

Pichler, Stefan; Ziebarth, Nicolas

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Labor market effects of U.S. sick pay mandates

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Labor Market Effects of U.S. Sick Pay Mandates

Stefan Pichler
ETH Zurich

Nicolas Ziebarth
Cornell University

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Stefan Pichler

ETH Zurich, KOF Swiss Economic Institute

Nicolas R. Ziebarth

Cornell University

email: nrz2@cornell.edu

September 21, 2018

ABSTRACT

This paper exploits temporal and spatial variation in the implementation of nine-city- and four state-level U.S. sick pay mandates to assess their labor market consequences. We use the synthetic control group method and traditional difference-in-differences models along with the Quarterly Census of Employment and Wages to estimate the causal effects of mandated sick pay on employment and wages. We do not find much evidence that employment or wages were significantly affected by the mandates that typically allow employees to earn one hour of paid sick leave per work week, up to seven days per year. Employment decreases of 2 percent lie outside the 92 percent confidence interval and wage decreases of 3 percent lie outside the 95 percent confidence interval.

JEL Classification Codes: I12, I13, I18, J22, J28, J32

Key Words: sick pay mandates, sick leave, medical leave, employer mandates, employment, wages, synthetic control group method (SCGM), Quarterly Census of Employment and Wages (QCEW0, United States (U.S.))

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Stefan Pichler
ETH Zurich, KOF Swiss Economic Institute*

Nicolas R. Ziebarth
Cornell University†

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*ETH Zurich, KOF Swiss Economic Institute, Leonhardstrasse 21, 8092 Zurich, Switzerland, phone: +41-(44)632-2507, fax: +41-(44)632-1218, e-mail: pichler@kof.ethz.ch

†Cornell University, Department of Policy Analysis and Management (PAM), 106 Martha Van Rensselaer Hall, Ithaca, NY 14850, USA, phone: +1-(607)255-1180, fax: +1-(607)255-4071, e-mail: nrz2@cornell.edu

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1 Introduction

Paid sick leave was an integral part of the first social insurance scheme 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 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 19th century, expenditures for paid sick leave initially accounted for more than half of all health insurance expenditures ([Busse and Riesberg, 2004](#)). Other European countries followed soon and also implemented paid sick leave. Today, virtually every European country provides universal access to paid sick leave.

The United States, Canada, and Japan are the only industrialized countries that do not provide universal access to paid sick leave. In these countries, sick pay is largely provided as a fringe benefit by employers on a voluntary basis ([Heymann et al., 2009](#)). In the United States, coverage rates are around 65 percent among full-time workers; low-income, part-time, and service sector workers have coverage rates of less than 20 percent ([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 10 percent of the workforce in the United States. In addition to concerns about inequality, worker well-being, and work productivity, a lack of sick leave coverage can induce contagious employees to work sick and spread diseases ([Pichler and Ziebarth, 2017](#)).

In the past decade, support for sick pay mandates has grown substantially in the United States. On the city level, the first sick pay mandates were implemented in San Francisco (2007), Washington, D.C. (2008), Seattle (2012), New York City (2014), Portland (2014), Newark (2014), Philadelphia (2015), and Oakland (2015). Several dozen cities, including Pittsburgh, Santa Monica, Los Angeles, or Chicago, have followed more recently (for an overview, see [A Better Balance \(2018\)](#)).

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; it only covers about 20 percent of the workforce ([Miller and Williams, 2015](#); [Connecticut Department of Labor](#)). We thank the following individuals and organizations for their helpful comments and suggestions: PAM, HEC Lausanne, the University of Linz (Economics Department), RAND Corporation in Santa Monica, CA, the University of Utah, the 12th Annual Conference on Empirical Legal Studies in Ithaca, NY, the 2017 Essen Health Conference, the Nordic Health Economics Study Group meeting (NHESG) in Uppsala, the 2017 meetings of the Southern Economic Association in Tampa, FL, and the Verein for Socialpolitik (VfS) in Münster. We also thank Katherine Wen for editing this paper. 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 is gratefully acknowledged. Neither we nor our employers have relevant or material financial interests that relate to the research described in this paper. We take responsibility for all remaining errors in and shortcomings of the paper.

bor, 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 pay mandates, effective July 2015 and January 2016, but exempt small businesses. In addition, Vermont, Arizona, and Washington State passed sick leave legislation very recently. Table 1 lists all citywide (nine in total) and statewide (four in total) 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 pay mandate that would cover employees in businesses with more than 15 employees (U.S. 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 seven days) per year. Paid sick leave—at the standard wage rate of 100 percent—could then be taken in case of own sickness or sickness of a relative, in most cases children.

The main source of controversy is the possibility that government mandated sick pay 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 United States. He argues that the case for a group-specific mandate may deviate from the textbook example because antidiscrimination laws or social norms may prohibit the free downward adjustment of wages for a specific identifiable group. Using the CPS, Gruber (1994) finds significant wage decreases for women of childbearing age, but no significant impact on labor supply.

The case of mandated sick pay may also deviate from the textbook example. Assuming flexible wages and absent administrative costs, earning one hour of paid sick leave per 30 hours worked equals a wage increase of $1/30$ or 3.3 percent per week for full-time employees. However, such a 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, empirical causal evidence on how work productivity changes when employees gain access to paid sick leave is lacking. It seems likely that sick employees cannot maintain full work productivity when working sick and that employees on sick leave will (partly) compensate for their lost productivity after their recovery. Hence, the calculated static wage increase of 3.3 percent appears to be an upper bound for marginal firms.

When ignoring administrative costs, changes in work productivity, and 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, antidiscrimination laws, minimum wages or because employees do not value sick leave, marginal employees might not get hired or even get fired. In addition, when small businesses are exempt from the mandate, some employers could reduce their workforce or split up their firms. 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 be significantly affected by sick pay mandates.

This paper empirically assesses how city and state-level sick pay mandates affected wages and employment in the United States. We use the Quarterly Census of Employment and Wages (QCEW) by the Bureau of Labor Statistics (BLS) for this evaluation. The QCEW is a census of all establishments that are covered by Unemployment Insurance and contains 97 percent of nonfarm employment in the United States. Our first QCEW data set records total monthly employment and quarterly wages at the county(-industry) level from January 2001 to June 2016. The second data set 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 U.S. regions and over time. To mimic pretreatment trends as closely as possible, we follow [Abadie and Gardeazabal \(2003\)](#) and [Abadie et al. \(2010\)](#) and build synthetic control groups using untreated regional units. To test hypotheses with single and multiple events, we use the approach in [Dube and Zipperer \(2015\)](#) and [Firpo and Possebom \(2017\)](#). In a recent review of the state of applied econometrics, [Athey and Imbens \(2017\)](#) call the synthetic control group method (SCGM) the most important innovation in program evaluation in the last 15 years.¹

The setting of this paper is well-suited for the application of the SCGM. First, when evaluating reforms at the county level, we can build synthetic controls using a large pool of more than 3,000 U.S. counties. To our knowledge, this is one of the very first papers to select donors out of the total pool of U.S. counties. It allows us to replicate the labor market dynamics of the treated counties very closely. Second, because the treated units are rather small and geographically dispersed, the assumption of no general equilibrium or spillover effects to neighboring regions seems justified. Third, we can match the labor market dynamics of the treated units for a long pre-reform time period. Fourth, we evaluate sick pay mandates in nine counties and four states. All these U.S. regions were treated with similar reforms

¹ Other papers that apply the SCGM or variants include [Billmeier and Nannicini \(2013\)](#); [Bohn et al. \(2014\)](#); [Bauhoff \(2014\)](#); [Bassok et al. \(2014\)](#); [Karlsson and Pichler \(2015\)](#), and [Restrepo and Rieger \(2016\)](#).

and the policies were implemented subsequently over a decade. Moreover, the regions are heterogeneous in terms of size and local labor markets and thus provide broad common support. As a result, the findings should have external validity for other U.S. counties with a similar industrial structure and policy environment. Finally, the main identification assumption of no systemic unobserved postreform labor market shocks is weak(er) when evaluating thirteen reforms over a decade.

Our findings do not provide much evidence that either employment or wages significantly and systematically increased or decreased postreform. The main point estimates have ambiguous signs and are relatively small in size. Joint tests let us exclude that employment decreases of 2 percent or more lie within the 92 percent confidence interval. Wage decreases of 3 percent or more lie outside the 95 percent confidence interval, which let us exclude that administrative and psychological costs are large (see above for a static back-of-the envelope calculation). Moreover, the results are very robust to alternative SCGM matching algorithms and when focusing on the most affected industries (such as the construction or hospitality sector). The findings are also supported by traditional Difference-in-Differences (DD) models and event studies. In sum, this paper follows [Abadie \(2018\)](#)'s call for "a visible reporting and discussion of non-significant results in empirical practice."

The next section summarizes the literature. [Section 3](#) discusses the U.S. sick pay mandates in more detail, and [Section 4](#) explains the data. The empirical approach and identifying assumptions are in [Section 5](#). [Section 6](#) discusses the empirical findings, and [Section 7](#) concludes.

2 Research on Sick Leave

Economic research on sick leave almost exclusively focuses on countries outside the United States. In the past, the simple reason has been a lack of policy variation and a lack of appropriate data. For example, high-quality administrative sick leave data exist in most Scandinavian countries ([Andr en, 2007](#); [Markussen et al., 2011](#); [Dale-Olsen, 2014](#)) but, in the United States, actual sick leave behavior is largely unobservable. There are a few exceptions. One exception is [Gilleskie \(1998\)](#), who exploits 1987 MEPS data 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, 2 percent of U.S. 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 sick leave coverage. [Ahn and Yelowitz \(2016\)](#) confirm that U.S. employees

take more sick leave when they have sick leave coverage. And [Colla et al. \(2014\)](#) find that, in San Francisco, 73 percent of all firms offered sick pay voluntarily before the mandate in 2006, and that this share had increased to 91 percent by 2009. Some reports suggest that the early mandates in San Francisco and D.C. did not have negative employment effects ([Boots et al., 2009](#); [Drum Major Institute for Public Policy, 2010](#); [Appelbaum and Milkman, 2011](#)). Using 2009 to 2012 data from the American Community Survey, [Ahn and Yelowitz \(2015\)](#) come to a similar conclusion for Connecticut.²

[Van Kammen \(2015\)](#) also uses the QCEW to evaluate U.S. sick pay mandates but a different methodological approach. His findings also differ from the findings in this paper. Using data from 2003 to 2014, [Van Kammen \(2015\)](#) restricts the sample to counties with more than 10,000 employees and 144 county-month observations, uses higher-order lags of employment as instruments, and finds evidence for generally negative employment effects (although they partly lack precision). However, using a balanced sample with select county-industry observations and interacting the treatment effects with the average number of sick days at the industry level in 2010, he concludes “the effect of paid sick days mandates is to shift employment from the least constrained industries [utilities] to the most constrained [accommodation and food services], having a practically small effect on overall county employment” ([Van Kammen 2015](#), p. 17).

Outside the United States, several empirical papers estimate the causal effects of variation in sick pay. These studies find that employees adjust their intensive labor supply in response ([Johansson and Palme, 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](#); [Kostol and Mogstad, 2014](#); [Borghans et al., 2014](#); [Burkhauser et al., 2016](#)). It is closer to U.S. studies on work-related accidents and diseases covered by Workers’ Compensation ([Meyer et al., 1995](#); [McInerney and Bronchetti, 2012](#); [Powell and Seabury, 2018](#); [Hansen, 2016](#)).

Other papers on sick leave investigate the role of general determinants ([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

²Similar to the findings in this paper, [Colla et al. \(2017\)](#) do not find evidence that the 2008 employer health benefit mandate for non-small employers had a substantial effect on employment and wages in San Francisco.

on the impact of sick leave on earnings (Sandy and Elliott, 2005; Markussen, 2012). In addition, some papers study the phenomenon of presenteeism explicitly (Brown and Sessions, 2004; Pauly et al., 2008; Barmby and Larguem, 2009; Pichler, 2015; Pichler and Ziebarth, 2017).

Finally, note that paid sick leave differs from paid vacation or paid maternity leave in both scope and aim (Rossin-Slater et al., 2013; Lalive et al., 2014; Baum and Ruhm, 2016; Dahl et al., 2016; Thomas, 2018). Whereas sick leave coverage 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 pay mandates 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, 2017; Stearns and White, 2018).

3 U.S. Sick Pay Mandates

The United States is one of three OECD 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).

The only existing federal law is *The Family and Medical Leave Act of 1993 (FMLA)*. It provides *unpaid* leave in case of pregnancy, own disease, or disease of a family member to employees who work at least 1,250 hours annually in businesses with at least 50 employees (see, for example, Tominey, 2016). Jorgensen and Appelbaum (2014) find that 49 million U.S. employees are ineligible for FMLA, 44 percent of all private sector employees. The findings in Susser and Ziebarth (2016) also suggest that many low-wage and service sector employees are either not aware of their FMLA rights or that they are not covered by the law. As a result of the sick pay mandates analyzed in this paper, most employees without firm-provided sick pay gained access to sick leave coverage.

Table 1 provides a summary of the mandates evaluated by this paper. The details of the bills differ from city to city and state to state, but basically all sick pay mandates are employer mandates. Several mandates exclude small firms or offer exemptions. Employees “earn” a sick pay credit (typically one hour per 30-40 hours worked up to 7 days per year) and, if unused, the credit rolls over to the next calendar year. Because employees need to accrue sick pay credit, most mandates explicitly state a 90-day accrual period in addition to waiting periods when changing jobs. Moreover, several bills that exempt small businesses still require them to let their employees accrue *unpaid* sick days instead of paid sick

days ([Massachusetts Attorney General's Office, 2016](#)). Several laws explicitly prohibit employers from requiring a doctor's note ([Polsky, 2016](#); [New York City Consumer Affairs, 2016](#)).

As [Table 1](#) shows, San Francisco was the first city to mandate paid sick leave effective February 5, 2007.³ Washington D.C. enacted its mandate effective November 13, 2008, and expanded the mandate on February 22, 2014 to include temporary workers and tipped employees. Seattle (September 1, 2012), Portland (January 1, 2014), New York City (April 1, 2014), and Philadelphia (May 13, 2015) followed.

Connecticut 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 percent of the workforce. The mandates of California (July 1, 2015), Massachusetts (July 1, 2015), and Oregon (January 1, 2016) are much more comprehensive (see [Table 1](#)).

4 Quarterly Census of Employment and Wages (QCEW)

The paper makes use of publicly available data from the QCEW, which is provided by the [Bureau of Labor Statistics \(BLS\) \(2018\)](#). The QCEW is based on an establishment census. It includes all establishments covered by Unemployment Insurance (UI)—97 percent of all U.S. civilian employment.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 at different levels of spatial and timely disaggregation. To evaluate reforms at the county and state level (see [Table 1](#)), we generate two data sets, one at the county level and one at the state level. Both the county and state-level data are available from January 2001 to June 2016. The raw data are reported by industry. Because the mandates mostly apply to the private sector, we generate variables that measure private sector employment and private sector wages. While, the QCEW is available at the state level by county-industry-firm-size, at the county level it is only available by county-industry.

³ In the case of San Francisco, two laws that went into effect January 2008 could potentially confound a clean assessment of the sick pay mandate. First, the minimum wage has been increased in predetermined steps annually from \$8.50 in 2004 to \$9.79 in 2009. Second, the *Health Care Security Ordinance* set minimum rates for employee health care spending by employers (those vary by firm size and for-profit status).

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

4.1 County-Level Data

Table 2 provides the summary statistic for the county-level data. The table shows summary statistics for 3,062 counties.⁵ As for employment, the data are at the monthly level, yielding a total of 548,992 county-month observations.⁶ As for wages, the data are at the quarterly level with a total of 182,992 county-quarter observations. Population counts are at the annual level with a total of 44,267 county-year observations (U.S. Census Bureau, 2016b).

We generate several outcome variables for the county-level analysis. The first main outcome variable is *Private Sector Employment*, which we obtain by dividing the total number of filled jobs at the monthly county level by the annual county level population. This yields private sector jobs as a share of the county population for each U.S. county on a monthly basis. Table 2 shows that the average private sector employment share is 27.1 percent; the average public sector employment share is 7.7 percent. This means that, on average, for every 100 residents in a county in the United States, 27 private sector jobs paying UI contributions are officially reported.

Note that individuals who hold multiple jobs are counted for every job that 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) that some counties have significantly higher employment ratios than others, and even employment ratios above 100 percent. Whereas the minimum value for *Private Sector Employment* is only 1.1 percent, the maximum value is 404 percent (Table 2).

As shown in Table 2, while the county-level data do not allow us to differentiate by firm size, we can differentiate by industry. Consequently, in extended analyses, we will test whether mandated sick pay resulted in weaker job growth in the construction (1.5 jobs per 100 pop.) and the leisure and hospitality industries (3.3 jobs per 100 pop.), as these two sectors were most affected by the mandates; prereform, around 70 percent of all employees in these sectors did not have access to paid sick leave (Susser and Ziebarth, 2016).

⁵In total, the United States has 3,143 counties or county-equivalents. The missing counties in our data are counties without any official establishment location, e.g., in very rural counties in Alaska (U.S. Census Bureau, 2016a).

⁶To obtain one consistent baseline data set, we do not include all available data points from January 2001 to June 2016 but only include observations that we also use in the traditional difference-in-differences Section 6.1 where we only consider data points up to 48 months prior to the treatment.

The second main outcome variable is *Weekly Wages*. Employers report total quarterly gross compensations, including bonus payments and stock options.⁷ Gross wages are then derived by dividing the total quarterly compensation by the total quarterly employment. Dividing additionally by the number of weeks yields 182,992 county wage observations at the weekly level in Table 2. The average weekly wage is \$599 (or \$31,200 per year), but the variation ranges from \$155 to \$4,542. Because Consumer Price Indices are not available at the county level, throughout the paper we use nominal wages. However, we net out wage seasonalities by regressing the wage dynamics of each county on a full set of quarter-year fixed effects.

In addition to these two main outcome measures, we use the industry structure variables in Table 2 to build synthetic control counties that resemble the treatment counties as closely as possible.

4.2 State-Level Data

Table 3 provides the summary statistic for the state-level data. When considering all 51 states, we obtain 8,981 state-month observations for employment and 2,992 state-quarter observations for wages. Using the state-level data, this paper evaluates the sick pay mandates in Connecticut, California, Massachusetts, and Oregon.⁸ The Connecticut mandate only applies to private firms with more than 49 employees in the service sector, and the mandates in Massachusetts and Oregon only apply to private firms with more than 9 employees (Table 1). Because the QCEW data are broken down by industry and firm size (at the state level), we carve out employment and wage dynamics for private firms with more than 49 employees in the service sector in Connecticut. For Oregon and Massachusetts, we generate all outcome variables for private firms with more than 9 employees.⁹

⁷ We send an inquiry to the BLS to double check which fringe benefits are included in the reported quarterly wage and received the following response on May 30, 2018: “Covered employers’ contributions to old-age, survivors, and disability insurance; health insurance; UI; workers compensation; and private pension and welfare funds are *not* reported as wages. Employee contributions for the same purposes, however, as well as money withheld for income taxes, union dues, and so forth, are reported, even though they are deducted from the workers gross pay.”

⁸ We do not include Washington D.C. in the state-level analysis because the synthetic control group fit was superior with counties.

⁹ Because the data by industry and firm size are only reported for the first quarter of each year, we impute values for the other quarters assuming that the first quarter ratios of, e.g., *<50 employees* vs. *>49 employees*, remain stable in the other three quarters. For two firm size categories in Delaware, we impute missing values for 2014.

Analogous to the county-level data, the upper panel of Table 3 shows that, overall, private sector employment was 37.3 and public sector employment 8.0 per 100 population at the state level. The average weekly wage was \$805, and the state population was on average 5.6 million.

The lower panel of Table 3 lists *Private Service Sector Employment >49 employees* as one main outcome variable for Connecticut. Across all U.S. states, for every 100 residents of a state, 15.2 people worked in the service sector and in establishments with more than 49 employees. *Private Sector Employment >9 employees* (31.2 per 100 pop.) is a main outcome variable for Oregon and Massachusetts. In contrast, general *Private Sector Employment* is one main outcome measure for California, where the mandate does not exempt small businesses.

4.3 Treatment and Control Regions

Treatment Regions. Table 1 lists treated cities and states. Whereas we evaluate all regions listed and also provide graphs for all regions, some regions (e.g., employment in Washington, D.C. , or wages in New York City and Hudson County) provide examples of where the SCGM is not a valid evaluation method due to a poor fit. For example, in the case of D.C. the fit is poor because of the following three reasons: 1) D.C. has a very unique employment structure with many nonprofit, public sector, and lobbying jobs. Thus, finding appropriate control counties for D.C. is very challenging. 2) D.C.'s original mandate had many exemptions that are difficult to model with our data (e.g., no health care or restaurant workers). Moreover, D.C. extended the mandate in September 2014, but retrospectively effective February 2014. 3) The first D.C. mandate became effective shortly after the Great Recession hit in October 2008. This makes it very challenging to disentangle labor market effects due to the mandate from the confounding effects of the Great recession. Because of the first reason, the recession also affected D.C. differently than most other U.S. counties.¹⁰ To deal with special cases such as D.C., we experiment with several alternative SCGM modeling approaches but remain cautious when drawing conclusions.

Table 1 lists all city mandates along with the relevant counties. However, city and county boundaries are not always identical. First, the case for San Francisco (SF) is clear given that the city boundaries equal the county boundaries.

¹⁰ As another example, Jersey City (Hudson County) has many small entrepreneurial businesses and a large Finance, Insurance, and Real Estate industry. It lies just across the Hudson River opposite Manhattan.

Second, we do not separately evaluate the five boroughs of New York City (NYC)¹¹ but, using the simple employment and wage averages of all five, aggregate them to one regional unit for three reasons: 1) The five boroughs together represent the entire area where the law formally applied. 2) Employment ratios and wages in Manhattan are extremely high and they are relatively low in the other boroughs. Moreover, most people who work in NYC live in one of the four surrounding counties and commute to Manhattan. 3) NYC can be seen as one integrated labor market and not five separate ones. For these reasons, we treat NYC as one statistical unit.

Third, in the case of Portland, Seattle, Newark, and Jersey 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). Seattle, Newark, and Jersey City 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 (U.S. Census Bureau, 2016c). The fact that these three cities only make up 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 cities as in the cases of SF and NYC).

If businesses relocated (due to the mandate) just outside the city boundaries but within the treatment county boundaries, our method would not be able to identify such “border jumping” for Portland, Seattle, Newark, and Jersey City. However, comparing the results for these treatment counties with SF and NYC indirectly tests whether firms relocated 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 (SF and NYC) but no impact for entire counties that surround the core cities (Portland, Seattle, Newark, Jersey City).

Control Regions. We employ the SCGM to model an ideal hypothetical control region for each treatment region. Table 2 lists *private sector employment*, *public sector employment*, *production employment* and *service sector employment*, along with employment shares of specific industries such as *manufacturing*, *education and health services*, or *leisure and hospitality*. In some modeling approaches, we will use all of these industry structure variables to find suitable control “donor” counties. In other words, in addition to identical prereform outcome dynamics, the SCGM algorithm selects control coun-

¹¹ These are Manhattan, Kings County (Brooklyn), Bronx County, Richmond County (Staten Island), and Queens County. We experimented with excluding Manhattan when averaging. The results are very similar and are available upon request.

ties with similar labor market and population structures than the treatment counties. Tables [A1](#), [A2](#), [A6](#), and [A7](#) list all donor counties and states chosen to replicate the pretreatment employment and wage dynamics of each treatment region as closely as possible. Section [5](#) below provides more details on the estimation procedure. We also provide evidence from traditional DD models that use all nontreated counties or states jointly as control units.

4.4 Sample Selection

The baseline data sets in Tables [2](#) and [3](#) are already restricted as follows: For each treatment region, we focus on four pretreatment years (48 months or 16 quarters). Moreover, depending on when exactly the mandate was enacted (Table [1](#)), the postreform periods differ by treatment regions.

The county-level data set in Table [2](#) contains 3,062 unique counties. For the county-level SCGM analysis (*not* the state-level SCGM analysis or the traditional DD analysis), we additionally preselect suitable donor counties. (We do this because running the SCGM with 3,062 donor counties would technically be unfeasible due to multiple equilibria and too many degrees of freedom.) Specifically, we separately rank all 3,062 available counties on the three dimensions *county population*, *private sector employment* and *private sector wages*. Then, we select counties ranked above and below the treated county using a bandwidth of 500 ranks for the first dimension *county population*. Next, we proceed with the same procedure on dimensions two and three using the variables *private sector employment* and *private sector wages*. Finally, we use the counties that overlap on all three dimensions and fall within a ranking bandwidth of +/- 500 ranks on each dimension. This preselection procedure results in about 200 potential control counties for each treatment county (for exact values, see the denominator in column (5) of Table [4](#)).

5 The Synthetic Control Group Method (SCGM)

To assess the causal effects of the sick pay mandates on employment and wages, we use [Abadie and Gardeazabal \(2003\)](#)'s SCGM, along with traditional DD models in robustness checks. The SCGM uses fractions of several natural control units to build an ideal—synthetic—control group whose prereform outcome dynamics mimic those of the treatment group ([Abadie et al., 2010](#)). Given the assumptions discussed below, differences in postreform outcome dynamics between the treatment and the synthetic control group then yield evidence on causal reform effects ([Athey and Imbens, 2017](#)).

In our context, following Table 1, the treatment units are counties or states that implemented sick pay mandates; the potential control units consist of the remaining U.S. counties or states. Because we analyze each treatment unit separately, the notation below refers to a single treatment and J control units.

Let y_{it}^0 denote the natural logarithm of the outcome ($y_{it}^0 = \ln(Y_{it}^0)$) that would have been observed in region 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 region i at time t , where the sick pay mandate was implemented at time $T_0 + 1$. We assume $y_{it}^1 = y_{it}^0 \forall t = 1, \dots, T_0, \forall i = 1, \dots, J + 1$.

Abadie et al. (2010) suggest that the following factor model represents the counterfactual y_{it}^0 :

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

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 treatment endogeneity because the treatment can be correlated with unobservables. However, the method still requires several identification assumptions.

First, in our case, one necessary assumption is that employment rates and wages in the control regions are not affected by the treatment. This implies the absence of spatial labor market spillovers. When evaluating counties, the treatment units are rather small and thus unlikely to trigger large labor market spillover effects. Also, in most cases, the treated counties are geographically distant from the control counties. Tables A1, A2, A6, and A7 list all counties and states used to build the synthetic control units. For example, the donor counties to evaluate employment dynamics in King County (WA) illustrate that the “no spatial labor market spillover” assumption is rather weak: to replicate King County (WA), the control donors are Fulton (GA), Somerset (NJ), Mecklenburg (NC), Durham (NC), Denver (CO), Madison (AL), Harris (TX), Midland (TX), Winnebago (WI), and Mercer (NJ).

Second, similar to traditional DD models, no unobserved shocks should affect the outcome differently for treatment and control groups in postreform periods. In our case, shocks violating this assumption would be other labor market policies that are correlated with sick pay mandates in treated regions (but not in control regions). The SCGM may consider such shocks (better than traditional methods) because the synthetic control units are, by construction, built to replicate the outcome dynamics of the treated unit (which includes unobservables affecting such outcome dynamics).

Third, again similar to traditional DD models, treatment-induced migration could lead to biases. If employment prospects worsened due to sick pay mandates and employees lost their jobs, they might migrate to more prosperous counties. Likewise, firms could relocate in response to mandates. For several reasons, economic migration is unlikely to be a major confounder in our context: Our data and outcome measures allow us to directly test for such possible migration pattern. In fact, it is precisely one objective of this paper to test for changes in employment *rates*. Recall that we use official population data and normalize employment. Because we stratify the effects by the time since implementation, we would identify negative employment effects due to migration over time. When evaluating county effects, the treatment counties are unlikely to contaminate the donor counties (which are chosen out of a total of 3,062 U.S. counties) by worker or firm relocation. In robustness checks, we also test for spillover effects on neighboring counties and states.

Lastly, in most SCGM settings, only one single treatment unit is evaluated. We rely on 13 different treatment regions—counties and states of different sizes. Single unobserved shocks may confound one county or state. But it is very unlikely that 13 treatment units, with staggered treatments from 2007 to 2015, were all coincidentally affected by random unobserved labor market shocks unrelated to the mandates.

5.1 Implementation

SCGM requires the estimation of two matrices: 1) V is the weighting matrix determining the relative predictive power of Z_i and of y_i^0 , and 2) W is a vector of nonnegative weights attached to the J control countries. The criterion to be minimized is

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

where \bar{X}_1 and \bar{X}_0 are vectors of averages over the pretreatment elements of Z_i and y_i for treated and control units, respectively. In our case, \bar{X}_1 and \bar{X}_0 include the variables in Tables 2 and 3. This means that, for the main county and state-level analysis, \bar{X}_1 and \bar{X}_0 include *private sector employment* and its subcategories *service sector employment* and *production sector employment* along with *public sector employment* as well as *private sector wages*. To avoid criticism of overfitting, we only include these variables at the following points in time before the treatment: 36 months, 24 months, 12 months, and 1 month. In industry-structure robustness checks, we additionally use the employment shares in *manu-*

facturing; professional and business services; education and health services; trade; transportation and utilities as well as leisure and hospitality.¹²

We obtain an optimal weight matrix $W^*(V)$ among all diagonal positive definite matrices, where the elements of V minimize the distance to the outcome. This optimal weight matrix minimizes the root of the mean squared prediction error (*RMSPE*) for prereform periods:

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

where T_0 represents the number of prereform time periods, i.e., in our case 48 months or 16 quarters. In alternative specifications, we use six instead of four prereform years and stop minimizing the *RMSPE* 24 months prior to the treatment.

5.2 Treatment Effects and Inference

In addition to calculating the *RMSPE* for the prereform periods, we also calculate the *RMSPEs* for the postreform periods and take the ratio of the two, as suggested by [Abadie et al. \(2010\)](#). Whereas the prereform *RMSPE* is an indicator to assess the *fit* of the synthetic control group, the ratio between post and prereform *RMSPE* indicates the *size* of a possible treatment effect. Assuming a stable model fit over time, a *RMSPE post/RMSPE pre* > 1 would indicate a larger post than prereform *RMSPE* and thus a treatment effect.

However, the *RMSPE Ratio* is only a measure of the relative treatment effect. The sign of the treatment effect remains ambiguous. Therefore, we calculate the *Percent Treatment Effect (PTE)* as

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

and the *Level Treatment Effect (LTE)* as

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

¹² In our main analysis, we do not consider the full set of industry-structure variables in \bar{X}_j due to memory and computing constraints.

In principle, the sign of the treatment effect could change over time. Then positive and negative effects would cancel each other out. Still, then the *PTE* and *LTE* would provide evidence on the cumulative sign and size of the long-run effect over all postreform periods.

To conduct inference, we follow [Abadie et al. \(2010\)](#) and run placebo estimates.¹³ Because we assess multiple treatments at different points in time, we first construct placebo estimates for each treatment unit. Then we rank the treated and all placebo estimates by their *RMSPE Ratios*. Following [Abadie et al. \(2010\)](#), the rank of the true treatment estimate relative to the N placebo estimates then determines the p-value of the H_0 hypothesis of no treatment effect ($H_0 : RMSPE Ratio_{Treat} \leq RMSPE Ratio_{Placebo}$). Mathematically, the p-value results from the percentile rank $p = \hat{F}(RMSPE Ratio_e)$ for the event e , where \hat{F} stands for the empirical cumulative distribution of all *RMSPE Ratios*, as obtained by the placebo estimates. For example, if the true treatment county had the highest rank among $99 + 1$ (placebo + treatment) counties, the p-value would be $1/100 = 0.01$, the treatment effect would be highly significant, and the H_0 of no treatment effect could be rejected. In the results section, we carry out this testing procedure for the *RMPSE Ratio* ([Firpo and Possebom, 2017](#)). Finally, we follow [Dube and Zipperer \(2015\)](#) and calculate joint p-values based on the sum of the single p-values using the Irwin-Hall distribution.

As in the standard parametric case, p-values can 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 to analyze how narrow the confidence intervals are. To do so, we follow the basic procedures in [Dube and Zipperer \(2015\)](#) and [Firpo and Possebom \(2017\)](#): We set the hypothetical average treatment effect over all postreform periods equal to z percent. Next, we recalculate the *RMSPE Ratio* with this hypothetical average treatment effect of z percent.¹⁴ Then we carry out all N placebo estimates as above to assess the probability that our treated unit (with the artificially set z percent treatment effect) originates from that distribution. Accordingly, we calculate p-values and test the null hypothesis that the treatment effect equals zero. Using the notation above, this means that we calculate modified p values, namely $p = \hat{F}(RMSPE Ratio_{ez})$.¹⁵ To provide additional intuition: In the SCGM setting, placebo estimates are usually produced to check whether the treated unit differs from the placebo units. The placebo units are, by definition, non-treated units with

¹³ An alternative would be subsampling methods ([Politis and Romano, 1994](#); [Saia, 2017](#)).

¹⁴ Because we only set the average effects to z percent, this method implicitly keeps the original variance between treated and synthetic control units.

¹⁵ [Dube and Zipperer \(2015\)](#) propose a similar test based on elasticities. Moreover, in a previous version ([Pichler and Ziebarth, 2016](#)) we construct the test statistic using the *LTE* and the *PTE*. However, as pointed out by an anonymous referee, [Firpo and Possebom \(2017\)](#) show in a simulation exercise that using the *RMSPE Ratio* is preferable because of statistical power.

a treatment effect of zero. Here, this basic idea is modified and we assign an artificial treatment effect of z percent. Then, as in the standard case, we assess the likelihood that the artificially treated unit stems from the distribution of nontreated placebo units. This procedure then shows whether we have enough power to reject the null of no effect, given that we assigned a known treatment effect of z percent.

6 Results

Section 6.1 begins by evaluating the overall employment and wage effects of the city-level mandates using our county-level QCEW data set and the SCGM. As falsification exercises, we run four alternative SCGM modeling approaches that yield robust findings and provide better fits for a few counties (and worse fits for others). Next, we provide complementary evidence from traditional DD models and event studies. The subsequent subsection provides heterogeneity tests by specifically investigating effects in the construction and hospitality sectors at the county level; those sectors were particularly affected by the mandates. We also test for spillover effects of the policy to counties neighboring the treatment counties. Then, Section 6.2 provides evidence on labor market effects at the state level. Analogous to the city level case, we also investigate alternative modeling approaches, the construction and hospitality sector, as well as evidence for spillover effects. Section 6.3 discusses potential explanations for possible heterogeneity in effect sizes.

6.1 Labor Market Effects of City-Level Mandates

County-Level Employment and Wage Effects Using the Standard SCGM Approach

Figure 1 shows the evolution of county-level employment in five treatment counties as listed in Table 1. (The equivalent graphs for the remaining four counties are in the appendix, in Figure A1.) In the left column of Figure 1, the solid lines represent the treatment counties and the dashed lines represent the synthetic control counties. The composition of each synthetic control county—the weights W of the J control counties—are in Table A1. The solid vertical lines at point zero on the x-axes represent the months when the sick pay mandates went into effect and were enforced. The dotted lines to the left indicate when the bills were passed; they test whether there is evidence of anticipation effects. The dotted lines to the right indicate the end of the accrual periods.

Figures 1 and A1 illustrate, first, substantial differences in employment rates. Whereas San Francisco and King County have employment rates of around 50 percent of the population, the rates for NYC and Philadelphia are below 40 percent. Second, the employment dynamics of treated and synthetic control counties are basically identical in the prereform periods, suggesting that the SCGM produces valid counterfactuals. (One obvious exception is Washington, D.C., in Figure A1.) Third, visually, it is difficult to identify sizable and systematic reform-related employment effects. In postreform periods, the employment dynamics appear to be identical for all cities displayed. Fourth, to quantitatively evaluate the SCGM fit between treated and controls, to assess potential employment effects, and to conduct inference, we follow Abadie and Gardeazabal (2003) and Abadie et al. (2010) and show all relevant statistics in Panel A of Table 4.

Column (1) of Table 4 shows the *Employment Ratio*, Y_{it}^1 , defined as private sector employment as a share of the county population—averaged over all prereform periods.

Column (2) shows the *RMSPEs* for prereform years as specified in 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 D.C. (which we disregard due to a poor fit but show for completeness in Figure A1), all prereform *RMSPEs* are very low—at around 1% of the outcome measure. This implies a very successful replication of the employment dynamics of the treatment counties by the SCGM. As a comparison, evaluating the effects of a tobacco control program on cigarette consumption in California, Abadie et al. (2010) have a prereform *RMSPE* of 3 relative to a mean of about 100.

Column (3) shows the *RMSPEs* for postreform years. They appear to be slightly larger than prereform. This conjecture is confirmed by the *RMPSE Ratios*, which are shown in column (4) and divide column (3) by column (2). The *RMPSE Ratios* lie between 1 for Alameda County and 4 for San Francisco. (As comparison, Abadie et al. (2010) report significant results and a ratio of 11.4.)

Next, we conduct inference using placebo methods (see Section 5). For each treatment county, as described in Section 4.4, we select nontreated placebo counties with similar labor markets and demographics. Then we replicate the standard SCGM procedure with each placebo county pretending it had been treated at the same time as the real treatment county. Column (5) illustrates the calculation of the p-values for the hypothesis $H_0 : RMSPE Ratio_{Treat} \leq RMSPE Ratio_{Placebo}$, which is simply the rank of the *RMSPE Ratio* of the treated county divided by *#Total Counties Assessed*. In other words: After cal-

culating the *RMPSE Ratio* for each placebo county and ranking all of them, we can assess the position of the true *RMPSE Ratio* for the treated county in the test statistic distribution (Abadie et al., 2010). As seen in column (5), the total number of SCGM runs for each treatment county varies between 83 and 199 (placebo + 1). Moreover, the ranks of the true treatment county lie between 23 (NYC) and 139 (Alameda). Accordingly, except for NYC ($p=0.13$), none of the p-values is even close to being statistically significant at conventional levels.

We also calculate the sum of all p-values (excluding D.C. because of a poor *pre-RMSPE* fit) and then evaluate their joint p-value based on the Irwin-Hall distribution (Dube and Zipperer, 2015). The overall p-value for the county-level employment effect is 0.25.

The right columns of Figures 1 and A1 display the permutation inference using placebo tests graphically. Following the convention in the literature, the graphs plot the differences in the logarithms of the employment ratios (solid black) along with the differences for all placebo SCGM runs (gray) with good fit ($\text{RMSPE}_{\text{placebo}} \leq \text{RMSPE}_{\text{Treat}} \cdot 2$). As seen, for prereform 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, which is indicated by the black solid vertical line, employment differentials between treated and control counties remain very small and straight flat for most counties. One exception is SF, where the differential appears to be even positive, although it is not significant in a statistical sense.

Column (6) of Table 4 shows the *Percent Treatment Effect (PTE)*, and column (7) shows the *Level Treatment Effect (LTE)* for the postreform periods; the LTE is private sector employment as a share of the county population. As seen, the signs of the calculated treatment effects are ambiguous (four are negative and four are positive); none are statistically significant at conventional levels.

Finally, columns (8) and (9) test whether we have enough statistical power to reject potential employment decreases of 3 and percent, respectively (see Section 5.2). Both columns provide p-values for the hypotheses $H_0 : \text{PTE}_{\text{Treat}} = 0$; inference is again based on the rank of the *RMSPE Ratio* (Firpo and Possebom, 2017). The bottom of Panel A provides the joint p-values for all counties (excluding D.C.), which is 0.08 in column (8), implying that we could identify employment decreases of 2% with 92% statistical probability.

Next we evaluate wage effects using graphs and test statistics in Figures 2 and A2 and in Panel A of Table 5. The structure follows that of employment effects. Recall that the wages are quarterly nominal wages that have been detrended of seasonal fluctuations (Section 4).

Figures 2 and A2 show positive wage dynamics representing rising nominal wages. Not only do the wage levels differ substantially between local labor markets, but so do the slopes representing wage growth. This is why we decided against further manipulation of the raw data, for example, 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, however, would not capture the properties of the local labor markets appropriately.

As illustrated by the many prereform *RMSPEs* below 0.03 (column [2] of Table 5), most treatment regions show very good pre-treatment fits among the treated and the synthetic control counties. However, using this standard SCGM approach, it was impossible to find synthetic control groups with “acceptable” fits for NYC and Hudson County.¹⁶ The reasons are the nonrepresentative wage levels in NYC (by far the highest wages among all treatment regions, column (1) of Table 5), as well as in Jersey City (Hudson County).

Furthermore, Table 5 shows the statistically insignificant *RMSPE Ratios* (column [5]) and that the PTEs fluctuate without any clear trend between -0.5% (Essex County) and +5.4% (Alameda County) in column (6). Overall, there is not much evidence for significantly weaker wage growth as a result of mandating sick pay. Visually, it is hard to detect substantial and systematic wage effects (Figures 2 and A2). According to the county joint tests (which exclude NYC and Hudson County because of the poor *pre-RMSPE* fit), we could identify wage decreases of 3 percent with a statistical probability of 95%. Below, in Section 6.3, we provide a detailed discussion of expected effect sizes. Because the mandates force employers to provide one hour of paid sick leave for every 30 to 40 hours worked, a static calculation that ignores administrative and psychological (“business climate”) costs would yield wage decreases of up to 3.3% for marginal firms.

¹⁶ There is no firm threshold for an “acceptable” fit defined by the literature. We consider the fit acceptable for an $RMSPE < 0.1$. Using our “alternative modeling approaches” below, we find acceptable fits. The same is true when we disregard Manhattan when evaluating NYC (available upon request). These alternative approaches also yield no evidence for treatment effects.

Alternative SCGM Modelling Approaches

Figure 3 visually compares our standard modeling approach with four alternative SCGM modeling approaches for five counties and employment (left columns) as well as wages (right column). Figure A3 shows the results for the remaining four treatment counties. The black solid lines are the benchmark and depict the differences in outcome dynamics using our standard SCGM procedure, i.e., they equal the right columns of Figures 1 and 2. (Note that we reset the SCGM algorithm and the selection of donor counties at the beginning of each of these alternative modeling approaches.)

The black dashed lines represent an approach that uses additional covariates on the industry-structure of the county to select synthetic control and placebo counties (see Table 2 for the covariates and Section 5.2 for the procedure). The gray solid lines use six instead of four pretreatment years; and the gray dashed lines use six instead of four pretreatment years but stop applying the SCGM algorithm two years before the law’s enactment. Finally, the light gray solid lines use a modified outcome variable based on the log difference between the outcome in the current period and the period before the law’s enactment. The main statistical indicators for each approach are in Table A3 (Appendix).

In summary, the findings of our main modeling approach are fairly robust to these alternative modeling approaches. All lines fluctuate closely and mostly in a parallel fashion. However, there are a few instances where these alternative approaches, in particular (v), help to improve the modeling fit for counties with a bad fit. This applies to employment in Washington, D.C. (Figure A3, left column, third row) and wages in Hudson County (Figure A3, right column, last row). In terms of content, these alternative modeling approaches with improved fit corroborate the main findings of no employment or wage effects. Also, while the modeling fit of a few treatment counties can be improved with alternative SCGM approaches, it worsens for other treatment counties. We conclude that the standard modeling approach performs reasonably well for the majority of counties, but alternative SCGM modeling techniques can help to improve the fit if this is not the case.

Traditional Difference-in-Differences Approaches

This subsection runs traditional DD models as robustness checks. We use the baseline QCEW data set as in Table 2 and keep all nontreated counties as control counties in the sample. Then we exploit variation

in the implementation of city-level mandates across counties and over time by estimating the following model:

$$y_{it} = \alpha + \beta TreatedCounty_i \times Law_t + \gamma TreatedCounty_i \times Law_t \times LawTime_t + \theta Z_{it} + \delta_t + \rho_i + \rho_s \times t + \epsilon_{it} \quad (6)$$

where $y_{it} = \ln(Y_{it})$ is the logarithm of the outcome variables in county i at time t as above. $TreatedCounty_i$ is a dummy that indicates counties that implemented sick pay mandates, and Law_t is a postreform dummy. Thus, β represents the standard average DD treatment effect for postreform periods. In some specifications, we additionally estimate a second treatment coefficient, γ , that provides information about the slope of the treatment effect over time for postreform periods (e.g., to consider the possibility that the effects increase slowly over time, see [Lafortune et al. \(2018\)](#) for a similar application).

δ_t represents month-year or quarter-year fixed effects, ρ_i are county fixed effects, and $\rho_s \times t$ are state-specific time trends. Z_{it} is a vector of county-year specific control variables.

Table 6 shows the results for 14 DD models, where each column in each panel represents on model as in Equation (6). Panel A shows the findings for employment, and Panel B shows the findings for wages. Even and uneven columns differ by the sets of covariates included, as indicated in the bottom of the table. We run three main specifications for both wages and employment, where $LawEffective_t$ represents the month when the mandate became effective, $LawPassed_t$ the month when the bill was passed, and $ProbationOver_t$ the month when the accrual period was over (see Section 3). In addition, column (7) reports two specifications where we simultaneously control for $TreatedCounty_t \times LawPassed_t$ and $TreatedCounty_t \times LawEffective_t$.¹⁷

None of the 14 main DD coefficient estimates is statistically significant from zero. Moreover, the approximate point estimates in percent of the outcome are relatively small and fluctuate around 0.5 percent. In addition, the estimates' signs are not consistently positive or negative but alternate.

¹⁷This is feasible as the time elapsed between the passage and the implementation of the mandates varies across regions. However, we abstained from additionally controlling for $TreatedCounty_t \times ProbationOver_t$ as the accrual period is almost always 90 days; thus we would run into multicollinearity issues, particularly with the quarterly wage data.

Finally, we plot two standard event studies in Figure 4. Technically, we replace the binary Law_t indicator in Equation (6) by a continuous time indicator counting the months (or quarters) up to and since the reform became effective. (Note that we also control for state-specific time trends in the event study specification.) The point estimates for these time dummies are then plotted in Figure 4, where zero on the x-axis indicates when the mandate became effective.

In line with the findings in Table 6, Figure 4 shows relatively smooth estimates without much trending. Almost none of the point estimates, either before or after the reform, is statistically significant from zero. This is illustrated by the gray bars, representing the 95 percent confidence interval, which fully covers the horizontal x-axis.

Evidence from the Construction and Hospitality Sectors

Now, we zoom into specific sectors of the economy. So far, we have evaluated employment and wage effects for entire counties and all sectors. However, it is known that some sectors were more affected by the mandates than others. In particular, in the construction and service sector industries, prereform sick leave coverage rates had been very low—only at around 30 percent (Susser and Ziebarth, 2016).

Figure 5 and Figures A4 to A7 show results for employment in the construction and hospitality. Due to space constraints, we graphically solely show results for San Francisco, Philadelphia, King County, NYC, Multnomah, and Essex County (the remaining graphs are available upon request). The test statistics for all counties and states are in Table A4. For each sector and outcome variable, we reset the SCGM algorithm and the selection of donor counties using only labor market outcomes of the specific sector under consideration.

First of all, focusing on specific industries within counties comes at the cost of slightly worse, but still acceptable, prereform $RMSPE$ fits (columns [2] and [7] of Table A4). The graphical evidence confirms this impression. Particularly for employment in the construction sectors of Philadelphia and King County, the SCGM does not perform well. But, given the very low levels of construction in these two counties (<1 percent), and the fact that we had to drop counties with zero construction employment (because we take the log before applying SCGM), this is perhaps not surprising. As for the other coun-

ties, we again do not see much evidence that employment systematically and significantly increased or decreased.

As for employment in the hospitality sector, the prereform *RMSPE* fit is better. However, the results still show no consistent pattern. Judged by the test statistics, most *PTEs* are positive (Table A4). The statistic for Washington, D.C., is statistically significant at the 6 percent level (with a mediocre fit) and the statistic for San Francisco is statistically significant at the 9 percent level. Graphically, there appears to be suggestive evidence that employment in the hospitality sector may have even *increased* in San Francisco (Figure 5, right column, third row). No such evidence exists for the other counties.

The case is very similar for wages dynamics in both sectors as shown by Figures 6, A6, and A7 as well as Table A4. When focusing on cases with a good *pre-RSMPE* fit, the evidence suggests either no effects (Philadelphia, Essex, and King County for Construction) or suggestive light evidence for rising wages (King and Essex County for Hospitality).

Testing for Spillover Effects on Neighboring Counties

The final robustness check tests whether there is any evidence for spillover effects of the mandates on neighboring counties. Although we do not find much evidence for systematic employment or wage effects, it is conceivable that some businesses relocated just outside the county boundaries to circumvent the mandate. It is also conceivable that hypothetical (positive or negative) labor market effects spread to neighboring counties.

Table A5 (Appendix) shows the results of the spillover tests for neighboring counties. First, although there are a few exceptions, columns (1) and (5) show very good prereform *RMSPE* fits. Second, as shown by columns (3) and (7), the *PTEs* do not have consistent signs: 18 out of 38 tested neighboring counties have negative employment signs, and the remainder have positive employment signs.

In terms of statistical significance, to conduct thorough statistical inference, one would have to calculate placebo estimates for all 38 neighboring counties and around 150 placebos for each neighbor (resulting in roughly 5,700 SCGM estimations). To avoid excessive calculations, we rely on the empirical distributions of the placebo estimates from Tables 4 and 5. The empirical distributions suggest that the cutoff for a p-value of 0.05 is at an *RMSPE Ratio* of 5.1 for employment and at an *RMSPE Ratio* of 6.7 for wages. The largest *RMSPE Ratios* in Table A5 are Contra Costa, CA (7.5), and Santa Clara, CA, (6.7) for employment (with no values exceeding 5 for wages); these two estimates are probably significant in a statistical sense. The point estimates suggest a negative employment effect for these counties,

which could imply that firms (and/or employees) relocated to the neighboring counties San Francisco or Alameda where the mandates applied. On the other hand, we advise caution as these are only 2 out of 38 cases, which lies within a conventional 6 percent statistical error probability rate.

6.2 Labor Market Effects of State-Level Mandates

State-Level Employment and Wage Effects

The graphical evidence for the state-level results is in Figure 7 (Employment) and Figure 8 (Wages). Panels B of Tables 4 (Employment) and 5 (Wages) show the test statistics analogous to the city-level case. Note that we are able to differentiate by firm size and industry and only focus on employment and wage effects in treated firms and industries, that is, private service sector firms with more than 49 employees in Connecticut and private sector firms with more than 9 employees in Massachusetts and Oregon. As above, Figures A8 and A9 (Appendix) provide robustness checks using four different modeling approaches.

Again, the graphs and test statistics provide clear and consistent evidence: First, the standard SCGM modeling approach performs well in most cases. In the few instances when the fit is not great (e.g. California and wages), the alternative modeling approaches clearly improve the fit. Second, visually and when studying the test statistics, there is no evidence for systematic labor market effects of substantial size and significance. None of the *RMSPE Ratios* is statistically significant at conventional levels (Panels B of Tables 4 and 5).¹⁸ Third, if the effect sizes were significant, most of them would suggest that the employment and wage effects are rather positive than negative.

Evidence from the Construction and Hospitality Sectors

In the appendix, we show separate findings for the construction and hospitality industry in Connecticut (Figure A10), California (Figure A11), Massachusetts (Figure A12), and Oregon (Figure A13).

Again, the findings from the city-level mandates hold up. With some exceptions (e.g., Massachusetts and Wages), the standard SCGM performs well and is able to closely replicate the labor market dynamics

¹⁸ Note that the Irwin Hall joint tests for counties *and* states at the bottom of Tables 4 and 5 implicitly assume that the cities and states were similarly affected by the mandates. However, because the industry structures and exceptions differ across regions (Table 1), this is not necessarily the case.

of the treatment states using fractions of other states as shown in Table A4. Moreover, the visual and analytic analysis does not provide evidence for significant employment or wage effects in any of the states or sectors.

Testing for Spillover Effects on Exempt Firms and Sectors

As a very final test we investigate whether *exempt* firms within a state (because of size or industry) may have been affected by the mandates. In other words, we replicate the spillover analysis from above but do not test effects on neighboring states but on exempt firms and sectors in the same state (results are in Table A8). The largest *RMSPE Ratio* is 4.0 for Connecticut. This is comparable to other insignificant ratios in Tables 4 and 5. Thus, the results are again robust in the sense that we are unable to identify statistically significant and systematic labor market effects for exempt industries and firms.

6.3 Discussion of Effect Sizes

As shown by Tables 4 and 5, overall, there is very little evidence that employment or wages varied systematically as a result of the city or state-level sick pay mandates. The SCGM inference procedure (see Section 5.2) almost never allows us to conclude that employment and wage dynamics have been significantly different in treated cities or states. Moreover, the sign and sizes of the *PTEs* and *LTEs* (columns [6] and [7]) do not follow a consistent pattern that, in our opinion, corroborates the main conclusion of no systematic employment or wage effects.

As discussed in the Introduction, the standard textbook example would predict negative wage effects as a result of mandated sick pay. A static calculation would yield wage decreases of up to 3.3 percent under several assumptions. Relaxing these assumptions would predict ambiguous wage effects, depending on the assumption. For example, how and whether sick leave affects work productivity is crucial but there exists no empirical causal evidence on this question. It could be reasonable to assume either, that overall work productivity increases or decreases when employees gain access to paid sick leave. Similarly, assumptions about (unobserved) administrative and psychological (“business climate”) costs appear to be crucial when making predictions about employment effects. Ultimately, we take the view that employment and wage effects are an empirical question. And we do not find evidence for systematic employment and wage effects.

There may be one or two exemptions, though. In Section 6.1, we find suggestive evidence for positive wage effects in the construction sector of Hudson County (NJ) and Alameda County (CA). Both effects

are marginally significant and have good SCGM fits. Similarly, we find suggestive evidence for positive employment effects in the hospitality sector of San Francisco (CA)¹⁹ and positive wage effects in the hospitality sector of King County (WA). (On the other hand, one could argue that these are only 4 marginal cases out of 52 in Table A4, which lies entirely within a 10 percent false positive rate.)

There are several possibilities to rationalize positive employment and wage effects as a result of sick pay mandates. First, it is possible that sick pay mandates correct market inefficiencies and effectively reduce negative externalities such as infection rates among coworkers or customers (Pichler and Ziebarth, 2017). In fact, paradoxically, overall sick leave rates may fall when employees gain access to paid sick leave (Stearns and White, 2018). When overall firm productivity goes up as a result of the mandate, it could explain stronger wage growth. Second, it could simply be the case that wages cannot flexibly adjust downward due to, for example minimum wage laws. Third, in a standard labor supply model, a higher wage (and higher employment) can be a result of a downward-shifting labor supply curve (e.g., because jobs become more attractive for employees) and an upward-shifting labor demand curve (e.g., because customers demand more services), see Boeri and van Ours (2008). Finally, there exists anecdotal evidence from qualitative employer surveys which were primarily conducted in San Francisco after the first mandate was implemented in 2007. Boots et al. (2009) interviewed 26 employers and found that most of those implemented the mandate with “minimal to moderate effects on their overall business and their bottom line.” Moreover, “about half of the employers [...] tried to offset or minimize their recent increased labor costs” by “changes in other benefits or delayed wage increases [...] (p.8)”

7 Discussion and Conclusion

Using the SCGM, this paper systematically evaluates the labor market consequences of nine city-level and four state-level sick pay mandates in the United States. The setting is well 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 synthetic control counties that map the labor market dynamics of the treated counties very closely. We also rely on many pretreatment observations; matching treated-control labor market dynamics over long prereform time periods strengthens the identifying assumptions of the SCGM. Because several of our treated units are very small and geographically dispersed, we can also plausibly assume the absence of general equilibrium and spillover effects from treated to control regions. Additionally, because we rely on many different treatment units with di-

¹⁹See footnote 3 for a discussion of potential confounding factors.

verse labor markets, our findings have a broad range of common support and arguably high external validity. Moreover, many treatment regions reduce the likelihood that unobserved shocks confounded postreform labor market dynamics systematically.

Opponents of sick pay mandates are mainly concerned with 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. This may be a function of how the U.S. 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 to 40 hours worked—that is, 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.

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. We show, however, that this was very likely not the case. Our estimates let us exclude employment losses of more than 2 percent and wage reductions of more than 3 percent at conventional statistical levels. While an even higher statistical precision would always be desirable, we agree with [Abadie \(2018\)](#) and believe that much can be learned from such nonsignificant findings, especially in this policy-relevant context. In our opinion, the overall findings from 9 city-level and four state-level mandates, in conjunction with a lack of systematically positive or negative point estimates (and rather small effect sizes), further corroborate our null findings.

This paper’s findings suggest that neither employment nor wage growth has been significantly affected by U.S. sick pay mandates. However, the limitations of this study should be kept in mind and require more research: Although we evaluate nice city-level and four state-level mandates, these regions are not random subsamples of all U.S. regions. They tend to be relatively prosperous regions, governed by Democrats, and have also more labor market regulations, higher minimum wages, and stricter employment projections. It is thus unclear whether the conclusions would also hold up in less prosperous regions and regions with fewer labor market regulations.

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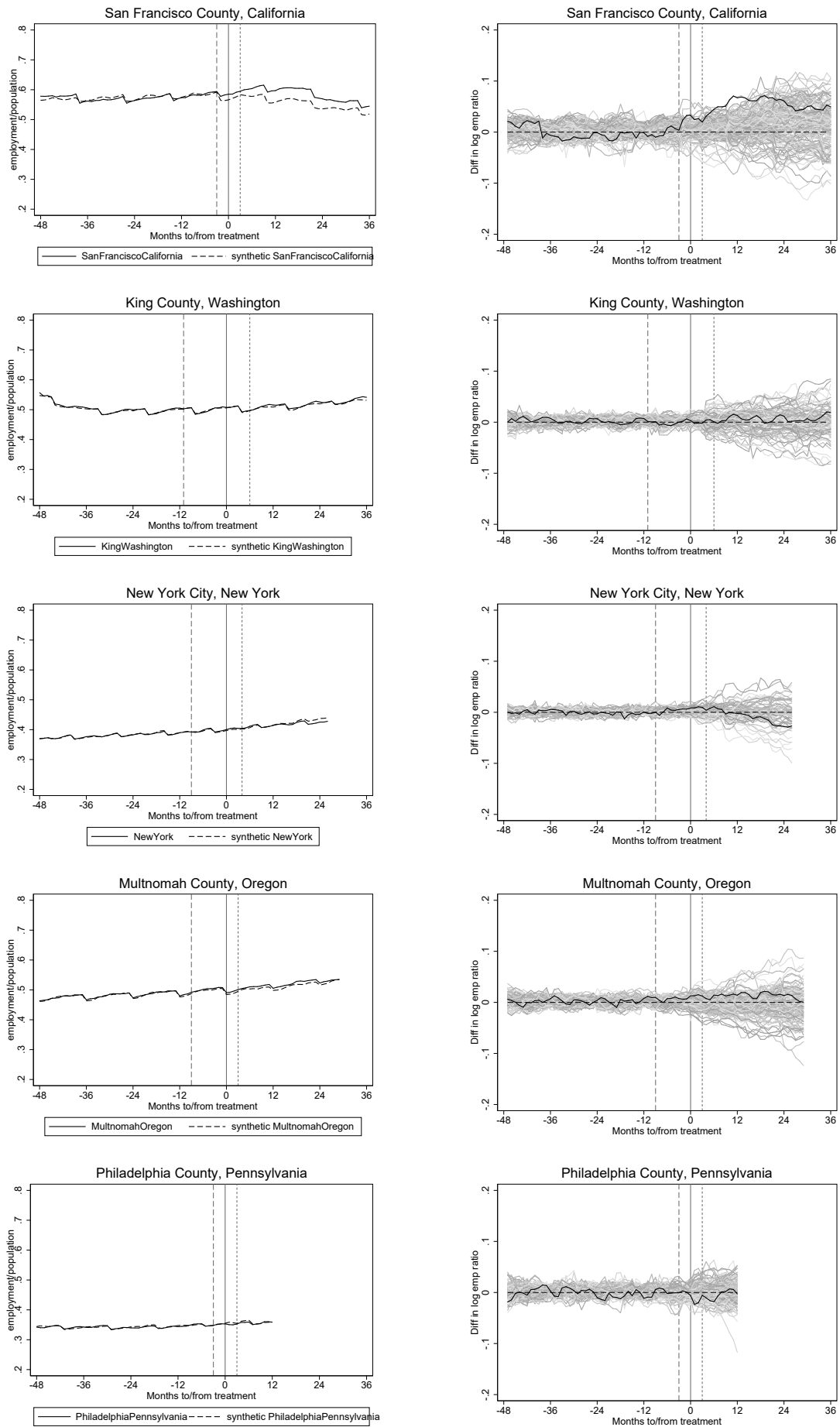
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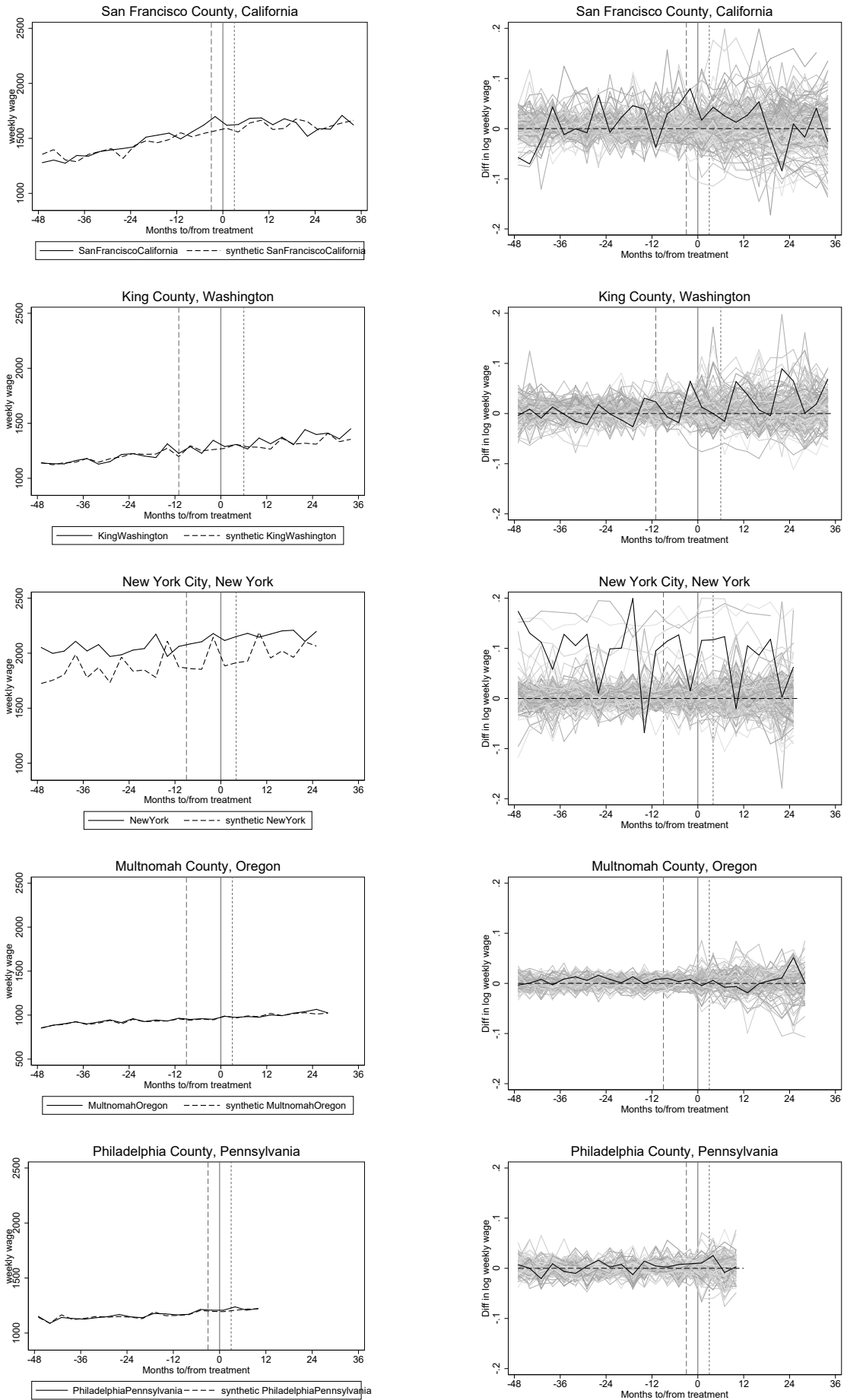
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Figure 1: Employment Ratios in Treated vs. Synthetic Control Counties



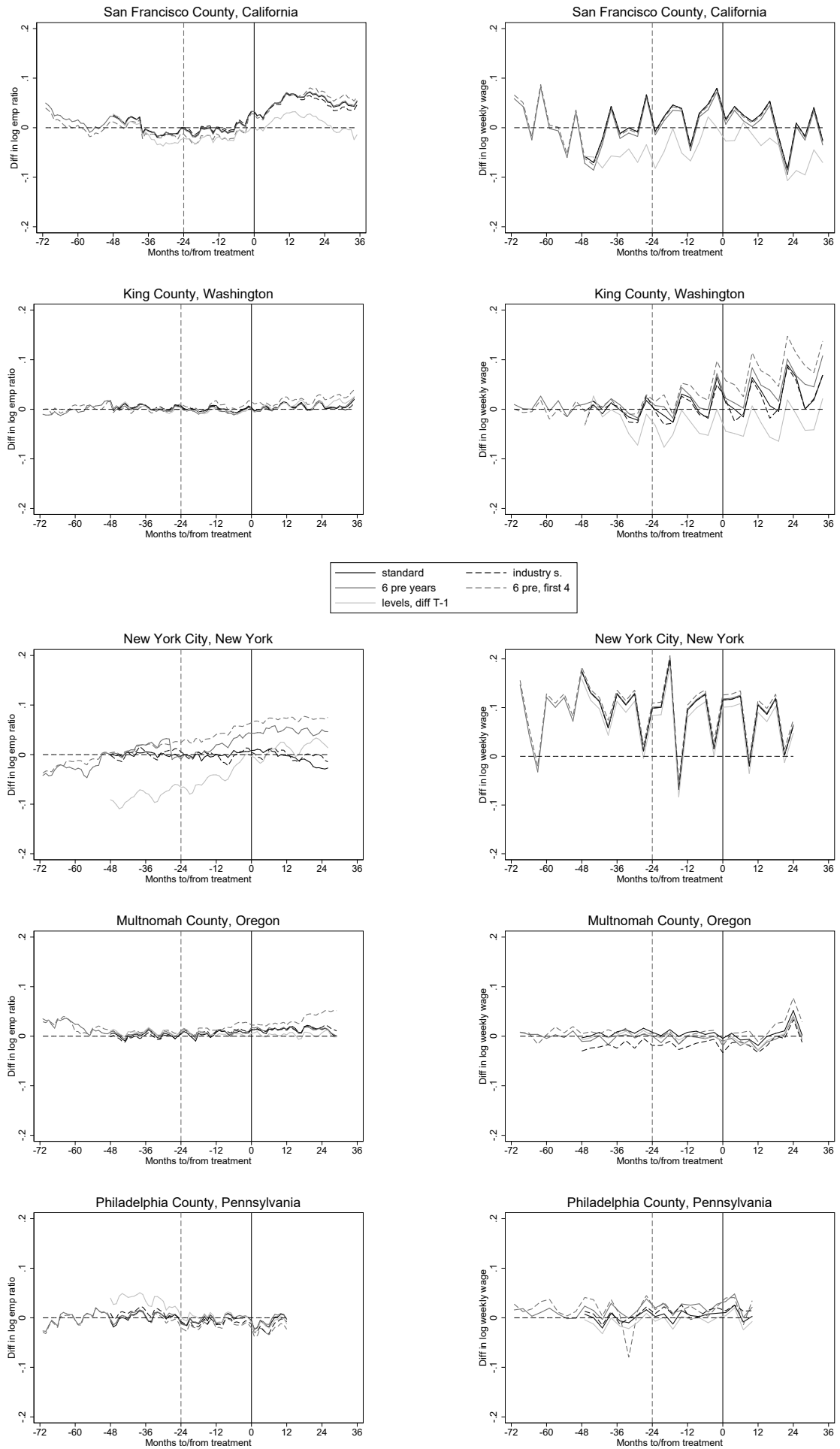
Note: The left column compares treated counties (solid lines) to the synthetic control counties (dashed lines). The composition of the synthetic control counties is in Table A1 (Online Appendix). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios between treatment and synthetic control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure 2: Weekly Wages in Treated vs. Synthetic Control Counties



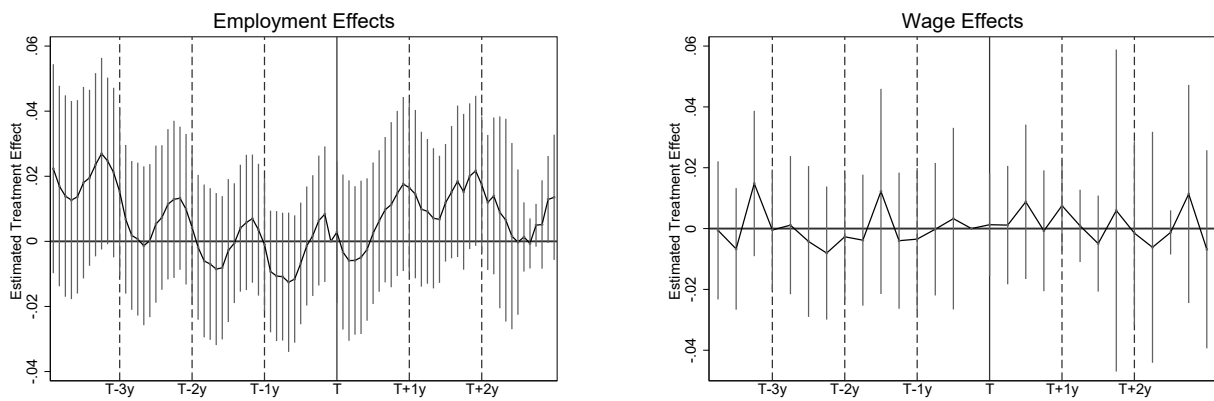
Note: The left column compares treated counties (solid lines) to the synthetic control counties (dashed lines). The composition of the synthetic control counties is in Tables A2 (Online Appendix). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the weekly wages between treated and synthetic control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure 3: Alternative SGCM Modeling—Employment (Left) and Wage (Right) Effects



Note: The lines always show the difference of the employment ratios (left column) and weekly wages (right column) between treatment and synthetic control groups. The solid black lines show our standard modeling approach (right columns of Figures 1 and 2). The black dashed lines select synthetic control counties based on additional industry-structure variables (Tables 2 and 3). The gray solid lines use six instead of four pretreatment years; the gray dashed lines use six instead of four years but stop applying the SGCM algorithm two years before the law’s enactment. The light gray solid lines use employment in levels, relative to employment in T-1. The left dashed vertical lines indicate when the law was passed and the solid vertical lines when the law became effective. Source: QCEW.

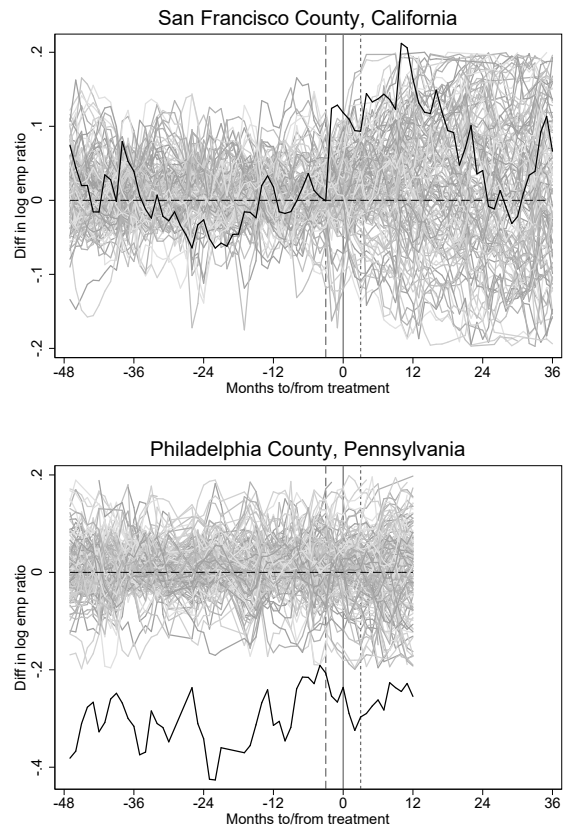
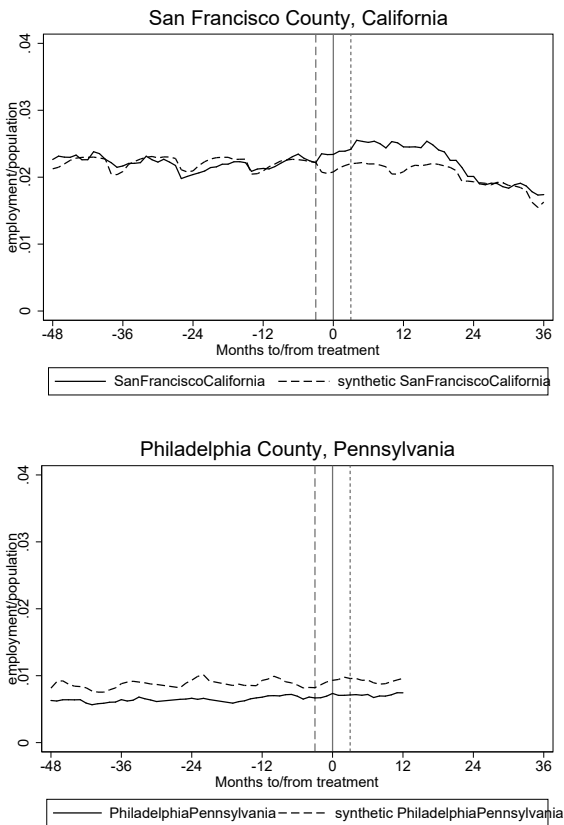
Figure 4: Event Studies from Traditional DD Models for County-Level Estimates



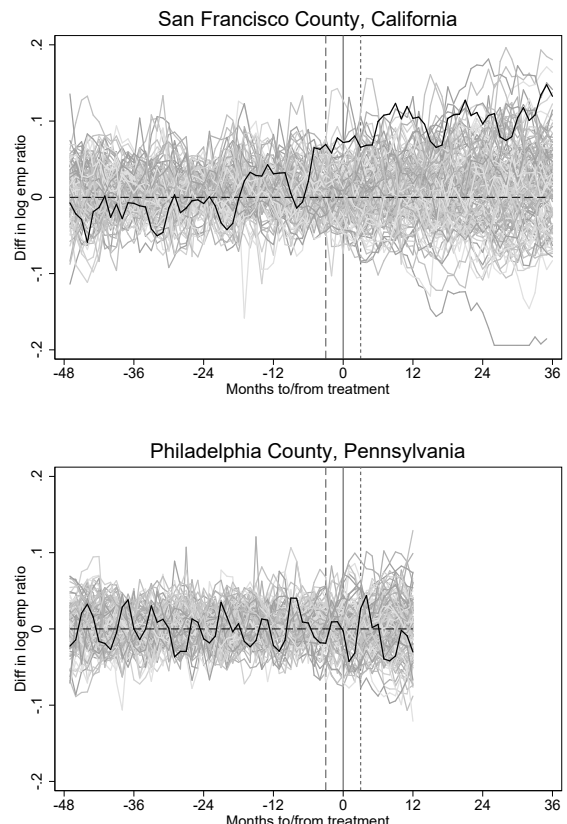
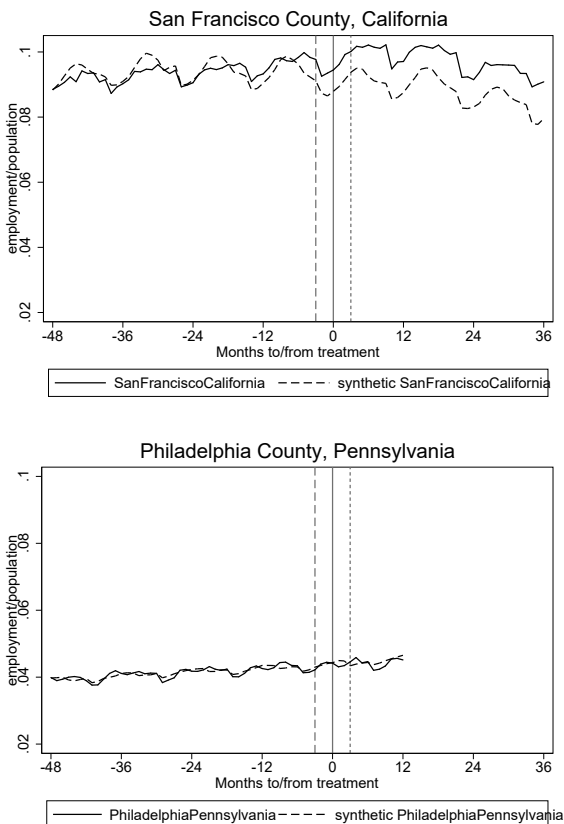
Note: The graphs show event studies based on traditional DD models similar to Equation (6), where the *TreatedCounty* dummy is replaced by a time indicator that counts from 48 months before, up to 36 months after the enactment of the city-level sick pay mandates. The errors terms are clustered at the county-level and the gray areas depict 95% confidence intervals. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure 5: Construction and Hospitality—Employment in SF and Philadelphia

A. Construction



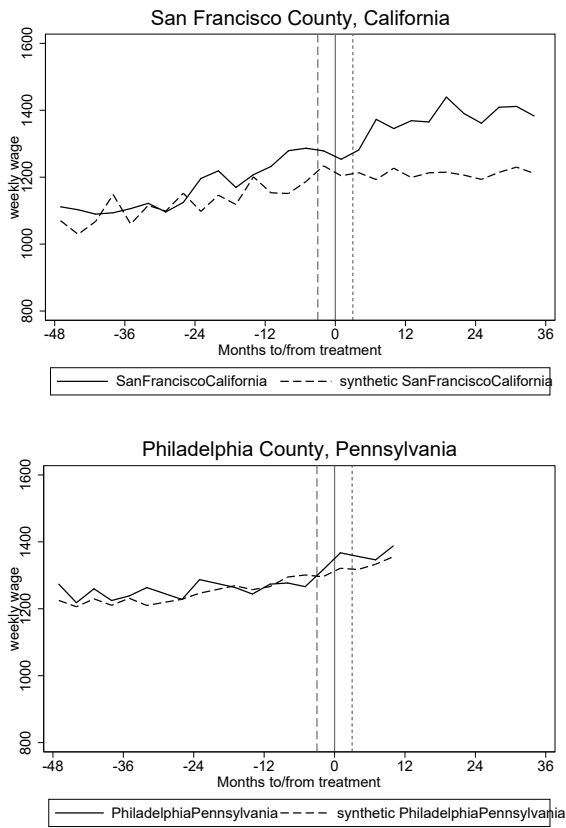
B. Hospitality



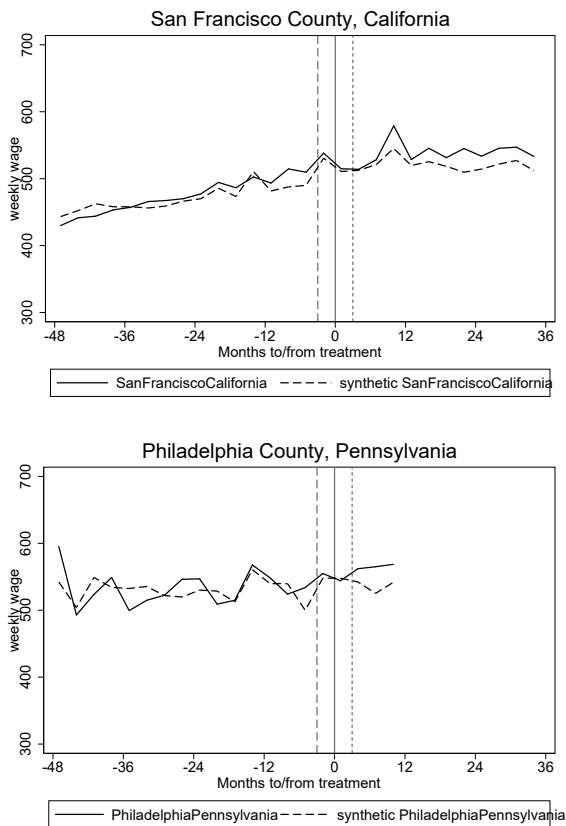
Note: The left column compares treated counties (solid lines) to the synthetic control counties (dashed lines). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios between treatment and synthetic control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure 6: Construction and Hospitality—Weekly Wages in SF and Philadelphia

A. Construction

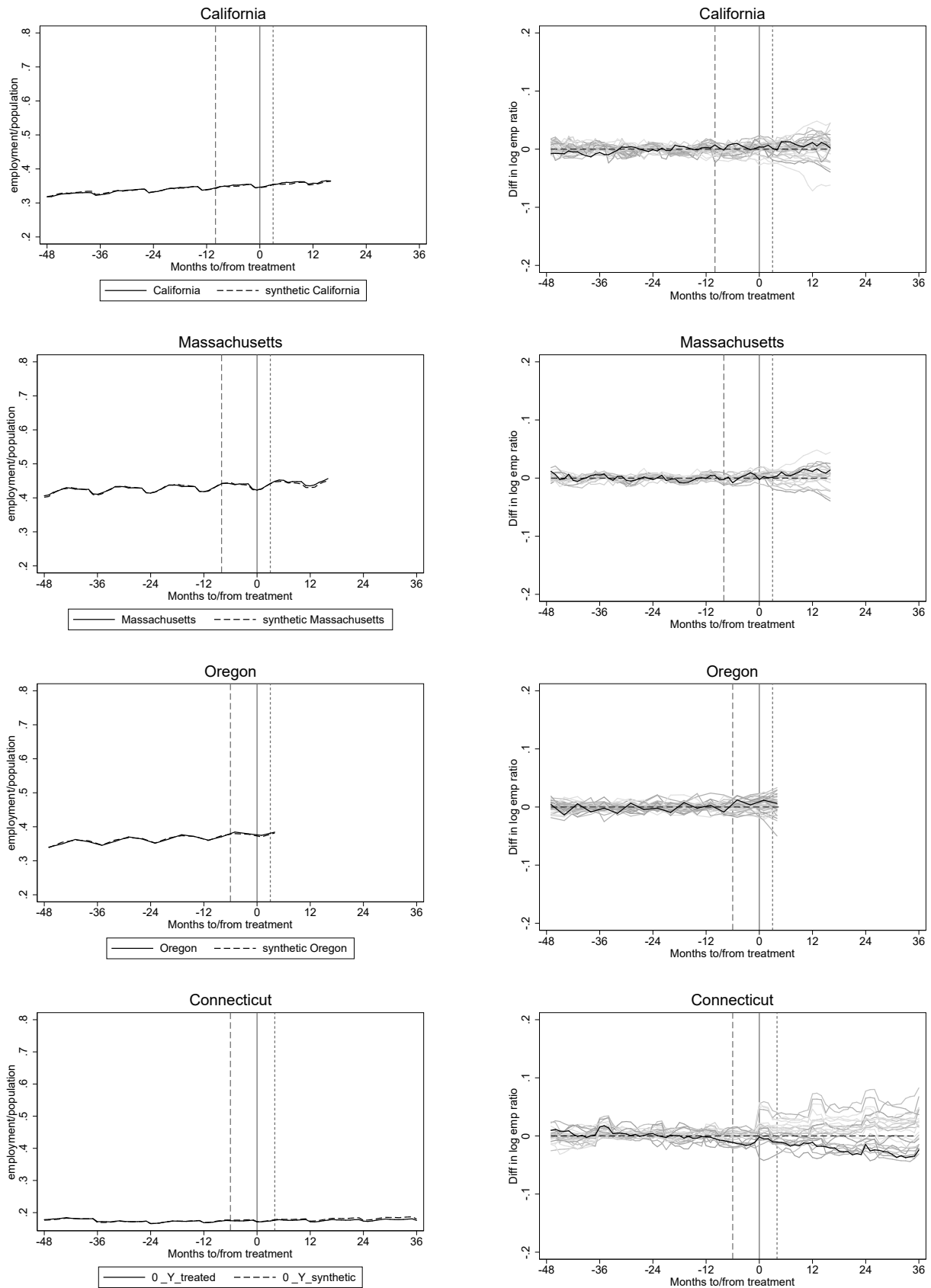


B. Hospitality



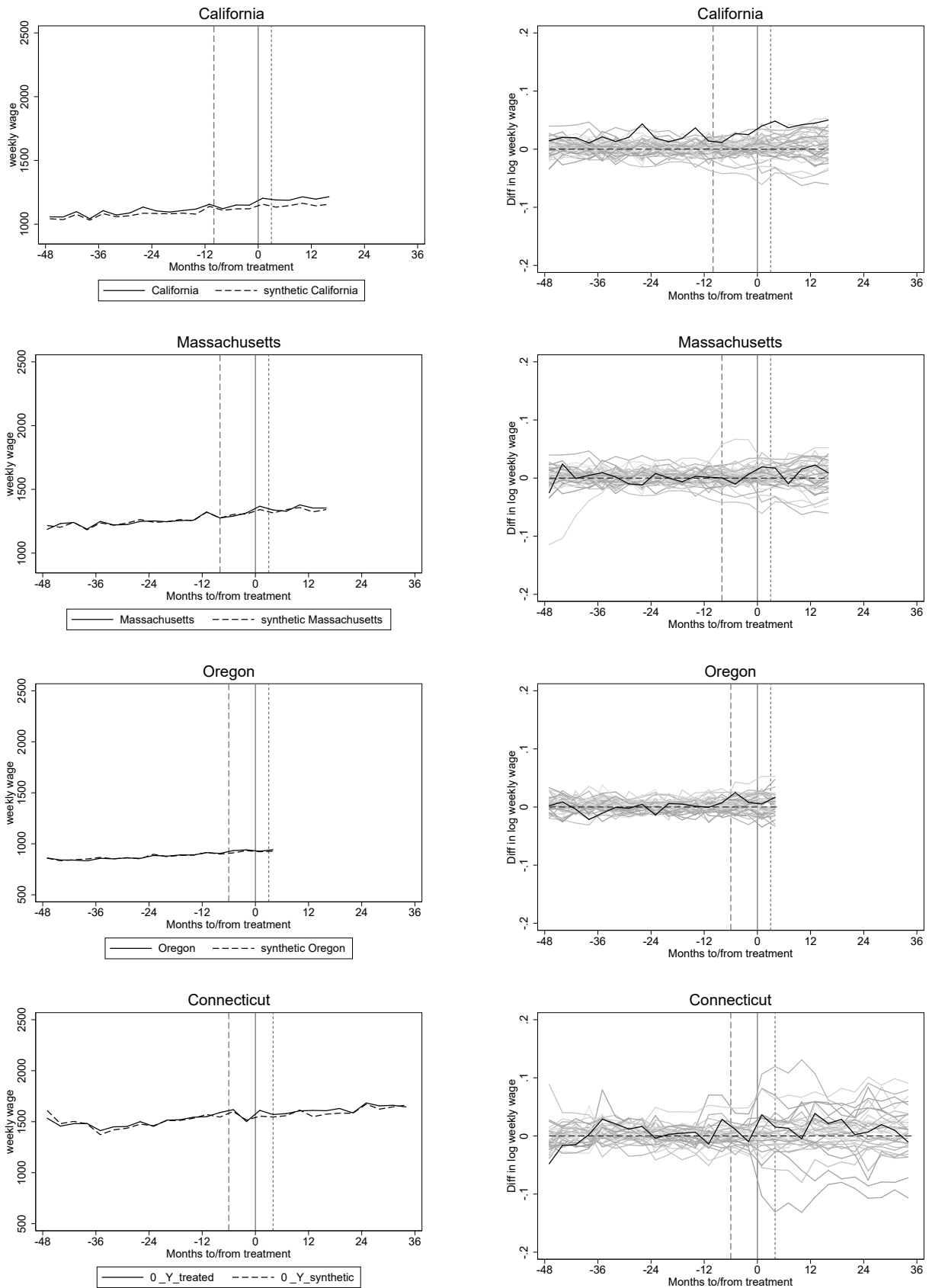
Note: The left column compares treated counties (solid line) to the synthetic control counties (dashed line). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the weekly wage between treated and synthetic control groups along with placebo estimates for counties with prereform *RMSPE*s smaller than two times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure 7: Employment Ratios in Treated vs. Synthetic Control States



Note: The left column compares treated states (solid line) to synthetic control states (dashed line). The composition of the synthetic control states is in Table A6 (Online Appendix). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios between treatment and synthetic control groups along with placebo estimates for counties with prereform *RMSPE*s smaller than 2 times the prereform *RMSPE* of the treated state (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. In Connecticut, the treatment group consists of private sector firms with at least 50 employees; in Oregon and Massachusetts, the treatment group consists of private sector firms with at least 10 employees. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure 8: Weekly Wages in Treated vs. Synthetic Control States



Note: The left column compares treated states (solid line) to the synthetic control states (dashed line). The composition of the synthetic control states is in Table A7 (Online Appendix). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the weekly wages between treatment and synthetic control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated state (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical line indicates when the law became effective, and the right dashed vertical lines indicate when the probation period was over. In Connecticut, the treatment group consists of private sector firms with at least 50 employees; in Oregon and Massachusetts, the treatment group consists of private sector firms with at least 10 employees. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Table 1: 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 Dec 18, 2013	Nov 13, 2008 Feb 22, 2014 (retrosp. in Sep 2014)	'qualified employees'; 1 hour of paid sick leave for every 43 hours, 90 days accrual period; up to 3 to 9 days depend. on firm size; own sickness or family; no health care or restaurant workers extension to 20,000 temporary workers and tipped employees
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.

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

Variable	Mean	Std. Dev.	Min.	Max.	N
Private sector, employment	0.271	0.136	0.011	4.038	548,992
Production sector	0.078	0.064	0	3.569	548,992
Construction	0.015	0.014	0	1.355	
Manufacturing	0.045	0.046	0	3.482	548,992
Service sector	0.191	0.11	0	2.989	548,992
Trade, transportation, and utilities	0.066	0.032	0	0.966	
Professional and business services	0.023	0.047	0	2.855	548,992
Education and health services employment	0.041	0.028	0	0.44	548,992
Leisure and hospitality employment	0.033	0.04	0	1.592	548,992
Public sector, employment	0.077	0.036	0.012	0.496	548,992
Private sector wages	599	182	155	4542	182,992
Population	82,799	240,169	258	8,495,194	44,267

Source: QCEW ([Bureau of Labor Statistics \(BLS\), 2018](#)), own calculation and illustration. [U.S. Census Bureau \(2016b\)](#) provided the county-level population data.

Table 3: Quarterly Census of Employment and Wages (QCEW), State Level: 2001-2016Q2

Variable	Mean	Std. Dev.	Min.	Max.	N
Private sector, employment	0.373	0.061	0.268	0.791	8,981
Production sector	0.073	0.02	0.014	0.141	8,981
Construction	0.023	0.007	0	0.059	8,981
Manufacturing	0.043	0.019	0	0.107	8,981
Service sector	0.297	0.061	0.179	0.765	8,981
Trade, transportation, and utilities	0.086	0.011	0.042	0.145	8,981
Professional and business services	0.055	0.026	0.027	0.26	8,981
Education and health services employment	0.062	0.018	0.031	0.173	8,981
Leisure and hospitality employment	0.048	0.016	0.03	0.148	8,981
Public sector, employment	0.08	0.041	0.044	0.414	8,981
Private sector wages	805	199	440	1624	2,992
Population	5,573,357	5,728,342	494,657	39,144,818	721
Private service sector empl., >49 empl.	0.152	0.051	0.047	0.557	8,981
Private non-service sector empl., >49 empl.	0.045	0.016	0.006	0.098	8,981
Private sector empl., >9 empl.	0.312	0.056	0.21	0.696	8,981
Private service sector wages, >49 empl.	839.829	263.662	423.173	2030.237	8,981
Private sector wages, >9 empl.	815.594	190.141	454.512	1667.3	8,981

Source: QCEW ([Bureau of Labor Statistics \(BLS\), 2018](#)), own calculation and illustration. [U.S. Census Bureau \(2016b\)](#) provided the state-level population data.

Table 4: Synthetic Control Group Method—the Effect of Sick Pay Mandates on Employment

	$\bar{Y}_{i,pre}^1$ (1)	RMSPE pre (2)	RMSPE post (3)	RMSPE Ratio (4)	Rank RMSPE/ #Placebos= P-Value (5)	PTE (6)	LTE (7)	P-value PTE<-3% (8)	P-value PTE<-2% (9)
A. Counties:									
San Francisco,CA	0.5742	0.0133	0.0536	4.0429	35/164=0.2134	0.0518	0.0298	0.4329	0.5854
King County, WA	0.5040	0.0055	0.0083	1.5041	89/155=0.5742	0.0058	0.0031	0.0323	0.1613
New York City,NY	0.3835	0.0041	0.0146	3.5908	23/175=0.1314	-0.0062	-0.0030	0.0171	0.0343
Multnomah, OR	0.4852	0.0061	0.0141	2.3237	61/185=0.3297	0.0131	0.0068	0.0541	0.1784
Essex County, NJ	0.3319	0.0147	0.0256	1.7445	85/158=0.538	-0.0232	-0.0078	0.4241	0.6139
Hudson County, NJ	0.2965	0.0267	0.0438	1.6393	41/83=0.494	-0.0419	-0.0132	0.6747	0.8795
Philadelphia, PA	0.3436	0.0081	0.0119	1.4800	79/175=0.4514	-0.0072	-0.0026	0.0514	0.1771
Alameda, CA	0.3638	0.0081	0.0081	0.9984	139/199=0.6985	0.0055	0.0021	0.0754	0.1960
District of Columbia	0.7752	0.1395	0.1902	1.3638	114/161				
Average / Sum Pval				2.0764	4.1387	-0.0003	0.0019	1.762	2.8259
P val Irwin Hall					0.3407			0.0023	0.0765
B. States (only treated firm sizes & industry):									
Connecticut (service sector, 50+)	0.1746	0.0076	0.0235	3.0716	9/47=0.1915	-0.0214	-0.0038	0.1489	0.2340
California	0.3376	0.0056	0.0080	1.4369	24/47=0.5106	0.0069	0.0025	0.0213	0.0638
Massachusetts (10+)	0.3628	0.0041	0.0042	1.0184	36/47=0.766	0.0008	0.0005	0.0213	0.0213
Oregon (10+)	0.2932	0.0069	0.0086	1.2425	26/47=0.5532	0.0082	0.0026	0.0638	0.1702
Average / Sum Pval				1.6923	2.0213	-0.0014	0.0005	0.2553	0.4893
P val Irwin Hall					0.5142			0.0002	0.0024
Total (Counties and States):									
Average / Sum Pval				2.0078	5.4519	-0.0006	0.0014	2.0173	3.3152
P val Irwin Hall					0.2940			0	0.0031

Source: QCEW (Bureau of Labor Statistics (BLS), 2018), 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 prereform years. Columns (2) and (3) display the *RMSPE* as in Equation (3) for pre and postreform 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). Columns (8) and (9) display the p-values of rejecting the null of no treatment effect after assigning hypothetical employment decreases of 3% and 2% (see main text for details). As for the joint tests and sum of all p-values, we exclude the District of Columbia due to a poor prereform *RMSPE* fit (values in cells are intentionally left blank). For more information, see the discussion on treatment regions in Section 4. For more information about the sick pay reforms, see Table 1.

Table 5: Synthetic Control Group Method—the Effect of Sick Pay Mandates on Weekly Wages

	$\bar{Y}_{i,pre}^1$ (1)	RMSPE pre (2)	RMSPE post (3)	RMSPE Ratio (4)	Rank RMSPE/ #Placebos= P-Value (5)	PTE (6)	LTE (7)	P-value PTE<-3% (8)	P-value PTE<-2% (9)
A. Counties:									
San Francisco,CA	1434.71	0.0423	0.0389	0.9201	144/164=0.878	0.0089	11.6254	0.8049	0.8598
King County, WA	1199.26	0.0207	0.0446	2.1524	28/155=0.1806	0.0296	38.9481	0.1806	0.2839
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	0.0162	0.0703
Essex County, NJ	1261.09	0.0160	0.0162	1.0101	112/158=0.7089	-0.0054	-7.3773	0.2152	0.3671
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	0.0629	0.1543
Alameda, CA	1231.76	0.0308	0.0546	1.7728	60/199=0.3015	0.0542	70.8706	0.6633	0.8794
District of Columbia	1320.95	0.0203	0.0146	0.7221	151/161=0.9379	-0.0021	-4.7810	0.2857	0.5280
Average / Sum Pval				1.3174	4.3220	0.0138	17.5663	2.2288	3.1428
P val Irwin Hall					0.6507			0.0483	0.3234
B. States (only treated firm sizes & industry):									
Connecticut (service sector, 50+)	1503.8880	0.0190	0.0209	1.0986	32/47=0.6809	0.0150	23.9702	0.4043	0.6383
California	1106.7480	0.0229	0.0441	1.9251	12/47=0.2553	0.0439	51.8478	0.5319	0.7660
Massachusetts (10+)	1260.1960	0.0706	0.0800	1.1335	22/47=0.4681	0.0718	96.0367	0.9149	0.9574
Oregon (10+)	918.6851	0.0127	0.0189	1.4880	10/47=0.2128	0.0171	17.7307	0.1277	0.1702
Average / Sum Pval				1.4113	1.6171	0.0369	47.3964	1.9788	2.5319
P val Irwin Hall					0.2608			0.4859	0.8144
Total (Counties and States):									
Average / Sum Pval				1.3983	5.2075	0.0222	28.4136	4.2076	5.6747
P val Irwin Hall					0.3815			0.0897	0.5714

Source: QCEW (Bureau of Labor Statistics (BLS), 2018), 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 prereform years. Columns (2) and (3) display the *RMSPE* as in Equation (3) for pre and postreform 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). Columns (8) and (9) display the p-values of rejecting the null of no treatment effect after assigning hypothetical weekly wage decreases of 3% and 2% (see main text for details). As for the joint tests and sum of all p-values, we exclude New York City and Hudson County due to a poor prereform *RMSPE* fit (values in cells are intentionally left blank). For more information, see the discussion on treatment regions in Section 4. For more information about the sick pay reforms, see Table 1.

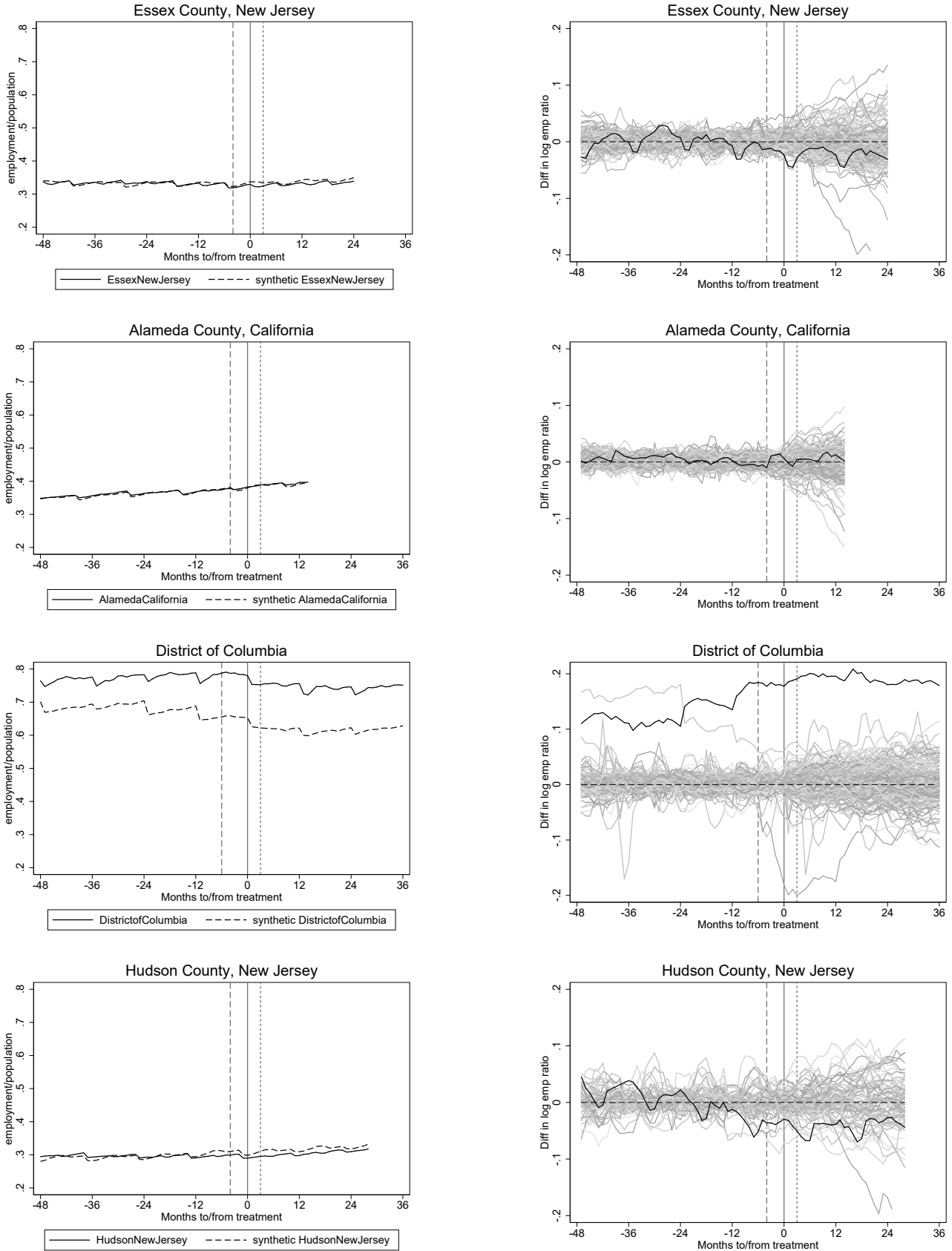
Table 6: Traditional DD Models—Effect of Mandates on Employment and Wages at the County Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Employment							
TreatedCounty × LawEffective	0.0168 (0.0130)	-0.0017 (0.0098)					0.0050 (0.0053)
TreatedCounty × LawEffective × LawEffectiveTime		0.0004 (0.0005)					
TreatedCounty × LawPassed			0.0149 (0.0129)	-0.0046 (0.0095)			-0.0053 (0.0067)
TreatedCounty × LawPassed × LawPassedTime				0.0005 (0.0004)			
TreatedCounty × ProbationOver					0.0189 (0.0134)	0.0005 (0.0090)	
TreatedCounty × ProbationOver × ProbationOverTime						0.0004 (0.0005)	
B. Wages							
TreatedCounty × LawEffective	0.0017 (0.0101)	-0.0022 (0.0079)					-0.0046 (0.0098)
TreatedCounty × LawEffective × LawEffectiveTime		0.0010 (0.0012)					
TreatedCounty × LawPassed			0.0021 (0.0104)	-0.0007 (0.0056)			0.0045 (0.0042)
TreatedCounty × LawPassed × LawPassedTime				0.0011 (0.0012)			
TreatedCounty × ProbationOver					0.0029 (0.0102)	-0.0012 (0.0099)	
TreatedCounty × ProbationOver × ProbationOverTime						0.0009 (0.0012)	
Month/quarter-year fixed effects	yes	yes	yes	yes	yes	yes	yes
County fixed effects	yes	yes	yes	yes	yes	yes	yes
State time trends (linear)		yes		yes		yes	yes
Log Public Sector + Log Pop		yes		yes		yes	yes

Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration; * p<0.1, ** p<0.05, *** p<0.01; standard errors in parentheses are clustered at the county level. Each column in each panel represents one model, estimated by OLS. *TreatedCounty* is a treatment indicator which is one for all counties listed in Table 1. *LawEffectiveTime*, *LawPassedTime* and *ProbationOverTime* are linear time trends that start when the law is effective (2), when the law is passed (4) and when probation is over (6). The models in Panel A use the logarithm of the population share of private sector employees as dependent variable. The models in Panel B use the logarithm of weekly wages as dependent variable. All models in Panel A have 548,992 employment-month observations and all models in Panel B have 182,992 wage-quarter observations. All regressions contain month-of-year (Panel A) or quarter-year (Panel B) fixed effects and county fixed effects as in Equation (6). The descriptive statistics of the baseline sample used here is in Table 2.

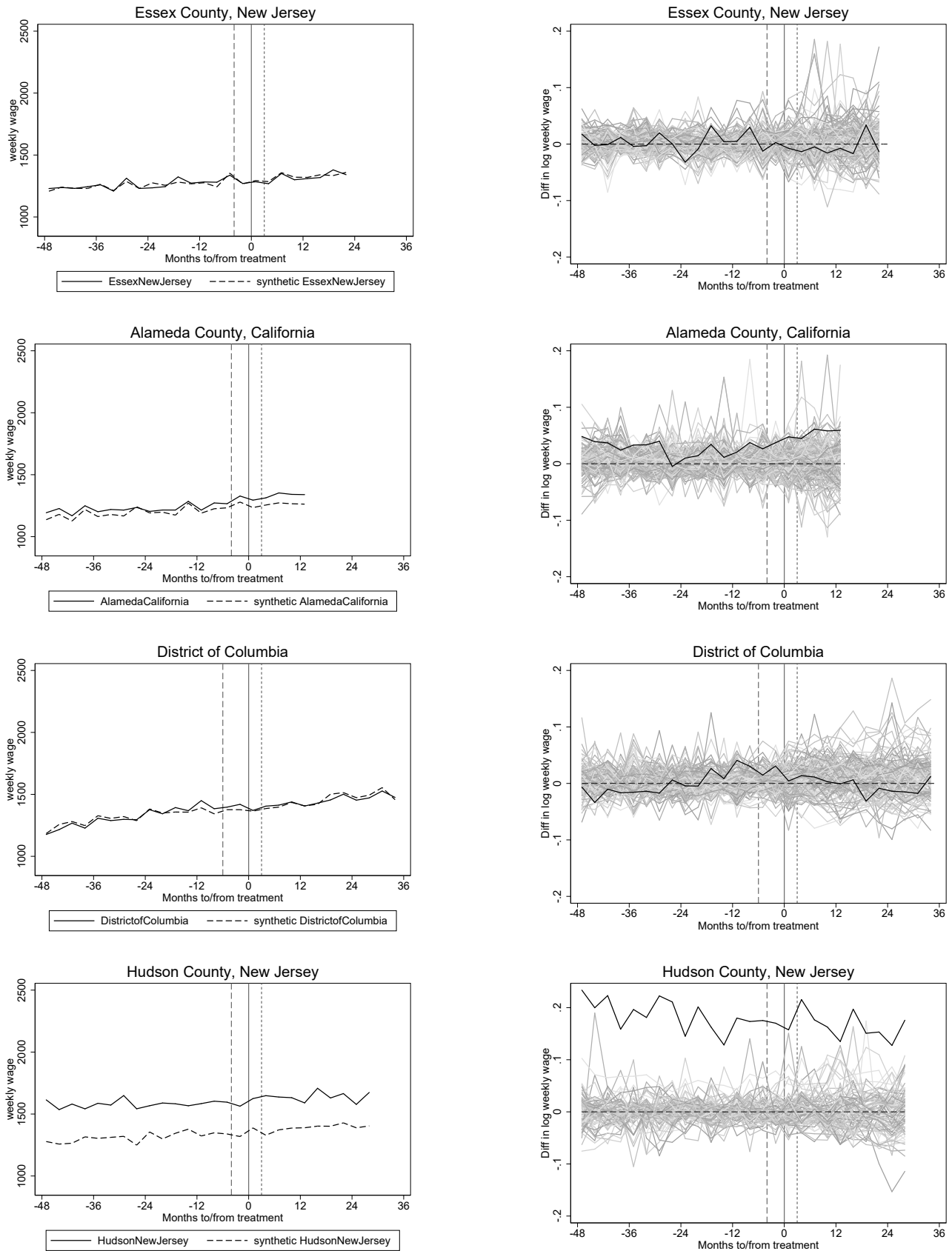
Online Appendix

Figure A1: Other Counties—Employment Ratios in Treated vs. Synthetic Control Regions



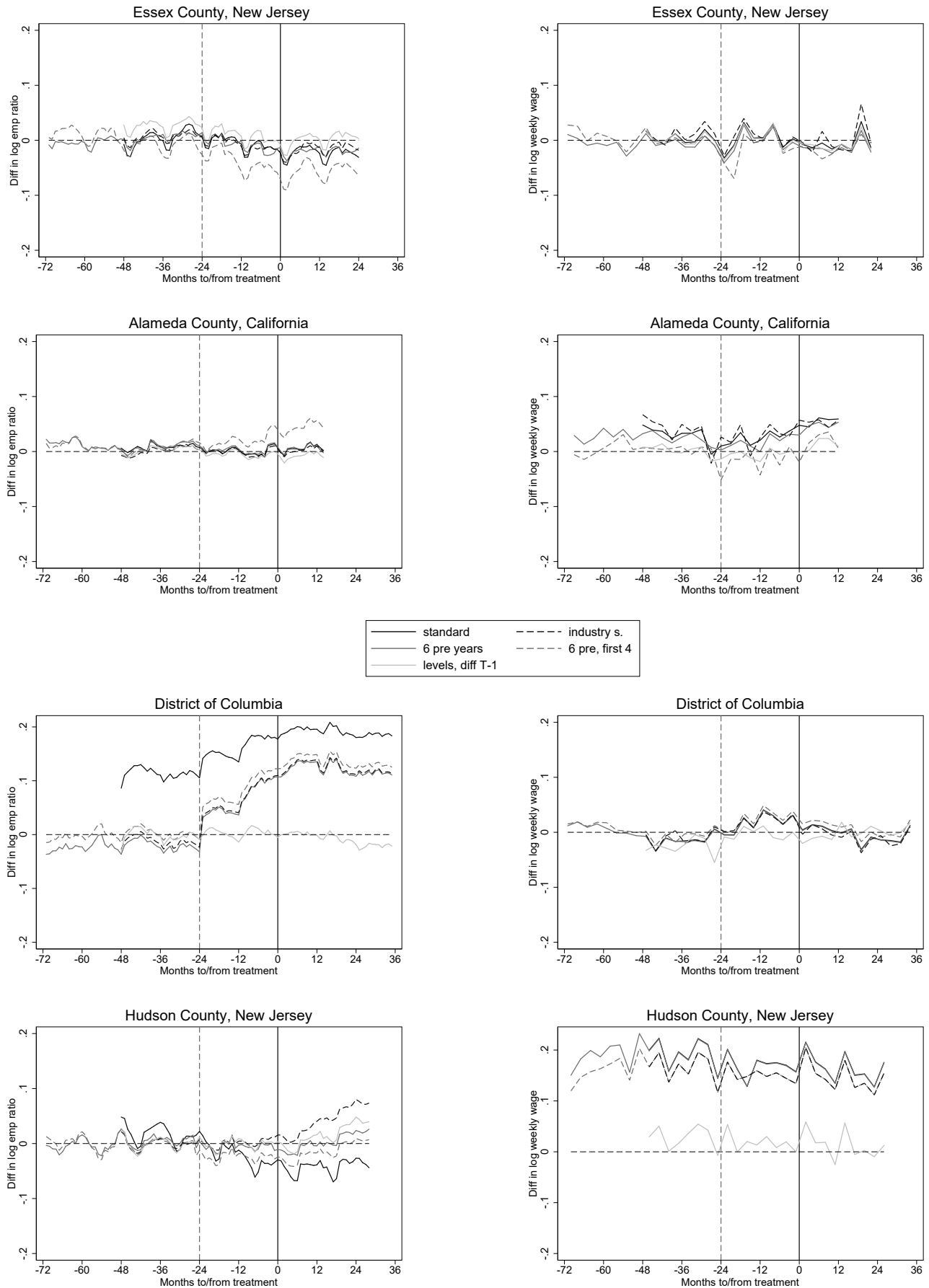
Note: The left column compares treated counties (solid line) to the synthetic control counties (dashed line). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios between treated and control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A2: Other Counties—Wages in Treated vs. Synthetic Control Regions



Note: The left column compares treated counties (solid line) to the synthetic control counties (dashed line). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. 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 prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

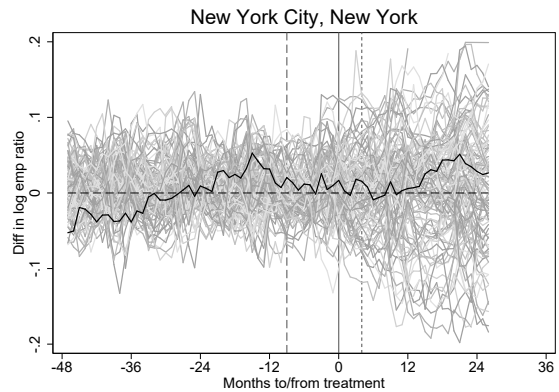
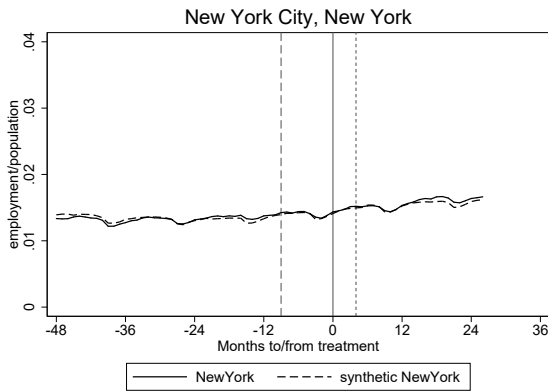
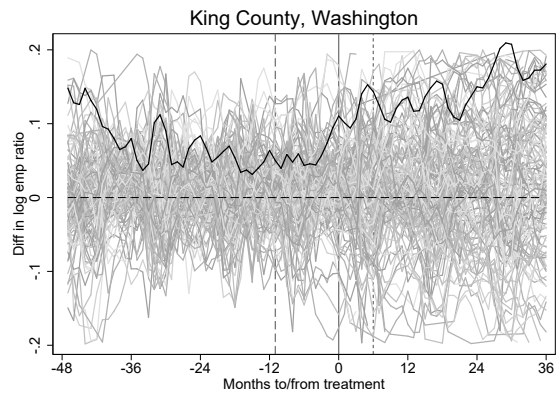
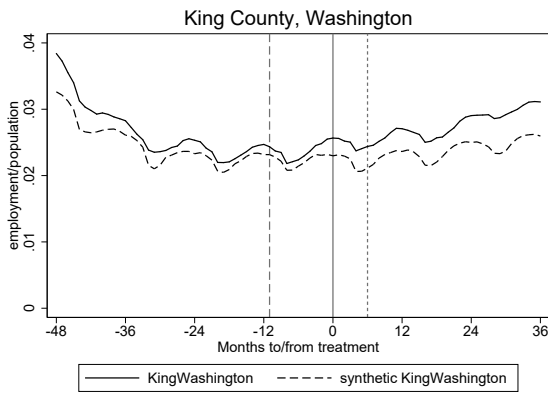
Figure A3: Other Counties Alternative SCGM Modeling Employment (left) and Wages (right)



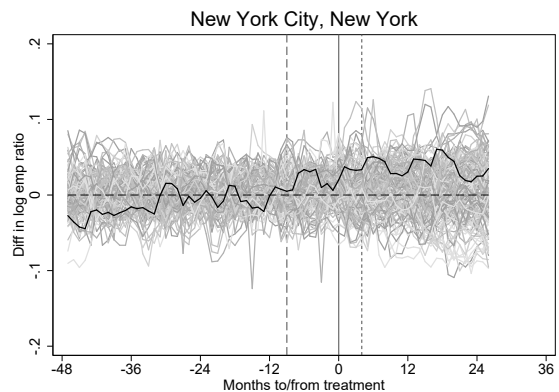
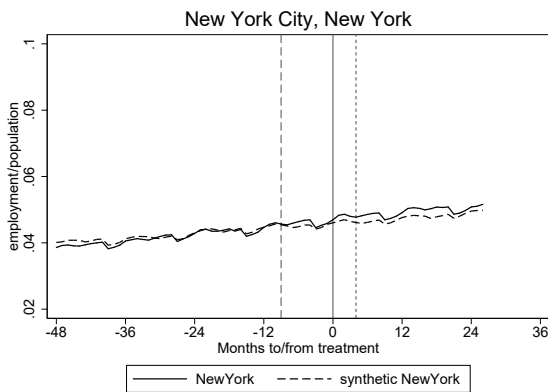
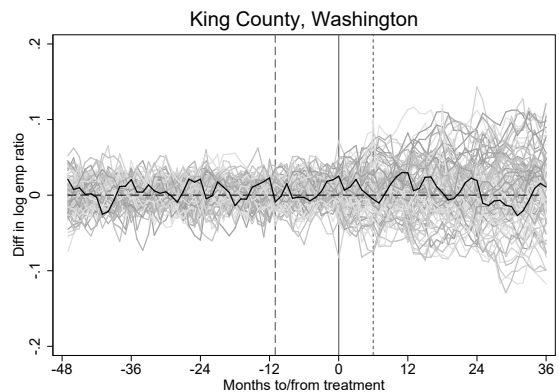
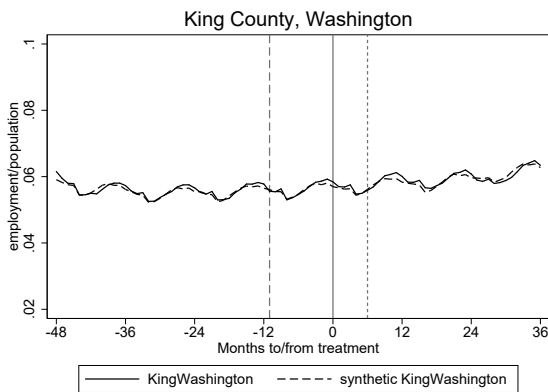
Note: The black dashed lines select synthetic control counties based on additional industry-structure variables (Tables 2 and 3). The gray solid lines use six instead of four pretreatment years; the gray dashed lines use six instead of four years but stops applying the SGCM algorithm two years before the law's enactment. The light gray solid lines use the outcome in levels, relative to T-1 (the period before the law's enactment). For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A4: Construction and Hospitality—Employment in King County and NYC

A. Construction



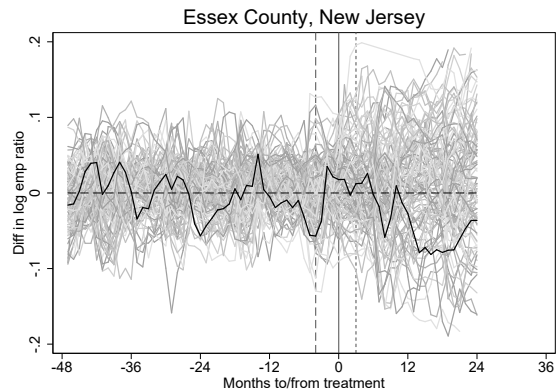
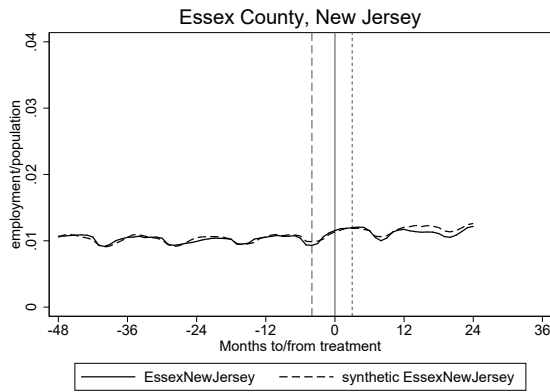
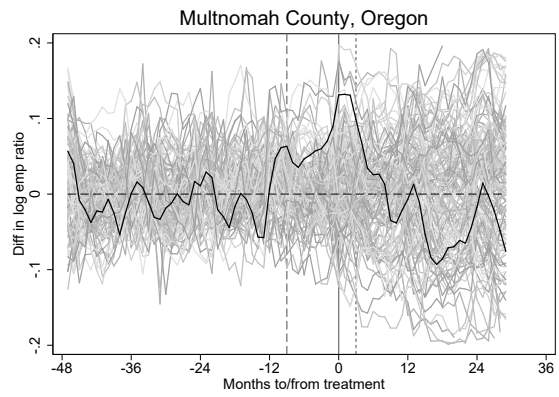
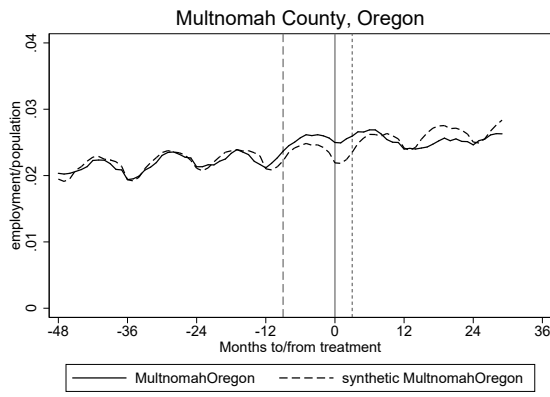
B. Hospitality



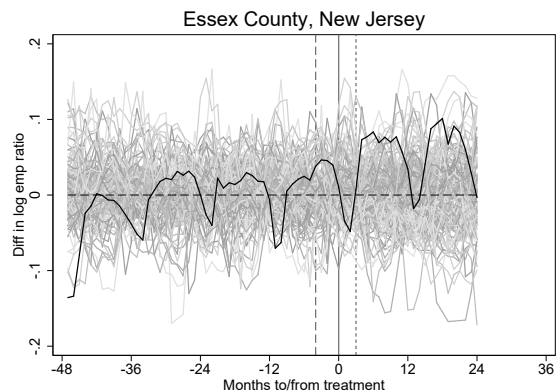
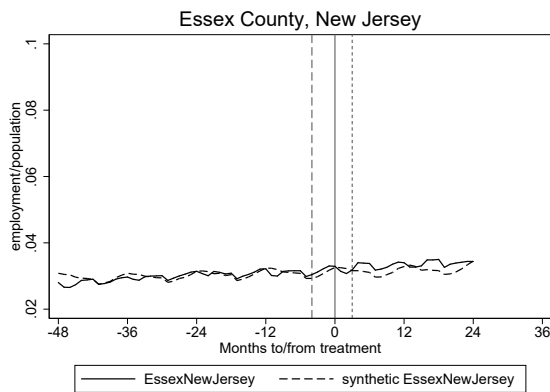
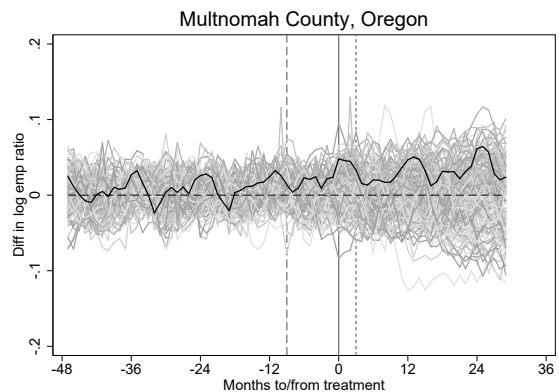
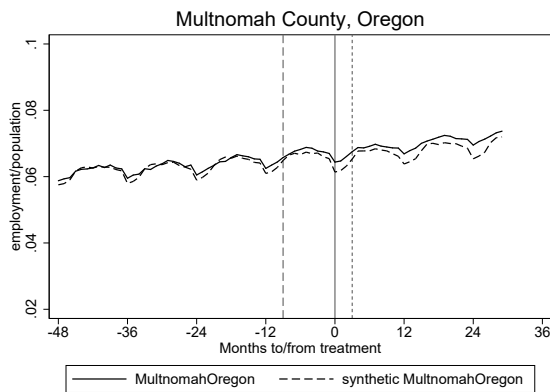
Note: The left column compares treated counties (solid lines) to the synthetic control counties (dashed lines). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios and the weekly wage between treated and synthetic control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A5: Construction and Hospitality—Employment in Multnomah County and Essex

A. Construction



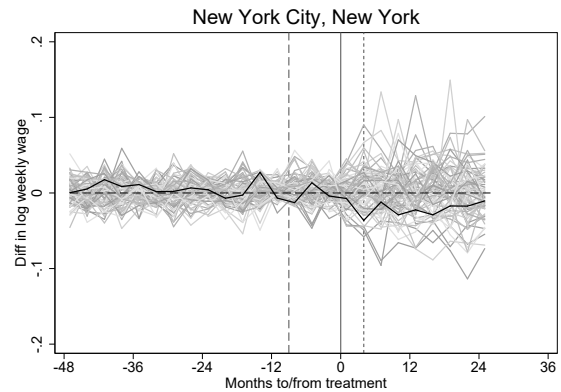
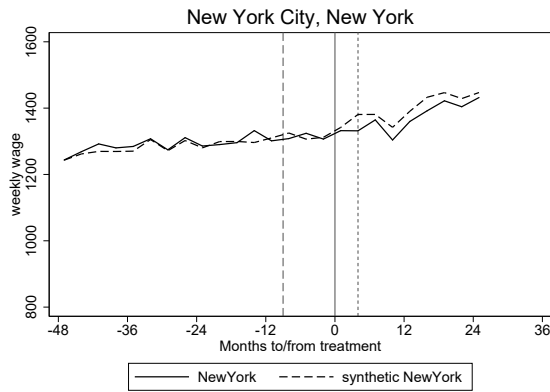
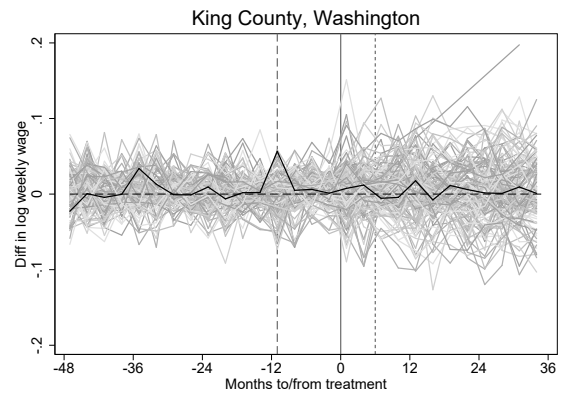
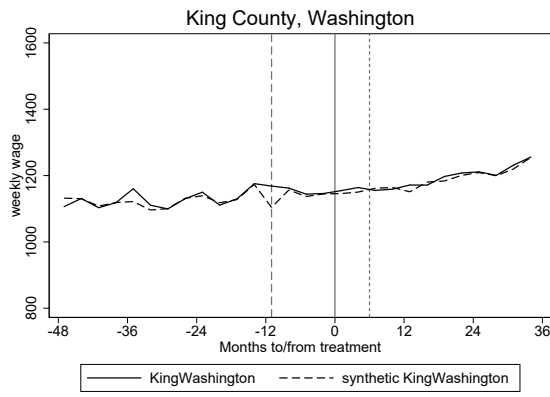
B. Hospitality



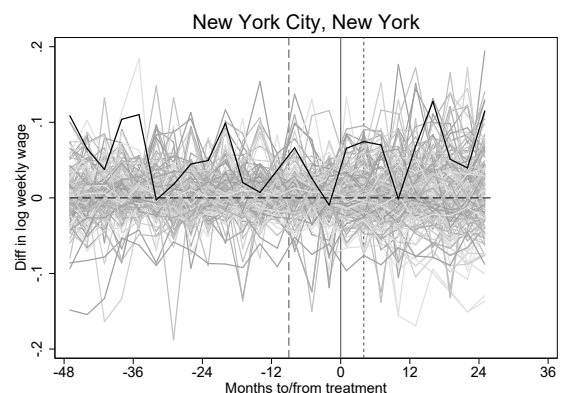
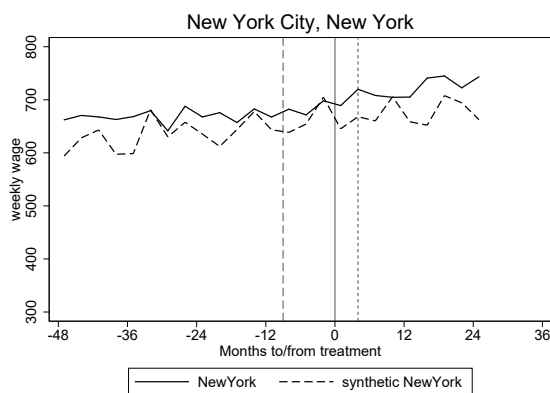
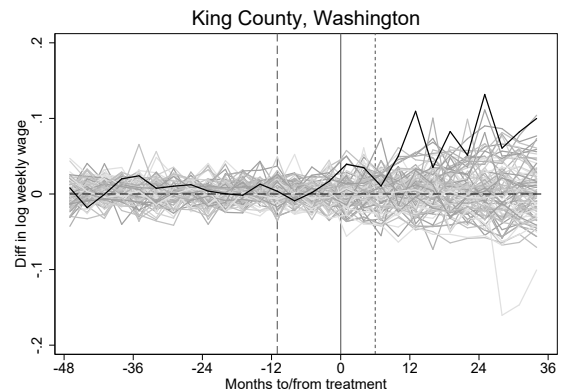
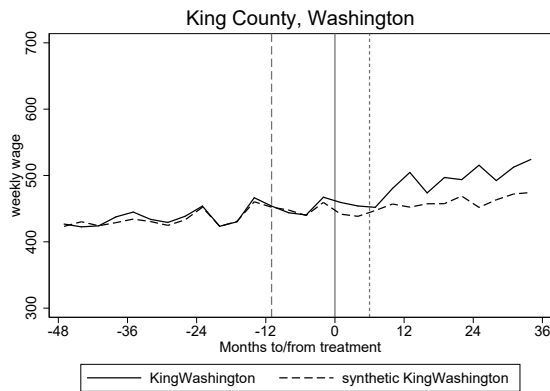
Note: The left column compares treated counties (solid lines) to the synthetic control counties (dashed lines). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios and the weekly wage between treated and synthetic control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A6: Construction and Hospitality—Weekly Wages in King County and NYC

A. Construction



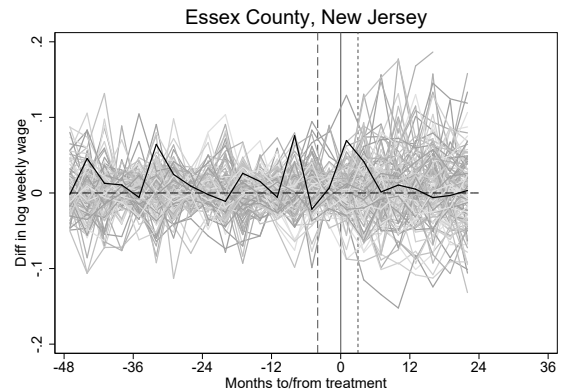
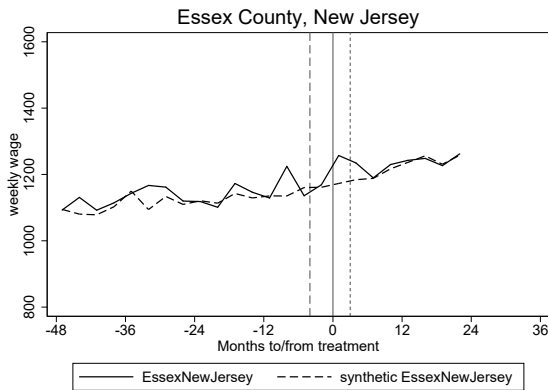
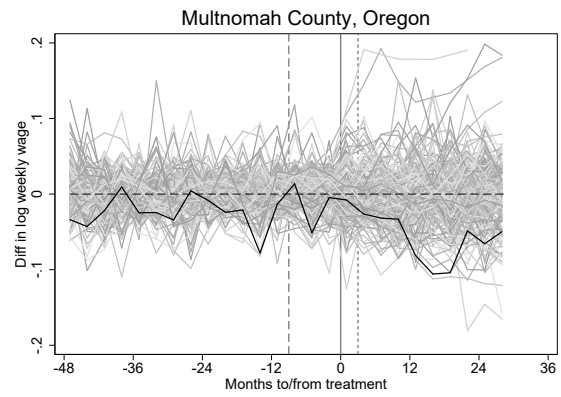
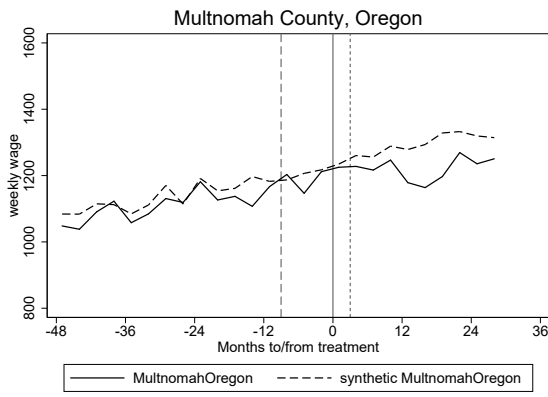
B. Hospitality



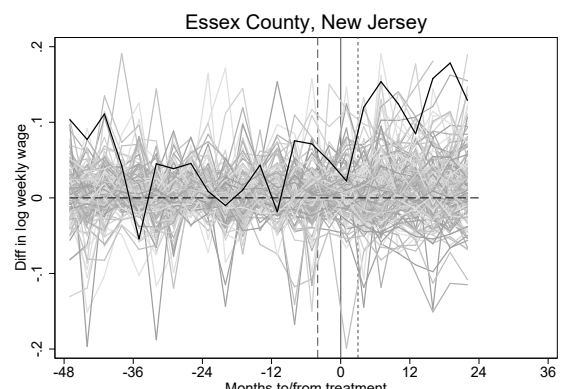
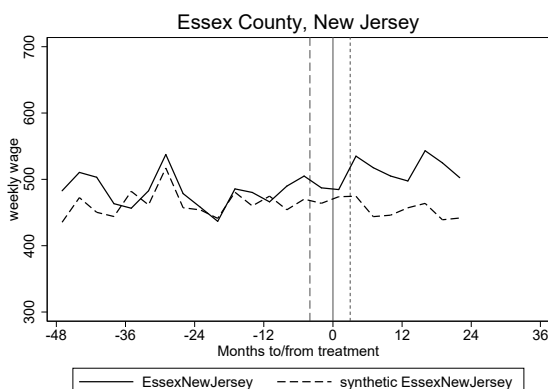
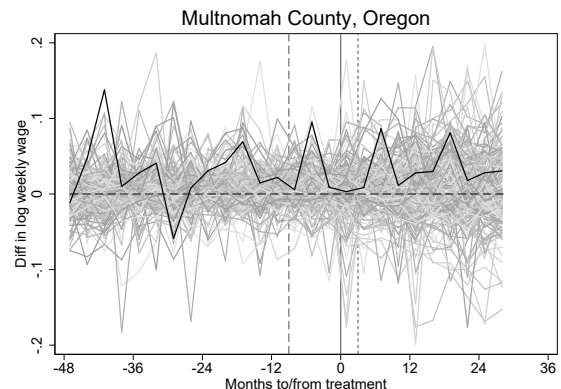
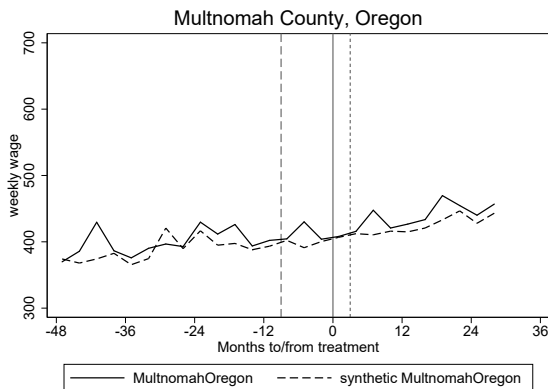
Note: The left column compares treated counties (solid lines) to the synthetic control counties (dashed lines). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios and the weekly wage between treated and control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A7: Construction and Hospitality—Weekly Wages in Multnomah County and Essex

A. Construction

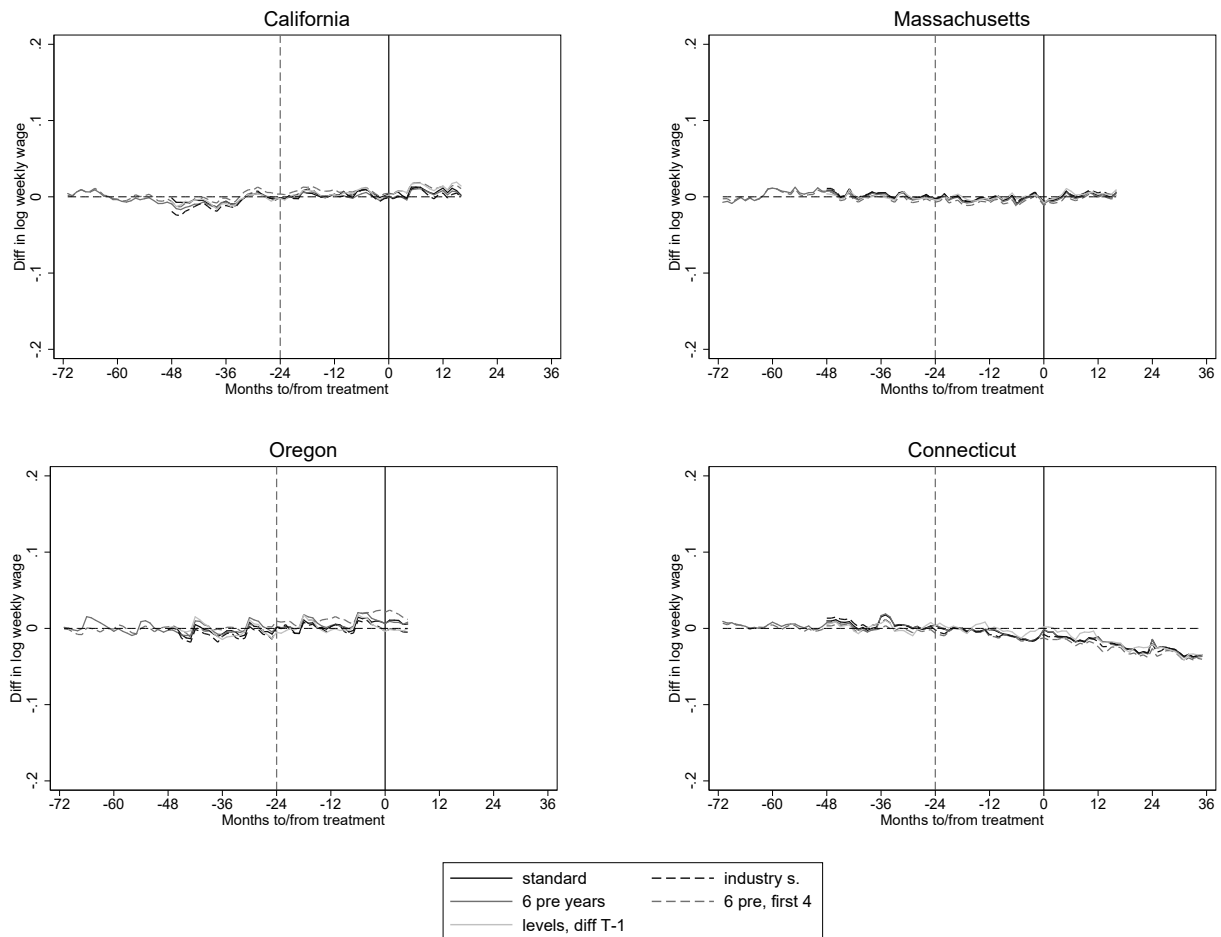


B. Hospitality



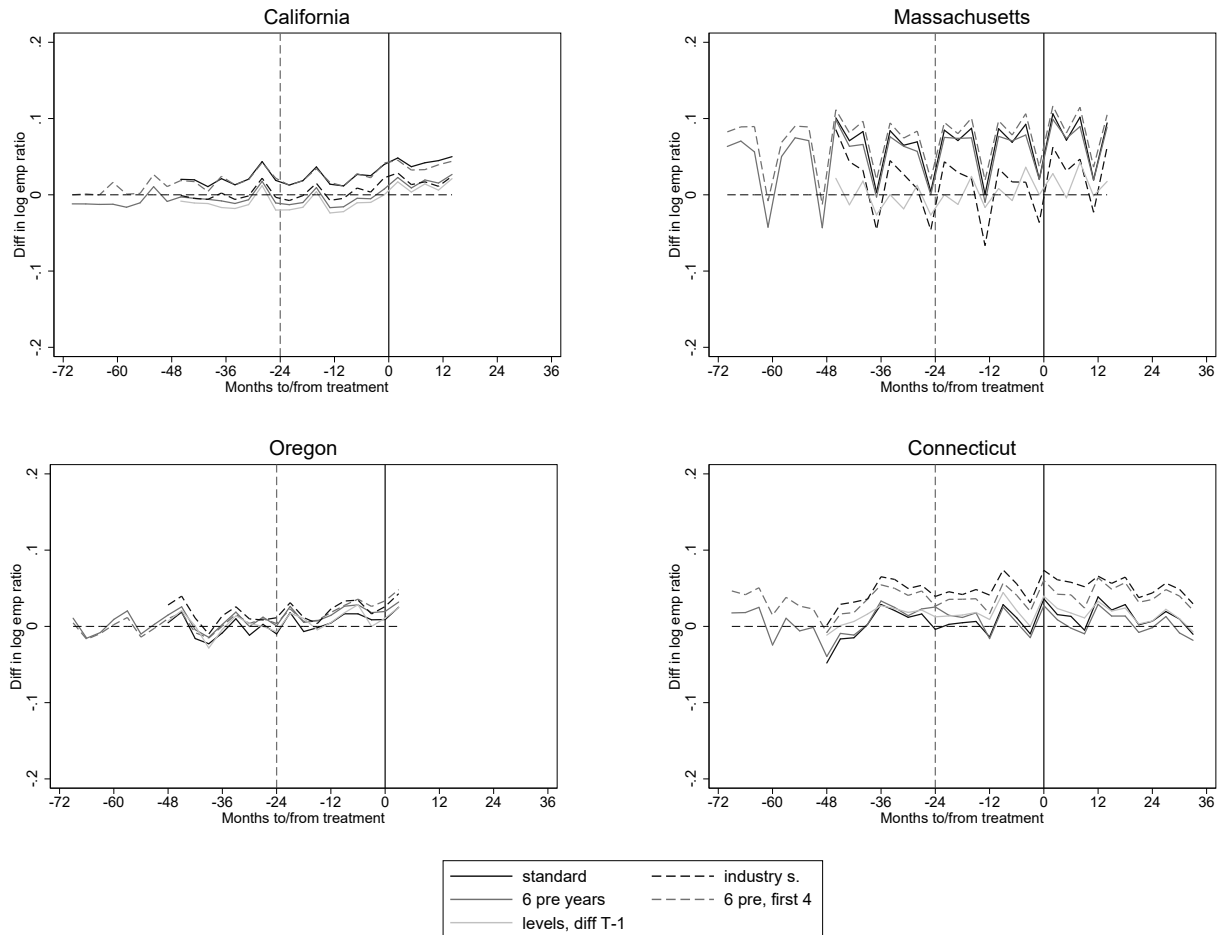
Note: The left column compares treated counties (solid lines) to the synthetic control counties (dashed lines). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithm of the employment ratios and the weekly wage between treated and synthetic control groups along with placebo estimates for counties with prereform *RMSPEs* smaller than 2 times the prereform *RMSPE* of the treated county (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A8: Alternative SGCM Modeling—Employment in States



Note: The lines always show the differences between treated and controls. The solid black line shows our standard modeling approach as shown in the right column of Figure 7. The black dashed lines select synthetic control counties based on additional industry-structure variables (Tables 2 and 3). The gray solid lines use six instead of four pretreatment years; the gray dashed line uses six instead of four years but stops applying the SGCM algorithm two years before the law’s enactment. The light gray solid lines use employment in levels, relative to employment in T-1 (the period before the law’s enactment) instead of the employment as a share of the population. The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicates when the law became effective, and the right dashed vertical lines indicate when the probation period was over. In Connecticut, the treatment group consists of private sector firms with at least 50 employees; in Oregon and Massachusetts, the treatment group consists of private sector firms with at least 10 employees. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

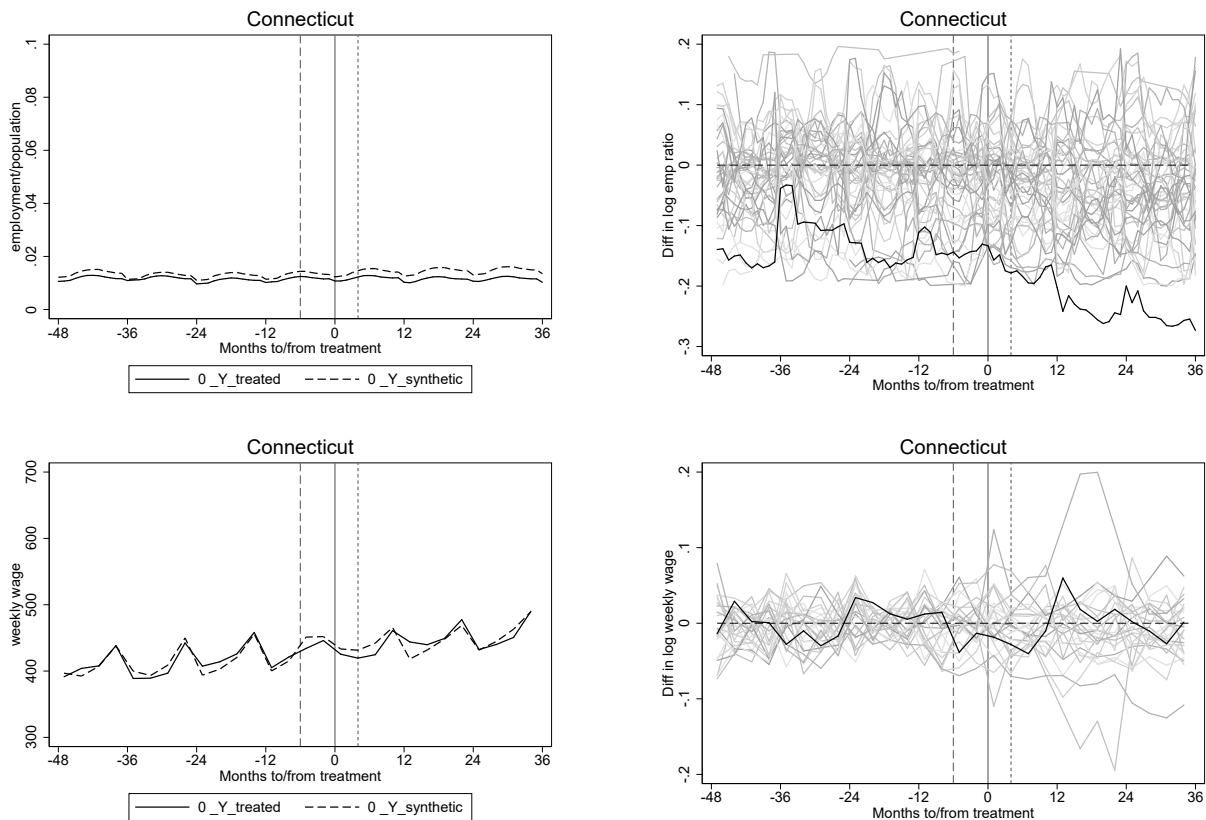
Figure A9: Alternative SGCM Modeling—Weekly Wages in States



Note: The solid black line shows our standard modeling approach as shown in the right column of Figure 8. The black dashed lines select synthetic control counties based on additional industry-structure variables (Tables 2 and 3). The gray solid lines use six instead of four pretreatment years; the gray dashed lines use six instead of four years but stops applying the SGCM algorithm two years before the law’s enactment. The light gray solid lines use wages in levels, relative to wages in T-1 (the period before the law’s enactment). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. In Connecticut, the treatment group consists of private sector firms with at least 50 employees; in Oregon and Massachusetts, the treatment group consists of private sector firms with at least 10 employees. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A10: Hospitality—Employment and Wages in Connecticut

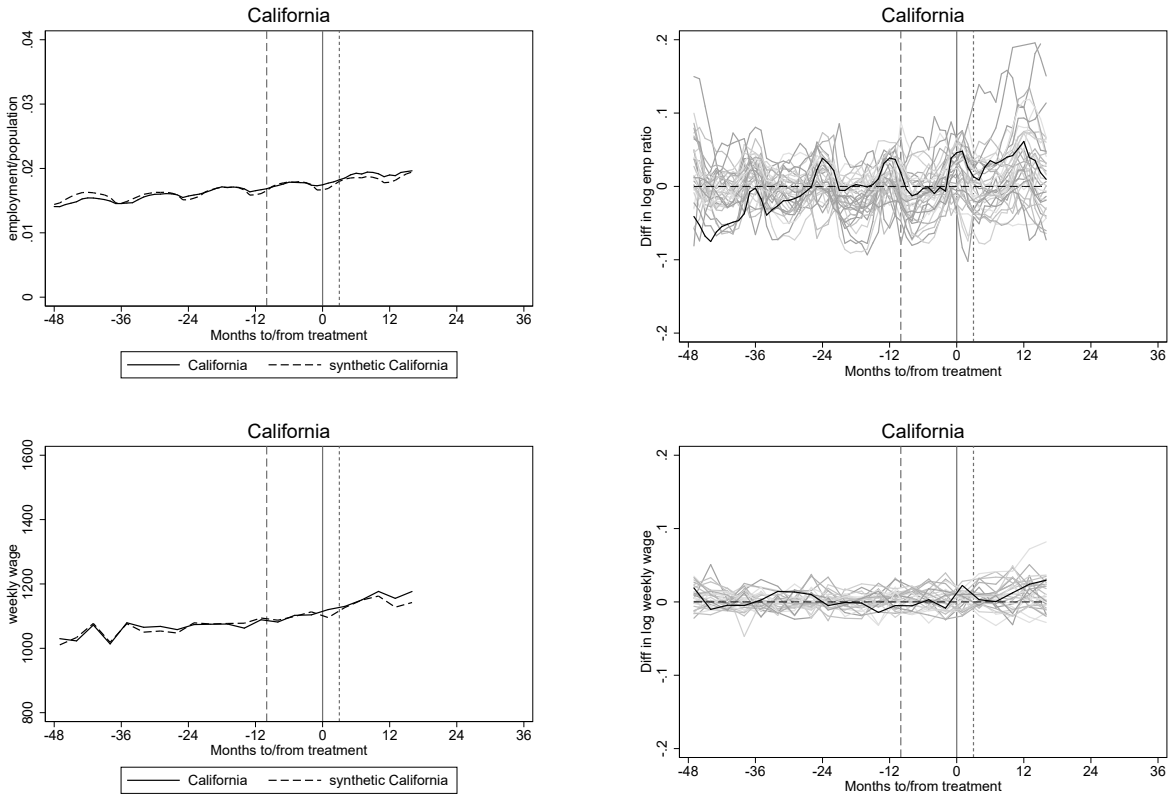
A. Hospitality



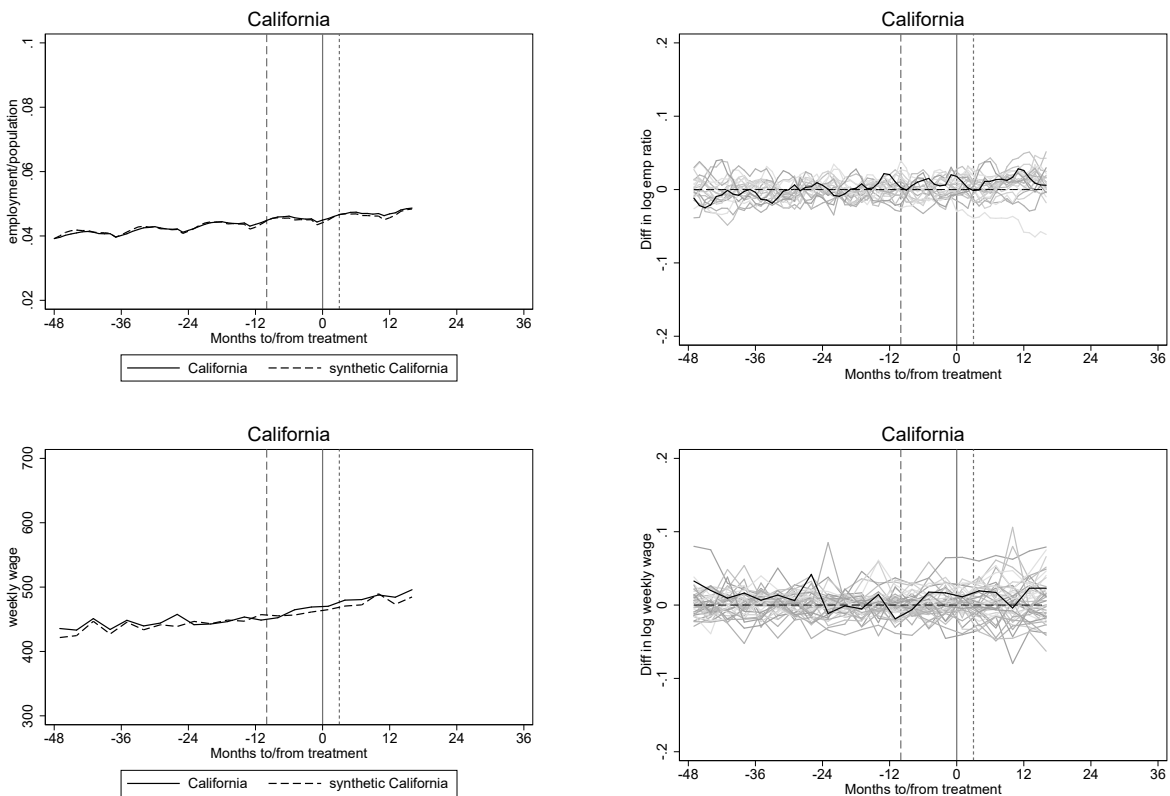
Note: The left column compares Connecticut (solid line) to the synthetic control state (dashed line). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithms of the outcome variable between Connecticut and the synthetic control state along with placebo estimates (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A11: Construction and Hospitality—Employment and Wages in California

A. Construction



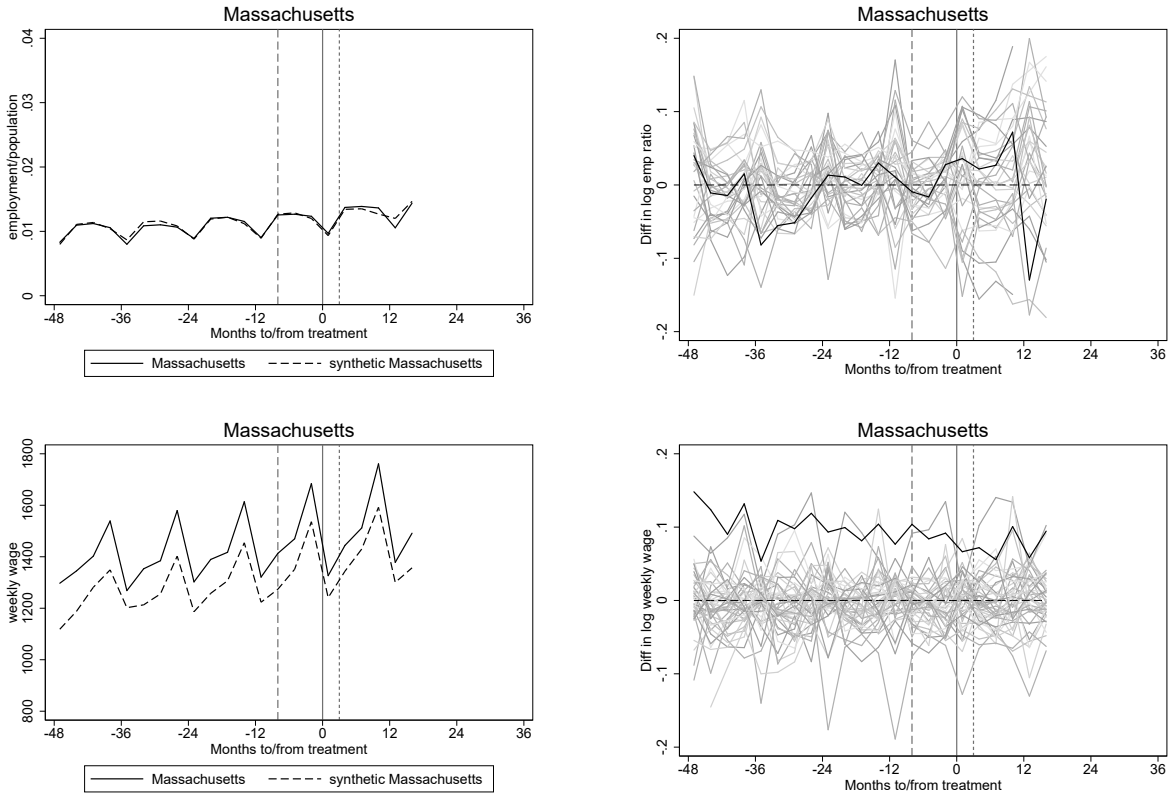
B. Hospitality



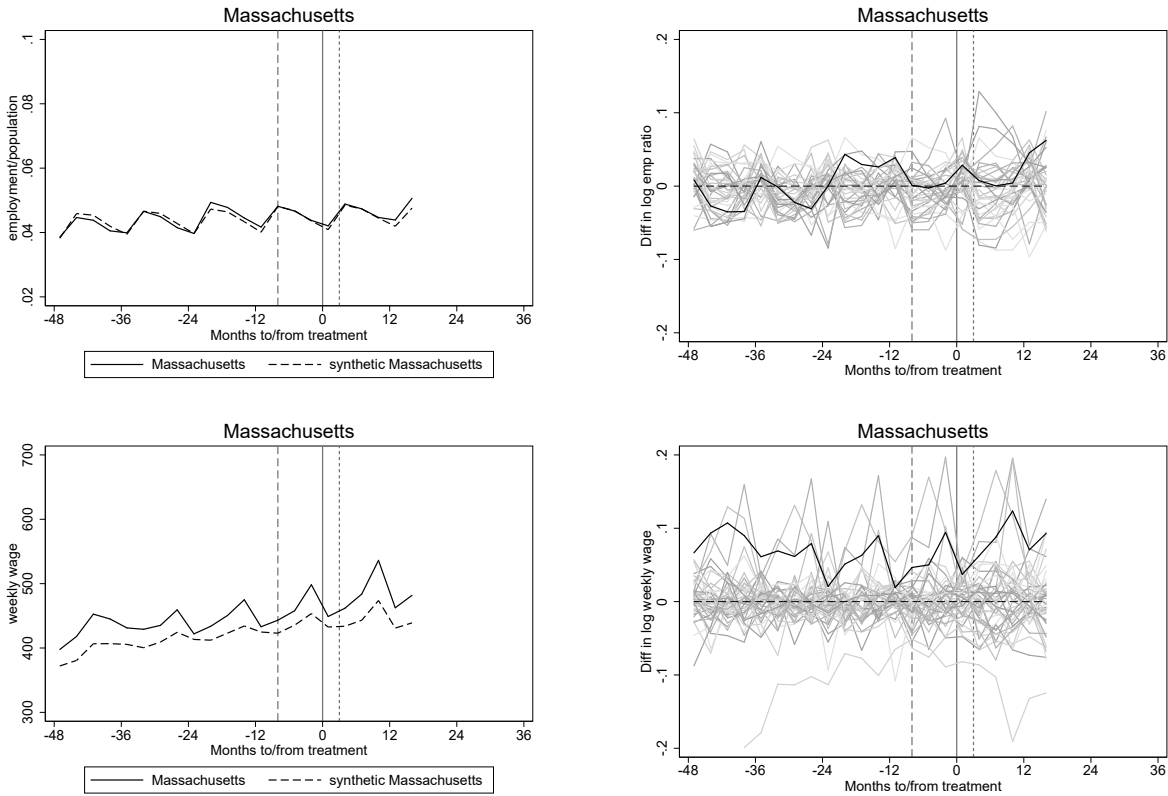
Note: The left column compares California (solid line) to the synthetic control state (dashed line). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithms of the outcome variable between California and the synthetic control state along with placebo estimates (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A12: Construction and Hospitality—Employment and Wages in Massachusetts

A. Construction



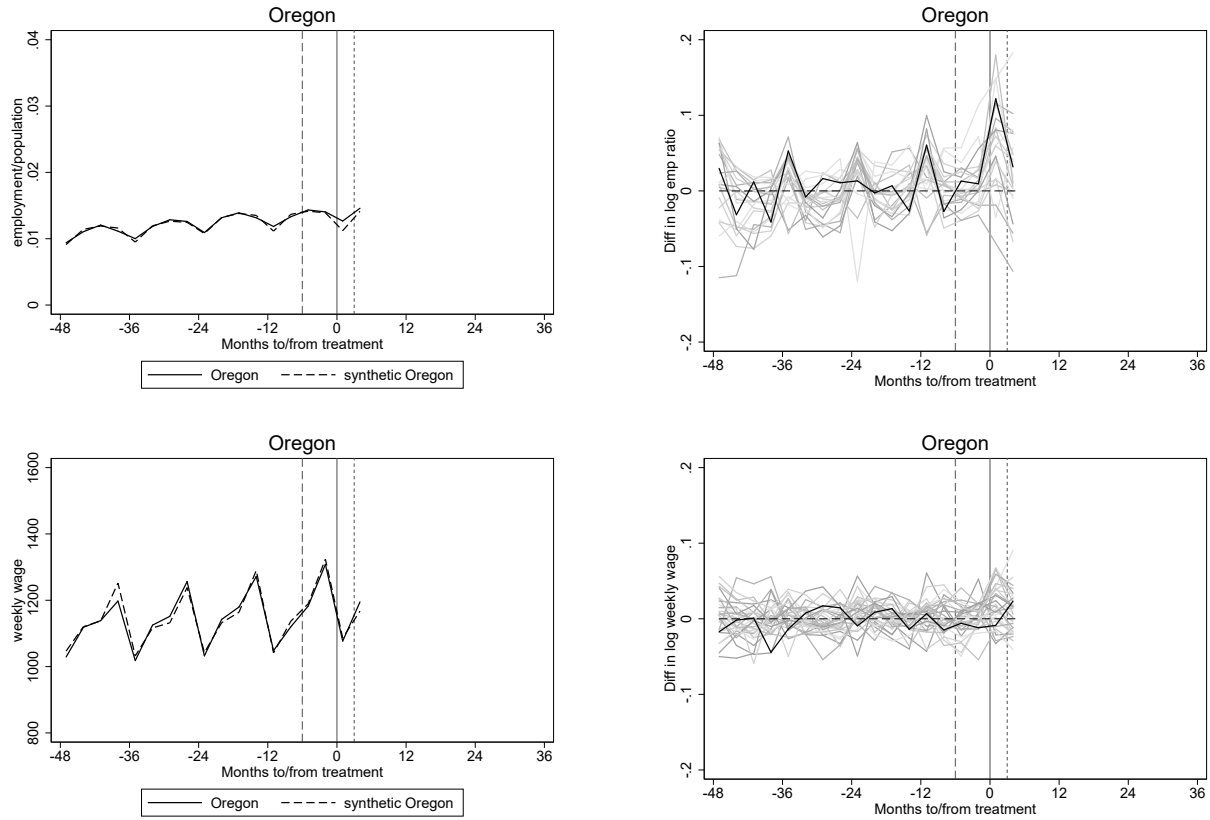
B. Hospitality



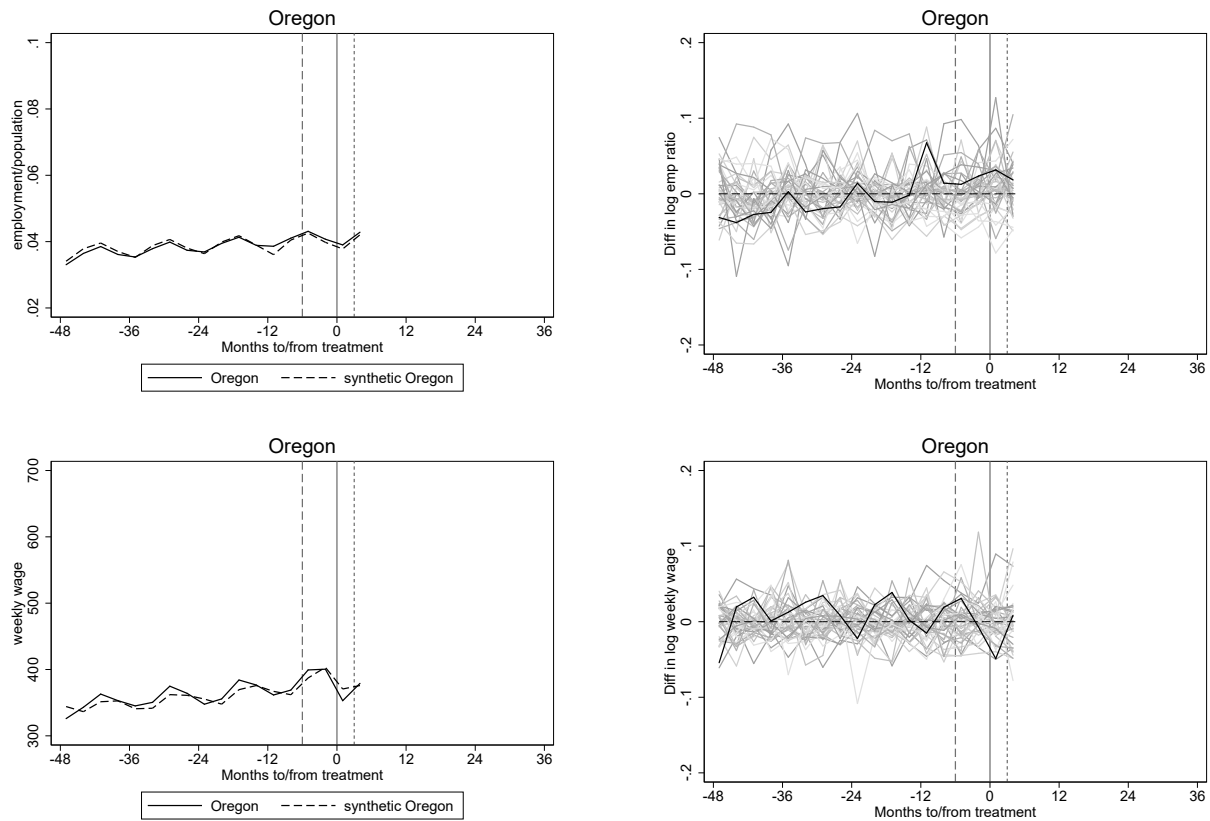
Note: The left column compares Massachusetts (solid line) to the synthetic control state (dashed line). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithms of the outcome variable between Massachusetts and the synthetic control state along with placebo estimates (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Figure A13: Construction and Hospitality—Employment and Wages in Oregon

A. Construction



B. Hospitality



Note: The left column compares Oregon (solid line) to the synthetic control state (dashed line). All SCGM analyses are in logs; graphs in the left column display the exponentiated values in levels. The right column shows the difference of the logarithms of the outcome variable between Oregon and the synthetic control group along with placebo estimates (gray lines). The left dashed vertical lines indicate when the law was passed, the middle solid vertical lines indicate when the law became effective, and the right dashed vertical lines indicate when the probation period was over. Table A4 shows the according statistics. For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

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
...
...

Note: The table shows the vector of weights W that minimizes the *RMSPE* (see Equation [2]) for all treated counties and log employment rate as dependent variable. These weights are used to construct the synthetic control counties in Figure 1 and Figure A1. The weights are also used to calculate the indicators in Table 4. ‘a’ indicates that the values 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. ... represents the missing counties (out of a total of 3,062). All counties with positive fractions indicate the donor share employed by the SCGM to replicate the treatment county in the column header. All fractions in one column (including the non-displayed “...”) add to 100%. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Table A2: 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

Note: The table shows the vector of weights W that minimizes the $RMSPE$ (see Equation [2]) for all treated counties and log wages as dependent variable. These weights are used to construct the synthetic counties in Figure 2 and Figure A2. The weights are also used to calculate the indicators in Table 5. 'a' indicates that the values 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 SCGM for the treatment county in the column header. Thus, all fractions in one column add to 100%. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Table A3: Synthetic Control Group Method—Alternative Modeling Approaches

	More Industry-Structure Controls				Normalize by T-1				6 Yrs Pretreatment				6 Yrs Pretreatment + Min. 4 Yrs			
	RMSPE		RMSPE		RMSPE		RMSPE		RMSPE		RMSPE		RMSPE		RMSPE	
	Pre	Ratio	PTE	LTE	Pre	Ratio	PTE	LTE	Pre	Ratio	PTE	LTE	Pre	Ratio	PTE	LTE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
A. Employment																
San Francisco,CA	0.013	3.624	0.047	0.027	0.022	0.814	0.009	0.010	0.019	2.921	0.053	0.030	0.019	3.172	0.057	0.033
King County, WA	0.006	1.301	0.005	0.003	0.006	2.200	0.009	0.010	0.007	1.328	0.006	0.003	0.007	3.170	0.020	0.010
New York City,NY	0.009	0.940	-0.001	-0.001	0.070	0.259	0.011	0.012	0.024	2.014	0.048	0.020	0.029	2.503	0.072	0.029
Multnomah, OR	0.008	1.947	0.014	0.008	0.008	0.724	0.003	0.003	0.015	0.926	0.013	0.007	0.018	2.008	0.034	0.018
Essex County, NJ	0.012	1.567	-0.016	-0.005	0.023	0.528	0.001	0.002	0.011	1.903	-0.020	-0.007	0.028	2.118	-0.057	-0.019
Hudson County, NJ	0.008	5.750	0.042	0.013	0.010	2.319	0.015	0.016	0.010	1.444	0.003	0.001	0.018	1.187	-0.014	-0.004
Philadelphia, PA	0.011	1.787	-0.017	-0.006	0.026	0.562	-0.012	-0.012	0.010	1.072	-0.005	-0.002	0.013	1.916	-0.024	-0.009
Alameda, CA	0.007	0.825	0.002	0.001	0.007	1.354	-0.008	-0.008	0.012	0.649	0.006	0.002	0.016	2.731	0.044	0.017
District of Columbia	0.051	2.419			0.010	1.424			0.044	2.732			0.050	2.725		
Average Counties		2.240	0.009	0.005		1.132	0.004	0.004		1.666	0.013	0.007		2.392	0.016	0.009
Connecticut	0.008	2.930	-0.023	-0.004	0.005	4.307	-0.020	-0.020	0.007	3.536	-0.022	-0.004	0.006	4.498	-0.028	-0.005
California	0.010	0.551	0.003	0.001	0.007	1.910	0.012	0.013	0.007	0.936	0.004	0.002	0.007	1.808	0.012	0.004
Massachusetts	0.004	1.189	0.003	0.001	0.003	1.780	0.003	0.004	0.005	0.762	0.000	0.000	0.006	0.840	-0.003	-0.001
Oregon	0.008	0.417	-0.003	-0.001	0.008	0.309	-0.002	-0.002	0.008	0.937	0.008	0.002	0.009	2.222	0.019	0.006
Average States		1.272	-0.005	-0.001		2.076	-0.002	-0.001		1.543	-0.003	0.000		2.342	0.000	0.001
Average C. & States		1.942	0.005	0.003		1.422	0.002	0.002		1.628	0.008	0.005		2.377	0.011	0.007
B. Wages																
San Francisco,CA	0.042	0.920	0.009	11.625	0.054	1.044	-0.045	-0.045	0.053	0.722	0.000	-3.785	0.054	0.716	0.006	6.755
King County, WA	0.023	1.857	0.025	32.831	0.040	0.992	-0.028	-0.029	0.020	2.971	0.050	66.726	0.026	3.450	0.083	109.085
New York City,NY	0.115	0.818			0.102	0.796			0.112	0.855			0.119	0.869		
Multnomah, OR	0.019	1.117	-0.011	-10.324	0.005	3.383	-0.004	-0.004	0.007	2.459	-0.005	-4.770	0.010	2.964	0.016	17.279
Essex County, NJ	0.019	1.378	0.001	9.929	0.017	0.906	-0.008	-0.009	0.016	1.141	-0.013	-17.412	0.022	0.890	-0.015	-19.666
Hudson County, NJ	0.162	0.920			0.033	0.879	0.013	0.014	0.187	0.892			0.162	0.923		
Philadelphia, PA	0.015	1.190	0.017	20.389	0.014	1.124	-0.002	-0.002	0.019	1.604	0.026	30.966	0.029	1.140	0.025	30.197
Alameda, CA	0.039	1.371	0.054	69.464	0.009	1.645	0.012	0.013	0.026	1.802	0.046	60.720	0.018	1.323	0.014	21.215
District of Columbia	0.019	0.861	-0.006	-10.867	0.023	0.495	-0.002	-0.002	0.018	0.845	-0.003	-6.370	0.019	0.815	0.007	9.007
Average Counties		1.159	0.013	16.292		1.252	-0.008	-0.008		1.477	0.014	18.011		1.454	0.019	24.839
Connecticut	0.047	1.173	0.054	84.235	0.019	1.092	0.017	0.018	0.020	0.755	0.005	6.995	0.038	1.188	0.043	67.718
California	0.009	2.227	0.019	22.730	0.015	0.871	0.011	0.012	0.011	1.710	0.017	21.390	0.020	2.003	0.039	45.964
Massachusetts	0.044	1.058	0.028	39.663	0.020	1.144	0.015	0.015	0.063	1.196	0.068	90.488	0.079	1.153	0.084	111.697
Oregon	0.022	1.593	0.034	34.088	0.014	1.364	0.018	0.018	0.015	1.749	0.026	25.595	0.015	2.731	0.041	39.843
Average States		1.513	0.034	45.179		1.118	0.015	0.016		1.352	0.029	36.117		1.769	0.052	66.306
Average C. & States		1.268	0.020	26.797		1.210	0.000	0.000		1.438	0.020	24.595		1.551	0.031	39.918

Note: All statistics displayed here are discussed in Section 5. Panel A shows the statistics for employment and Panel B shows the statistics for wages. The first four columns show the results when using additional industry-structure controls in the SCGM algorithm (see lower part of Tables 2 and 3). The next four columns show the results when implementing the SCGM on differences in the dependent variable from the period T-1, before the enactment of the mandate. Columns (9) to (12) show the results when using six instead of four years of pretreatment data for each treated region. And columns (13) to (16) use six pretreatment years but stop applying the minimization algorithm two years before the enactment of the mandate. The analogous figures are in Figures 3, A3, A8, and A9. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Table A4: Synthetic Control Group Method—Construction and Hospitality/Leisure Sectors

	Construction					Hospitality/Leisure				
	$\bar{Y}_{i,pre}^1$ (1)	RMSPE pre (2)	RMSPE Ratio (3)	P-Val (4)	PTE (5)	$\bar{Y}_{i,pre}^1$ (6)	RMSPE Pre (7)	RMSPE Ratio (8)	P-Val (9)	PTE (10)
A. Employment										
SF, CA	0.022	0.044	2.422	0.474	0.087	0.094	0.032	3.135	0.085	0.099
King County, WA	0.026	0.080	1.834	0.445	0.143	0.056	0.013	1.161	0.684	0.005
NYC, NY	0.013	0.025	0.983	0.749	0.018	0.042	0.021	1.898	0.229	0.037
Multnomah, OR	0.022	0.037	1.736	0.400	-0.007	0.064	0.017	2.127	0.211	0.033
Essex County, NJ	0.010	0.027	1.816	0.413	-0.033	0.030	0.043	1.471	0.373	0.047
Hudson County, NJ	0.006	0.468	0.823			0.025	0.029	1.987		
Philadelphia, PA	0.006	0.306	0.871			0.041	0.020	1.422		
Alameda, CA	0.022	0.043	0.407	0.984	0.015	0.039	0.014	0.978	0.613	0.005
DC	0.022	0.082	1.927	0.612	-0.129	0.096	0.037	3.283	0.062	0.117
All Counties			1.424	4.077	0.037			1.940	2.710	0.033
P-Val Irwin Hall				0.771					0.019	
CT (service sector, 50+)	0.002	not treated				0.012	0.136	1.646		
CA	0.016	0.031	1.185	0.596	0.033	0.043	0.011	1.334	0.298	0.012
MA (10+)	0.011	0.033	1.858	0.297	-0.001	0.044	0.026	1.164	0.383	0.023
OR (10+)	0.012	0.027	2.999	0.148	0.070	0.038	0.027	0.901	0.659	0.021
All States			2.014	1.043	0.034			1.261	1.341	0.018
P-Val Irwin Hall				0.189					0.382	
County & State			1.539	5.407	0.020			1.731	4.051	0.037
P-Val Irwin Hall				0.462					0.025	
B. Wages										
SF, CA	1166	0.055	2.260	0.260	0.118	476	0.026	1.426	0.439	0.032
King County, WA	1133	0.019	0.454	0.993	0.004	439	0.012	6.267	0.007	0.064
NYC, NY	1294	0.011	2.078	0.240	-0.020	671	0.062	1.235	0.566	0.068
Multnomah, OR	1123	0.032	2.010	0.291	-0.055	402	0.053	0.801	0.854	0.032
Essex County, NJ	1137	0.030	0.946	0.780	0.015	482	0.059	2.156	0.196	0.118
Hudson County, NJ	1270	0.032	2.677	0.075	0.085	425	0.027	0.980	0.675	0.008
Philadelphia, PA	1257	0.022	1.173	0.581	0.024	533	0.042	1.086	0.623	0.036
Alameda, CA	1315	0.014	2.980	0.064	0.037	460	0.052	2.726	0.075	0.133
DC	981	0.142	0.900			562	0.229	1.012		
All Counties			1.720	3.283	0.026			1.965	3.434	0.061
P-Val Irwin Hall				0.193					0.248	
CT (service sector, 50+)	1489	not treated				417	0.021	1.242	0.425	-0.001
CA	1069	0.009	1.973	0.128	0.015	448	0.017	1.029	0.723	0.015
MA (10+)	1424	0.101	0.763			444	0.070	1.215		
OR (10+)	1145	0.016	1.115	0.425	0.007	363	0.026	1.367	0.319	-0.020
All States			1.284	0.553	0.011			1.203	2.000	0.019
P-Val Irwin Hall				0.153					0.500	
County & State			1.611	3.836	0.023			1.775	5.434	0.047
P-Val Irwin Hall				0.103					0.288	

Note: All statistics displayed are discussed in Section 5. Panel A shows the statistics for employment and Panel B shows the statistics for wages. Blank cells indicate a poor *pre-RMSPE* fit; those regions are not included in joint tests. The first five columns show the results for the construction sector and the last five columns show the results for the hospitality and leisure sector. The analogous figures are in Figures 5, A3, A8, and A9. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Table A5: Test for Spillover Effects on Neighboring Counties of Treated Counties

County	Treated Neighbor	Employment				Wages			
		RMSPE Pre (1)	RMSPE Ratio (2)	PTE (3)	LTE (4)	RMSPE Pre (5)	RMSPE Ratio (6)	PTE (7)	LTE (8)
Marin, CA	San Francisco, CA	0.012	4.446	-0.048	-0.018	0.035	1.483	-0.042	-45.996
San Mateo, CA	San Francisco, CA	0.013	1.339	0.006	0.002	0.046	1.729	0.011	19.600
Alameda, CA	San Francisco, CA	0.009	1.926	-0.001	-0.001	0.015	1.029	0.011	12.530
Contra Costa, CA	San Francisco, CA	0.010	7.453	-0.066	-0.019	0.020	2.072	0.008	8.397
Kitsap, WA	King, WA	0.009	1.559	-0.006	-0.001	0.009	2.935	0.025	17.082
Pierce, WA	King, WA	0.012	1.067	0.010	0.003	0.010	1.289	0.003	2.588
Kittitas, WA	King, WA	0.035	0.922	0.006	0.001	0.012	2.170	0.014	7.722
Chelan, WA	King, WA	0.080	1.114	0.055	0.027	0.014	1.443	0.016	10.255
Snohomish, WA	King, WA	0.012	2.434	-0.016	-0.005	0.029	1.468	0.032	34.551
Nassau, NY	Bronx, NY	0.011	1.119	-0.004	-0.002	0.036	1.513	0.050	53.508
Westchester, NY	Bronx, NY	0.008	1.257	0.001	0.000	0.014	1.431	0.002	4.034
Rockland, NY	Bronx, NY	0.007	2.470	0.015	0.005	0.018	4.088	-0.019	-16.899
Clark, WA	Multnomah, OR	0.007	1.593	0.007	0.002	0.010	1.193	0.000	0.447
Columbia, OR	Multnomah, OR	0.023	2.255	0.044	0.008	0.013	1.785	0.010	6.694
WA, OR	Multnomah, OR	0.006	2.452	0.003	0.001	0.020	2.866	0.037	51.031
Clackamas, OR	Multnomah, OR	0.010	1.499	0.007	0.002	0.015	1.360	0.005	5.878
Hood River, OR	Multnomah, OR	0.117	1.545	0.162	0.079	0.024	2.833	0.063	38.859
Skamania, WA	Multnomah, OR	0.054	1.935	0.096	0.012	0.031	1.225	-0.016	-8.690
Bergen, NJ	Essex, NJ	0.010	1.138	-0.007	-0.003	0.019	1.724	-0.029	-37.681
Passaic, NJ	Essex, NJ	0.012	4.515	-0.052	-0.014	0.021	1.183	0.009	9.264
Union, NJ	Essex, NJ	0.017	3.807	-0.061	-0.022	0.028	1.541	0.034	44.881
Bergen, NJ	Hudson, NJ	0.012	1.155	-0.009	-0.004	0.023	0.758	0.013	16.034
Passaic, NJ	Hudson, NJ	0.011	5.103	-0.054	-0.015	0.015	1.205	-0.003	-2.216
Morris, NJ	Hudson, NJ	0.012	0.509	0.000	0.000	0.020	1.695	0.025	42.492
Union, NJ	Hudson, NJ	0.010	6.634	-0.059	-0.021	0.024	2.116	0.037	52.706
Bucks, PA	Philadelphia, PA	0.007	0.913	-0.001	-0.001	0.009	1.202	-0.008	-8.441
Montgomery, PA	Philadelphia, PA	0.009	0.620	-0.002	-0.001	0.017	0.639	0.004	4.196
Delaware, PA	Philadelphia, PA	0.009	1.838	-0.015	-0.005	0.012	1.398	0.010	11.671
Burlington, NJ	Philadelphia, PA	0.008	2.712	0.020	0.008	0.026	1.176	0.027	26.566
Cameron, PA	Philadelphia, PA	0.031	2.456	-0.073	-0.025	0.033	0.650	-0.008	-7.263
Contra Costa, CA	Alameda, CA	0.010	3.069	-0.029	-0.008	0.051	0.916	0.041	54.512
San Mateo, CA	Alameda, CA	0.020	1.054	0.019	0.009	0.197	0.434	0.081	165.503
Santa Clara, CA	Alameda, CA	0.011	6.730	0.069	0.034	0.074	2.062	0.148	319.458
Stanislaus, CA	Alameda, CA	0.014	0.846	0.001	0.001	0.018	1.760	0.030	24.084
San Joaquin, CA	Alameda, CA	0.023	0.929	0.014	0.004	0.007	3.519	0.018	14.146
Arlington, VA	District of Col.	0.019	1.940	0.037	0.021	0.025	0.815	0.011	16.524
Montgomery, PA	District of Col.	0.005	3.135	0.001	0.000	0.022	1.313	0.015	18.460
Pr. George's, MD	District of Col.	0.011	2.407	-0.021	-0.005	0.011	2.030	-0.011	-10.491
	Average	0.018	2.366	0.001	0.001	0.027	1.633	0.017	25.158

Note: The tables shows the effects on neighbors of treated counties. Columns (1) to (4) show the results for employment and columns (5) to (8) show the results for wages. Columns (1) and (5) display the prereform RMSPE as in Equation (3). Columns (2) and (6) display the RMSPE Ratio (RMSPE post/RMSPE pre). Finally, columns (3) and (7), and (4) and (8) show the PTE and LTE as in Equations (4) and (5). Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.

Table A6: 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

Note: The table shows the vector of weights W that minimizes the *RMSPE* (see Equation [2]) for all treated states and log employment rate as dependent variable. These weights are used to construct the synthetic control states in Figure 7. The weights are also used to calculate the indicators in Table 4. '0' indicates that the state is a potential donor state but has not actually been used as a donor. All states with positive fractions indicate the donor share employed by the SCGM for the treatment state in the column header. All fractions in each column add to 100%. Source: QCEW ([Bureau of Labor Statistics \(BLS\), 2018](#)), own calculation and illustration.

Table A7: States for Synthetic Control Group—Weekly 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

Note: The table shows the vector of weights W that minimizes the *RMSPE* (see Equation (2)) for all treated states and log wages as dependent variable. These weights are used to construct the synthetic control states in Figure 8. The weights are also used to calculate the indicators in Table 5. '0' indicates that the state is a potential donor state but has not actually been used as a donor. All states with positive fractions indicate the donor share employed by the SCGM for the treatment state in the column header. All fractions in each column add to 100%. Source: QCEW ([Bureau of Labor Statistics \(BLS\), 2018](#)), own calculation and illustration.

Table A8: Test for Spillover Effects on Exempt Small Firms in Treated States

State	$\bar{Y}_{i,pre}^1$ (1)	RMSPE pre (2)	RMSPE post (3)	RMSPE Ratio (4)	PTE (5)	LTE (6)
A. Employment						
Connecticut	0.148	0.007	0.029	4.011	0.028	0.001
Massachusetts	0.065	0.007	0.026	3.703	0.024	0.001
Oregon	0.070	0.009	0.031	3.395	0.029	0.001
B. Wages						
Connecticut	1002	0.101	0.050	0.492	0.045	19.304
Massachusetts	862	0.016	0.025	1.599	-0.019	13.117
Oregon	686	0.014	0.016	1.078	0.016	1.146

Note: The tables tests the impact of the mandates on exempt small firms. For Connecticut these are small businesses in the service sector (<50 employees). For Massachusetts and Oregon, these are small businesses with less than 10 employees. Column (1) displays the outcome in levels for each treated state averaged over all prereform years. Columns (2) and (3) display the *RMSPE* as in Equation (3) for pre and postreform years. Column (4) displays the *RMSPE Ratio* (RMSPE post/RMSPE pre). Column (5) and (6) show the *PTE* and *LTE* as in Equations (4) and (5). For more information about the sick pay reforms, see Table 1. Source: QCEW (Bureau of Labor Statistics (BLS), 2018), own calculation and illustration.