen, for pre-reform periods, the solid black line fluctuates very closely around the horizontal zero line implying that the synthetic control units very closely map the employment dynamics of the treatment units. After the reform implementation, as indicated by the black solid vertical line, employment differentials between treated and control counties remain very small and straight flat for almost all counties and states. One exception is SF where the differential even appears to be positive although this is not true in a statistical sense.

The other exception is the state of Connecticut whose sick pay mandate only applied to full-time service sector workers in non-small firms. The graphs for Connecticut are in Figure 3. Concerning employment effects, one observes a very good fit in pre-reform years, the RMSPE pre value is just 0.0076 (column 2 of Table 3). As is already observable in the right upper graph, post-reform, it looks like the employment dynamic would develop weaker in Connecticut's service sector firms with more than 49 employees (relative to the synthetic control state). The decrease in employment losses kicked in just after the reform became effective in 2012 and the weaker job growth has been increasing smoothly but steadily over time. This visual assessment is confirmed by the *RMSPE Ratio* of 3 (column 4) and the negative point estimate implying an employment decrease of 2.1% (column 6) which is statistically significant at the twelve percent level (column 8).

Not just for Connecticut but all treatment regions, column (6) of Table 3 shows the *Percent Treatment Effect (PTE)* for the post-reform period, whereas column (7) shows the *Level Treatment Effect (LTE)* in natural units, i.e., private sector employment as a share of the total population. As seen, the signs of the calculated treatment effects are ambiguous (five are negative and seven positive); only the one for Connecticut comes close to statistical significance at conventional levels.

These PTE significance levels are displayed in column (8) and show the results of the placebo testing procedures for the PTE.[6] The p-value of the joint test for the cities is 0.43 and those for the stats 0.37 meaning that, when tested jointly, we clearly cannot reject the null of no negative employment effects as a result of US sick pay mandates.

Finally, column (9) of Table 3 shows p-values for alternative statistical tests, namely a hypothetical treatment effect of -1%. The null hypotheses here is $H_0 : \text{PTE}_{Treat} \geq -0.01$. We first added this hypothetical treatment effect to each placebo iteration, and recalculate then the p-value.[7] As seen, the joint p-value for all cities is 0.0782, implying that we can reject with 92% probability potential employment decreases of more than 1% as a result

[6] Note that the number of placebos in the denominator slightly changes (as compared to column 5); column (7) only uses placebo counties with a good pre-treatment fit with $\text{RMSPE}_{Placebo} \leq \text{RMSPE}_{Treat} \cdot 2$.

[7] Dube and Zipperer (2015) suggests adding the negative treatment effect to the treated county, which yields exactly the same result.

of the sick pay mandates; for the four states we can reject that with 90% likelihood and, for both cities and states together, our statistical power even increases to above 97%.

6.2 Evaluating Wage Effects of the US Sick Pay Mandates

Next we analyze wage effects. The results are visually shown in Figures 2 and 3 and test statistics are in Table 4. The structure follows the structure for employment effects. Recall that the wages are quarterly nominal wages that have been de-trended of seasonal fluctuations (Section 4).

The left column of Figure 2 shows the relative wage dynamics for ten treatment regions. We observe a positive wage trend representing increasing nominal wages. Interestingly, not only do the wage levels differ substantially between local labor markets, but so do the slopes representing wage growth. This is one of the reasons why we decided against further manipulation of the raw data, e.g., correcting for the consumer price index. First, the SCGM is able to precisely replicate local and time-variant differences in wage dynamics. Actually it is a method that is very well suited for such purposes. Second, because no monthly (or quarterly) county level CPI measure is available, one would have to convert nominal wages into presumably 'real' wages using a common discount rate which would not capture the properties of the local labor market appropriately.

[Insert Figure 2 about here]

Although most treatment regions show acceptable and partly very good pre-treatment fits between wage dynamics in the treated and the synthetic control regions, for two regions it was impossible to find synthetic control groups with acceptable fits. These two regions are NYC (Figure 2) and Hudson County (Figure B2) in which Jersey county lies. For these two regions, we abstain from making any statements about causal wage effects of sick pay mandates. The reason for the failure to even create *synthetic* control groups choosing from more than 3000 counties illustrates the very non-representative wage levels and dynamics in NYC (with by far the highest wages among all treatment regions, column 1 of Table 4) as well as Jersey City with its Finance, Insurance, and Real Estate industry just across the Hudson river from Manhattan.[8]

[8]Jersey City has also many small entrepreneurial small businesses.

However, except for NYC and Jersey City, the other county and state wage dynamics shown in Figure 2 could be replicated quite accurately with all pre-RMSPEs below 10 and mostly below 3 (column 2 of Table 4). The statistically insignificant *RMSPE Ratios* fluctuate without any clear trend between 0.7 and 2.2. This is also represented by the PTEs which are either very small in size or positive. However, none of the single PTE p-values is statistically significant (column 9).

[Insert Table 4 about here]

Overall, there is not much evidence for significant wage decreases as a result of mandating sick leave. Visually, it is hard to detect substantial and systematic effects and all test statistical let us exclude wage decreases with statistical certainty. The separate joint tests for cities and states allow us to exclude, with error probabilities of just 1.15% and 0.07%, that wages decreased by at least 1% due to the sick pay mandates.

7 Discussion and Conclusion

This paper systematically evaluates the labor market consequences of nine city and four state sick pay mandates in the US using the Synthetic Control Group Method (SCGM). The setting is ideally suited for the SCGM. First, especially when evaluating counties, we have a very rich pool of donor counties—in fact thousands of them—which we can exploit to build convincing synthetic control counties that map the labor market dynamics of the treated counties very closely. We also rely on many pre-treatment observations; matching treated-control labor market dynamics over a long pre-reform time periods strengthens the identifying assumptions of SCMG. Because some of our treated units are very small and geographically dispersed, we can also convincingly assume the absence of relevant general equilibrium and spillover effects from treated to control regions. Additionally, because we rely on many different treatment group—all of which have very diverse labor markets—our findings have a broad range of common support and arguably high external validity. Also, many treatment regions reduce the likelihood that unobserved shocks confounded post-reform labor market dynamics systematically.

The main concern of opponents of sick pay mandates are negative employment or wage effects. We do not find much evidence that employment and wage growth have been substantially and significantly dampened by mandating employers to allow employees to earn paid sick leave. The precisely estimated zero effects on employment and wage growth may be a function of how the US laws are designed: In fact, they seem to be more incentive-compatible than their European counterparts and minimize shirking behavior, a main concern of opponents. The reason for this incentive-compatibility is that paid sick days are personalized and employees 'earn' them. For every 30-40 hours worked—i.e., for every week a full-time employee works—employees earn one hour of paid sick leave. This means that employees earn about one day of paid sick leave for every two months worked, up to typically seven days per year. Unused sick days roll over to the next year. Because earned sick days represent a personalized insurance credit for future health shocks (similar to health savings accounts) that are likely to occur (e.g. flu or disease of child), we expect shirking to play a minimal role for most employees. Static calculations suggest that the US version of earning sick days equals a fringe benefit that is worth up to 3.3% of the wage. The static calculation is an upper bound because it assumes that employees fully exhaust their sick days, could maintain 100% work productivity when working sick, and would not compensate for the lost work productivity due to sick leave after their recovery. However, wages and employment could still be significantly affected due to administrative burdens or psychological effects when employers overestimate the actual relevance for their businesses.

This paper's findings suggest that neither employment nor wage growth has been significantly affected by US sick pay mandates. We can exclude with at least 97% statistical precision that employment or wages have decreased by more than 1%. One exception could be Connecticut where the law was the least comprehensive and only applied to 20% of the workforce—that is, full-time service sector employees in firms with at least 50 employees. Here we find some suggestive evidence that, as compared to the same sector in the other US states, employment growth has been lagging behind as a result of the mandate.

The US needs more economic research on sick leave.

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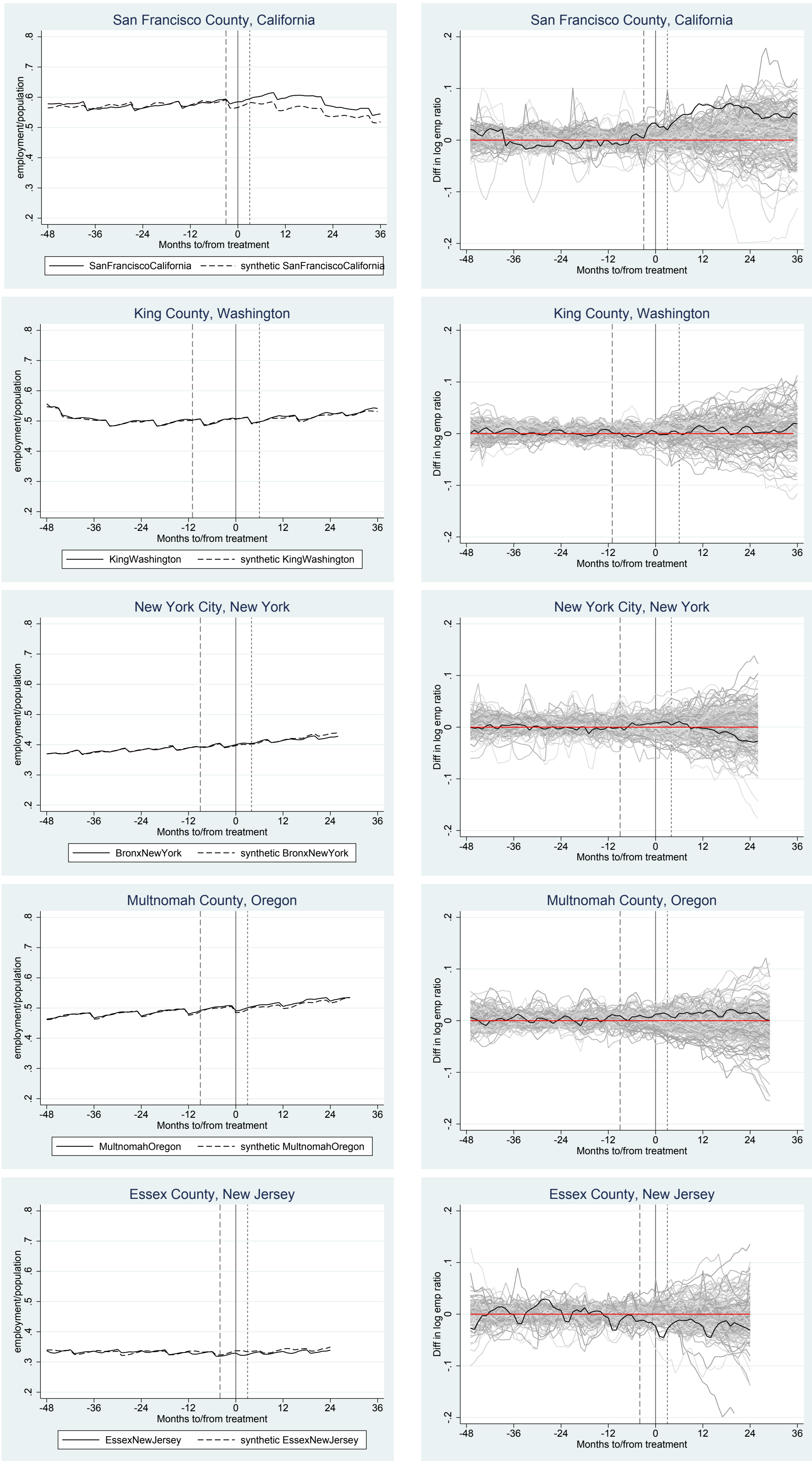
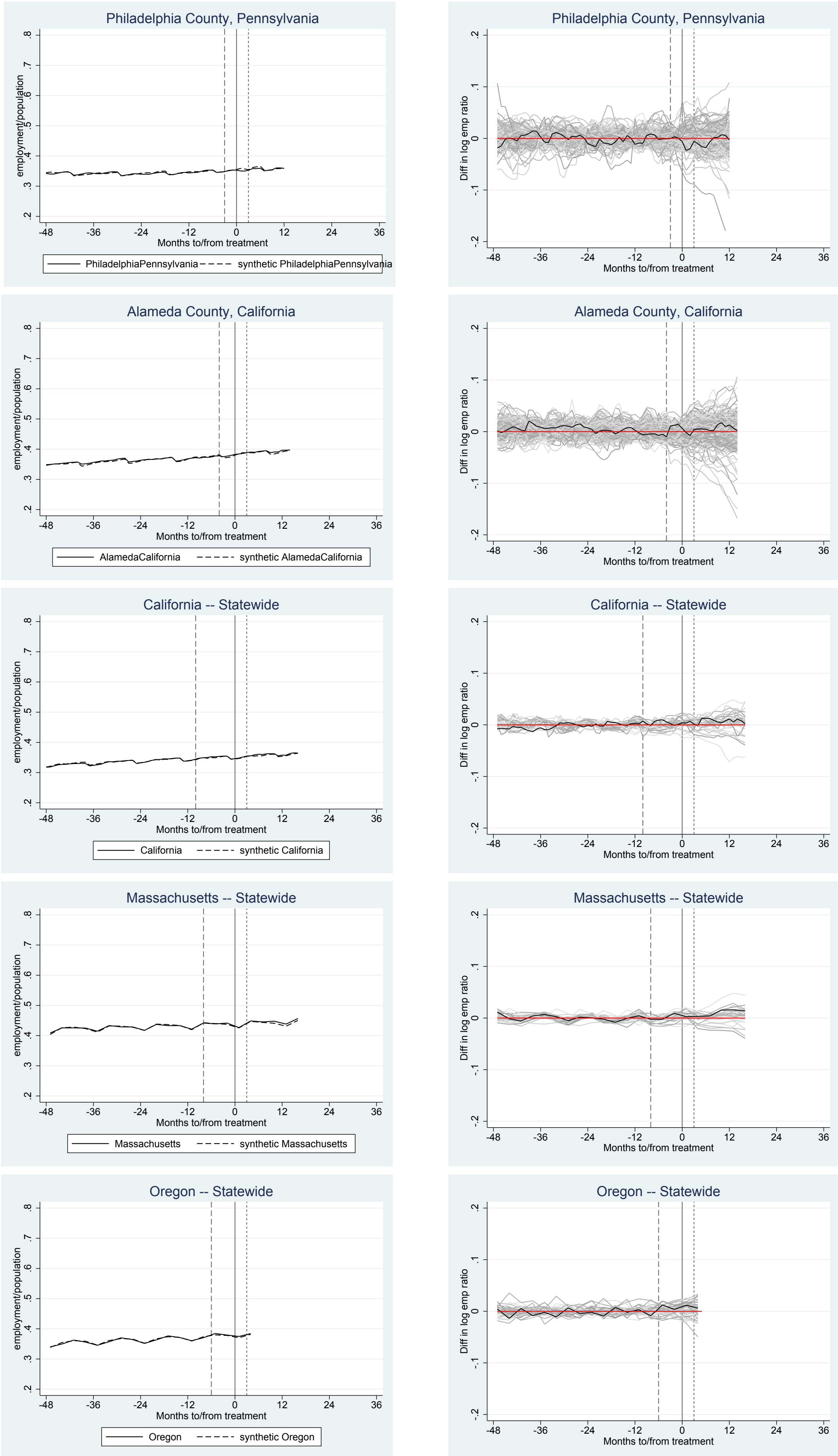
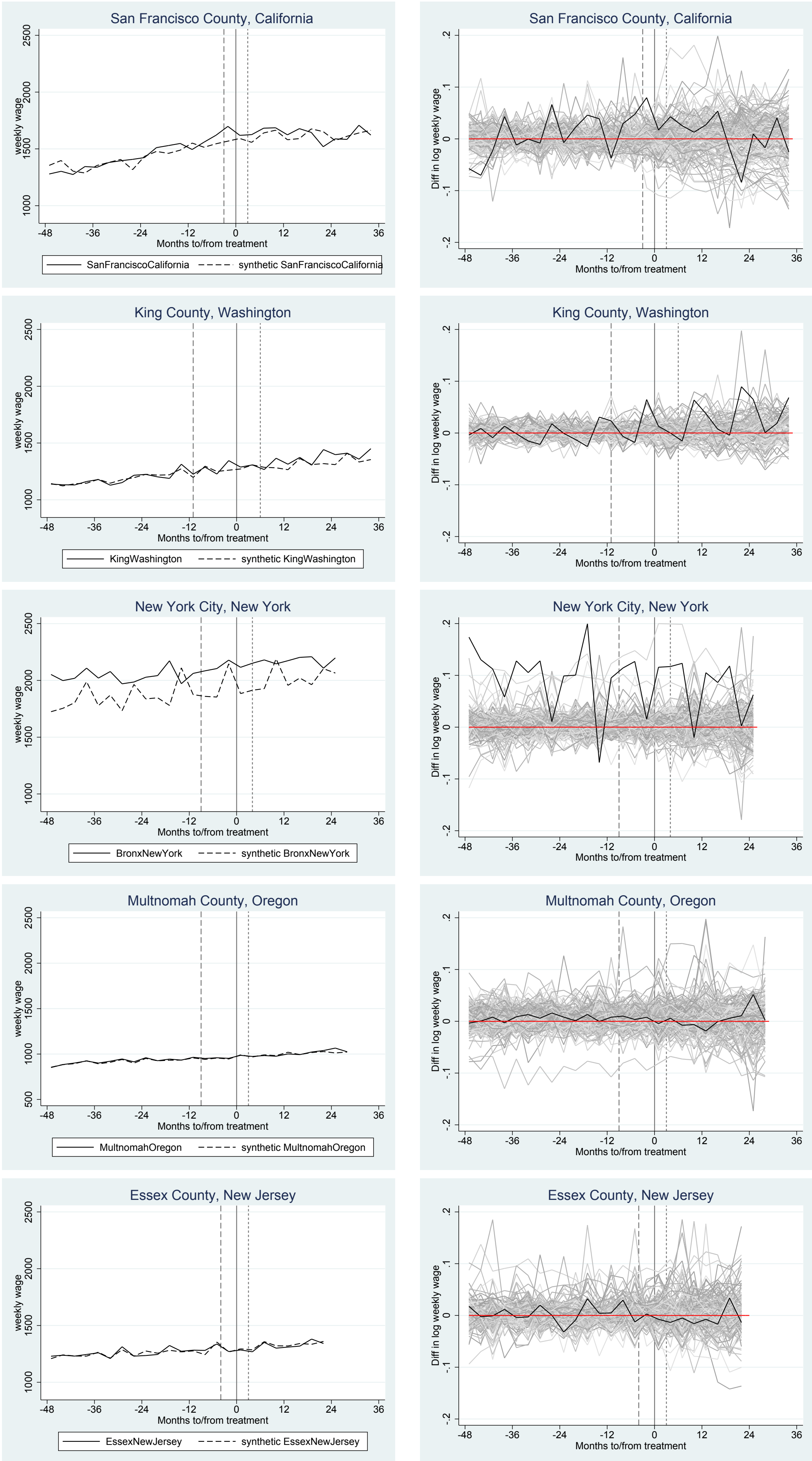
Figure 1: Development of Employment Ratios in Treated vs. Synthetic Control Regions

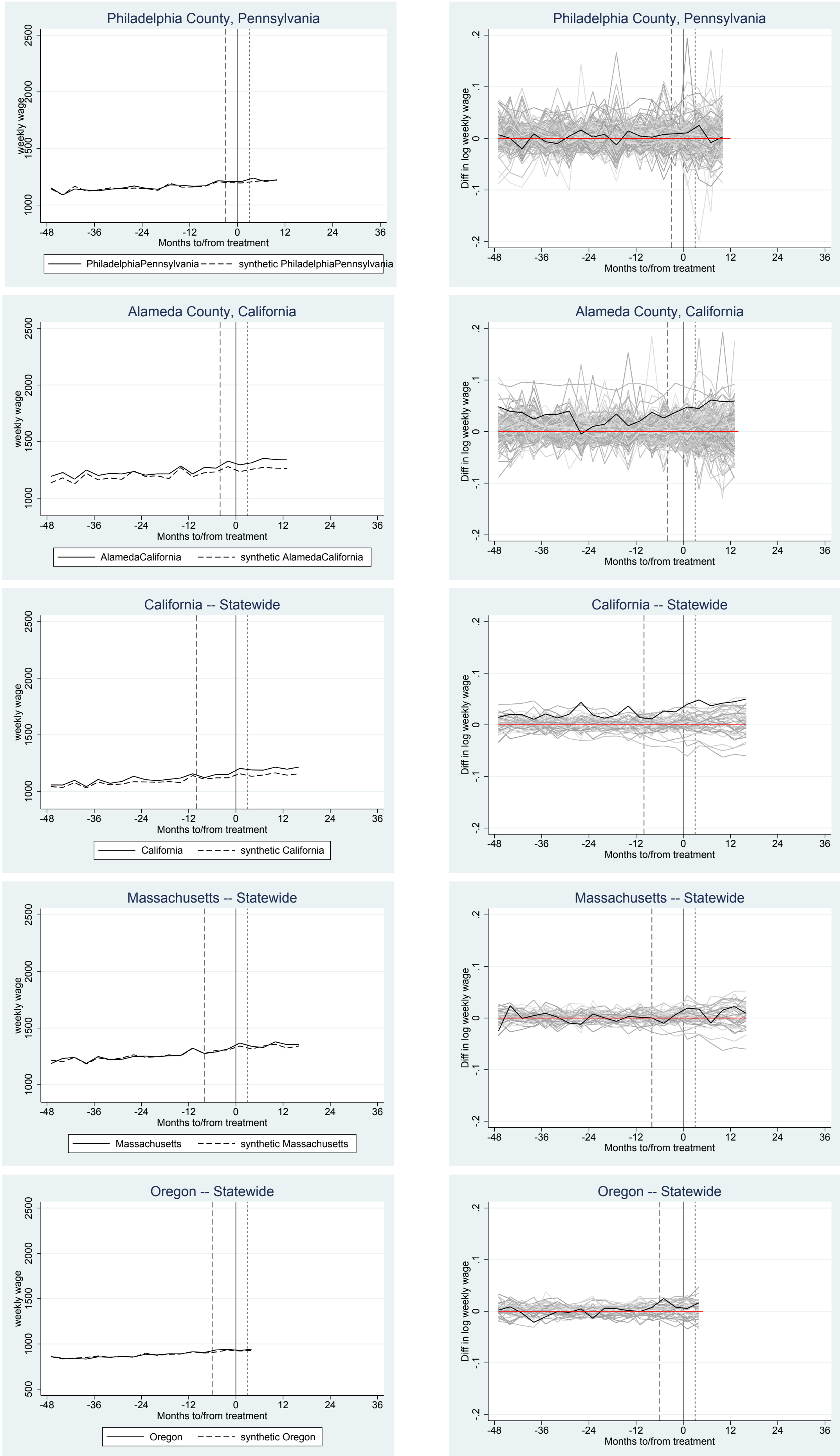




The left column compares the development for the treated counties and states (solid line) to the synthetic control counties and states (dashed line). The composition of the synthetic control counties is in Tables A1 and A2. The right column shows the difference of the logarithm of the employment ratios between treated and control groups along with placebo estimates for counties with an RMSPE smaller than 2 times the RMSPE of the treated county (gray lines). For more information about the sick pay reforms, see Table B1.

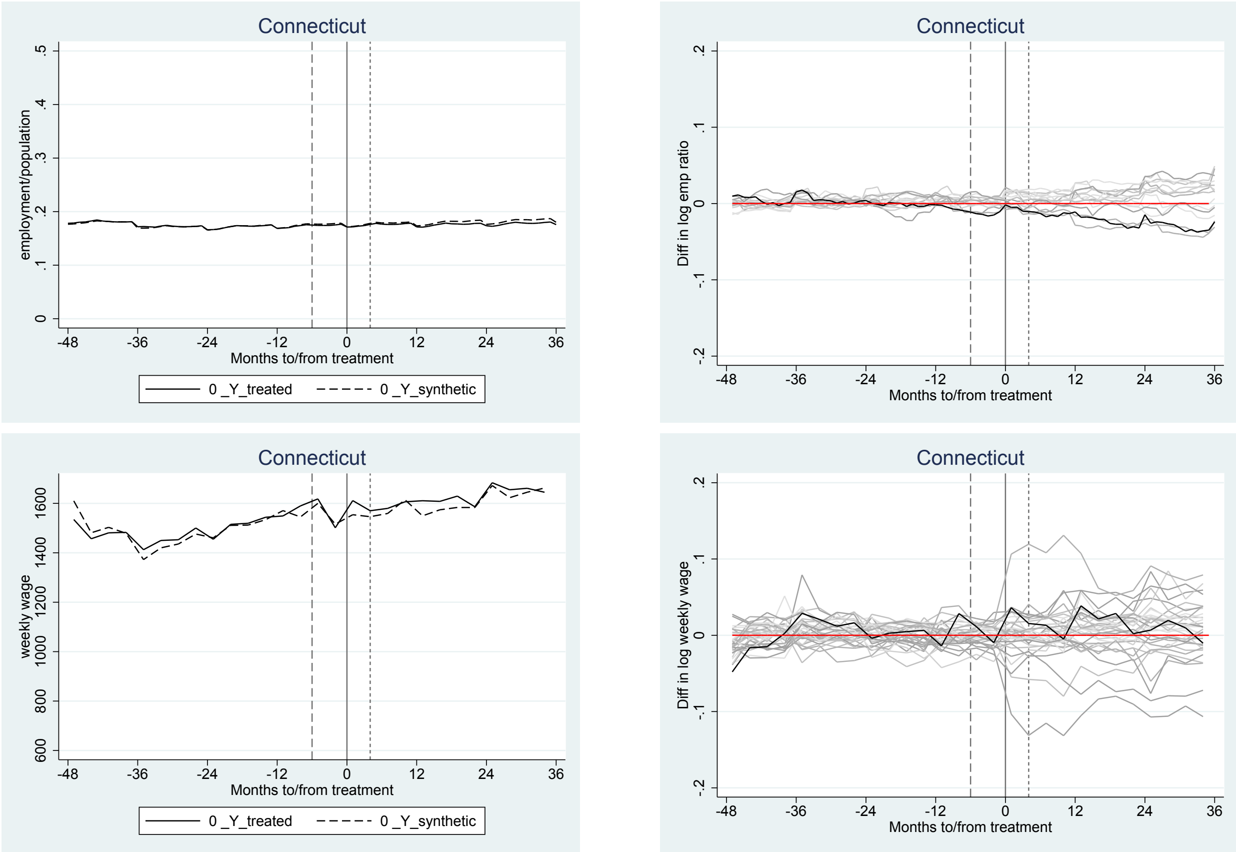
Figure 2: Development of Weekly Wages in Treated vs. Synthetic Control Regions





The left column compares the development for the treated the treated counties and states (solid lines) to the synthetic control counties and states (dashed lines). The composition of the synthetic control units is in Tables A3 and A4. The right column shows the difference of the logarithm of weekly wages between treated and control units along with placebo estimates for counties with an RMSPE smaller than 2 times the RMSPE of the treated county (gray lines). For more information about the sick pay reforms, see Table B1.

Figure 3: Labor Market Development in Connecticut vs. Synthetic Control States: Service Sector Firms >49 Employees



The left column shows the development for Connecticut (solid lines) vs. the synthetic control group state (dashed lines). The composition of the synthetic control state is in Table **??**. The right column shows the difference of the logarithm of the employment ratios along with placebo estimates for states with an RMSPE smaller than 2 times the RMSPE of the treated county (gray lines). The first row shows the result for the employment ratio and the second row shows results for wages. For more information about the sick pay reforms, see Table B1.

Table 1: Quarterly Census of Employment and Wages (QCEW), County Level: 2001-2016Q2

Variable	Mean	Std. Dev.	Min.	Max.	N
Private sector employment	0.272	0.135	0.011	4.038	569,004
Public sector employment	0.077	0.036	0.012	0.496	569,004
Production employment	0.078	0.064	0	3.569	569,004
Service employment	0.192	0.109	0	2.989	569,004
Private sector wages	602.042	186.417	154.871	4541.787	189,720
Population	97,573.51	338,869.507	258	10,109,436	45,888

Source: QCEW (Bureau of Labor Statistics (BLS), 2016), own calculation and illustration. County level population data are taken from (United States Census Bureau, 2016b).

Table 2: Quarterly Census of Employment and Wages (QCEW), State Level: 2001-2016Q2

Variable	Mean	Std. Dev.	Min.	Max.	N
Private sector employment	0.376	0.065	0.268	0.791	9,486
Service employment	0.3	0.066	0.179	0.765	9,486
Production employment	0.073	0.02	0.014	0.141	9,486
Public sector employment	0.081	0.046	0.044	0.414	9,486
Private sector wages	812.583	200.205	440.281	1623.866	3,162
Population	5,952,186.341	6,665,414.549	494,657	39,144,818	765
Private service sector empl., >49 empl.	0.154	0.054	0.047	0.557	9,486
Private non-service sector empl., >49 empl.	0.045	0.016	0.006	0.098	9486
Private sector empl., >9 empl.	0.315	0.06	9,486		
Public sector employment	0.081	0.046	0.044	0.414	9486
Private service sector wages, >49 empl.	850.950	265.452	423.173	2030.237	3,162
Private sector wages, >9 empl.	822.451	190.876	3,162		

Source: QCEW (Bureau of Labor Statistics (BLS), 2016), own calculation and illustration. State level population data are taken from (United States Census Bureau, 2016b).

Table 3: Key Statistics—Using the Synthetic Control Group Method to Assess Changes in Employment after the Implementation of Sick Pay Mandates

	$\bar{Y}^1_{i,pre}$ **(1)**	**RMSPE pre (2)**	**RMSPE post (3)**	**RMSPE Ratio (4)**	**Rank RMSPE/ #Placebos= P-Value (5)**	**PTE (6)**	**LTE (7)**	**Rank PTE/ #Placebos= P-Value (8)**	**P-value PTE<-3% (9)**	**P-value LTE<-2ppt (10)**
Counties:										
San Francisco, CA	0.5742	0.0133	0.0536	4.0429	35/164=0.2134	0.0518	0.0298	149/156=0.9551	0.0256	0.0064
King County, WA	0.5040	0.0055	0.0083	1.5041	89/155=0.5742	0.0058	0.0031	53/90=0.5889	0.1778	0.1000
New York City, NY	0.3835	0.0041	0.0146	3.5908	23/175=0.1314	-0.0062	-0.0030	20/61=0.3279	0.3770	0.1639
Multnomah, OR	0.4852	0.0061	0.0141	2.3237	61/185=0.3297	0.0131	0.0068	110/126=0.873	0.0556	0.0397
Essex County, NJ	0.3319	0.0147	0.0256	1.7445	85/158=0.538	-0.0232	-0.0078	24/150=0.16	0.7267	0.3267
Hudson County, NJ	0.2965	0.0267	0.0438	1.6393	41/83=0.494	-0.0419	-0.0132	7/81=0.0864	0.8765	0.7284
Philadelphia, PA	0.3436	0.0081	0.0119	1.4800	79/175=0.4514	-0.0072	-0.0026	36/137=0.2628	0.4161	0.1825
Alameda, CA	0.3638	0.0081	0.0081	0.9984	139/199=0.6985	0.0055	0.0021	98/161=0.6087	0.1801	0.0683
District of Columbia	0.7752	0.1395	0.1902	1.3638	114/161					
Average MPE/ Sum Pval					3.4306	-0.0003	0.0019	3.8628	2.8354	1.6159
P val Irwin Hall					0.2462			0.4345	0.0782	0.0011
States:										
Connecticut	0.1746	0.0076	0.0235	3.0716	9/47=0.1915	-0.0214	-0.0038	3/25=0.12	0.7200	0.1200
California	0.3376	0.0056	0.0080	1.4369	24/47=0.5106	0.0069	0.0025	21/32=0.6563	0.0938	0.0313
Massachusetts	0.3628	0.0041	0.0042	1.0184	36/47=0.766	0.0008	0.0005	8/21=0.381	0.2381	0.0952
Oregon	0.2932	0.0069	0.0086	1.2425	26/47=0.5532	0.0082	0.0026	24/37=0.6486	0.1892	0.0541
Average MPE/ Sum Pval					2.0213	-0.0014	0.0005	1.8059	1.2410	0.3005
P val Irwin Hall					0.5142			0.3729	0.0983	0.0003
Total (Counties and States):										
Sum Pval					5.4519	-0.0006	0.0014	5.6686	4.0765	1.9164
P val Irwin Hall					0.2940			0.3717	0.0268	$5.1139*10^{-6}$

Source: QCEW (Bureau of Labor Statistics (BLS), 2016), own calculation and illustration. All statistics displayed here are discussed in Section 5. Column (1) displays the outcome measure in levels for each treated county averaged over all pre-reform years. Columns (2) and (3) display the *RMSPE* as in equation (3) for pre and post-reform years, respectively. Column (4) displays the *RMSPE Ratio* [RMSPE post/RMSPE pre]. Column (5) calculates the p-value of the *RMSPE Ratio* for all treated counties using the indicated number of placebo estimates. Columns (6) and (7) show the *PTE* and *LTE* as in equations (4) and (5). Column (8) calculates the p-value of the *PTE* for all treated counties using the indicated number of placebo estimates. Columns (9) and (10) display the p-values of hypothetical employment decreases of 3% and 2ppt respectively (see main text and Figure **??** for more details). As for the joint tests and sum of all p-values per county, we exclude the District of Columbia due to a poor pre RMSPE fit. For more information, see the discussion on treatment regions in Section 4. For more information about the sick pay reforms, see Table B1.

Table 4: Key Statistics—Using the Synthetic Control Group Method to Assess Changes in Weekly Wages after the Implementation of Sick Pay Mandates

	$\bar{Y}^1_{i,pre}$ (1)	RMSPE pre (2)	RMSPE post (3)	RMSPE Ratio (4)	Rank RMSPE/ #Placebos= P-Value (5)	PTE (6)	LTE (7)	Rank PTE/ #Placebos= P-Value (8)	P-value PTE<-3% (9)	P-value LTE<-€40 (10)
Counties:										
San Francisco, CA	1434.71	0.0423	0.0389	0.9201	144/164=0.878	0.0089	11.6254	87/162=0.537	0.3148	0.2469
King County, WA	1199.26	0.0207	0.0446	2.1524	28/155=0.1806	0.0296	38.9481	131/151=0.8675	0.0861	0.0662
New York City, NY	2054.66	0.1146	0.0937	0.8183	153/175					
Multnomah, OR	927.31	0.0083	0.0182	2.2004	35/185=0.1892	0.0039	4.3896	45/91=0.4945	0.2308	0.2308
Essex County, NJ	1261.09	0.0160	0.0162	1.0101	112/158=0.7089	-0.0054	-7.3773	42/140=0.3	0.4500	0.5000
Hudson County, NJ	1579.94	0.1857	0.1675	0.9018	66/83					
Philadelphia, PA	1153.10	0.0104	0.0141	1.3589	69/175=0.3943	0.0077	9.2885	73/111=0.6577	0.1712	0.1351
Alameda, CA	1231.76	0.0308	0.0546	1.7728	60/199=0.3015	0.0542	70.8706	194/195=0.9949	0.0103	0.0051
District of Columbia	1320.95	0.0203	0.0146	0.7221	151/161=0.9379	-0.0021	-4.7810	39/144=0.2708	0.5278	0.5694
Average MPE/ Sum Pval					3.5904	0.0138	17.5663	4.1225	1.7909	1.7536
P val Irwin Hall					0.5461			0.788448	0.0115	0.0099
States:										
Connecticut	1503.8880	0.0190	0.0209	1.0986	32/47=0.6809	0.0150	23.9702	23/38=0.6053	0.2105	0.1316
California	1106.7480	0.0229	0.0441	1.9251	12/47=0.2553	0.0439	51.8478	45/45=1	0.0222	0.0222
Massachusetts	1260.1960	0.0706	0.0800	1.1335	22/47=0.4681	0.0718	96.0367	45/47=0.9574	0.0426	0.0426
Oregon	918.6851	0.0127	0.0189	1.4880	10/47=0.2128	0.0171	17.7307	31/36=0.8611	0.0833	0.0833
Average MPE/ Sum Pval					1.6170	0.0369	47.3964	3.4238	0.3586	0.2797
P val Irwin Hall					0.2607			0.9954	0.0007	0.0003
Total (Counties and States):										
Average MPE/ Sum Pval					5.2074	0.0230	28.8579	7.5463	2.1495	2.0333
P val Irwin Hall					0.3815			0.9844	0.0001	0.0001

Source: QCEW (Bureau of Labor Statistics (BLS), 2016), own calculation and illustration. All statistics displayed here are discussed in Section 5. Column (1) displays the outcome measure in levels for each treated county averaged over all pre-reform years. Columns (2) and (3) display the *RMSPE* as in equation (3) for pre and post-reform years, respectively. Column (4) displays the *RMSPE Ratio* [RMSPE post/RMSPE pre]. Column (5) calculates the p-value of the *RMSPE Ratio* for all treated counties using the indicated number of placebo estimates. Columns (6) and (7) show the *PTE* and *LTE* as in equations (4) and (5). Column (8) calculates the p-value of the *PTE* for all treated counties using the indicated number of placebo estimates. Columns (9) and (10) display the p-values of hypothetical weekly wage decreases of 3% and €40 respectively (see main text and Figure **??** for more details). As for the joint tests and sum of all p-values per county, we exclude the District of Columbia due to a poor pre RMSPE fit. For more information, see the discussion on treatment regions in Section 4. For more information about the sick pay reforms, see Table B1.

Appendix A

Table A1: Counties for Synthetic Control Group: Employment

	San Francisco	King (WA)	NYC (NY)	Multnomah (OR)	Essex (NJ)	Hudson (NJ)	Philadelphia (PA)	Alameda (CA)	DC (DC)
Arlington, VA	0.308	0.000	0.085	0.000	a	a	a	0.000	0.481
Montgomery, MD	0.000	0.000	0.000	0.000	0.539	0.000	0.267	0.000	0
Fulton, GA	0.218	0.001	0.000	0.000	a	a	a	0.000	0.519
Somerset, NJ	0.474	0.082	0.000	0.000	a	a	0.000	0.157	0
DeKalb, GA	a	a	0.000	a	0.000	0.000	0.667	0.000	a
Miami-Dade, FL	a	a	0.56	a	0.000	0.000	0.000	0.000	a
Douglas, CO	a	a	a	a	0.000	0.551	0.000	a	a
Westchester, NY	0.000	0.000	0.000	0.000	0.000	0.265	0.000	0.191	0
Williamson, TN	0.000	0.000	0.355	0.082	0.000	a	0.000	0.000	0
Mecklenburg, NC	0.000	0.402	0.000	0.000	a	a	a	0.000	0
El Paso, CO	a	a	a	a	0.262	0.000	0.000	0.000	a
Rutherford, TN	0.000	a	0.000	a	0.000	0.000	0.000	0.245	a
Durham, NC	0.000	0.132	0.000	0.064	a	a	a	0.000	0
Ada, ID	0.000	0.000	0.000	0.189	0.000	a	0.000	0.000	0
Collin, TX	a	a	0.000	a	0.000	0.000	0.000	0.187	a
St. Mary’s, MD	a	a	a	a	a	0.184	a	a	a
Polk, IA	0.000	0.000	0.000	0.175	a	a	a	0.000	0
Albany, NY	0.000	0.000	0.000	0.149	a	a	a	0.000	0
Kent, MI	0.000	0.000	0.000	0.139	a	a	a	0.000	0
Denver, CO	0.000	0.123	0.000	0.000	a	a	a	0.000	0
Benton, AR	0.000	0.000	0.000	0.000	0.000	a	0.000	0.101	0
Strafford, NH	a	a	a	a	0.095	0.000	0.000	a	a
Madison, AL	0.000	0.093	0.000	0.000	0.000	a	0.000	0.000	0
Albemarle, VA	a	a	a	a	0.089	0.000	0.000	0.000	a
Travis, TX	0.000	0.000	0.000	0.057	0.000	a	0.000	0.029	0
Washtenaw, MI	0.000	a	0.000	0.000	0.015	0.000	0.000	0.057	a
Fairfax, VA	0.000	0.000	0.000	0.000	0.000	a	0.065	0.000	0
Harris, TX	0.000	0.063	0.000	0.000	0.000	a	0.000	0.000	0
Johnson, KS	0.000	0.000	0.000	0.061	a	a	a	0.000	0
Lake, IL	0.000	0.04	0.000	0.000	0.000	a	0.000	0.000	0
Sangamon, IL	a	a	a	0.036	a	a	a	a	a
Midland, TX	0.000	0.036	0.000	0.000	a	a	a	0.000	0
Cass, ND	a	0.000	0.000	0.035	a	a	a	0.000	a
Ascension, LA	a	a	a	a	0.000	0.000	0.000	0.026	a
Winnebago, WI	0.000	0.018	0.000	0.000	a	a	0.000	0.000	0
Fayette, KY	0.000	0.000	0.000	0.013	0.000	a	0.000	0.000	0
Mercer, NJ	0.000	0.01	0.000	0.000	0.000	a	0.000	0.000	0
San Juan, NM	a	a	a	a	0.000	0.000	0.000	0.006	a

Sources: QCEW, own calculation and illustration. ’a’ indicates that the variables for employment, wages, and county population do not lie within the region of support of the treatment county. Thus these counties are not considered as potential “donors.” ’0’ indicates that the county is a potential control county donor but has not actually been used as a donor. All counties with positive fractions indicate the donor share employed by the synthetic control group method for the treatment county in the column header. Thus, all fractions in one column add to 100%.

Table A2: States for Synthetic Control Group: Employment

	Connecticut	California	Massachusetts	Oregon
Michigan	0.000	0.513	0.000	0.38
Minnesota	0.000	0.000	0.867	0.000
New York	0.698	0.129	0.000	0.000
Arizona	0.000	0.31	0.000	0.000
Wisconsin	0.261	0.000	0.000	0.000
Utah	0.000	0.000	0.000	0.234
Washington	0.000	0.000	0.000	0.161
Nevada	0.000	0.000	0.133	0.000
Wyoming	0.000	0.000	0.000	0.094
Idaho	0.000	0.000	0.000	0.076
South Carolina	0.000	0.000	0.000	0.055
Georgia	0.000	0.048	0.000	0.000
Rhode Island	0.029	0.000	0.000	0.000
Iowa	0.012	0.000	0.000	0.000

Table A3: Counties for Synthetic Control Groups: Weekly Wages

	San Francisco	King (WA)	NYC (NY)	Multnomah (OR)	Essex (NJ)	Hudson (NJ)	Philadelphia (PA)	Alameda (CA)	DC (DC)
Westchester, NY	0.000	0.000	0.000	0.000	0.251	0.874	0.264	0.332	0
Somerset, NJ	0.608	0.000	1	0.000	a	a	0.000	0.089	0
Arlington, VA	0.392	0.051	0.000	0.000	a	a	a	0.000	0.739
DeKalb, GA	a	a	0.000	a	0.000	0.000	0.6	0.000	a
Montgomery, MD	0.000	0.000	0.000	0.000	0.457	0.000	0.03	0.000	0
Fulton, GA	0.000	0.169	0.000	0.000	a	a	a	0.000	0.261
Cass, ND	a	0.089	0.000	0.166	a	a	a	0.000	a
Travis, TX	0.000	0.000	0.000	0.193	0.000	a	0.000	0.044	0
Harris, TX	0.000	0.227	0.000	0.000	0.000	a	0.000	0.000	0
Polk, IA	0.000	0.000	0.000	0.187	a	a	a	0.000	0
Macomb, MI	a	a	a	a	0.000	0.000	0.000	0.186	a
Lake, IL	0.000	0.157	0.000	0.000	0.000	a	0.000	0.000	0
Benton, AR	0.000	0.000	0.000	0.000	0.000	a	0.000	0.148	0
Douglas, CO	a	a	a	a	0.000	0.126	0.000	a	a
Hunterdon, NJ	a	a	a	a	0.125	0.000	0.000	0.000	a
Montgomery, PA	0.000	0.117	0.000	0.000	a	a	a	0.000	0
Washtenaw, MI	0.000	a	0.000	0.048	0.063	0.000	0.000	0.000	a
Fairfax, VA	0.000	0.044	0.000	0.000	0.000	a	0.064	0.000	0
Mercer, NJ	0.000	0.000	0.000	0.000	0.04	a	0.000	0.061	0
San Juan, NM	a	a	a	a	0.000	0.000	0.000	0.097	a
Ada, ID	0.000	0.000	0.000	0.093	0.000	a	0.000	0.000	0
Anne Arundel, MD	a	0.000	0.000	0.089	0.000	0.000	0.000	0.000	a
Steuben, NY	a	a	a	a	0.064	0.000	a	a	a
Durham, NC	0.000	0.048	0.000	0.015	a	a	a	0.000	0
Madison, AL	0.000	0.056	0.000	0.000	0.000	a	0.000	0.000	0
Albany, NY	0.000	0.000	0.000	0.055	a	a	a	0.000	0
Kent, MI	0.000	0.000	0.000	0.048	a	a	a	0.000	0
Orange, FL	0.000	0.000	0.000	0.043	a	a	a	0.000	0
Ascension, LA	a	a	a	a	0.000	0.000	0.000	0.042	a
Terrebonne, LA	0.000	0.042	0.000	0.000	0.000	a	0.000	0.000	0
Alexandria, VA	0.000	0.000	0.000	0.000	a	a	0.042	0.000	0
Olmsted, MN	0.000	0.000	0.000	0.037	a	a	a	0.000	0
St. Louis, MO	0.000	0.000	0.000	0.023	a	a	a	0.000	0
Sangamon, IL	a	a	a	0.003	a	a	a	a	a

Sources: **QCEW**, own calculation and illustration. 'a' indicates that the variables for employment, wages, and county population do not lie within the region of support of the treatment county. Thus these counties are not considered as potential "donors." '0 'indicates that the county is a potential control county donor but has not actually been used as a donor. All counties with positive fractions indicate the donor share employed by the synthetic control group method for the treatment county in the column header. Thus, all fractions in one column add to 100%.

Table A4: States for Synthetic Control Group: Wages

	Connecticut	California	Massachusetts	Oregon
Michigan	0.38	0.38	0.272	0.000
Utah	0.234	0.234	0.334	0.213
Washington	0.161	0.161	0.169	0.33
Wyoming	0.094	0.094	0.027	0.035
Indiana	0.000	0.000	0.000	0.21
Florida	0.000	0.000	0.000	0.161
Idaho	0.076	0.076	0.003	0.000
South Carolina	0.055	0.055	0.032	0.000
Iowa	0.000	0.000	0.08	0.000
Maine	0.000	0.000	0.065	0.000
Montana	0.000	0.000	0.000	0.051
Colorado	0.000	0.000	0.018	0.000

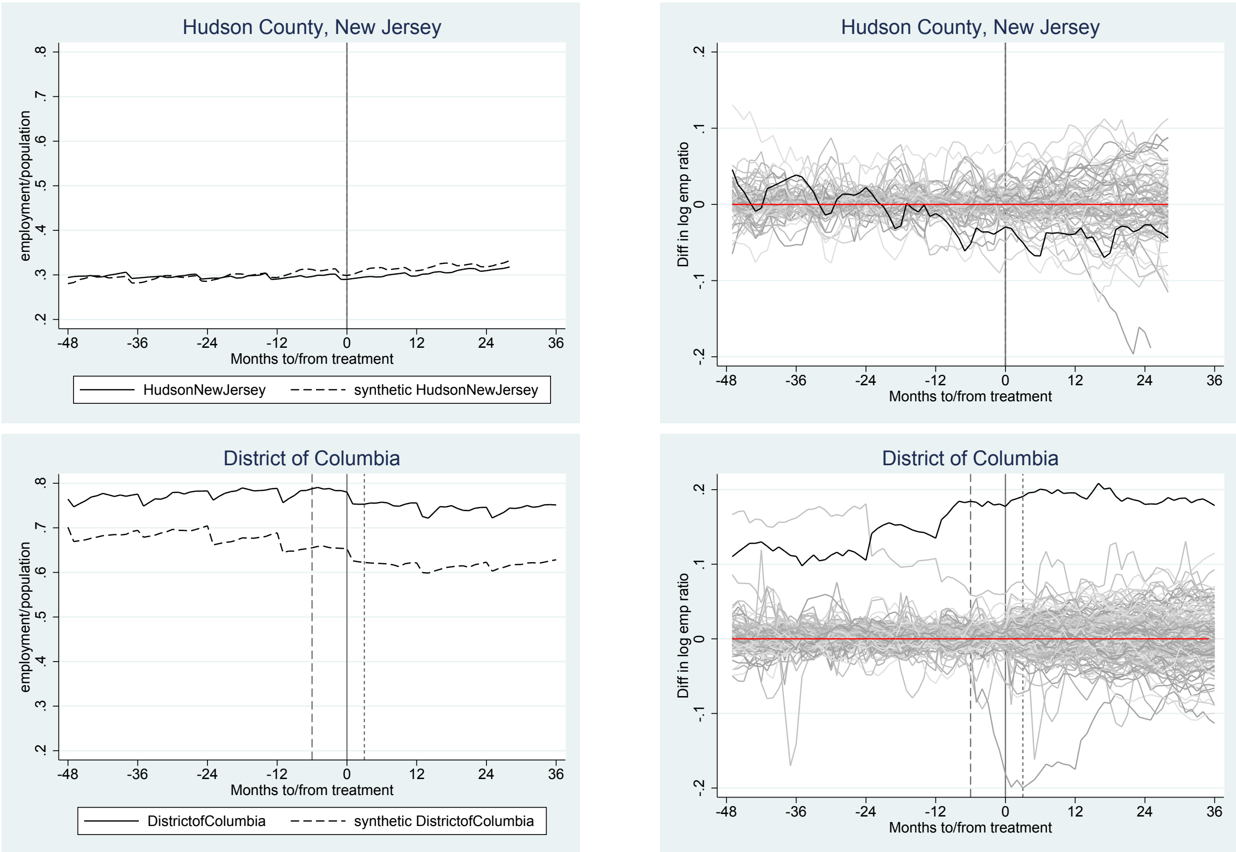
Appendix B

Table B1: Overview of Employer Sick Pay Mandates in the US

Region (1)	County (2)	Law Passed (3)	Law Effective (4)	Content (5)
San Francisco, CA	SF	Nov 7, 2006	Feb 5, 2007	all employees including part-time and temporary; 1 hour of paid sick leave for every 30 hours worked; up to 5 to 9 days depending on firm size; for own sickness or family member; 90 days accrual period
Washington, DC	DC	May 13, 2008	Nov 13, 2008	'qualified employees'; 1 hour of paid sick leave for every 43 hours, 90 days accrual period; up to 3 to 9 days depend. on firm size; own sickness or family; no health care or restaurant workers
		Dec 18, 2013	Feb 22, 2014 (retrosp. in Sep 2014)	extension to 20,000 temporary workers and tipped employees
Connecticut		July 1, 2011	Jan 1, 2012	full-time service sector employees in firms>49 employees (20% of workforce); 1 hour for every 40 hours; up to 5 days; own sickness or family member, 680 hours accrual period (4 months)
Seattle, WA	King	Sep 12, 2011	Sep 1, 2012	all employees in firms with >4 full-time employees; 1 hour for every 30 or 40 hours worked; up to 5 to 13 days depending on firm size, for own sickness or family member; 180 days accrual period
New York, NY	Bronx, Kings, New York, Queens, Richmond	June 26, 2013 Jan 17, 2014 extended	April 1, 2014	employees w >80 hours p.a in firms >4 employees or 1 domestic worker; 1 hour for every 30 hours; up to 40 hours; own sickness or family member; 120 days accrual period
Portland, OR	Multnomah	March 13, 2013	Jan 1 2014	employees w >250 hours p.a. in firms >5 employees; 1 hour for every 30 hours; up to 40 hours; own sickness or family member
Jersey City, NJ	Hudson	Sep 26, 2013 Oct 28, 2015 extended	Jan 22, 2014	all employees in private firms with >9 employees; 1 hour for every 30 hours; up to 40 hours; own sickness or family; 90 days accrual period
Newark, NJ	Essex	Jan 29, 2014	May 29, 2014	all employees in private companies; 1 hour for every 30 hours; 90 days accrual period; up to 24 to 40 hours depending on size; own sickness or family
Philadelphia, PA	Philadelphia	Feb 12, 2015	May 13, 2015	all employees in firms >9 employees; 1 hour for every 40 hours; up to 40 hours; own sickness or family member; 90 days accrual period
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 in firms >10 employees; 1 hour for every 40 hours; up to 40 hours; own sickness or family member; 90 days accrual period
Oakland, CA	Alameda	Nov 4, 2014	March 2, 2015	all employees in firms >9 employees; 1 hour for every 30 hours; 90 days accrual period; up to 40 to 72 hours depending on firm size; own sickness or family member
Oregon		June 22, 2015	Jan 1, 2016	all employees in firms >9 employees; 1 hour for every 30 hours; 90 days accrual period; up to 40 hours; own sickness or family member

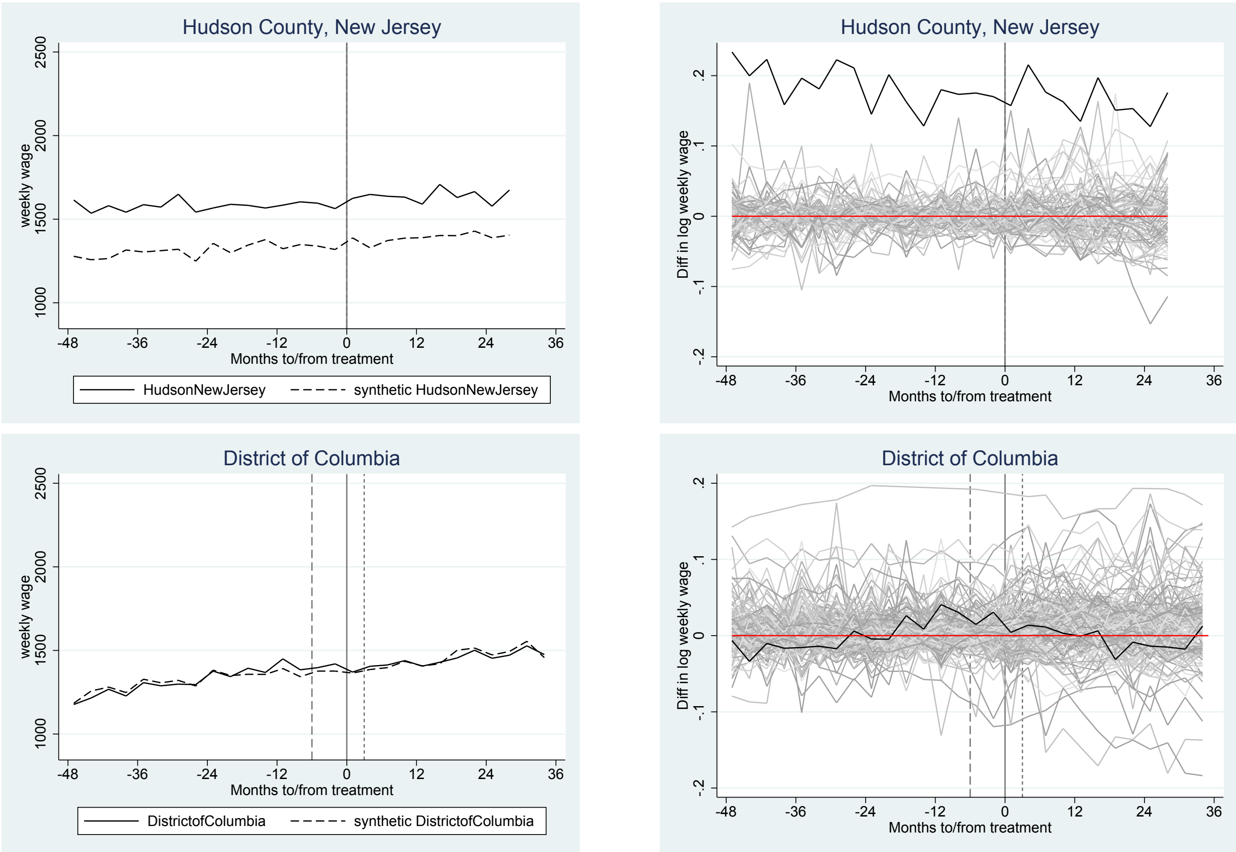
Source: several sources, own collection, own illustration.

Figure B1: Employment in Hudson County, DC and Their Synthetic Control Counties



The left column shows the development for the two treated counties (sold lines) vs. the synthetic control group counties (dashed lines). The composition of the synthetic control counties is in Table A1. The right column shows the difference of the logarithm of the employment ratios between treated and control counties (treated-controls) along with all placebo estimates for counties with an RMSPE smaller than 2 times the RMSPE of the treated county (gray lines). For more information about the sick pay reforms, see Table B1.

Figure B2: Wages in Hudson County, DC and Their Synthetic Control Counties



The left column shows the development for the two treated counties (sold lines) vs. the synthetic control group counties (dashed lines). The composition of the synthetic control counties is in Table A3. The right column shows the difference of the logarithm of weekly wages between treated and control counties (treated-controls) along with all placebo estimates for counties with an RMSPE smaller than 2 times the RMSPE of the treated county (gray lines). For more information about the sick pay reforms, see Table B1.