The Labor Market Effects of Sick Days Mandates

Ben Van Kammen

Author's Address:
Ben Van Kammen
Purdue University
Krannert Building, Room 531
403 West State Street
West Lafayette, IN 47907-2056

Author's Email and Phone:
bvankamm@purdue.edu; Phone: 1(414) 559-2652

Abstract: This paper uses local ordinances to measure the effects of mandated paid sick days on employment and wages. Using the Quarterly Census of Employment and Wages, modest decreases in employment and wages are observed in the mandate counties relative to a control group of places without an ordinance. Analysis of industry-county data reveals that the mandates shift employment from the least constrained industries to the most constrained, as measured by the prevalence of pre-mandate sick days by industry. This evidence suggests that the costs of sick days mandates are paid partly by a positive productivity spillover within the most constrained, e.g., food service, industries. The results are robust to falsification tests that assess the exogeneity of treatment, as well as unrestrictive assumptions about time trends, covariates, and duration (lags of treatment) dependence of the effects. JEL Codes: J33, J38, H75, I18.

Keywords: sick leave, sick days, mandate, differences in differences, fringe benefits, employee health, program evaluation.

1. INTRODUCTION

The most convincing argument in favor of paid sick days mandates asserts that they correct a negative public health externality by encouraging contagiously ill workers to quarantine themselves at home. These merits have motivated most nations, i.e., at least 145 (Heymann 2007), to mandate that some paid sick days be part of employees' compensation. The United States has not, presumably judging that employers would offer paid sick days voluntarily in lieu of other compensation to any employees that want them.

Along with other job amenities, paid sick time is a non-wage employment benefit transacted in an implicit market. Equilibrium in this market could exhibit the optimal incidence of sick time provision, in which case mandating a different level would likely harm employment. Alternatively firms may under-provide sick time if it has un-internalized spillover benefits. It is uncontroversial that co-workers contribute to good health in their workplaces when they stay home with contagious illnesses. Firms' interests are aligned with limiting the spread of illnesses and internalizing their productivity effects on co-workers, thus paying sick workers to stay home is part of maximizing employers' profits. Since firms have no incentive to internalize spillovers to other firms' employees, though, under-provision of paid leave follows if health externalities extend beyond co-workers at the same firm.

Government mandates in other countries, and an increasing number of states and municipalities in the United States, reveal concerns that the prevalence of paid sick days is sub-optimal. If these mandates correct a market failure, external productivity gains could compensate for the burden of compliance and increase employment at the constrained firms.

This paper measures the employment and wage effects of local paid sick days mandates by employing a differences in differences (hereafter DD) strategy. Many researchers [e.g., Card

and Krueger (1994), Klerman and Leibowitz (1997) and Ruhm (1998), Baker and Milligan (2008)] have used DD to analyze similar policies. I compare the outcomes in U.S. localities that have enacted laws mandating paid sick days to places that did not do so. My estimates indicate small negative effects of sick days mandates, less than 1 percent, on both county-level employment and wages. Analysis of industries within each county, however, reveals heterogeneity on the basis of how constrained each industry was by the law. The laws shifted employment from industries with relatively high pre-mandate sick days prevalence to industries with relatively low sick days prevalence. Though I do not reject the null of zero effect on the industries' wages, the estimates suggest wages decreased in the most constrained industries, in absolute terms and relative to the least constrained industries. For the latter group, the point estimates indicate a positive wage effect.

Employment increases in the constrained industries indicate that mandated paid sick days laws, indeed, correct a market failure. Advocates of the mandates sometimes tout them as a way of relieving employees from choosing between health and income, however, employees do pay for mandated sick days with lower wages. Sick days mandates fail to pull more people into the labor market on net, since employment gains in constrained industries are offset by losses in less constrained industries. Functionally the sick days mandate is like a subsidy to the industries that didn't already give employees sick days.

Section 2 of this paper summarizes the preceding literature on benefits mandates and espouses the theoretical effects of mandating employee benefits. Section 3 describes the data and methods I use to measure the effects of mandates. Section 4 conveys the results of the methods used, and section 5 summarizes the conclusions drawn from those results. Though

some international statues make a distinction between the terms based on the length of the absence, throughout the paper I use “paid sick days” and “paid sick leave” interchangeably.

2. BACKGROUND, LITERATURE REVIEW, AND DISCUSSION OF THEORY

A municipality may pass a law, through a referendum for example, requiring private employers to provide sick days to their employees at a specific rate, i.e., paid hours per hours worked, and subject to a maximum number of sick days. The timeline (figure 1) shows examples of such laws, and an overview of their provisions is on table 1.

{Place table 1 about here}

The places that legislate and enforce mandates are the treatment group in this analysis. San Francisco passed the first law of this kind in 2006, and it took effect in February of 2007. Washington, D.C., introduced a law shortly after that, passed it, and implemented it in November of 2008. Milwaukee passed a sick days mandate in a referendum but it was subsequently overturned by the State of Wisconsin. Popularity has increased since 2012 with the State of Connecticut, Seattle, Portland (OR), New York City, and several cities in New Jersey enacting mandates. California and Oregon will enact statewide mandates in 2015, and a handful of local mandates will be enacted in the near future (National Partnership for Women and Families), but some time must elapse before their effects can be observed. Additionally the Federal government (the Healthy Families Act) and 25 states have bills proposing mandated sick days (ibid).

{Place figure 1 about here}

Consider what to expect when measuring these laws’ effects on labor employment and wages. The key theoretical forebear of this topic is Summers’s (1989) “Simple Economics of

Mandated Benefits," which allows for three possible effects on employment. First if employees value a mandated benefit equal to its cost, employees will effectively pay for it by accepting reduced wages at the identical level of employment. Second mandating a worthless benefit has the same effect as a tax on labor: shifting labor demand downward by the cost of the benefit without an off-setting shift in supply. A final possibility is mandating a benefit that is more valuable to employees than it costs, which increases employment. If this is observed, it's incumbent on the observer to explain why the mandate was necessary, considering that it reveals unmade mutually beneficial trades between employers and employees.

As noted by Summers (178) using the example of health insurance, this may occur if employees' good health has externalities that firms have no interest in resolving. As with a standard positive externality, paid sick time is under-provided if its good health benefits spill over to employees at different firms. Then correcting the externality attracts people, i.e., those at the margin in terms of their preferred mix of wage and benefits, to the constrained firms from other jobs or from outside the labor force. If the mandated level of sick leave is chosen wisely, this explains why a positive employment effect is foreseeable.

Adverse selection could further explain the under-provision of sick days. Specifically a mandate can have net positive effect on labor markets if it corrects an information asymmetry, as proposed by Aghion and Hermalin (1990). Perhaps firms expect that offering sick days will attract unusually sickly applicants. But if all employers must offer sick days by law, the sickly population diffuses among employers according to mutual benefit instead of pooling by the ones that offer leave voluntarily. In this manner, a sick days mandate "destroys the choice" as well as the market failure.

Predictions about the effect of a benefits mandate can be extended to particular firms within a geographical market. For example disemployment is more likely if employees cannot pay for the mandated benefit because of a minimum wage. Marks (2011) and Simon and Kaestner (2004) analyze the effects of minimum wage increases on provision of fringe benefits, hypothesizing that they could be a buffer against disemployment; the reverse of this process could occur with mandated benefits. More directly the effect of the mandate should differ between industries that already give most employees paid leave, i.e., where the law is loosely binding, and those that don't. Although I do not use the information on average firm size in the QCEW in this paper, the laws are explicitly less binding on smaller firms. The variation across 2 digit industries in average firm size is coarser than desired for showing differential effects.

Following the health insurance example in Summers, three forces could contribute to the effect of a sick days mandate: the cost of complying with it, the value of sick days to employees that get them as a result of the mandate, and the positive health externality. Markets without pre-mandate sick days provision would potentially experience all three; markets with sick leave already would experience only the externality. This predicts that industries with abundant paid sick leave prior to the mandate move up the supply curve as a result of the law and experience higher wages and productivity. Employment in such a market would also increase unless applicants were pulled away toward the more constrained markets. Markets without pre-mandate paid sick leave could experience employment and wage increases or decreases depending on the magnitudes of the three forces. Namely the externality and employees' valuation of sick days tend to increase employment; the cost of compliance tends to decrease employment; the cost and valuation tend to decrease wages, and the externality tends to increase wages.

Measuring these effects is more complicated than reading a labor supply and demand graph, though. Mitchell (1990) suggests the possibility of substituting mandated benefits for other benefits, rather than wages. Her paper additionally contains a useful summary of previous estimation techniques for measuring wage-benefit substitutability. Mitchell also recognizes that a mandate affects the relative prices of labor in addition to the input price level that firms face. Low-skilled workers are more likely to be affected by a benefits mandate because they are less likely to receive fringe benefits voluntarily. Requiring employers to provide benefits to all employees does nothing for the employees that already receive said benefits but increases the costs of employing low-skilled workers. So substitution among types of workers (away from low-skilled) might mitigate a mandate's disemployment effect. More on the wage-benefit substitution mechanism can be found in Woodbury (1983), Feldman (1993), Simon (2001), Olson (2002), or Marks (2011). Studies on more specific groups like low-wage workers (Lee and Warren 1999; Sherstyuk, Wachsman, and Russo 2007), and teens (Kaestner 1996) are also available, and the most common mandated benefit they examine is health insurance.

Some research has been performed on measuring the cost improving worker health directly. Markussen (2011) studied Norwegian individuals using matched employee-physician data and instrumental variables methods, finding that spells of sick leave have a negative relationship with lagged measures of earnings. Another of his papers (2010) endeavors to uncover the optimal wage replacement rate for employees on sick leave. This is not interpreted as an estimate of sick leave's compensating wage differential, though, since Norwegian workers have social insurance that provides 52 weeks of paid leave. Instead sick leave's wage effects are interpreted as depreciated human capital and signals of productivity. A survey of the value of sick days can be found in Earle and Heymann (2006). One quantitative estimate of

"presenteeism" costs, lost productivity when workers are sick at work, comes from Goetzel, et al. (2004). Some of the authors' larger estimates suggest that on a per worker-year basis, illnesses result in $100 to $400 of lost productivity. But many conditions studied in that paper are chronic illnesses that are unlikely to improve with a sick leave allowance. Lovell (2004) focuses on the public health benefit instead of productivity, but a precise estimate (measured in GDP or worker-days) of the value is absent. Even if the mandate harmed labor market efficiency, the public health benefits are potentially great enough to offset those costs and make it a beneficial policy. As evidence that the San Francisco law improved public health, my findings are preceded by Drago and Lovell (2011) who, in employee surveys, found reductions in the prevalence of sick workers and of sick children attending school.

In terms of methods and subject, the five papers that are most similar to this one are: Ruhm's (1998) study of parental leave mandates in European countries, Klerman and Leibowitz's (1997) study of maternity leave mandates in 12 American states between 1987 and 1993, Ziebarth and Karlsson's (2010) study of changes to sick leave mandates in Germany during 1996-97, Colla, Dow, and Dube's (forthcoming) evaluation of the San Francisco Health Care Security Ordinance, and Petro's (2010) case study of the San Francisco sick days ordinance.

The first two papers rely on DD as well as DDD since they also exploit differences between males' and females' uses of parental leave (Ruhm) and between new mothers and mothers of older children (Klerman and Leibowitz) to measure the policies' effects. Petro does a less formal version of this, focusing on small firms and the retail and food service industries. All four papers use DD methods in which a group is treated with a change to its mandated level of leave, and that group is compared to another that is untreated.

Ziebarth and Karlsson emphasize labor costs more than employment levels and find significant decreases in the utilization of sick days under the less generous mandate. Ruhm's results indicate that mandated parental leave increases female employment-population ratio by 1.3 to 1.8 percentage points. Klerman and Leibowitz conclude that maternal leave statutes have a negligible effect on female employment. Petro's conclusion is that employment grew relatively rapidly in San Francisco county compared to five neighboring counties, and that the growth rate of business establishments (large and small) in San Francisco outpaced neighboring counties during the period following the mandate.

Colla, Dow and Dube study a "pay or play" health insurance mandate enacted in San Francisco in 2008. Though it did not coincide with the paid sick days mandate, the timing is near enough that controlling for its effects in this paper is important. Moreover CDD use the same data source for their paper that I use and find similar effects associated with San Francisco's Health Care Security Ordinance, i.e., small and probably negative effects on employment and wages.

3. **METHODS AND DATA**

A. County Analysis

I use panel data to estimate the following model:

$$Y_{it} = \alpha_i + \alpha_t + \sum_s \beta_s SICKMANDATE_{i,t-s} + \gamma_i t + \delta' X_{it} + \phi Y_{i,t-1} + \varepsilon_{it}, \qquad (1)$$

where, *i* indexes counties, and *s* and *t* index months. *Y* is the natural logarithm of employment. In the subsequent regression measuring the wage effect, *Y* is the log average weekly real wage. I

include county and time fixed effects, represented by α_i and α_t, and idiosyncratic time trends, γ_i. X is a vector of control variables: log of county population, logs of federal, state, and local government employment, minimum wage, and population growth variables. Minimum wage is observed for the state in which county *i* is located and in the year of which month *t* is part. San Francisco has (and a few other counties near the end of the sample have) a higher local minimum wage and differs from the rest of the state. Since San Francisco enacted its health insurance mandate (Colla, Dow, Dube) at approximately the same time as its sick leave mandate, I control for the dollar value per hour of mandated health insurance contributions and try not to confound the two laws' effects. SICKMANDATE is an indicator for whether a mandate has been enacted; β is the effect of the mandate. The sum of coefficients (including lags) estimates how the employment effect accumulates as time passes.[1]

I assume and verify with a test that the mandate indicator is strictly exogenous in (1), i.e., after conditioning on the fixed effects, time trends, and (instrumented) autoregressive term. The leads of the mandate indicator are the basis for a falsification test described by Rothstein (2010) and Koedel and Betts (2011). If leads of the treatment indicator are not excludable in the estimates, it is a signal that the exogeneity assumption fails. In addition to performing this on all specifications, I test restrictions on the coefficients in (1): restricting the treatment effect to being independent ($\beta_s = \beta \; \forall \; s$) of time elapsed since the law's enactment, restricting the autocorrelation in Y ($\phi = 0$), and restricting the treatment and control counties to having common time trends. No mandate has been repealed so far, so the SICK indicators stay on once turned on and I cannot compare switching on to switching off effects.

[1] This methodology follows McCrary (2007), particularly for the interpretation of coefficients. Also see Wolfers (2006), Jacobson, Lalonde and Sullivan (1993).

Estimates of β, however, are not qualitatively sensitive to stronger assumptions outlined above, e.g., about autoregression in employment or wages. An addition to the robustness of the estimates is that the residuals are clustered when computing the standard errors, a technique proposed by Arellano (1987). This addresses the effects of arbitrary correlation among observations of the same place. All the standard errors presented from estimates of employment and wage in this paper are of the cluster (by county or by industry-county where applicable) robust variety.

B. Industry Analysis

To reinforce the county estimates, I estimate a model at the industry-county level. This enables me to interact the treatment variables with measures of each industry's proportion of workers affected by the mandate. Specifically I estimate the following:

$$Y_{ijt} = \alpha_{ij} + \alpha_t + \sum_s \left(\beta_{1s} + \beta_{2s} VOL_j\right) SICKMANDATE_{it-s} + \gamma_{ij} t + \delta' X_{it} + \phi Y_{ij,t-1} + \varepsilon_{ijt}. \quad (2)$$

Y is the log of employment (subsequently of average wage) in industry "j" in county "i" in month "t". The mandate indicator is defined as before, but in this equation it is interacted with the industry characteristic "VOL_j," proportion of workers in that industry that already transacted paid sick days from their employers voluntarily.

{{Place Table 2 about here}}

Following estimation of the duration-independent version of (2), I contrast the effects on the minimally constrained $\left(\hat{\beta}_1 + \hat{\beta}_2 * 0.93\right)$ and maximally constrained $\left(\hat{\beta}_1 + \hat{\beta}_2 * 0.27\right)$ industries as an illustration of how the effect varies depending on initial conditions. Utilities is

the industry with 93% voluntary sick days, and Accommodation and Food Services has 27%. Within the treatment counties, industries with near-universal sick days should experience no wage-fringe trade as a result of the mandate, but they could get positive (productivity-enhancing) health externalities. An industry with low prevalence of sick days would also experience the health externality (if anything, even more intensely) and a wage-fringe trade. If the health externality makes the mandated sick days "cheap" for workers, though, the most constrained industries become more attractive, relative to the minimally constrained ones, in terms of wage-fringe combinations. Thus employment may shift from the least constrained industries to the most constrained, within the mandate counties.

C. Data

Data on employment and wages are available from the QCEW, compiled by the Bureau of Labor Statistics. The QCEW contains the employment in each county, disaggregated by NAICS industry. Employment is observed on a monthly basis, so the data set consists of month-county observations. Average weekly wage per county is observed each quarter and can also be tabulated by industry. In addition, the QCEW reports the number of government employees (local, state, and Federal separately) in each county. Other sources of data for the main estimates come from the U.S. Census (county birth, domestic and international migration rates) and the Department of Labor (minimum wage by state and year). National Compensation Survey (NCS, March 2010) tables provide statistics on the prevalence of paid sick days by industry, using the NAICS 2 digit classification.

{{Place Table 3 about here}}

As the time span for the sample, I use 2003 to 2014 inclusive. San Francisco's mandate was proposed in August 2006 and passed in November 2006, so a substantial number of periods are observed to establish a pre-treatment trend and use leads of the mandate indicator. The most recent mandate in the sample is Newark, NJ in May 2014, so the treatment lags are limited to 7 months if all 19 counties are to be used to measure their effects.

Aside from the variables already mentioned, time trends have autonomous effects on employment. All of the economic variables discussed are time-variant; business cycles, national population growth, globalization, et al., affect them, and the trends can differ across locations. To capture additional variation over time that might confound the interpretation of the estimates, I include month (quarter for the wage regressions) by year indicators and two varieties of time trends (alternately): treatment-specific and county-specific [industry-county-specific for (2)]. In both models, α_t and γ capture these influences.

4. **ESTIMATION RESULTS**

A. Main Results

It's probable that a small decrease in employment accompanies paid sick days mandates and has a magnitude of less than ½ percent. This comes from the most precise of the estimates on table 4 that pass the falsification test, i.e., column 4. This specification is model (1), with a restriction on the lagged effects of the treatment. Using this estimate, it's possible to sign the effect with only a little over 90% confidence, though. The other estimates are same-signed and similar in magnitude but with less precision.

{{Place Table 4 about here}}

Lagged employment seems to be the decisive covariate in passing the falsification test, based on the excludability of 8 leads of the treatment indicator. Adding controls for population growth and other labor market policies neither improves nor harms (columns 2 and 4) the performance on the test compared to conditioning only on lagged employment (and instrumenting using the 2nd and 3rd lags). Nor does relaxing restrictions on the time trends (column 6) have much effect on the falsification test.

Comparing this to the Petro (2010) finding, his results indicate that San Francisco County's employment increased 4.7% during the same period and that employment in the five counties surrounding San Francisco decreased 2.5%. Petro's finding is that employment growth in San Francisco was 7.2 percentage points higher than neighboring counties, but the failure to account for both population growth and time trends limits what can be causally attributable to the sick days policy. When I estimate (1) using only California counties, I estimate the causal effect of San Francisco's paid sick days mandate is a ¾ point decrease. The estimate is noisy, but the upper bound of the confidence interval is a 1 point increase, whereas the lower bound is a 2.5% decrease. The causal effect of a paid sick days mandate on county employment is much smaller and possibly opposite in sign from the earlier estimate.

My point estimates of the effect on county average wage are consistent with employees paying for mandated paid sick days with lower wages. The specifications that pass the falsification test indicate a decrease in wages between two-thirds of a point and a full point (columns 1, 2, and 6). They are, however, even less precise than the employment effects and impossible to sign with high confidence.

{{Place Table 5 about here}}

The negative effect on county wages could be as large as 2 percentage points, and my data could be failing to measure it accurately enough. The null hypothesized zero effect is well within the confidence interval, though, and I conclude that the effect on wages is practically small regardless of its sign. This challenges the prediction that employees pay for mandated benefits that are valuable. Specifically it suggests the mandates have a positive productivity effect that offsets part of, maybe even most of, the cost of complying with them.

{{Place figure 2 about here}}

Combining the two results, a small negative effect on employment and a small (indeterminate sign) effect on wages, the mandated benefits theory holds but with something pushing the wage back up the supply curve. Given the nature of the law, I would label this a health externality. Focusing on industries within mandate counties clarifies the basis for this conclusion, and I now present the different effects across the 18 two digit industries that differ with respect to pre-mandate sick days provision.

B. Industry Analysis

In model (2), I distinguish among industries according to the fraction of their workers that receives paid sick days voluntarily and interact this (VOL_j) variable with the treatment indicator.[2] The employment results on table 6 and wage results on table 7 reveal more labor market activity within counties than is apparent from the preceding tables. I reach this conclusion from estimating the coefficients $\beta_1$and β_2 in the industry equation with β_1 representing the effect on an (hypothetical) industry with no pre-mandate paid sick days, and $\beta_1 + \beta_2$, representing the effect on an industry with universal pre-mandate paid sick days. Since

[2] The full interaction is not identified since VOL_j is temporally invariant in my data. Only cross-sectional variation in the 2010 NCS is used.

no industry has such extreme values for VOL_j, I report the effects for the minimum and maximum industries in the data. The samples, covariates, adjustments to standard errors (clustered on industry-county), and dependent variables (logs of employment and wages, now at the 2 digit industry level) are the same as previously.

{{Place Table 6 about here}}

The employment effect is the most interesting finding. Though the county average effect is small and negative, it is not uniform across industries. Particularly the mandate shifts employment from the minimally constrained industries to the more constrained (think “fast food”) industries. For these opposing effects, I reject the null in almost every specification. Columns 2 and 5, the specifications that pass the falsification test, indicate a 0.5% to 2% increase in the most constrained industries and a comparable magnitude decrease in employment in the least constrained industries. The other estimates confirm the signs of the employment changes but exaggerate their sizes (columns 1, 3, 4, 7 and 8). Once again controlling and instrumenting for lagged employment is important to avoid overstating the effects of the mandates.

Turning to the effect on wages, again there is evidence that the effect differs according to the prevalence of pre-mandate sick days in the industry. This time the effect favors the least constrained, as expected since they bear less costs of compliance. While I can say with high confidence, based on columns 1, 3, and 4, that there is a difference between the minimally and maximally constrained industries, putting them on opposite sides of zero is just out of reach. All of the confidence intervals include zero. But for the Food and Accommodation Services industry, zero is very close to the upper bound in most specifications, for the Utilities industry zero is very close to the lower bound.

{{Place Table 7 about here}}

The averaging of these effects over the county accounts for the small county wage effects on table 5, but the average again masks within county wage changes. I conclude that the most likely outcome is a less than 2% wage decrease in Food and Accommodation's wages and a less than 2% wage increase in Utilities (and other similar) industry wages. Putting together the employment and wage results, paid sick days mandates decrease employment and increase wages in industries that already have a lot of paid sick days, and they increase employment and decrease wages in industries that do not have a lot of paid sick days.

C. Robustness Checks

The results are not qualitatively sensitive to the specificity of time trends or the inclusion of time varying controls, including other policy variables like the minimum wage. The employment results rely on controlling and instrumenting for lagged employment, but the wage results do not. Allowing the treatment effect to vary with time elapsed since the mandate's enactment ("duration dependent" specifications) neither improves nor contradicts the results. As a way of envisioning how the most and least constrained industries diverge, in terms of employment, after the mandate is enacted, observe figures 3a and 3b. These are not based on the most precise specifications, so the point estimates and confidence intervals are not emphasized. But they convey the shape of the policy's dynamic impact on employment.

{{Place Figures 3a and 3b about here}}

In addition to testing restrictions on the time trends, treatment lags, and covariates, I have varied the number of lags and leads, the timing of treatment, and the sample used in the estimation. Qualitatively similar results hold regardless of whether treatment originates on the date the mandate was passed into law, on the date it becomes binding (as in my tables), or

includes both. Since most of the mandate counties are metropolitan, it may be desirable to control for county and industry size in a more direct way, i.e., by excluding smaller counties or industries. The results are not sensitive to using a smaller sample of counties (industries) with higher minimum population (employment).

I end this section with a clarification: the units of measure for labor should technically be hours, not employment level. There is no measure in the QCEW of hours worked. Since there is a maximum number of sick hours employees can earn, employers may have an incentive to schedule more hours per employee, crossing the threshold where they stop earning paid leave. If firms do this with their mandated employees, the effect on hours would be larger than the measured effect, which implicitly holds hours per worker constant. Consequently the possibility of changing hours per worker does not contradict the conclusion.

5. DISCUSSION AND CONCLUSIONS

The evidence in this paper is consistent with the employment increase scenario in Summers (1989). Thus employees have revealed a higher value for paid sick days than it costs for employers to provide them. I hypothesize that the justification for the mandates is that the costs of complying with them were offset by other firms' positive spillover effects on employee health and productivity.

Though the county level estimates only crudely capture it, the effect of paid sick days mandates is to shift employment from the least constrained industries to the most constrained, having a practically small effect on overall county employment. Additionally the mandates increase the wages of employees in the least constrained industries and employees in the most constrained industries pay, at least partly, for them with lower wages.

Focusing just on the constrained industry, the wage decrease is expected but the employment increase is not, unless employees value the benefit beyond its cost of provision. If that's the case, though, a mandate should not have been necessary. Furthermore the more employees value a benefit, the more they pay for it if it's mandated. If the cost of complying is 3.3% of wages (many mandates stipulate 1 paid hour for every 30 worked), employees would have to pay at least this much to offset the demand shift and for an employment increase to be observed. The observed wage decrease is not that large.

But the employment increase coupled with the modest wage decrease can easily be explained if there are productivity enhancing externalities that come from mandated paid sick days. These would offset the costs of compliance, shifting demand upward, and moving up the labor supply curve. A health externality would also account for the higher wages in the least constrained industries, as they gain productivity from the improved health of, say, the people serving them lunch.

To sum up, offering paid sick days makes the erstwhile non-providing industries relatively more attractive and increases labor supply to them at the expense of the already providing industries. Providing paid sick days is costly to the constrained industries. But part of the cost is offset by a positive health externality in the "fast food" industries, and it shows up as a pure productivity gain to other industries that already had sick days. It may sound odd, but the mandates' effects on constrained industries is like an employment subsidy: firms can attract more employees by offering something valuable that is mostly paid for by someone else (other firms, in the form of a positive health externality). This explains why a mandate would be necessary; firms in these industries ignore their external health effects when (not) offering paid sick days.

Mandating sick days was not necessary in every industry, though. Aggregate statistics (BLS 2010) reveal why not. The industries with conspicuously low incidence of paid sick days have traits that predict low incidence in the aggregate: part time employees and low average wages.[3] The same pattern is observable with numerous fringe benefits; they're luxury goods. Low wage employees value them, but they value wages more when asked to choose.

Forcing them to make a different choice sounds malicious, unless of course the mandated luxury is offered at a de facto discounted price that they would willingly pay. Though I question the wisdom of trying to attract more people to these jobs, the productivity gains to employees in the other industries are obviously a favorable outcome. It would be better, though, if the mandate attracted people from outside the labor force, rather than from other jobs. Still, if I could choose a luxury non-wage employment amenity to mandate, it would be tough to think of a better alternative to paid sick days.

[3] 26% of part time employees have paid sick days, compared to 74% of full time employees. 32% of employees in the lowest quartile of the distribution have paid sick days, compared to 66%, 75%, and 84% in (and strictly increasing across) the three upper quartiles.

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Table 1: Paid sick leave mandates provisions.

	Typical Requirement	Notable Exeptions
Sick Leave Accrues at	1 hr. per 30 worked	Small Employers (< 50 employees) exempt (CT) or (< 10 employees) must provide upaid leave (Portland, New York, Jersey City)
Maximum Leave Can Accrue	40 hours	Higher caps for "large" employers, e.g., 72 hours

Summary of Paid Sick Days Statutes from National Partnership for Women and Families: http://www.nationalpartnership.org/research-library/work-family/psd/paid-sick-days-statutes.pdf. San Francisco (>9), Washington, D.C. (>99), and Seattle (>49) are examples of laws with higher caps for larger employers.

Table 2: Industry Characteristics.

Industry	**Proportion Voluntary Sick Days**	**Average Employees per Firm**	**Average Weekly Wage (Relative to Population)**
Mining	0.68	5.71	129.4%
Utilities	0.93	51.66	170.9%
Construction	0.36	11.30	99.9%
Manufacturing	0.61	22.89	117.4%
Wholesale Trade	0.78	9.44	114.5%
Retail Trade	0.51	13.60	63.6%
Transportation and Warehousing	0.68	13.15	94.9%
Information	0.87	27.60	108.4%
Finance and Insurance	0.92	20.91	118.1%
Real Estate and Rental and Leasing	0.81	8.95	72.5%
Professional, Scientific & Technical Services	0.83	12.30	118.1%
Management of Companies and Enterprises	0.92	57.55	146.0%
Administrative and Support and Waste Management and Remediation Services	0.40	26.03	62.5%
Educational Services	0.75	54.63	66.9%
Health Care and Social Assistance	0.79	24.12	93.6%
Arts, Entertainment, and Recreation	0.39	22.81	42.6%
Accommodation and Food Services	0.27	21.65	33.3%
Other Services (except Public Administration)	0.53	4.98	60.8%
All Industries Pooled	**0.62**	**13.57**	**100.0%**

Statistics on voluntary leave come from the 2010 March NCS (Bureau of Labor Statistics). These are interacted with the treatment variables to perform the "industry analysis" in the Methods section (part B). They are national averages that are not necessarily precise for each county in the sample. Average employment per firm is constructed as follows: private employment in industry (j), county (i), divided by number of "establishments" in industry, place (from QCEW). Average weekly wage is expressed as a fraction of the county average weekly wage (from 2003 QCEW data). The latter averages are for the pooled sample over time.

Table 3: Means and conditional means of key variables.

	Non-Treatment Counties			Treatment Counties		
	10th %ile	Mean	90th %ile	10th %ile	Mean	90th %ile
Private Employment (Level)	11,668	78,922	173,270	32,259	377,371	937,876
Share Food & Accommodation	7.43%	11.18%	15.28%	6.20%	8.81%	11.99%
Population	37,689	214,760	486,640	153,059	908,187	2,244,238
Average Weekly Real Wage (Level)	$442.02	$559.93	$711.36	$580.48	$844.65	$1,246.05
Food & Accommodation (Ratio to Cty. Avg.)	23.71%	30.38%	37.54%	24.13%	30.40%	36.53%
Federal Gov't Employment (Ratio to Private)	0.58%	2.40%	5.14%	0.72%	4.25%	3.67%
State Gov't Employment (Ratio to Private)	0.25%	4.46%	9.75%	1.06%	4.99%	8.78%
Local Gov't Employment (Ratio to Private)	6.96%	13.98%	22.31%	<0.01%	9.73%	19.60%
Births per 1000 Population	8.70	12.26	16.14	7.84	11.66	15.39
International 'In' Migration per 1000	0.15	1.47	3.65	1.18	4.71	9.17
Domestic 'In' Migration per 1000	-7.54	2.23	14.70	-18.62	-5.46	5.03
Minimum Wage, 2014 ($/hr.)	$7.25	$7.62	$8.25	$8.00	$8.67	$9.50

The sample consists of all U.S. Counties with a time series average employment of at least 10,000 per month. There are 1213 counties; 3 (Adams and Tazewell counties in Illinois and Bristol county in Rhode Island) have incomplete time series in the QCEW data for reasons of confidentiality-based censoring. These counties are also excluded from the sample, leaving 1210 counties with (12 squared=) 144 observations each. The above statistics are based on a pooling of these counties’ time series and averaging over the total number (1210*144=174,240) of observations.

Table 4: Effect of sick days mandate on county employment.

	Effect on Log Private Sector Employment							
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Mandate Implemented (β)	**-0.0071**	**-0.0152**	**-0.0015**	**-0.0025***	**-0.0019**	**-0.0014**	**-0.0028**	**-0.0048**
	(.0106)	(.0098)	(.0013)	(.0013)	(.0015)	(.0018)	(.0045)	(.013)
Implied % Change Employment	-	-	-	**-0.24%**	-	-	-	-
Time*100	0.0666***	0.0221***	-0.0019***	-0.0042***	-0.1683***			
	(.0036)	(.0055)	(.0002)	(.0004)	(.0086)			
{Treatment=1}*Time*100	0.0977***	0.0705***	0.0025**	0.0022*	-0.1626***			
	(.0187)	(.021)	(.0013)	(.0013)	(.0087)			
Log of Employment (t-1)			0.9234***	0.9013***	0.9003***		0.4682***	0.4782***
			(.0008)	(.0009)	(.0009)		(.0073)	(.0075)
p value: exclusion of treatment leads	<0.001	<0.001	0.989	0.984	0.980	<0.001	0.952	0.969
Sample	All Counties ≥ 10,000 Employed							
Observations	174,240	174,240	170,610	170,610	165,770	173,030	170,610	164,560
Panels	1210	1210	1210	1210	1210	1210	1210	1210
Month Fixed Effects	Yes							
Controls	None	All; No AR	AR	All	All	All; No AR	All	All
Time Trends	Treatment & Control	Treatment & Control	Treatment & Control	Treatment & Control	Treatment & Control	County Specific	County Specific	County Specific
Duration Dependent Effect	No	No	No	No	Yes	No	No	Yes

The dependent variable in all specifications is county private sector log of employment. Counties are included in the sample only if they have a full time series of 144 observations each and county employment (averaged over time series) of at least 10,000. The coefficient of interest is "Mandate Implemented"—which corresponds to the treatment indicator for a sick leave mandate. Coefficients are estimated by FE or IVFE (columns 3-5, 7-8) regression. All parentheses contain county cluster robust standard errors. "All" control variables refers to the minimum wage, San Francisco's mandated health insurance contribution per the Health Care Security Ordinance, the log of population, components of population growth rates, and logs of government employment. In the specifications that use duration dependent treatment effects, the linear combination (sum) of the treatment and its lags' coefficients is reported. In specifications with the lagged employment as a regressor, the second and third lags are used as its instruments.

* Significant at α=0.1, ** Significant at α=0.05, *** Significant at α=0.01.

Table 5: Effect of sick days mandate on county average weekly wage.

	Effect on Log Private Sector Average Wage							
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Mandate Implemented (β)	**-0.0068**	**-0.0094**	**-0.0046**	**-0.0042**	**0.0016**	**-0.0094**	**-0.0156**	**-0.0328**
	(.0092)	(.0092)	(.0058)	(.0059)	(.0065)	(.0372)	(.0135)	(.0235)
Implied % Change Wages	-	-	-	-	-	-	-	-
Time*1000	0.0886***	0.0851***	-1.774***	-1.765***	-1.765***			
	(.0022)	(.0038)	(.0332)	(.0333)	(.0333)			
{Treatment=1}*Time*1000	.0677***	0.0610***	-1.778***	-1.770***	-1.771***			
	(.0125)	(.0126)	(.0336)	(.0337)	(.0337)			
Log of Wage (t-1)			0.8892***	0.8885***	0.8887***		0.2120***	0.2482***
			(.0136)	(.0129)	(.0129)		(.0434)	(.0525)
p value: exclusion of treatment leads	0.335	0.411	0.002	0.002	0.003	0.164	0.009	0.005
Sample	All Counties ≥ 10,000 Employed							
Observations	58,080	58,080	54,450	54,450	54,450	56,870	54,450	54,450
Panels	1210	1210	1210	1210	1210	1210	1210	1210
Quarter Fixed Effects	Yes							
Controls	None	All; No AR	AR	All	All	All; No AR	All	All
Time Trends	Treatment & Control	Treatment & Control	Treatment & Control	Treatment & Control	Treatment & Control	County Specific	County Specific	County Specific
Duration Dependent Effect	No	No	No	No	Yes	No	No	Yes

The dependent variable in these regressions is log of average weekly wage, and the sample is the same as the employment regressions on table 4. The sample size is precisely 1/3 as large, though, since average wages are only observed for quarters, not individual months. All standard errors (in parentheses) are clustered at the county level. "All" control variables refers to the minimum wage, San Francisco's mandated health insurance contribution per the Health Care Security Ordinance, the log of population, components of population growth rates, and logs of government employment. In the specifications that use duration dependent treatment effects, the linear combination (sum) of the treatment and its lags' coefficients is reported. In specifications with the lagged wage as a regressor, the second and third lags are used as its instruments.
* Significant at α=0.1, ** Significant at α=0.05, *** Significant at α=0.01.

Table 6: Industry analysis of effects on employment.

		Effect on Log Industry Employment							
		1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Mandate Implemented (β1)		0.1515***	0.0229***	0.1467***	0.1388***	0.0216***	-0.0172*	0.0491***	0.1099**
		(.0274)	(.0043)	(.0274)	(.0272)	(.0044)	(.0093)	(.0155)	(.0511)
Mandate*Unaffected (β2)		-0.2239***	-0.0368***	-0.2221***	-0.2196***	-0.0370***	0.0228*	-0.0450***	-0.1754**
		(.0438)	(.006)	(.0437)	(.0432)	(.006)	(.0136)	(.009)	(.0733)
Implied % Change (Constrained Industry)	Lower	**6.10%**	**0.73%**	**5.62%**	**4.88%**	**0.59%**	**-2.24%**	**1.89%**	**-0.19%**
	Upper	**13.06%**	**1.89%**	**12.60%**	**11.79%**	**1.75%**	**0.10%**	**8.26%**	**13.54%**
Implied % Change (Minimally Constrained Industry)	Lower	**-8.42%**	**-1.57%**	**-8.67%**	**-9.15%**	**-1.72%**	**-0.51%**	**-6.07%**	**-9.70%**
	Upper	**-2.53%**	**-0.68%**	**-2.86%**	**-3.42%**	**-0.83%**	**1.32%**	**-2.70%**	**-0.44%**
p value: exclusion of treatment leads		<0.001	0.163	<0.001	<0.001	0.158	<0.001	<0.001	0.604
Sample		Complete Time Series; ≥ 50 Employed							
Observations		2,450,736	2,399,679	2,450,736	2,450,736	2,399,679	2,433,717	2,314,584	2,314,584
Panels		17,019							
Month Fixed Effets		Yes							
Controls		None	AR	Policy	All; No AR	All	All; No AR	All; No AR	All
Time Trends		Industry, Treatment & Control	Industry, Treatment & Control	Industry, Treatment & Control	Industry, Treatment & Control	Industry, Treatment & Control	Industry, County	Industry, County	Industry, County
Duration Dependent Effect		No	No	No	No	No	No	Yes	Yes

Estimates of model (2). The sample is 2 digit industries within the (1210) counties analyzed on the previous tables. I exclude agriculture and the residual industry (99, "unclassified") category, and I restrict the sample to industry-counties with at least 50 employees and a complete time series. Of the (18*1210=21,780) possible pairs, 17,019 are included in the analysis. As a robustness check, the industry-counties with mostly complete (72<n<144) time series are included in the sample. To balance the panel and avoid the possibility of cyclical patterns biasing the sample selection, the censored months are imputed as a time invariant share of the county employment in that month. 95% confidence intervals are reported for the effects on the minimally (Utilities, 93% have paid sick days) and maximally (Accommodation & Food Service, 27% have paid sick days) constrained industries, in terms of voluntary transaction of sick days. The standard errors in this table are clustered around the industry-county unit of observation.

* Significant at α=0.1,

** Significant at α=0.05,

*** Significant at α=0.01.

Table 7: Industry analysis of effects on wage growth.

		Effect on Industry Log Weekly Wage					
		1	**2**	**3**	**4**	**5**	**6**
Mandate Implemented (β1)		-0.0204	0.0058	-0.0274	-0.0258	-0.0229	-0.0191
		(.0175)	(.0092)	(.0167)	(.0164)	(.0184)	(.0275)
Mandate*Unaffected (β2)		0.0411*	-0.0070	0.0437*	0.0436*	0.0423	0.0277
		(.0248)	(.0127)	(.0237)	(.0233)	(.0258)	(.0484)
Implied % Change (Constrained Industry)	Lower	**-3.08%**	**-0.81%**	**-3.59%**	**-3.41%**	**-3.45%**	**-4.09%**
	Upper	**1.29%**	**1.61%**	**0.54%**	**0.65%**	**1.21%**	**1.86%**
Implied % Change (Minimally Constrained Industry)	Lower	**0.25%**	**-1.01%**	**-0.19%**	**-0.05%**	**-0.06%**	**-3.20%**
	Upper	**3.38%**	**0.89%**	**2.88%**	**3.02%**	**3.39%**	**4.69%**
p value: exclusion of treatment leads		0.199	<0.001	0.266	0.249	0.295	0.013
Sample		Complete Time Series; ≥ 50 Employed					
Observations		816,912	765,855	816,912	816,912	782,874	799,893
Panels		17,019					
Quarter Fixed Effets		Yes					
Controls		None	AR	Policy	All; No AR	All; No AR	All; No AR
Time Trends		Industry, Treatment & Control	Industry, Treatment & Control	Industry, Treatment & Control	Industry, Treatment & Control	Industry, Treatment & Control	Industry, County
Duration Dependent Effect		No	No	No	No	Yes	No

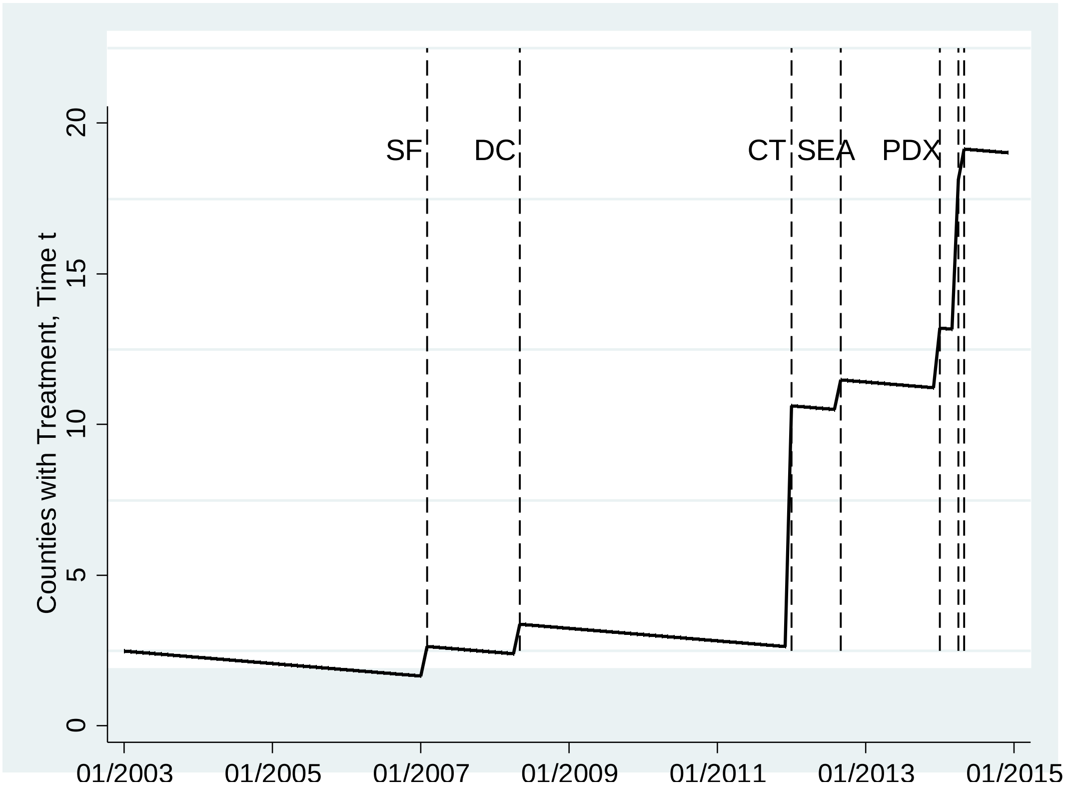
Estimates of model (2). The sample is 2 digit industries within the (1210) counties analyzed on the previous tables. I exclude the residual industry (99, "unclassified") category, and I restrict the sample to industry-counties with a time series mean of at least 50 employees and a complete time series. 95% confidence intervals are reported for the effects on the minimally (Utilities, 93% have paid sick days) and maximally (Accommodation & Food Service, 29% have paid sick days) constrained industries, in terms of voluntary transaction of sick days. Estimates for both industries' effects get noisier when I relax restrictions on the lags of the treatment indicator (column 5) and on time trends (column 6), but there is a statistically significant negative effect on the constrained industry in the most precise specifications. The upper bound of the confidence interval is in every specification close to zero. Mandated benefits theory predicts this result, as workers in constrained industries pay for mandated sick days. It is gratifying that the specifications showing this are also the ones with favorable falsification test results (3-5). In the minimally constrained industry, two specifications reject the null in favor of a positive effect, which if real could be explained by movement up the labor demand curve as employment shifts toward more constrained industries (taken together with results in table 6). In the specifications that use duration dependent treatment effects, the linear combination (sum) of the treatment and its lags' coefficients is reported. The standard errors in this table are clustered around the industry-county unit of observation.

* Significant at α=0.1

** Significant at α=0.05

*** Significant at α=0.01

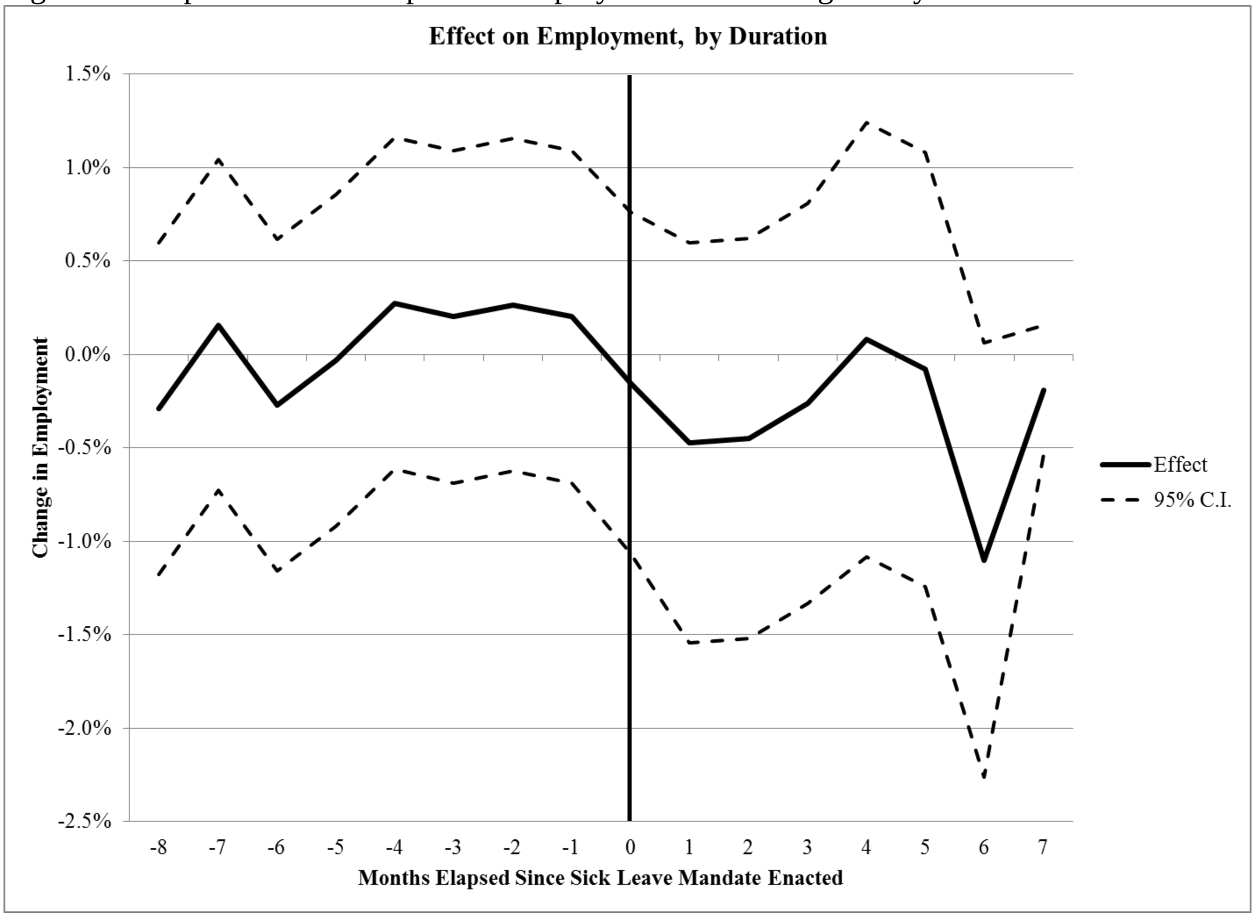
Figure 1: Timeline of mandates.



The timing of when the sick leave mandates went into effect. The two most recent on the right of the graph are New York City and Newark (Essex County), NJ.

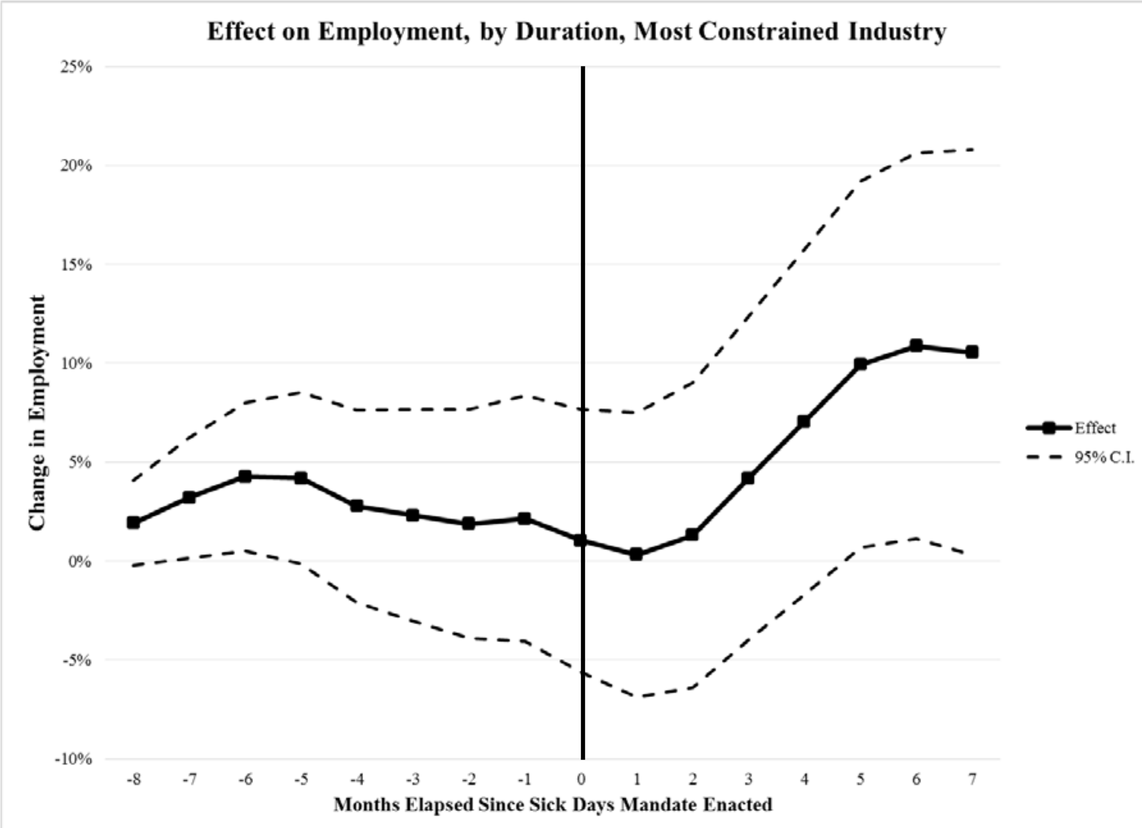
Appendix: Duration Dependence

Figure 2: Graph of duration-dependent employment effect using county estimates.



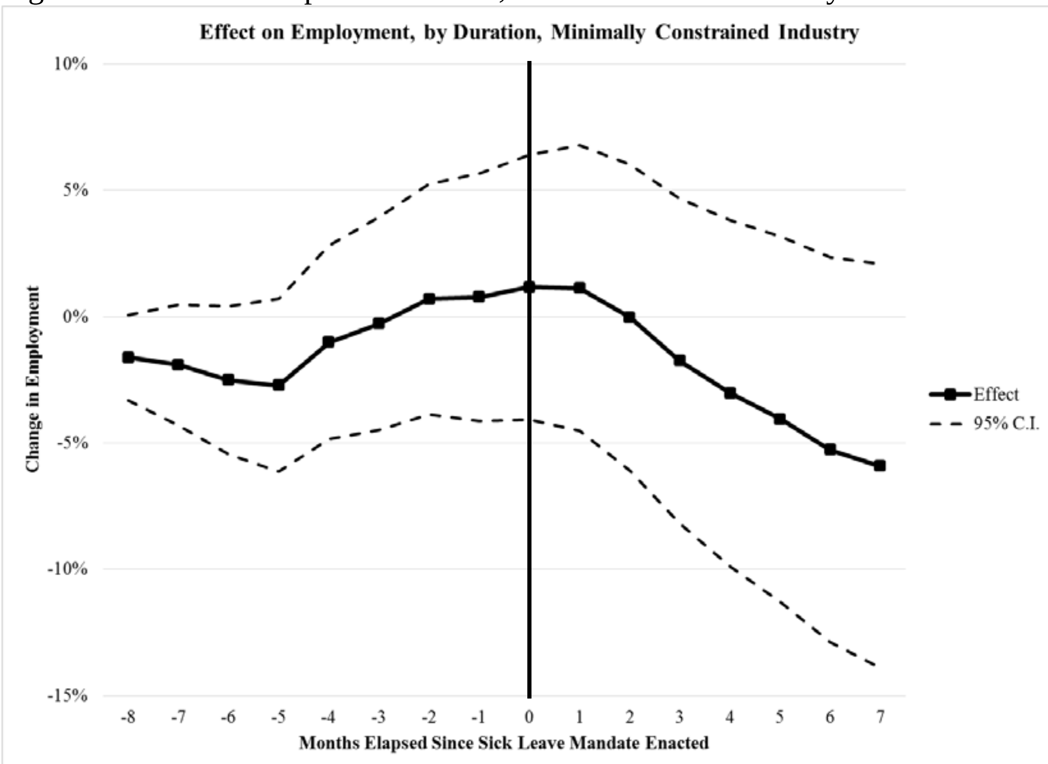
This graph uses the estimates from column 7 on table 4 to plot the cumulative effect of a sick days mandate on employment "s" months after it is enacted. Each plot is the point estimate of the linear combination of several coefficients on leads and lags of the treatment indicator. The lags go up to 7 months, since this is the minimum number of months of treatment observed in the sample. Columns 4 and 8 (using "all" controls), also estimating duration-dependent effects, reveal a similar pattern, i.e., a small negative effect after the mandate is enacted.

Figure 3a: Duration-dependent effects, most constrained industry.



This figure (a is "fast food" and b is "utilities") shows estimates from column 8 on table 6. It plots a running sum of the treatment and interaction (with VOL_j) coefficients, beginning from the most advanced lead up to and including period "s" which is noted on the horizontal axis. The emphasis here is not on the magnitude of the effect or the precision. Together with figure 3b, my intention is to show how employment in these two industries diverges after a sick days mandate is enacted in their county. Period zero is the month the mandate is enacted. The effect of the mandate is to increase employment in the most constrained industries and decrease it in the least constrained.

Figure 3b: Duration-dependent effects, least constrained industry.



This figure (a is "fast food" and b is "utilities") shows estimates from column 8 on table 6. It plots a running sum of the treatment and interaction (with VOL_j) coefficients, beginning from the most advanced lead up to and including period "s" which is noted on the horizontal axis. The emphasis here is not on the magnitude of the effect or the precision. Together with figure 3a, my intention is to show how employment in these two industries diverges after a sick days mandate is enacted in their county. Period zero is the month the mandate is enacted. The effect of the mandate is to increase employment in the most constrained industries and decrease it in the least constrained.