Do Paid Sick Leave Mandates Increase Productivity?

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Abstract

This paper exploits the staggered implementation of paid sick leave (PSL) mandates to assess their real effects on U.S. corporations. We find that mandatory access to sick pay leads to higher labor productivity and firm profitability. These performance improvements concentrate in industries that require more physical presence in the workplace, which suggests that PSL generates a positive health externality. The effects are also more pronounced for firms with more expensive labor force, indicating that the productivity benefits of sick pay are greater for employees with high human capital, and in counties with higher social capital, where the risk of absenteeism may be less severe.

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1 Introduction

Employers need their staff to stay healthy so that they keep being productive. At the same time, many employers foster a corporate culture that prizes *presenteeism* and pressures employees to be in the workplace even when they are sick. Sick employees often face a difficult trade-off: if they stay home, they chance negative consequences for their career; if they keep going to work, they might slow down their own recovery and spread the infection in the workplace to co-workers. This trade–off is directly affected by the firm's policies on sick leaves. Specifically, without paid sick leaves (PSL), staying home has an extra cost, namely, the loss of days of earnings; and thus the employee's decision is likely to be biased against taking days off work.

While the absence of a PSL provision is clearly making the employee worse off, predicting how PSL might influence employers is not as simple. On the one hand, paying staff when they are off work is an additional cost for firms; and providing PSL may induce opportunistic use of sick leaves. On the other hand, offering PSL might be part of the efficient compensation contract to incentivize employees; and allowing sick workers to take paid leave can speed up their recovery and can also prevent the spread of the disease in the workplace, creating a *positive health externality*.

This paper investigates the impact of PSL on firm performance within a difference-in-differences research design which takes advantage of the staggered implementation of state-, county- and city-level PSL mandates in the United States. Using the CRSP/Compustat Merged (CCM) data over the 2004–2019 period, we compare changes in performance of firms headquartered in areas covered by the PSL mandates (the treated firms) to a set of control firms headquartered in areas without such mandates. We support our main analysis with establishment-level data over the same 2004–2019 period from Data Axle (formerly Infogroup).

In the CCM data, we find that, after the implementation of the PSL mandates, labor productivity (defined as the logarithm of sales per employee) increases by around 6 percentage points (pp) and ROA (defined as EBITDA over assets) increases by around 1.6 pp, compared with firms in areas not covered by the mandates. Results hold across different measures of productivity and profitability, including sales over assets, Total Factor Productivity (TFP), gross profit over assets, EBIT over assets, and net income over assets. Using the sample from Data Axle (formerly Infogroup), we show that PSL increases labor productivity even at the establishment level.

With the help of a model, we identify three main channels through which the implementation of the PSL mandates might affect firm performance. First, at the core of our story is the *health externality* channel: the expanded PSL provision enhances firm performance by improving health in the community and thus reducing the probability that employees become sick. In other words, by countering presenteeism, PSL mandates help contain the spread of contagious diseases: direct empirical evidence of this effect is provided by Pichler, Wen, and Ziebarth (2021), Pichler and Ziebarth (2017), and Pichler and Ziebarth (2020). Consistent with this hypothesis, we find that the performance improvements concentrate in industries that are likely to be more exposed to the health externality channel: industries with a lower share of jobs that can be done remotely and industries that were most affected by workplace breakouts of Covid–19.

Second, we explore the role of *human capital* in affecting the effects of PSL mandates. Typically, employees with greater human capital are associated with higher wages and have a greater impact on firm outcomes (Acemoglu and Autor, 2011). In the context of mandatory PSL provision, as the reduction in the probability of being sick translates into higher worker productivity, the impact is greater for employees whose effort is more valuable. As predicted by the model, we find that the effects on performance are more pronounced for firms operating in industries with more expensive labor force and for firms with higher capital per employee.

Third, PSL mandates may themselves generate a *moral hazard* problem and thus hurt firm performance. Johansson and Palme (2005) and Ziebarth and Karlsson (2010) find that, while more generous sick pay reduces presenteeism, it also increases absenteeism because workers with minor diseases, or no disease at all, may take advantage of the generous PSL policy. To evaluate empirically if absenteeism attenuates the positive effects of PSL on firm performance, we turn to the concept of social capital. Literature in sociology and economics suggests that the strength of cooperative norms and the density of social networks in geographical areas discourage opportunistic behavior (e.g., Coleman, 1988; Knack and Keefer, 1997; Guiso, Sapienza, and Zingales, 2011). We use social capital as a measure for the potential severity of the moral hazard problem. We find that the positive effects of implementing PSL mandates are more pronounced for firms headquartered in counties with higher social capital. We interpret this result as evidence that the benefits of PSL are greater when the moral hazard problem associated with the self-reporting of sick leaves by employees is less severe.

We probe our analysis with a battery of robustness tests. The findings are robust to the use of the stacked sample approach proposed by Cengiz et al. (2019), which can circumvent the potential bias (due to treatment effect heterogeneity) associated with the two-way fixed effects (TWFE) estimator in staggered difference-in-differences (DiD) designs. We further verify that the treatment effect estimates are not plagued by the weighting issues of the TWFE estimator in our staggered DiD setting: we employ two alternative estimators proposed by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). Additionally, our results strengthen if we exclude firms headquartered in cities and counties that have their own PSL mandates (where policy spillover may potentially attenuate our results); when we exclude firms that operate in the IT industry (where there should be little health externality since most jobs can be done remotely); and when we exclude firms operating in industries that tend to have geographically dispersed operations (and thus could be less affected by regulatory changes in the area where their headquartered is located).

We find that the positive effects of the PSL mandates are similar across industries that differ in terms of voluntary provisions of PSL. This result is consistent with our model and indicates that voluntary and mandatory PSL are not perfect substitutes: the externality benefits of PSL mandates are felt similarly by firms that have voluntary PSL provisions and those that do not. We also rule out the alternative possibility that the PSL mandates causes firms to alter the relative use of labor versus capital. This might be the efficient reaction of firms if PSL is merely an increase in labor cost without any benefits. We find that PSL mandates have no impact on the capital-to-labor ratio used by firms. We also do not find support for the view that PSL changes labor market conditions by making it easier for firms to attract and retain skilled employees. Although the data is at the state-industry level (and not at the firm level), we find that PSL mandates have no effect on the hiring and firing flows. This finding confirms our hypothesis that PSL increases labor productivity independently from changes in the labor force. We also explore the political economy of the PSL mandates, finding no evidence that PSL mandates are correlated with demographic and macroeconomic variables.

Overall, our paper supports the conclusion that PSL mandates represent a Pareto improvement (by benefiting both employees and employers). This finding contributes to the recent debate on the effectiveness and efficacy of PSL during the Covid-19 crisis; and adds to the long-standing debate over the pros and cons of universal access to PSL in the United States.

This paper contributes to two strands of literature. First, we build on the work on the labor market implications of mandated health benefits. While both Pichler and Ziebarth (2020) and Wething (2022) do not find much evidence of significant impacts on employment and wages, our paper documents a sizable increase in labor productivity and firm profitability after the implementation of the PSL mandates. Pichler, Wen, and Ziebarth (2021) present evidence that mandating PSL at the state level leads to positive welfare-relevant externalities: the influenza-like illness (ILI) transmission rates are reduced by 11% or 290 ILI cases per 100,000 patients per week in the first year, with the reduction in ILI activity increasing cumulatively over the three years following the implementation of PSL mandates.[1] In

[1]Other studies reach similar conclusions, finding that PSL mandates improve public health at the city level (Pichler and Ziebarth, 2017) as well as during the recent COVID-19 pandemic (Pichler and Ziebarth,

this regard, our paper confirms the positive health effect by further sharpening this insight through the *positive health externality* of PSL mandates, which limit the spread of contagious diseases and preserve aggregate productivity in the workplace. Almeida et al. (2023) find that the Patient Protection and Affordable Care Act (PPACA) significantly increases health insurance premiums for employees in company-sponsored plans but has no effect on firm performance. Firms respond by shifting their workforce composition from full-time employees to other types of workers not subject to the PPACA mandates. In contrast, our study shows that PSL mandates, while also designed to improve employee health, can enhance firm performance without distorting employment policies. In addition, there is a small literature on the corporate finance implications of employee health, typically focusing on the CEOs. Bennedsen, Perez-Gonzalez, and Wolfenzon (2020) document the negative effect of worsening CEO health on firm performance by using managers' hospitalization records. Keloharju, Knüpfer, and Tåg (2020) find that CEO health plays a vital role in CEO appointment and retention decisions. More broadly looking at the impact of corporate activity on employees, Garcia-Gomez et al. (2020) show that buyout-related restructuring has a strong negative impact on the careers and human capital of employees with health problems. Their work reveals the dark side of buyouts for unhealthy employees. In contrast, we emphasize the bright side of promoting employee health and show that good employee health can lead to more productive workers.

Second, our paper also contributes to a rapidly growing literature on the financial implications of labor market frictions for corporations.[2] The most closely related work to ours is Bennett et al. (2022). The authors study the adoption of Paid Family Leave (PFL) laws in the U.S. from 2002 to 2018. The PFL laws require firms to offer their employees long-term paid leave for a family or medical event. The authors show that the productivity of firms in states that passed the PFL laws increases by about 4 pp relative to firms in adjacent counties that were not subject to the change. PSL is fundamentally different from PFL.

2020).

[2]For a recent survey, see Pagano (2020).

PSL is usually reserved for short-term health-related needs and preventive care, for example a cold or flu, recovering from outpatient or dental procedures. PFL, on the other hand, relates to a long-term paid absence from work to care for ill family members, as well as when a parent has a new child. Moreover, the moral hazard implications differ significantly, as it is much simpler to feign a minor illness than a pregnancy. Indeed, PFL and PSL have different effects on incentives and absences, hence on firm performance. While Bennett et al. (2022) argue that the improvement of firm performance following the PFL laws is due to improved access to a broader talent pool, we show that our results are mainly driven by the increased employee effort and the positive health externalities caused by PSL mandates. As a robustness check, we explicitly control for the impact of PFL acts and show that our findings remain unchanged. This confirms that PSL has an independent impact on labor productivity from PFL.

The paper proceeds as follows. In Section 2, we present a simple principal-agent model. In Section 3, we describe the data and the empirical strategy. Section 4 contains the empirical evidence on the effects of mandating PSL on firm performance. In Section 5, we explore the underlying economic mechanisms. Section 6 presents the results of the robustness tests. The conclusion is in Section 7.

2 Model

In this section we present a model to study the basic trade-off in the choice of PSL from the perspective of a private corporation. This allows us to develop a set of empirical predictions and help with the interpretation of the findings.

2.1 Setup

Consider a principal-agent setting in which a firm hires one employee who needs to exert effort over a given period of time (say a month) to complete a project. In case of success, the firm produces $Y > 0$; in case of failure, the output is 0. The probability of success is $\int_0^1 e_t dt$, where $e_t \in \{0, 1\}$ for each time $t \in [0, 1]$. The cost of effort is $\gamma > 0$.

The employee is risk neutral and effort is unobservable. The firm uses compensation to incentivize the employee: it pays a wage $w > 0$ to the employee if the project is successful; and 0 otherwise. The firm has limited liability and the employee has no wealth.

The employee is sick at time t with probability $\sigma \in [0, 1]$. If unwell, the employee suffers a disutility $\delta \in [0, \gamma]$ from going to work and their effort is unproductive (i.e., it does not increase the probability of success). Whether they are sick is only known to the employee: the employee may engage in absenteeism, defined as being absent from work for reasons unrelated to their own illness. Specifically, the employee may be subject to an "absenteeism shock" with probability $\mu \in [0, 1]$. If they are subject to this shock, employees receive a private benefit δ if they choose to stay home. Employees who are sick or opportunistic may decide to stay home: let $h_t \in \{0, 1\}$ be the decision to stay home at a given point in time ($h_t = 1$) or to go to work ($h_t = 0$).

Firms prefer employees to be present: a parameter $\alpha \geq 0$ captures the loss of value for the firm if an employee is not present. Specifically, we assume that for a fraction $\hat{p}$ of companies, absenteeism is very costly, i.e., $\alpha = \delta + \epsilon$ (with $\epsilon > 0$), and for the remaining fraction $1 - \hat{p}$, absenteeism has a low cost, i.e., $\alpha = 0$. To reduce absenteeism, firms may choose to deduct $d \in [0, w]$ from the employee's pay each time the employee stays home. The parameter d captures the Paid Sick Leave (PSL) provision: a more generous provision is represented by a lower deduction d. In what follows, we will compare the case in which the firm chooses d (privately optimal) and then the case in which a social planner (i.e., the city, county or state) chooses d (socially optimal).

Finally, we assume that the probability of being sick σ – while a parameter at the level of an individual firm – is a function of the aggregate decisions in the community on what the employees do when they are sick. Specifically, going to work when unwell helps spread the disease and so increases the probability of becoming sick at the community level: $\sigma = s + \beta W$, where s is the average level of health risk, $\beta \in [0, 1-s]$ and W is the aggregate proportion of workers in the community that go to work when sick. This assumption introduces a positive externality (captured by the parameter β) in the choice of PSL.

2.2 Effort decision

The model is solved by backward induction, starting from the employee's decision whether to exert effort at any point in time t. The employee's utility has three components: the employee receives a private benefit δ with probability μ; if the employee is sick and/or opportunistic, they have to decide whether to stay home or not; if the employee is not sick and not opportunistic, their key decision is whether to exert effort or not. The realized utility over the time period is thus:

$$U = \mu\delta - \int_{S\cup O} (1 - h_t)\delta dt + \int_{S'\cap O'} e_t(w - \gamma - dH)dt, \tag{1}$$

where S indicates the set of times t when the employee is sick, O indicates the set of times t when the employee can be opportunistic, S' and O' are the complements of S and O respectively, $H = \int_{S\cup O} h_t dt$ is the total time that the employee stays home. In expectation, the probability of being sick or opportunistic is $\int_{S\cup O} dt = \sigma + \mu - \sigma\mu$ and the probability of not being sick or opportunistic is $\int_{S'\cap O'} dt = 1 - \sigma - \mu + \sigma\mu \equiv \pi$. Notice that π is decreasing in σ and μ.

It is immediate to see that workers exert effort as long as the salary net of the penalties for staying home $(w - dH)$ exceeds the cost of effort γ:

$$e^* \equiv e_t = \begin{cases} 1 & \text{if } w \geq \gamma + dH \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

2.3 Staying home decision

We proceed backwards to the employee's decision whether to stay home. There are two cases to consider: when the incentive compatibility (IC) condition in equation (2) is satisfied: i.e.,

$$w \geq \gamma + dH \tag{3}$$

or when it is not satisfied.

If the IC condition (3) is not met, $h_t = 1$ and the expected utility is $E(U) = \mu\delta$. If the IC condition (3) is met, the utility (1) becomes:

$$U = \mu\delta + \pi \left(w - \gamma - \int_{S \cup O} h_t d dt \right) - \int_{S \cup O} (1 - h_t)\delta dt. \tag{4}$$

So,

$$h^* \equiv h_t = \begin{cases} 1 & \text{if } d \leq \delta/\pi \\ 0 & \text{otherwise} \end{cases} \tag{5}$$

and

$$H^* \equiv H = \begin{cases} \sigma + \mu - \sigma\mu = 1 - \pi & \text{if } d \leq \delta/\pi \\ 0 & \text{otherwise} \end{cases} \tag{6}$$

Intuitively, the worker chooses to stay home at a point in time t is the monetary penalty d is smaller than the disutility from going to work δ scaled by the probability of not being sick or opportunistic.

2.4 Efficiency wage

Summarizing the results so far, the worker's expected utility takes three possible values:

$$E(U) = \begin{cases} \pi[w - \gamma - d(1-\pi)] + \mu\delta & \text{if } w \geq \gamma + d(1-\pi) \text{ \& } d \leq \delta/\pi \\ \pi(w-\gamma) - \delta(1-\pi) + \mu\delta & \text{if } w \geq \gamma \text{ \& } d > \delta/\pi \\ \mu\delta & \text{otherwise} \end{cases} \tag{7}$$

The participation constraint requires that $E(U) \geq \mu\delta$. Omitting the case in which the IC constraint is violated (which is dominated from the firm's perspective), the optimal choice of wage is:

$$w^* \equiv w = \begin{cases} \gamma + d(1-\pi) & \text{if } d \leq \delta/\pi \\ \gamma + \delta(1-\pi)/\pi & \text{if } d > \delta/\pi \end{cases} \tag{8}$$

2.5 Privately optimal choice of PSL

The firm's expected profit is:

$$P = \pi e^*(Y - w^* + dH^*) - \alpha H^*, \tag{9}$$

where $\pi = 1 - \sigma - \mu + \sigma\mu$, e^* is given in equation (2), w^* is given in equation (8), and H^* is given in equation (6).

Combining these equations, the private choice of PSL is between two sets of values for d:

$$P = \begin{cases} \pi(Y-\gamma) - (1-\pi)\alpha \equiv P_\alpha^H(\pi) & \text{if } d \leq \delta/\pi \\ \pi(Y-\gamma) - (1-\pi)\delta \equiv P_\alpha^L(\pi) & \text{if } d > \delta/\pi \end{cases} \tag{10}$$

Comparing $P_\alpha^H(\pi)$ and $P_\alpha^L(\pi)$, we have the first result:

Proposition 1 *The privately optimal choice of PSL depends on the net cost of a work absence ($\alpha - \delta$): firms choose (i) more generous PSL (i.e., $d^* \leq \delta/\pi$) when absences are less* *costly to the firm than they benefit an individual ($\alpha < \delta$), and (ii) less generous PSL (i.e., $d^* > \delta/\pi$) when absences are more costly to the firm than they benefit an individual ($\alpha > \delta$).*

Because a fraction $\hat{p}$ of companies have $\alpha = \delta + \epsilon$, and the remaining fraction $1 - \hat{p}$ have $\alpha = 0$, Proposition 1 has an immediate corollary:

Corollary 1 *The privately optimal choice of PSL is different across firms: a fraction $\hat{p}$ privately offer low PSL; while the remaining fraction $1 - \hat{p}$ voluntarily offer a relatively high PSL.*

Although the effort decision is independent of the decision to stay home, the incentive contract plays an important role as it makes the firm internalize the worker's disutility from going to work when sick and the loss of income from staying home. This implies that the private choice of PSL ultimately depends only on the trade-off between the firm's preference towards presenteeism and the worker's private benefit from absenteeism.

2.6 Socially optimal choice of PSL

The model contains an important source of externality and thus a possible reason for regulatory intervention: the probability of being unwell σ is a function of the aggregate decision in the community on what the employees do when they are sick. Specifically, going to work when unwell helps spread the disease and so increases the probability of becoming sick at the community level: $\sigma = s + \beta W$, where W is the aggregate proportion of workers in the community that go to work when sick. Following the results in Corollary 1, W is equal to the proportion of firms that offer low PSL provisions ($d > \delta/\pi$).

Because of the positive externality associated with PSL, a social planner (i.e., the city, county or state) may choose to intervene and increase the PSL provision, requiring that all employers in the state set $d \leq \underline{d}$. The state chooses $\underline{d}$ to maximize the aggregate welfare in the community. The choice is between two options:

(1) Proceeding without regulation: in such case, Corollary 1 applies and so, $W = \hat{p}$ (i.e., the proportion of companies with $\alpha = \delta + \epsilon$), $\sigma = s + \beta\hat{p}$ and the employee's total effort is low: $\pi = 1 - (s + \beta\hat{p})(1 - \mu) - \mu \equiv \pi_L$. Therefore, the associated social welfare – which is also the average firm profitability – is:

$$\hat{P} = \pi_L(Y - \gamma) - (1 - \pi_L)\hat{p}\delta \equiv S_0 \tag{11}$$

This is also equivalent to setting regulation that is never binding (i.e., setting $\underline{d} > \delta/\pi_L$).

(2) Setting regulation that is binding for at least some firms, i.e., setting $\underline{d} \leq \delta/\pi_H$: in that case, $W = 0$, $\sigma = s$ and employee's total effort is $\pi = 1 - s(1 - \mu) - \mu \equiv \pi_H$. The associated social welfare is:

$$\hat{P} = \pi_H(Y - \gamma) - (1 - \pi_H)\hat{p}(\delta + \epsilon) \equiv S_1 \tag{12}$$

Comparing S_0 and S_1, it is clear that regulation is socially optimal if the externality is large relative to the moral hazard opportunity:

$$\beta > \frac{\epsilon(\mu + s - s\mu)}{(1 - \mu)(Y - \gamma + \hat{p}\delta)} \equiv \beta_0 > 0, \tag{13}$$

and so we can conclude with the following result:

Proposition 2 *A PSL mandate is socially valuable (i.e., $\underline{d}^* \leq \delta/\pi_H$) if equation (13) is satisfied: i.e., the health externality (β) and the value of human capital ($Y - \gamma$) are relatively large compared with the scope for opportunism (μ) and the net value of presenteeism (ϵ).*

The key role of a PSL mandate is to tackle the positive externality from staying home when sick, which reduces the spread of diseases in the community and thus the probability that an individual worker become sick.

2.7 Empirical predictions

The model delivers a number of empirical predictions on firm profitability and labor productivity, which will be tested in the rest of the paper. For that purpose, it is useful to define two cutoffs for β: $\beta_1 \equiv [\epsilon(\mu+s-s\mu)]/[\hat{p}(1-\mu)(Y-\gamma+\delta)]$ and $\beta_2 \equiv [(\delta+\epsilon)(\mu+s-s\mu)]/[\hat{p}(1-\mu)Y]$. Under mild conditions on the parameters, we can show that $\beta_1 > \beta_2 > \beta_0$.[3]

Looking at the first-order effect of the reform on profitability, because the average firm profitability is also the social welfare, we immediately have:

Prediction 1 (Firm Profitability) *A PSL mandate should increase the average firm profitability. If the externality effect is sufficiently large ($\beta > \beta_1$), all firms experience an increase in profitability ($P_\alpha^H(\pi_H) > P_\alpha^L(\pi_L)$); otherwise, only the firms that are already offering voluntary PSL provision are better off ($P_\alpha^H(\pi_H) - P_\alpha^H(\pi_L) > 0$).*

A similar prediction holds for labor productivity that can be defined in this context as:

$$X = \pi e^* Y - \alpha H^* = \begin{cases} \pi Y - (1-\pi)\alpha \equiv X_\alpha^H(\pi) & \text{if } d \leq \delta/\pi \\ \pi Y \equiv X_\alpha^L(\pi) & \text{if } d > \delta/\pi \end{cases}, \tag{14}$$

where $\pi = 1 - \sigma - \mu + \sigma\mu$:

Prediction 2 (Labor Productivity) *A PSL mandate should increase the average labor productivity. If the externality effect is large enough ($\beta > \beta_2$), all firms experience an increase in labor productivity ($X_\alpha^H(\pi_H) > X_\alpha^L(\pi_L)$); otherwise, only the firms that are already offering voluntary PSL provision are better off ($X_\alpha^H(\pi_H) - X_\alpha^H(\pi_L) > 0$).*

The impact of a PSL mandate on labor productivity and firm profitability depends on three critical parameters: the health externality (β), the employee's human capital ($Y - \gamma$ or Y), and the opportunism (μ). For each of these parameters, the model has clear predictions:

[3] The conditions are that δ is not too large ($\delta < [\sqrt{(Y-\gamma+\epsilon)^2 + 4\gamma\epsilon} - (Y-\gamma+\epsilon)]/2$) and Y is not too small ($Y > \gamma/(1-\hat{p})$).

Prediction 3 (Health Externality) *Firms exposed to a greater health externality experience a greater increase in labor productivity and firm profitability following a PSL mandate:* $\partial(X_\alpha^H(\pi_H) - X_\alpha^L(\pi_L))/\partial\beta > 0$ *and* $\partial(X_\alpha^H(\pi_H) - X_\alpha^H(\pi_L))/\partial\beta > 0$*; similarly,* $\partial(P_\alpha^H(\pi_H) - P_\alpha^L(\pi_L))/\partial\beta > 0$ *and* $\partial(P_\alpha^H(\pi_H) - P_\alpha^H(\pi_L))/\partial\beta > 0$.

Prediction 4 (Human Capital) *Firms with higher human capital experience a greater increase in labor productivity and firm profitability following a PSL mandate:* $\partial(X_\alpha^H(\pi_H) - X_\alpha^L(\pi_L))/\partial Y > 0$ *and* $\partial(X_\alpha^H(\pi_H) - X_\alpha^H(\pi_L))/\partial Y > 0$*; similarly,* $\partial(P_\alpha^H(\pi_H) - P_\alpha^L(\pi_L))/\partial(Y - \gamma) > 0$ *and* $\partial(P_\alpha^H(\pi_H) - P_\alpha^H(\pi_L))/\partial(Y - \gamma) > 0$.

Prediction 5 (Moral Hazard) *Firms exposed to a smaller risk of opportunistic employees experience a greater increase in labor productivity and firm profitability following a PSL mandate:* $\partial(X_\alpha^H(\pi_H) - X_\alpha^L(\pi_L))/\partial\mu < 0$ *and* $\partial(X_\alpha^H(\pi_H) - X_\alpha^H(\pi_L))/\partial\mu < 0$*; similarly,* $\partial(P_\alpha^H(\pi_H) - P_\alpha^L(\pi_L))/\partial\mu < 0$ *and* $\partial(P_\alpha^H(\pi_H) - P_\alpha^H(\pi_L))/\partial\mu < 0$.

3 Data and Methodology

In this section we describe the data we use, dedicating particular attention to the PSL mandates; and we introduce the empirical methodology adopted in the paper.

3.1 Sample Selection

Our sample consists of all firms in the CRSP/Compustat Merged (CCM) database (excluding utilities (SIC codes 6000-6999), financials (SIC codes 4900-4999), and non-classifiable establishments (SIC code 9900-9999)) that are incorporated and headquartered in the United States. The sample starts in 2004, four years before the first implementation of the state-level PSL mandate, Washington D.C. in 2008. The sample ends in 2019 because the temporary federal-level Families First Coronavirus Response Act (FFCRA) was introduced on April 1, 2020 when the Covid-19 pandemic resulted in significant global social and economic disruption. FFCRA was adopted to provide universal PSL to employees for reasons related to Covid-19 (U.S. Department of Labor, 2020).

During the sample period, 12 states, 2 counties, and 23 cities passed and subsequently implemented PSL mandates.[4] To ensure that our results are not driven by outliers, all firm-level continuous variables are winsorized at their 1st and 99th percentiles. State-level macroeconomic and industry-level data are from the Bureau of Economic Analysis. The final sample has 42,333 firm-year observations. Detailed variable definitions are in the Appendix. Table 1 presents summary statistics for the main variables used in our empirical analysis.

3.2 Paid Sick Leave Mandates

PSL is a component of the social insurance system, which makes sure that employees are protected against wage losses when they are unable to work due to illness or injury (Marie and Vall Castelló, 2020; Maclean, Pichler, and Ziebarth, 2021). However, unlike other developed economies, U.S. workers do not have universal access to PSL (OECD, 2020). Employees in the U.S. rely on employer policies to provide voluntarily paid leave for short-term illness as part of the fringe benefits offered to their employees (Heymann et al., 2010).[5] In 2011, approximately half of the U.S. employees did not have PSL and up to 3 million employees had to work sick every week in the U.S. (Susser and Ziebarth, 2016). The coverage rate has been increasing, but still around 25% of the workers have not been offered PSL by their employers by 2020 (Bureau of Labor Statistics, 2021). Nevertheless, compulsory PSL mandates and firm policies are not perfect substitutes. Heymann et al. (2010) assert that without PSL mandates, many Americans still choose to work sick even though PSL comes

[4]During our sample period, 6 cities in California either implemented their own PSL mandates or amended/expanded the state's PSL mandate as a separate mandate: Berkeley (2015), Emeryville (2015), Oakland (2015), Los Angeles (2016), San Diego (2016), and Santa Monica (2017). Since these reforms take effect either contemporaneously with or after the implementation of the California state mandate (2015), we use 2015 as the year of treatment for firms located in these cities.

[5]Typically, U.S. employers only voluntarily offer accrued time off that is specifically for sick leave (sometimes as a means to make the companies more attractive to top talent), and the inequality in coverage across different occupations is striking. For instance, 97% of private sector employees in the finance and insurance industry have access to sick pay. In contrast, among low-income, part-time, and service sector workers, the majority cannot take PSL (Bureau of Labor Statistics, 2019).

with their employment because employers may financially penalize employees when they use the voluntarily provided PSL. Susser and Ziebarth (2016) also argue that the workforce presenteeism behavior can stem from workplace and business pressure. Also, since PSL is only provided on a voluntary basis, there is substantial inequality in PSL coverage across different jobs (Maclean, Pichler, and Ziebarth, 2021). For instance, in 2018, the PSL coverage rate in financial industries was 97%, while only around 37% of private-sector employees in accommodation and food services industries had access to PSL (Bureau of Labor Statistics, 2019).

In recent years, many states, counties and cities across the country have started to implement PSL mandates. Washington D.C. was among the first to adopt the PSL mandate which took effect in November 2008. Subsequently, 11 states have implemented state-level PSL mandates. In addition to the state-level PSL mandates, some cities and counties also have implemented their own PSL mandates ahead of their relevant states. Since these cities and counties play a crucial role in the U.S. economy, we include their PSL mandates in our baseline analysis. Table 2 lists all the PSL mandates and reports the years on which each mandate was enacted and became effective. Employees earn one hour of PSL for every 30 to 40 hours worked with a maximum of about 7 days per year. PSL allows employees to first accumulate, and then use these paid sick days when they (or their relatives) are sick (Pichler, Wen, and Ziebarth, 2021). Although the details of these mandates are slightly different, all existing mandates are employer-based and can apply to both part-time and full-time employees.

In our robustness tests, following Maclean, Pichler, and Ziebarth (2021), we focus on state-level mandates. To alleviate the concern that there may be policy spillover to neighboring cities/counties, we drop companies headquartered in these cities/counties with separate PSL mandates. Our main findings remain robust and, in fact, become stronger after excluding firms with separate county- and city-level sick pay mandates (see Section 6.2 for more details).

3.3 Empirical Methodology

3.3.1 Traditional Staggered DiD

We use inter-area variation in the implementation of the PSL mandates to examine the effects of offering sick pay to employees on firm performance. We implement a staggered DiD design to explore the causal effects of the PSL mandate on labor productivity and firm profitability by comparing outcomes in states/counties/cities with and without PSL mandates over the same period. Specifically, several variants of the following TWFE DiD model are estimated:

$$y_{it} = \beta \times \mathrm{PSL}_{\ell t}(0,1) + \Gamma' \times X_{it-1} + \eta_i + \delta_{jt} + \epsilon_{it}, \tag{15}$$

where the dependent variable y_{it} denotes the measure of labor productivity or profitability at firm i (which belongs to industry j in locality ℓ) in year t. We use the logarithm of sales over lagged number of employees as our main productivity measure and EBITDA over lagged total assets as the measure of profitability. Our main variable of interest is $\mathrm{PSL}_{\ell t}(0,1)$, an indicator variable set to one if a firm's headquarter locality ℓ has implemented a state-, county- or city-level paid sick leave mandate since year t, and zero otherwise.[6] Since the mandates are only slightly different, the PSL indicator treats all laws equally and the details of different PSL regulations will not be compared.

In our regression analysis, we include the standard firm-level control variables (lagged by one year) identified in the previous literature: firm size (defined as the logarithm of total assets), labor size (defined as the logarithm of number of employees), firm's growth stage (defined as the logarithm of firm age), financial constraint (defined as the sum of long-term debt and short-term liabilities over total assets), investments (defined as capital expenditure over total assets), tangibility (defined as net property, plant, and equipment (PPE) over total assets) and cash holdings (defined as cash holdings over total assets). To control for the

[6] Thus, our identification strategy is based on the year when a PSL mandate becomes effective, a convention in the PSL literature (see, e.g., Maclean, Pichler, and Ziebarth, 2021). See Table 2 for more details.

differences in macroeconomic conditions across states, we include lagged state GDP growth, the logarithm of lagged state GDP, lagged state unemployment rate, and the logarithm of lagged union coverage rate in all our regression tests. The regression model also includes firm fixed effects η_i (which control for time-invariant omitted firm characteristics) and industry-year fixed effects δ_{jt} (which control for possible industry-specific time trends that could affect firms' policies and the likelihood that the state implements the PSL mandate). The standard errors in our regression models are adjusted for heteroskedasticity and clustered by state, the level at which most of the PSL mandates are implemented.

To investigate the economic channels, in Section 5 we will make use of the cross-sectional heterogeneity in the sample. For that purpose, we will adopt a difference-in-difference-in-differences (DDD) approach by estimating several variants of the following specification:

$$y_{it} = \beta \times \mathrm{PSL}_{\ell t}(0,1) \times H_{it}(0,1) + \Gamma' \times X_{it-1} + \phi \times H_{it}(0,1) + \eta_i + \xi_{lt} + \epsilon_{it}. \tag{16}$$

Compared with specification (15), the parameter of interest β is the coefficient on the interaction term between $PSL_{\ell t}(0,1)$ and a dummy variable ($H_{it}(0,1)$) that classifies firm i into binary groups that we expect ex-ante to be differently affected by the PSL mandates. Importantly, the specification (16) allows to control for locality times year fixed effect ξ_{lt}, thus absorbing any omitted variable that could bias our estimation. As in specification (15), the dependent variables are measures of labor productivity or firm profitability; and the standard errors are adjusted for heteroskedasticity and clustered by state, the level at which most of the major PSL mandates are implemented. We control for firm fixed effects (η_i), the same set of firm-level variables and state-level variables; and we also include the dummy variable $H_{it}(0,1)$ by itself.

3.3.2 Stacked Sample DiD

Recent literature on staggered DiD designs (see, e.g., Roth et al., 2023) has highlighted that, in the presence of treatment effect heterogeneity, coefficients from standard TWFE models may fail to be unbiased estimates of the treatment effects. Essentially, TWFE regressions make both 'clean' comparisons between treated and not-yet-treated units, as well as 'forbidden' comparisons between units that are both already treated. When treatment effects are heterogeneous, these 'forbidden' comparisons can lead to a biased estimate of the average unit-level treatment effect. To circumvent the potential biases resulting from the 'forbidden' comparisons, we employ a stacked sample DiD approach proposed by Cengiz et al. (2019) which estimates a convex weighted average of the event-level treatment effects. Specifically, we begin by identifying nine distinct treatment years during which at least one state, county, or city-level Paid Sick Leave (PSL) mandate takes effect.[7]

For each of these treatment years, we follow Cengiz et al. (2019) and consider an event window that spans from three years before to four years after the implementation of the mandates. In each event window, we classify firms located in areas implementing a PSL mandate that year as the treatment group. For the control group, we include firms located in areas that have never implemented a PSL mandate, as well as firms in areas that have not yet implemented a PSL mandate within the relevant event window. This mitigates concerns about potential biases resulting from the 'forbidden' comparisons. We then append the control group to the treatment group for each treatment year to form a single cohort c. Finally, we stack all nine cohorts to create the comprehensive sample for our DiD analysis. We estimate a similar DiD model to Equation (15), controlling for both cohort-firm and cohort-year fixed effects. For the cross-sectional tests, we estimate a DDD model similar to Equation (16), controlling for both cohort-firm and cohort-locality-year fixed effects. The contrast variable $H_{ict}(0,1)$ now classifies firm i into binary groups within cohort c that we expect ex-ante to be affected differently by the PSL mandates. Like before, we also control

[7]The nine treatment years are 2007, 2008, 2012, 2014, 2015, 2016, 2017, 2018, and 2019.

for $H_{ict}(0,1)$ by itself. In both models, we cluster our standard errors by state, the level at which most of the major PSL mandates are implemented.

4 Baseline Results

In this section we present the results of the DiD analysis of the impact of the PSL mandate on firm productivity and profitability. First, we conduct the analysis on our main sample, which consists of the CCM universe of listed firms. We estimate the traditional TWFE model and then repeat the analysis on the stacked sample. Subsequently, we perform the same analyses on the establishment-level data obtained from Data Axle (formerly Infogroup).

4.1 Firm-Level Results

We begin our analysis by examining the effect of PSL mandates on firm performance measured as labor productivity and firm profitability. Table 3 presents the baseline results of the analysis using sales (in millions) over lagged number of employees as our measure of labor productivity and EBITDA over lagged assets as our measure of profitability (ROA).

In Panel A, we estimate the traditional TWFE model using the CCM dataset. Column (1) shows a positive and significant relationship between the implementation of PSL and labor productivity when we only include state-level controls along with firm and year fixed effects. In Column (2), we also add firm controls: firm size, labor size, tangibility, age, investments, cash holdings, and leverage. In Column (3), we further include industry-times-year fixed effects to control for time-varying industry characteristics. Across all specifications, we find that the coefficients on PSL remain positive and are both statistically and economically significant. By focusing on the richer specification reported in Column (3), the implementation of PSL mandates is associated on average with a 4.2 pp increase in labor productivity. To understand the economic effect of this estimate, notice that the within-firm standard deviation for labor productivity is 0.415. The uncovered effect represents an increase equal to 10.1% (= 0.042/0.415) of one standard deviation of labor productivity. We find similar results for ROA, measured as EBITDA over lagged assets, across all specifications from Columns (4) to (6). Focusing on Column (6), the economic magnitude of the effect is that the profitability increases by 1.2 pp after the implementation of PSL mandates. As the within-firm standard deviation for ROA is 0.115, this represents an increase of 10.4% (= 0.012/0.115) of one standard deviation of profitability.

The identifying assumption of the DiD design is that the implementation of PSL mandates is exogenous to firm performance. We investigate the dynamic effects of PSL mandates on labor productivity in Figure 1 and ROA in Figure 2. The two figures plot the coefficients and the 95% confidence intervals from a similar regression model as in Columns (2) and (4) which replaces $\text{PSL}_{\ell t}(0,1)$ with a set of lead- and lag-indicators: $\text{PSL}^{\leq -5}$, PSL^{-4}, PSL^{-3}, PSL^{-2}, PSL^{0}, PSL^{+1}, PSL^{+2}, PSL^{+3}, and $\text{PSL}^{\geq +4}$. Specifically, PSL^{τ}, with $\tau \in \{-4, -3, -2,\ 0, +1, +2, +3\}$, equals one if the state/county/city in which the firm is headquartered is τ years away from the implementation year of a PSL mandate, and zero otherwise. Moreover, $\text{PSL}^{\leq -5}$ equals one if the state/county/city in which the firm is headquartered will implement a PSL mandate in 5 or more years, and zero otherwise; and $\text{PSL}^{\geq 4}$ equals one if the state/county/city in which the firm is headquartered implemented a PSL mandate 4 or more years ago, and zero otherwise.[8]

In general, the PSL mandate exhibits a positive and persistent effect on labor productivity (in Figure 1) and ROA (in Figure 2). The coefficients on $\text{PSL}^{\leq -5}$, PSL^{-4}, PSL^{-3}, and PSL^{-2} allow us to assess whether there is any pre-treatment difference in firm performance between the treated and the control firms, and/or any evidence of reverse causality concerns. We find that none of the coefficients on these indicators are statistically different from zero, confirming that the parallel trends assumption holds, and the labor productivity and ROA of firms in implemented states, counties, and cities only increase after the implementation of the PSL mandates.

[8]We omit PSL^{-1} from the regression model, enabling direct comparison of the effects of other years with the effect in year $t = -1$.

To alleviate the concern that our estimates from the TWFE model may be contaminated by the problematic comparisons between later and earlier treated firms, we estimate the staked-sample DiD model described in Section 3.2.2. Consistent with the results in Panel A, in Panel B of Table 3 we find that the implementation of the PSL mandates leads to significant increases in labor productivity and ROA across all three columns. Focusing on the most restrictive specification (Column (3)), treatment firms experience increases of 6.6 pp in labor productivity and 1.73 pp in ROA following the reform, both of which are significant at the 1% level. Furthermore, the dynamic treatment effects plotted in Figures A3 and A4 in the Online Appendix show that the parallel trends assumption of the stacked sample DiD model is satisfied for both labor productivity and firm profitability. Taken together, the traditional TWFE estimator in our setting is unlikely to suffer from the severe biases discussed in the recent DiD literature. Therefore, we primarily use the original CCM sample for our subsequent cross-sectional tests. Results of the same set of cross-sectional tests using the stacked sample are reported in the Online Appendix.

In Table 4, we show that our baseline results hold across different measures of productivity and firm profitability in both the original CCM and the stacked samples. In Panels A and B, the dependent variables are the logarithm of sales over lagged assets in Column (1), TFP-IT in Column (2), TFP-ACF in Column (3), gross profit over lagged assets in Column (4), EBIT over lagged assets in Column (5) and net income over lagged assets in Column (6), respectively.[9] We add state and firm controls as well as firm and year fixed effects in all regressions. Across most specifications, we find that the coefficients on the PSL dummy are positive and statistically different from zero at the 5% level in both panels.

[9]TFP-IT and TFP-ACF are measured as the residuals from the estimated log-linearized Cobb-Douglas production function using CCM firm data following İmrohoroğlu and Tüzel (2014) and Ackerberg, Caves, and Frazer (2015), respectively.

4.2 Establishment-Level Results

Given that companies often have dispersed operations across different regions, whereas PSL mandates may only apply to specific locations where employees are based, we assess the robustness of our findings by utilizing more precise data on employee locations and the revenue generated by them. In Table 5, we use establishment-level data from Data Axle (formerly Infogroup). In most states, a minimum number of employees is required for the PSL mandates to be binding. Therefore, to ensure comparability across different regions, we choose the two largest thresholds in our sample construction: 50 (Connecticut and Michigan) and 18 (Rhode Island). We include establishments with at least 50 employees in Columns (1) and (2), and establishments with at least 18 employees in Columns (3) and (4).[10] We exclude establishments which belong to utility (NAICS code 22) and finance (NAICS code 52) industries. We drop establishment-year observations with (i) missing sales, (ii) missing number of employees, and (iii) missing industry classification. We also exclude inactive establishments (sales or number of employees equals zero). Labor productivity at the establishment level is measured as the logarithm of sales (in thousands) over lagged number of employees.

The DiD estimates in Panel A of Table 5 show that the implementation of the PSL mandates leads to an increase of 4.9 to 5.8 pp in labor productivity for establishments with at least 50 employees (Columns (1) and (2)), and a similar increase of 6.2 to 6.8 pp for establishments with at least 18 employees (Columns (3) and (4)). It is interesting to note that the magnitude of the estimates is very close to that of the estimates using the CCM data (in Table 3), which lends support to the external validity of our findings on public firms. Figures 3 and 4 plot the dynamic treatment effects based on a variant of the model in Columns (1) and (3) which replaces the DiD dummy with similar leads and lags as in Figures 1 and 2, using establishments with at least 50 and 18 employees. In both cases, we

[10]The thresholds used by the PSL mandates of other states include 15 (Arizona and Maryland), 10 (Massachusetts and Oregon), and 5 (Vermont). California, Washington, Washington D.C., and New Jersey do not employ an explicit threshold for their mandates to be binding. Our results remain robust when using all of these alternative thresholds.

find that none of the pre-treatment coefficients are statistically different from zero and the strong and persistent increases in labor productivity only occur after the implementation of the PSL mandates, which again validate the parallel trends assumption. In Panel B of Table 5, we replicate the analysis on a stacked sample with an event window spanning three years before and four years after the implementation of each PSL mandate. We find that the labor productivity gains associated with the implementation of the PSL mandates range from 3.4 to 4.1 pp for establishments with at least 50 employees, and from 3.6 to 4.1 pp for those with at least 18 employees, all of which are significant at the 1% level. Figures A5 and A6 in the Online Appendix confirm that the parallel trends assumption holds for both samples with different employee cutoffs. Overall, these results further support our findings that PSL mandates contribute to performance improvement, even at the establishment level.

4.3 Interpretation of the Baseline Results

The results presented so far show that the PSL mandates have a positive impact on labor productivity and firm profitability, which holds across different empirical methodologies and data sets. In terms of the economic mechanism behind this finding, the model presented in Section 2 offers a specific explanation: PSL mandates reduce the probability of disease spread in the community, thereby decreasing the likelihood of employees being unwell and, consequently, less productive at work. The claim that PSL mandates improve public health is supported by extensive empirical evidence.[11] We show that this benefit extends to workers' productivity and firm profitability. In Section 5, we will explore three cross-sectional implications of the model to establish further support for it. However, it is important to note that there could be other explanations for the results presented so far.

One potential channel – consistent with the increase in labor productivity (but not the effect on firm profitability) – is that the PSL mandates may alter the relative cost of production inputs. If the mandates increase labor costs, firms could optimally react by reducing

[11]See, e.g., Pichler and Ziebarth (2017), Pichler and Ziebarth (2020), and Pichler, Wen, and Ziebarth (2021).

the demand for labor and increase the use of capital as their production inputs. A shift from labor to capital could also lead to a mechanical increase in labor productivity, measured as sales per employee, due to fewer employees being employed. If that is the case, the interpretation of our findings would be incorrect. In Table A8 of the Online Appendix we examine if there is any change in the use of capital relative to employees following the implementation of the PSL mandates. We measure the relative use of capital and labor as the logarithm of the ratio of total assets over the number of employees. Estimating a specification similar to the one in Table 3, we find that PSL mandates have no significant effect on the use of capital relative to labor. This finding lends more support to our interpretation that PSL increases labor productivity independently from changes in the amount of labor or capital as production inputs.

Another alternative (and complementary) economic channel consistent with the evidence so far is that PSL may change the labor market conditions by making it easier for firms to attract and retain skilled employees. We cannot directly test this hypothesis as it would require access to firm level data on labor composition. However, we can try to shed light on its plausibility with the hiring and firing data at the state-industry level obtained from the Census Bureau's Quarterly Workforce Indicators (QWI). In Table A9 of the Online Appendix, we show that the implementation of the PSL mandates has no effect on the hiring and firing activities at the state-industry level. Although the data is not at the firm level – and so we cannot rule out an effect of PSL on firm's ability to attract and retain employees – the results are not supportive of this alternative explanation.

5 Cross-Sectional Tests

The model presented in Section 2 identifies three different, complementary channels through which the implementation of PSL mandates might affect firm performance. First, the model predicts when the health externality is stronger, firms should experience a greater increase in labor productivity and firm profitability following the PSL mandates. Second, the model predicts that the effect on labor productivity and firm profitability from the PSL mandates is greater in firms with higher human capital. Third, the improvements in performance should be smaller for firms that are more exposed to opportunistic behavior from their employees. To test these predictions, we exploit variations across firm, industry and region characteristics to estimate difference-in-difference-in-differences (DDD) regression models.

5.1 Health Externality

Prediction 3 of the model states that firms exposed to a greater health externality should experience a larger increase in labor productivity and firm profitability following a PSL mandate. There is direct empirical evidence (Pichler and Ziebarth, 2017; Pichler and Ziebarth, 2020; Pichler, Wen, and Ziebarth, 2021) that PSL mandates create a *positive health externality* for the sick employees' coworkers in their workplace, since access to PSL encourages employees with contagious diseases to take a few days off at home to prevent the transmission of the virus. Because the seasonal flu vaccination coverage among adults remains only around 40% from 2011-2018, workplace presenteeism is arguably among the most important ways through which flu epidemics spread (Susser and Ziebarth, 2016; Centers for Disease Control and Prevention, 2020). Nevertheless, workers from different industries differ in their exposures to influenza virus due to the nature of the work they perform. In other words, the size of the health externality is not the same across industries. For instance, there are sectors that allow and support employees to work remotely (e.g., from home) with a limited impact on their productivity. Flexible working locations reduce the likelihood that sick employees show up in the workplace and spread the virus to their co-workers. On the contrary, others sectors cannot accommodate remote work unless they accept a significant loss of productivity. Employees whose jobs entail physical presence are more likely to exhibit workplace presenteeism behavior if they lack access to PSL. Their colleagues will also face greater health risk during the outbreak period of the contagious diseases. Accordingly, introducing PSL mandates should have a larger positive effect on the performance of firms operating in industries with *fewer* shares of jobs that can be done remotely.

To test the model's prediction, we first use the share of jobs that cannot be done remotely in each 2-digit NAICS sector identified in Dingel and Neiman (2020) to proxy for the degree of workplace health externality. Panel A of Table 6 presents the results of this analysis. *High Physical Presence* is an indicator variable set to one if the firm belongs to a sector that has below or equal to the median share of jobs that can be done remotely each year. The coefficients on the interaction term are positive and significant across all specifications, indicating that the PSL mandates lead to more pronounced increases in productivity and probability of firms with *less* workplace flexibility.[12]

Second, workplaces with on-site staff and operations are more likely to suffer from outbreaks of contagious diseases such as Covid-19. By analyzing a data set of 698 nonresidential, nonhealthcare workplace Covid-19 outbreaks in Los Angeles County, California, Contreras et al. (2021) find that nearly 60% of such outbreaks occurred in 3 industries: manufacturing (26.4%), retail trade (19.6%), and transportation and warehousing (10.5%). The high-density environments and close contact in production lines, long shifts, shared equipment, and common spaces increase the risk of exposure to the coronavirus in these sectors. The workplace outbreak data can help us to identify more vulnerable industries, and the positive externality of mandating PSL should be more salient in these industries. Across all specifications, Panel B of Table 6 show that the improvements in performance are indeed more pronounced among firms operating in industries that are more likely to be severely affected by workplace outbreaks of contagious disease. To this end, mandating PSL in these industries can effectively reduce the spread of contagious diseases among employees.

Overall, the results in this subsection confirm the positive health externality in workplace as the key driver of the subsequent performance enhancement due to PSL mandates.

[12] Our results are robust to the use of the share of jobs that cannot be done remotely in each 3-digit NAICS industry.

5.2 Human Capital

Prediction 4 of the model suggests that the effect on labor productivity and firm profitability from the PSL mandates should be greater in firms with higher human capital. To measure difference in human capital, we adopt two complementary approaches: we classify firms depending on their industry's average wage and their capital-to-labor ratio.

To classify firms in high- versus low-wage industries, we start by calculating the *weighted average wage per industry* as follows: we divide the total amount of wages paid to part-time employees by the number of part-time employees to get the average wage of part-time employees, and divide the total amount of wages paid to full-time employees by the number of full-time employees to get the average wage of full-time employees, both in the same 2-digit NAICS sector in each year. Then we calculate the annual weighted average wage based on the fractions of part-time employees' and full-time employees' wages to all employees' wages in the same 2-digit NAICS sector. Employees in industries with a higher weighted average wage per employee receive higher wages on average. Finally, we generate an indicator (*High Wage*) that is set to one if the *industry weighted average wage* is above or equal to the median of all industries in a given year. Panel A of Table 7 presents the results of the DDD specification based on Equation (16) using the indicators as our key contrast variables.

The results provided in Panel A of Table 7 show that the implementation of PSL mandates has a greater impact on the performance of firms in industries whose employees have higher wages. In Columns (1) and (4), the coefficients of the interaction between the implementation of PSL and the *High Wage* dummy are positive and significant at the 1% level while the positive coefficients of the treatment indicator PSL$(0, 1)$ are either weakly significant or insignificant. This indicates that the positive relationship between PSL provision and firm performance (both labor productivity and ROA) is concentrated among firms in high-paying industries. To address concerns that there could be time-varying state-specific variables that both correlate with our treatment dummy and affect performance, we include state-year fixed effects in Columns (2) and (5). We find that our results still hold after controlling for the state-year fixed effects. The results are also robust in Columns (3) and (6), where we include city-year fixed effects (which absorb PSL as an independent variable).

As an alternative, maybe more direct, measure of the human capital of employees, in Panel B of Table 7, we use total assets per employee to measure the firm-level capital-to-labor ratio. Hanka (1998) and Masulis, Wang, and Xie (2020) use this variable and find that employees are paid more at firms with higher capital per employee. We create an indicator (*High K/L*) that is set to one if its value is above the sample median in a given year. We find that all coefficients on the interaction term of the above-median capital per employee indicator and PSL dummy is positive and significant at 1% level. These findings suggest more pronounced performance improvements for firms that pay higher wages. The results are qualitatively the same as those in Panel A, confirming that the positive effect of PSL provision on firm performance is indeed stronger for firms with high-skilled employees. They also provide further assurance that our results are robust to controlling for all state-year and city-year specific heterogeneity.[13]

5.3 Moral Hazard

A potential concern is that the PSL provision may give rise to a new moral hazard problem: once PSL is offered, workers with minor diseases or even no disease at all may adapt their absence behavior to obtain sick pay opportunistically. As stated in Prediction 5 of the model, we expect that a lower increase in labor productivity and firm profitability whenever the risk for opportunistic behavior is greater.

We use county-level social capital to proxy for the likelihood of the moral hazard issue. There is a large literature in sociology and economics which has provided ample evidence that social capital, as captured by the strength of cooperative norms and the density of social networks in geographical areas, discourages opportunistic behaviors. Social capital also promotes effective communication, cooperation and attendant behaviors, and imposes

[13] We obtain similar results when we classify industries into high or low K/L industries based on the 2-digit SIC industry median K/L ratio each year.

punishment against deviant behavior (e.g., Coleman, 1988; Knack and Keefer, 1997; Guiso, Sapienza, and Zingales, 2011). Individuals are highly influenced by the communities where they inhabit. They have to accept the prevalent social norms, and desire to avoid any potential cost of deviating from the norms. Since the cost of perpetrating with opportunistic behaviors is higher for employees in regions with stronger norms and denser social networks, they are more likely to be honest and socially responsible (Hoi, Wu, and Zhang, 2019). As a result, firms in counties with higher social capital will exhibit less moral hazard and absenteeism behavior, and we expect the positive effect to be stronger for firms in these regions.

Following Rupasingha et al. (2006), Hasan et al. (2017), and Hoi, Wu, and Zhang (2019), we employ the data from the Northeast Regional Center for Rural Development (NRCRD) at Pennsylvania State University to estimate the county-level social capital which captures the variation of local corporate norms, social network and local moral standards in the U.S. in the years of 1997, 2005, 2009 and 2014. The variable of social capital is constructed by using the first principal component from a factor analysis based on Pvote (voter turnouts in presidential elections), Respn (response rates in US census survey), Assn (total numbers of social organizations), and Nccs (total numbers of nonprofit organizations). Since NRCRD only provides data in the years 1997, 2005, 2009 and 2014, first we compute county averages and then we set *High SK* equal to one if the social capital of the county where a company is headquartered is above the mean score across all counties, and zero otherwise.

The results of the DDD specification based on Equation (16) are presented in Table 8. From Columns (1) and (3), the coefficients of the interaction term PSL$(0,1)\times$*High SK* are all positive. As shown in Columns (2) and (4), after we further control for the state-year fixed effects, the positive coefficients of the interaction term become significant at 1% level. The results show a stronger positive effect of the PSL mandates on performance for firms with high-moral employees.

6 Robustness Tests

In this section, we discuss additional tests to check the robustness of our main findings: we adopt different DiD estimators; We discuss the results from the cross-sectional tests using the stacked sample; we examine different cuts of the data; we explore differences between voluntary and mandatory PSL provisions; we show that the results do not change when we explicitly control for changes in Paid Family Leave; and we perform a placebo test. All tables and figures related to this analysis are reported in the Online Appendix.

6.1 Staggered DiD Estimators

To provide further support that our treatment effect estimates are not plagued by the weighting issues of the TWFE estimator in staggered DiD designs, we employ two alternative estimators following the approaches by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). Both consider identification, aggregation, estimation, and inference procedures for the average treatment effects on the treated (ATT) in DiD designs with differential treatment timing and heterogeneity in treatment effects by time of implementation. We start with Callaway and Sant'Anna (2021) and estimate the aggregated group-time ATT where in our case groups are cohorts of firms treated in the same year. The results are presented in Columns (1) and (2) – for labor productivity – and Columns (3) and (4) – for firm profitability – of Table A1 of the Online Appendix. The estimated treatment effects on labor productivity are larger, and the effects on firm profitability are similar to the results we have obtained in Table 3.

Next, we perform an event study using the alternative estimators proposed by Sun and Abraham (2021) to test the parallel trends assumption and check the dynamics of the treatment effects in a event window that starts 5 years prior to the implementation of the PSL mandates and ends 4 years after the implementation. We report the results in Figures A1 and A2 of the Online Appendix. In general, the parallel trends assumption is satisfied for the DiD tests of both the productivity and the profitability. Moreover, we find that the magnitudes of the treatment effects start to grow persistently from the treatment implementation year, providing reassurance of the validity of our identification strategy and the original TWFE estimates.

6.2 Stacked Sample Analysis

In Panels B of Tables 3 - 5, we reported the result of the stacked sample analysis for the main findings. In the Online Appendix, we report the results of adopting a stacked sample approach in the cross-sectional tests. In Table A2, we replicate the analysis of differences in health externality reported in Table 6 using the stacked sample. We do the same in Table A3 (for the analysis of the differences in human capital reported in Table 7) and in Table A4 (for the analysis of the differences in social capital reported in Table 8). Across all these tables the results are similar to the ones discussed in Section 5.

In the Online Appendix, we also report the results on the dynamic effects with the stacked sample: In Figure A3 (A4), we plot the dynamic effects in the CCM data for labor productivity (ROA), respectively; in Figure A5 (A6), we plot the dynamic effects for the establishment-level data for more than 50 employees (18 employees), respectively. Across all these figures the results are similar to the ones reported in Figures 1 - 4.

6.3 Sample Selection

In our main regression, we include firms which are headquartered in cities or counties with separate city- or county- level PSL mandates from the sample. The coexistence of both state-level and city/county-level PSL mandates may complicate the interpretation of the results. Following Maclean, Pichler, and Ziebarth (2021), in Columns (1) - (2), Table A5 of the Online Appendix, we exclude these firms in the sample as a robustness test.[14] The key

[14]To ensure that the sample we are considering here is not affected by any county or city-level PSL mandates, we also exclude the 6 cities in California that implement their own PSL mandates either contemporaneously with or after the implementation of the state mandate (i.e., Berkeley, Emeryville, Oakland,

independent variable is PSL(0,1), which equals one for the years since a state/county/city has implemented the PSL mandate, and equals to zero otherwise. The results are qualitatively similar to our findings in Table 3.

In Columns (3) - (4) of Table A5, we drop observations for the IT industry (SIC code 737), as it is likely to be significantly different from the other industries in terms of labor flexibility: remote working is simpler to accommodate than in any other sector and frequently permitted. As our prior is that PSL mandates should not really matter in the IT sector, it is important to check that results are not driven but that industry. The results are very similar to those reported in Table 3 and thus unaffected by the removal of the observations for $t = 0$.

Our identification relies on the assumption that a large portion of the employees are affected by the PSL mandates set by the state, county and city in which the firm is headquartered. In some cases, this may not be a reasonable assumption as firm's employees could be wide-spread across the country and only a small proportion of them would be affected by the regulatory change in the locality in which the company's headquarters are located. To ensure that the implementation of the PSL mandates can affect employees sufficiently and effectively, we follow Agrawal and Matsa (2013) and exclude firms operating in geographically dispersed industries. These industries are retail, wholesale and transportation. Since employees of firms operating in these industries are more likely to work in different states, we drop these firm observations to restrict our sample to firms whose operations are more likely to be concentrated in the same state as their headquarters. The results, presented in Columns (5) - (6) of Table A5, are virtually unchanged from those reported in Table 3.

6.4 Voluntary versus Mandated PSL

We have shown that mandating PSL can effectively improve labor productivity and firm profitability. Although firms can voluntarily provide PSL, and often do, our argument is that

Los Angeles, San Diego, and Santa Monica) as mentioned in Footnote 3. Adding these firms back yields qualitatively similar results.

the existence of positive externalities implies private under-provision of PSL as individual employers fail to internalize the externality and underinvest in PSL. Mandatory regulation will be needed to ensure that PSL is at the socially optimal level.

In other words, our model argues that the voluntary and mandatory provisions of PSL are not perfect substitutes: the firms that are already providing PSL and those that are not should experience a similar increase in labor productivity and firm profitability following the PSL mandates. We cannot test this prediction directly, as we do not know which companies in our sample provide PSL. Therefore, in Table A6 of the Online Appendix, we follow Callison and Pesko (2017) and define low-access industries to be those with less than their sample mean share of workers that can access PSL prior to the enactment of the mandates. We run the same specification as in Table 3 with the added interaction term of *PSL(0,1)* × *Low Access*. We find that the coefficients on the interaction term are not statistically different from 0, while the coefficients on $PSL(0,1)$ by itself are positive and statistically different from zero at the 5% level (in Columns (1) and (4) when they are not absorbed by the fixed effects). This result supports the model's prediction that PSL mandates address an externality problem that private provision of PSL cannot solve.

6.5 PSL and PFL

In a closely related paper, Bennett et al. (2022) examine the implementation of Paid Family Leave (PFL) laws in the U.S. from 2002 to 2018.[15] The PFL laws require firms to offer their employees long-term paid leave for a family or medical event. The authors find that productivity improves by 4 pp in the states that passed the PFL laws relative to the adjacent states. Conceptually, PSL is fundamentally different from PFL: PSL is meant for short-term health-related needs; while PFL relates to a long-term paid absence from work to care for ill family members or new-born children. As a consequence, we expect that PFL and PSL have different effects on incentives and absences, hence on firm performance.

[15] Four states have implemented the PFL laws during our sample period: California (2004), New Jersey (2009), Rhode Island (2014), and New York (2018).

To assuage the concern that the effect we are measuring is due to the implementation of PFL rather than PSL provisions, in Table A7 of the Online Appendix, we replicate the analysis reported in Table 3 while controlling for the implementation of PFL acts. As the results are virtually unchanged, we can confirm that PSL has an independent and economically meaningful impact on labor productivity and firm profitability.

6.6 The Political Economy of PSL Mandates

The adoption of the PSL mandates is a political decision. As we do not know the precise motivations for this decision, a concern is that we are missing an omitted variable that is both correlated with firm performance and the adoption of the PSL mandates. The most effective way to handle this concern is to control for state-year fixed effects. We do so, in the DDD analysis reported in Tables 6 - 8 but cannot do it by construction in our basic DiD analysis in Tables 3. In those regressions we have a number of state-level controls (GDP growth, ln(GDP), Unemployment Rate and ln(Union Coverage Rate)) but it may not be enough.

Callison and Pesko (2022) study the determinants of PSL adoptions using county-year-level data from the 2005-2018 period. They show that a rich set of observable demographic variables fails to explain variations in the adoption of PSL laws. Their findings support the validity of our identification strategy. In Table A10 of the Online Appendix we further strengthen the analysis of Callison and Pesko (2022): we show that the passage of PSL mandates is uncorrelated with a set of state-level economic variables, including $\ln(\text{GDP})_{t-1}$, GDP Growth_{t-1}, Unemployment Rate_{t-1}, $\ln(\text{Population})_{t-1}$, and $\ln(\text{Union Coverage})_{t-1}$.

6.7 Placebo Test

A further concern is that our results are picking up noise in the data or some general trend towards higher productivity and profitability. One way to address this concern is to perform a placebo test: to do so we randomly assign the same number of (pseudo) treatment firms as that of the true treatment firms in each cohort with the same year of PSL mandate implementation, and re-estimates the placebo effects. We repeat the procedure for 1000 times and plot the distribution of the placebo estimates. The results are presented in Figures A7 and A8 of the Online Appendix: we find that the treatment effects for the placebo test (for both labor productivity and firm profitability) are nicely normally distributed around 0. This is indeed what we expected to see as, on average, the placebo treatments should have no effect on firm performance. We further perform a two-sided test to calculate the likelihood (p-value) of observing the effects found in our DiD analysis, assuming the null hypothesis that the treatment effects on both labor productivity and firm profitability are zero. The p-values for both labor productivity ($p = 0.002$) and ROA ($p = 0.000$) reject the null hypotheses at the 1% significance level.

7 Conclusion

The United States are among the very few developed economies and in fact one of the only three OECD countries that do not provide workers with universal access to PSL (OECD, 2020). Yet, 75% of Americans support sick pay mandates, with majority support across party affiliation (Kim, 2010; Swanson and Jamieson, 2013). In 2020, 25% of the U.S. workforce still had neither employer- nor government-provided PSL (Bureau of Labor Statistics, 2021). It was not until the outbreak of the recent Covid-19 pandemic crisis that the U.S. Congress eventually passed a bipartisan Families First Coronavirus Response Act (FFCRA) that contains up to two weeks of temporary emergency sick leave for employees in private firms with up to 500 employees. The Act became effective from April 1, 2020, but expired soon after, on December 31, 2020 (U.S. Department of Labor, 2020).

Our paper makes a case for mandating PSL at the federal level. We show that the state-, county-, and city-level PSL mandates increased labor productivity and firm profitability. In this sense, economies affected by the PSL mandates appeared to have experienced a Pareto improvement. We also show that the positive effects of PSL were stronger for (i) firms in industries that have less workplace flexibility and tend to require physical presence, (ii) firms with greater human capital, and (iii) firms in counties with higher social capital.

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Appendix: Variable Definitions

Dependent Variables:

Labor Productivity: The natural logarithm of the value of sales over lagged number of employees.

ROA: EBITDA over lagged total assets.

ln(Sales/Assets): The natural logarithm of the value of sales over lagged total assets.

Gross Profit/Assets: Gross profit over lagged total assets.

EBIT/Assets: EBIT over lagged total assets.

Net Income/Assets: Net income over lagged total assets.

TFP-IT: Total factor productivity (TFP) is estimated following İmrohoroğlu and Tüzel (2014).

TFP-ACF: Total factor productivity (TFP) is estimated following Ackerberg, Caves, and Frazer (2015).

ln(Capital/Labor): The natural logarithm of the ratio of total assets to number of employees.

DiD Dummy:

PSL(0,1): An indicator variable set to one if the state, county, or city in which a firm is headquartered has implemented a PSL mandate by year t, and zero otherwise.

Firm Controls:

ln(Assets): The natural logarithm of total assets.

ln(Number of Employees): The natural logarithm of number of employees (in thousand).

ln(Age): The natural logarithm of number of years, calculated as number of days divided by 365.25, since the firm first appeared in CRSP.

Tangibility: Net property, plant and equipment over total assets.

Cash Holdings: Cash holdings over total assets.

Capital Expenditures: Capital expenditures over total assets.

State Controls:

ln(GDP): The natural logarithm of annual GDP in each state.

GDP Growth: $(\text{GDP}_t - \text{GDP}_{t-1})/\text{GDP}_{t-1}$.

Unemployment Rate: Annual percentage unemployment rate in each state.

ln(Union Coverage): The natural logarithm of the percentage union coverage rate in each state.

PFL: An indicator variable set to one if the state in which a firm is headquartered has implemented the Paid Family Leave (PFL) act by year t, and zero otherwise. These states include California (2004), New Jersey (2009), Rhode Island (2014), and New York (2018).

Contrast Variables:

High Physical Presence: An indicator variable set to one if the firm is in an industry with above or equal to median share of jobs that cannot be done remotely in each 2-digit NAICS sector based on Dingel and Neiman (2020), and zero otherwise.

High Outbreaks: An indicator variable that takes one for industries highly affected by workplace Coronavirus outbreaks. These industries include manufacturing (2-digit NAICS code 31-33), retail trade (2-digit NAICS code 44-45), and transportation and warehousing (2-digit NAICS code 48-49) as identified by Contreras et al. (2021), and zero otherwise.

High Wage: An indicator variable set to one if the firm is in an industry with above or equal to median weighted average wage where the weighted average wage is the total amount of wage paid to part-time and full-time employees divided by the number of part-time and full-time employees in the same 2-digit NAICS sector.

High K/L: An indicator variable set to one if the firm has above or equal to median capital-to-labor ratio in a given year, and zero otherwise.

High SK: Following Rupasingha et al. (2006), Social Capital is the first principal component from a principal component analysis based on 1) percentage of voters who voted in presidential elections, 2) response rate to the Census Bureau's decennial census, 3) sum of tax-exempt non-profit organizations divided by populations per 10,000, and 4) sum of social organizations divided by populations per 100,000. The Social Capital variable is estimated at county level in 1997, 2005, 2009, and 2014, and made available by the Northeast Regional Center for Rural Development (NRCRD) at the Pennsylvania State University. We use the four-year average social capital score as a proxy for the strength of social capital of each county. High SK is an indicator variable set to one if a firm is headquartered in a county with above or equal to mean strength of social capital of all counties, and zero otherwise.

Low Access: An indicator variable set to one if the firm is in one of the following industries classified in Callison and Pesko (2017), which have less than their sample mean share of workers who can access PSL prior to the enactment of the mandates: agriculture, forestry, fishing and hunting (2-digit NAICS code 11), construction (2-digit NAICS code 23), administrative and support and waste management and remediation services (2-digit NAICS code 56), arts, entertainment, and recreation (2-digit NAICS code 71), accommodation and food services (2-digit NAICS code 72), and other services (except public administration) (2-digit NAICS code 81), and zero otherwise.

Table 1: Summary Statistics

This table reports summary statistics for firm-, state-, and establishment-level variables. The sample for variables at the firm-year level consists of firms in the CCM database (excluding financials, utilities, and non-classifiable establishments) from 2004–2019. Firm-level continuous variables are winsorized at the 1st and 99th percentiles. The sample for variables at the establishment-year level consists of firms in the Data Axle (formerly Infogroup) database (excluding financials, utilities, and non-classifiable establishments) from 2004–2019. All variables are defined in the Appendix.

	N	Mean	SD	P25	P50	P75
Dependent Variables						
Labor Productivity	42,333	-1.310	1.025	-1.805	-1.303	-0.770
ROA	42,333	0.057	0.246	0.029	0.110	0.176
ln(Sales/Assets)	41,792	-0.215	1.007	-0.586	-0.054	0.401
TFP-IT	30,075	-0.336	0.510	-0.572	-0.324	-0.072
TFP-ACF	30,028	-0.201	0.557	-0.485	-0.214	0.100
Gross Profit/Assets	41,792	0.365	0.337	0.200	0.341	0.524
EBIT/Assets	41,792	0.010	0.243	-0.017	0.067	0.127
Net Income/Assets	41,792	-0.035	0.248	-0.056	0.033	0.083
ln(Capital/Labor)	42,168	-1.065	1.181	-1.802	-1.107	-0.415
DiD Dummy						
PSL(0,1)	42,333	0.116	0.320	0.000	0.000	0.000
Lagged Firm Controls						
ln(Assets)	41,792	6.163	2.035	4.668	6.135	7.580
ln(Number of Employees)	41,792	7.256	2.147	5.659	7.313	8.810
ln(Age)	41,792	2.638	0.829	2.065	2.727	3.263
Tangibility	41,792	0.234	0.228	0.062	0.150	0.331
Cash Holdings	41,792	0.161	0.176	0.037	0.102	0.218
Leverage	41,792	0.211	0.218	0.007	0.164	0.331
Capital Expenditure	41,792	0.048	0.058	0.014	0.029	0.057
Lagged State Controls						
ln(GDP)	42,333	13.115	0.888	12.519	13.069	13.820
GDP Growth	42,333	0.044	0.029	0.031	0.047	0.064
Unemployment Rate	42,333	6.178	2.060	4.700	5.500	7.300
ln(Union Coverage)	42,333	2.378	0.585	1.740	2.632	2.815
PFL	42,333	0.197	0.398	0.000	0.000	0.000
Contrast Variables						
High Physical Presence	41,792	0.648	0.478	0.000	1.000	1.000
High Outbreaks	41,792	0.603	0.489	0.000	1.000	1.000
High Wage	41,792	0.541	0.498	0.000	1.000	1.000
High K/L	41,627	0.500	0.500	0.000	1.000	1.000
High SK	40,854	0.196	0.397	0.000	0.000	0.000
Low Access	41,792	0.079	0.270	0.000	0.000	0.000
Establishment-Level						
$\geq$ 50 Employees)						
Labor Productivity	4,831,883	5.157	1.101	4.394	5.139	5.644
PSL(0,1)	4,831,883	0.078	0.268	0.000	0.000	0.000
$\geq$ 18 Employees)						
Labor Productivity	14,634,364	5.043	1.057	4.200	5.059	5.553
PSL(0,1)	14,634,364	0.079	0.270	0.000	0.000	0.000

Table 2: Paid Sick Leave Mandates

This table reports the years in which each Paid Sick Leave mandate was enacted and became effective. The treatment year is the year when a PSL mandate becomes effective.

	Year Enacted	**Year Effective**
State:		
Arizona	2016	2017
California	2014	2015
Connecticut	2011	2012
Maryland	2018	2018
Massachusetts	2014	2015
Michigan	2018	2019
New Jersey	2018	2018
Oregon	2015	2016
Rhode Island	2017	2018
Vermont	2016	2017
Washington	2016	2018
Washington, DC	2008	2008
County:		
Cook, IL	2016	2017
Montgomery, MD	2015	2016
City:		
Bloomfield, NJ	2015	2015
Chicago, IL	2016	2017
East Orange, NJ	2014	2015
Elizabeth, NJ	2015	2016
Irvington, NJ	2014	2015
Jersey City, NJ	2013	2014
Minneapolis, MN	2016	2017
Montclair, NJ	2014	2015
Morristown, NJ	2016	2017
Newark, NJ	2014	2014
New Brunswick, NJ	2015	2016
New York, NY	2014	2014
Passaic, NJ	2014	2015
Paterson, NJ	2014	2015
Philadelphia, PA	2015	2015
Plainfield, NJ	2016	2016
Portland, OR	2013	2014
Saint Paul, MN	2016	2018
San Francisco, CA	2006	2007
Seattle, WA	2011	2012
Spokane, WA	2016	2017
Tacoma, WA	2015	2016
Trenton, NJ	2014	2015

Table 3: PSL and Firm Performance

This table reports the DiD estimates of the effect of the implementation of the state-, county- and city-level PSL mandates on firm performance over the 2004–2019 period. The dependent variable in Columns (1) - (3) is labor productivity, defined as $\ln(\text{Sales}_t/\text{Number of Employees}_{t-1})$. The dependent variable in Columns (4) - (6) is ROA, defined as EBITDA over lagged total assets. The key independent variable is $\text{PSL}(0,1)$ which equals one if the state, county, or city a firm headquartered in has implemented a PSL mandate, and zero otherwise. In Panel A, we use the original CCM sample. In Panel B, we use a stacked sample approach with an event window spanning three years before and four years after the implementation of each PSL mandate. We define cohorts based on each distinct treatment year in which at least one state, county, or city implements a PSL mandate, and append them to construct the stacked sample. In each cohort, the treatment group consists of firms subject to a PSL mandate implemented in the same year by their state, county, or city. The control group consists of firms that are never treated and those that have not yet been treated during the event window. Firm and state controls include $\ln(\text{Assets})_{t-1}$, Tangibility_{t-1}, Cash Holdings_{t-1}, Leverage_{t-1}, $\ln(\text{Number of Employees})_{t-1}$, Capital Expenditure_{t-1}, $\ln(\text{Age})_t$, GDP Growth_{t-1}, $\ln(\text{GDP})_{t-1}$, Unemployment Rate_{t-1}, and $\ln(\text{Union Coverage})_{t-1}$. All variables are defined in the Appendix. Industry fixed effects are defined by the 2-digit SIC code. Standard errors clustered by state are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Original Sample

	Labor Productivity: $\ln(\frac{\text{Sales}}{\#\text{Emp}})$			ROA: $\frac{\text{EBITDA}}{\text{Assets}}$		
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1)	0.057***	0.066***	0.042**	0.019**	0.020**	0.012*
	(0.021)	(0.023)	(0.018)	(0.009)	(0.009)	(0.007)
Firm Controls	NO	YES	YES	NO	YES	YES
State Controls	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	NO	YES	YES	NO
Industry-Year FE	NO	NO	YES	NO	NO	YES
Adj. R^2	0.822	0.846	0.849	0.757	0.761	0.769
Observations	42,333	41,792	41,727	42,333	41,792	41,727

Panel B: Stacked Sample [-3, 4]

	Labor Productivity: $\ln(\frac{\text{Sales}}{\#\text{Emp}})$			ROA: $\frac{\text{EBITDA}}{\text{Assets}}$		
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1)	0.081***	0.083***	0.066***	0.023**	0.024**	0.017***
	(0.029)	(0.029)	(0.021)	(0.009)	(0.010)	(0.006)
Firm Controls	NO	YES	YES	NO	YES	YES
State Controls	YES	YES	YES	YES	YES	YES
Cohort-Firm FE	YES	YES	YES	YES	YES	YES
Cohort-Year FE	YES	YES	NO	YES	YES	NO
Cohort-Industry-Year FE	NO	NO	YES	NO	NO	YES
Adj. R^2	0.878	0.894	0.896	0.787	0.789	0.799
Observations	115,901	114,317	114,111	115,901	114,317	114,111

Table 4: Alternative Measures of Productivity and Profitability

This table reports the DiD estimates of the effect of the implementation of the state-, county- and city-level PSL mandates on alternative measures of productivity and profitability over the 2004–2019 period. The dependent variables are $\ln(\text{Sales}_t/\text{Assets}_{t-1})$ (in Column (1)); TFP-IT (in Column (2)); TFP-ACF (in Column (3)); Gross profit over lagged assets (in Column (4)); EBIT over lagged assets (in Column (5)); Net income over lagged assets (in Column (6)). The key independent variable is PSL(0, 1) which equals one if the state, county, or city a firm headquartered in has implemented a PSL mandate, and zero otherwise. In Panel A, we use the original CCM sample. In Panel B, we use a stacked sample approach with an event window spanning three years before and four years after the implementation of each PSL mandate. We define cohorts based on each distinct treatment year in which at least one state, county, or city implements a PSL mandate, and append them to construct the stacked sample. In each cohort, the treatment group consists of firms subject to a PSL mandate implemented in the same year by their state, county, or city. The control group consists of firms that are never treated and those that have not yet been treated during the event window. Firm and state controls include $\ln(\text{Assets})_{t-1}$, Tangibility_{t-1}, $\text{Cash Holdings}_{t-1}$, Leverage_{t-1}, $\ln(\text{Number of Employees})_{t-1}$, $\text{Capital Expenditure}_{t-1}$, $\ln(\text{Age})_t$, GDP Growth_{t-1}, $\ln(\text{GDP})_{t-1}$, $\text{Unemployment Rate}_{t-1}$, and $\ln(\text{Union Coverage})_{t-1}$. All variables are defined in the Appendix. Standard errors clustered by state are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Original Sample

	$\ln(\frac{\text{Sales}}{\text{Assets}})$	TFP-IT	TFP-ACF	$\frac{\text{Gross Profit}}{\text{Assets}}$	$\frac{\text{EBIT}}{\text{Assets}}$	$\frac{\text{Net Income}}{\text{Assets}}$
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1)	0.069***	0.034**	0.049**	0.029***	0.020**	0.017*
	(0.025)	(0.016)	(0.020)	(0.010)	(0.009)	(0.009)
Firm Controls	YES	YES	YES	YES	YES	YES
State Controls	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Adj. R^2	0.839	0.683	0.686	0.791	0.756	0.680
Observations	41,792	30,075	30,028	41,792	41,792	41,792

Panel B: Stacked Sample [-3, 4]

	$\ln(\frac{\text{Sales}}{\text{Assets}})$	TFP-IT	TFP-ACF	$\frac{\text{Gross Profit}}{\text{Assets}}$	$\frac{\text{EBIT}}{\text{Assets}}$	$\frac{\text{Net Income}}{\text{Assets}}$
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1)	0.089***	0.039**	0.049**	0.036***	0.023**	0.021**
	(0.030)	(0.016)	(0.025)	(0.011)	(0.010)	(0.010)
Firm Controls	YES	YES	YES	YES	YES	YES
State Controls	YES	YES	YES	YES	YES	YES
Cohort-Firm FE	YES	YES	YES	YES	YES	YES
Cohort-Year FE	YES	YES	YES	YES	YES	YES
Adj. R^2	0.878	0.739	0.742	0.836	0.786	0.702
Observations	114,317	83,718	83,564	114,317	114,317	114,317

Table 5: Establishment-Level Labor Productivity

This table reports the DiD estimates of the effect of the implementation of the state-, county- and city-level PSL mandates on establishment-level labor productivity over the 2004–2019 period. The dependent variable is the logarithm of sales (in thousand) over lagged number of employees. The key independent variable is PSL$(0, 1)$ which equals one if the establishment's state, county, or city has implemented a PSL mandate, and zero otherwise. In Panel A, we use the original Data Axle sample. In Panel B, we use a stacked sample approach with an event window spanning three years before and four years after the implementation of each PSL mandate. We define cohorts based on each distinct treatment year in which at least one state, county, or city implements a PSL mandate, and append them to construct the stacked sample. In each cohort, the treatment group consists of establishments subject to a PSL mandate implemented in the same year by their state, county, or city. The control group consists of establishments that are never treated and those that have not yet been treated during the event window. In both panels, we include all active establishments with at least 50 employees in Columns (1) and (2), and use all active establishment with at least 18 employees in Columns (3) and (4). Industry fixed effects are defined at the 2-digit NAICS level. Standard errors clustered by state are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Original Sample

	$\geq$ 50 Employees		$\geq$ 18 Employees	
	(1)	(2)	(3)	(4)
PSL(0,1)	0.049***	0.059***	0.062***	0.068***
	(0.010)	(0.010)	(0.012)	(0.011)
Establishment FE	YES	YES	YES	YES
Year FE	YES	NO	YES	NO
Industry-Year FE	NO	YES	NO	YES
Adj. R^2	0.689	0.728	0.762	0.803
Observations	4,831,883	4,831,881	14,634,364	14,634,362

Panel B: Stacked Sample [-3, 4]

	$\geq$ 50 Employees		$\geq$ 18 Employees	
	(1)	(2)	(3)	(4)
PSL(0,1)	0.034***	0.041***	0.036***	0.041***
	(0.007)	(0.008)	(0.009)	(0.010)
Cohort-Establishment FE	YES	YES	YES	YES
Cohort-Year FE	YES	NO	YES	NO
Cohort-Industry-Year FE	NO	YES	NO	YES
Adj. R^2	0.737	0.765	0.805	0.834
Observations	14,975,745	14,975,734	45,734,833	45,734,822

Table 6: Cross-Sectional Differences in Health Externalities

This table reports the results from OLS regression relating firm performance to the implementation of the PSL mandates over the 2004–2019 period. The dependent variable in Columns (1) - (3) is ln(Sales_t/Number of Employees_{t-1}). The dependent variable in Columns (4) - (6) is EBITDA over lagged total assets. PSL$(0,1)$ is an indicator variable set to one if the state, county, or city a firm headquartered in has implemented a PSL mandate, and zero otherwise. In Panel A, we use data from Dingel and Neiman (2020) on the share of jobs that can be done remotely in each 2-digit NAICS sector as a proxy for the degree of physical presence requirement in the industry. *High Physical Presence* is an indicator variable set to one if the firm operates in a sector with a share of jobs that can be done remotely below or equal to the sample industry-year median, and zero otherwise. In Panel B, we interact PSL(0,1) with *High Outbreaks*, an indicator variable set to one if the firm operates in the manufacturing, retail trade, or transportation and warehousing industry, and zero otherwise. Firm and state controls include $\ln(\text{Assets})_{t-1}$, Tangibility_{t-1}, Cash Holdings_{t-1}, Leverage_{t-1}, ln(Number of $\text{Employees})_{t-1}$, Capital Expenditure_{t-1}, $\ln(\text{Age})_t$, GDP Growth_{t-1}, $\ln(\text{GDP})_{t-1}$, Unemployment Rate_{t-1}, and ln(Union $\text{Coverage})_{t-1}$. All variables are defined in the Appendix. Standard errors clustered by state are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Workplace Flexibility

	Labor Productivity			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1) × High Ph. Presence	0.122***	0.119***	0.136***	0.026***	0.025***	0.033***
	(0.034)	(0.033)	(0.046)	(0.008)	(0.006)	(0.008)
PSL(0,1)	-0.017	-0.093**		0.002	-0.015*	
	(0.019)	(0.041)		(0.005)	(0.009)	
Firm Controls	YES	YES	YES	YES	YES	YES
State Controls	YES	NO	NO	YES	NO	NO
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	NO	YES	NO	NO
State-Year FE	NO	YES	NO	NO	YES	NO
City-Year FE	NO	NO	YES	NO	NO	YES
Adj. R^2	0.846	0.846	0.847	0.761	0.762	0.767
Observations	41,792	41,747	33,293	41,792	41,747	33,293

Panel B: Health Risk (Vulnerability to Contagious Diseases (COVID-19))

	Labor Productivity			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1) $\times$ *High Outbreaks*	0.119***	0.117***	0.124***	0.025***	0.023***	0.029***
	(0.035)	(0.034)	(0.045)	(0.008)	(0.007)	(0.007)
PSL(0,1)	-0.011	-0.087**		0.004	-0.013	
	(0.018)	(0.041)		(0.005)	(0.009)	
Firm Controls	YES	YES	YES	YES	YES	YES
State Controls	YES	NO	NO	YES	NO	NO
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	NO	YES	NO	NO
State-Year FE	NO	YES	NO	NO	YES	NO
City-Year FE	NO	NO	YES	NO	NO	YES
Adj. R^2	0.846	0.846	0.847	0.761	0.762	0.767
Observations	41,792	41,747	33,293	41,792	41,747	33,293

Table 7: Cross-Sectional Differences in Human Capital

This table reports the results from OLS regression relating firm performance to the implementation of the PSL mandates over the 2004–2019 period. The dependent variable ln(Sales$_t$/Number of Employees$_{t-1}$) in Columns (1) - (3) and EBITDA over lagged total assets in Columns (4) - (6). PSL$(0,1)$ is an indicator variable set to one if the state, county, or city a firm headquartered in has implemented a PSL mandate, and zero otherwise. In Panel A, *High Wage* is an indicator variable set to one if a firm operates in a sector where the average wage is above or equal to the sample median of the 2-digit NAICS sector-year average wages, and zero otherwise. In Panel B, *High K/L* is an indicator variable set to one if the firm's capital-to-labor ratio is above the median in a year, and zero otherwise. Firm and state controls include ln(Assets)$_{t-1}$, Tangibility$_{t-1}$, Cash Holdings$_{t-1}$, Leverage$_{t-1}$, ln(Number of Employees)$_{t-1}$, Capital Expenditure$_{t-1}$, ln(Age)$_t$, and High Wage$_t$ in Panel A and High K/L$_t$ in Panel B. State controls include GDP Growth$_{t-1}$, ln(GDP)$_{t-1}$, Unemployment Rate$_{t-1}$, and ln(Union Coverage)$_{t-1}$. All variables are defined in the Appendix. Standard errors clustered by state are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Panel A: Industry Average Wage per Year

	Labor Productivity			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1) × High Wage	0.118***	0.114***	0.134**	0.027***	0.026***	0.033***
	(0.033)	(0.034)	(0.051)	(0.006)	(0.005)	(0.007)
PSL(0,1)	-0.003	-0.075**		0.003	-0.012	
	(0.016)	(0.035)		(0.006)	(0.008)	
Firm Controls	YES	YES	YES	YES	YES	YES
State Controls	YES	NO	NO	YES	NO	NO
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	NO	YES	NO	NO
State-Year FE	NO	YES	NO	NO	YES	NO
City-Year FE	NO	NO	YES	NO	NO	YES
Adj. R^2	0.846	0.846	0.847	0.761	0.762	0.767
Observations	41,792	41,747	33,293	41,792	41,747	33,293

Panel B: Firm-Level Capital-to-Labor Ratio

	Labor Productivity			ROA		
	(1)	(2)	(3)	(4)	(5)	(6)
PSL(0,1) × *High K/L*	0.111***	0.097***	0.092***	0.027***	0.021***	0.026**
	(0.023)	(0.024)	(0.028)	(0.006)	(0.006)	(0.010)
PSL(0,1)	0.007	-0.075***		0.006	-0.012	
	(0.020)	(0.025)		(0.008)	(0.010)	
Firm Controls	YES	YES	YES	YES	YES	YES
State Controls	YES	NO	NO	YES	NO	NO
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	NO	YES	NO	NO
State-Year FE	NO	YES	NO	NO	YES	NO
City-Year FE	NO	NO	YES	NO	NO	YES
Adj. R^2	0.847	0.847	0.848	0.762	0.763	0.768
Observations	41,627	41,582	33,128	41,627	41,582	33,128

Table 8: Cross-Sectional Differences in Social Capital

This table reports the DiD estimates of the effect of the implementation of the state-, county- and city-level PSL mandates on firm performance over the 2004–2019 period. The dependent variable in Columns (1) and (2) is ln(Sales$_t$/Number of Employees$_{t-1}$). The dependent variable in Columns (3) and (4) is EBITDA over lagged total assets. The key independent variable is PSL$(0,1)$ which equals one if the state, county, or city a firm headquartered in has implemented a PSL mandate, and zero otherwise. We interact PSL(0,1) with High SK, which is a dummy variable set to one if the company is headquartered in a high social-capital county, and zero otherwise. Firm and state controls include ln(Assets)$_{t-1}$, Tangibility$_{t-1}$, Cash Holdings$_{t-1}$, Leverage$_{t-1}$, ln(Number of Employees)$_{t-1}$, Capital Expenditure$_{t-1}$, ln(Age)$_t$, GDP Growth$_{t-1}$, ln(GDP)$_{t-1}$, Unemployment Rate$_{t-1}$, and ln(Union Coverage)$_{t-1}$. All variables are defined in the Appendix. Standard errors clustered by state are reported in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Labor Productivity		ROA	
	(1)	(2)	(3)	(4)
PSL(0,1) × High SK	0.057	0.100**	0.015	0.033**
	(0.062)	(0.040)	(0.019)	(0.014)
PSL(0,1)	0.061**	-0.050*	0.019	-0.008
	(0.028)	(0.025)	(0.011)	(0.008)
Firm Controls	YES	YES	YES	YES
State Controls	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
State-Year FE	NO	YES	NO	YES
Adj. R^2	0.847	0.847	0.761	0.762
Observations	40,854	40,809	40,854	40,809

Figure 1: Dynamic Effects with CCM Data: Labor Productivity

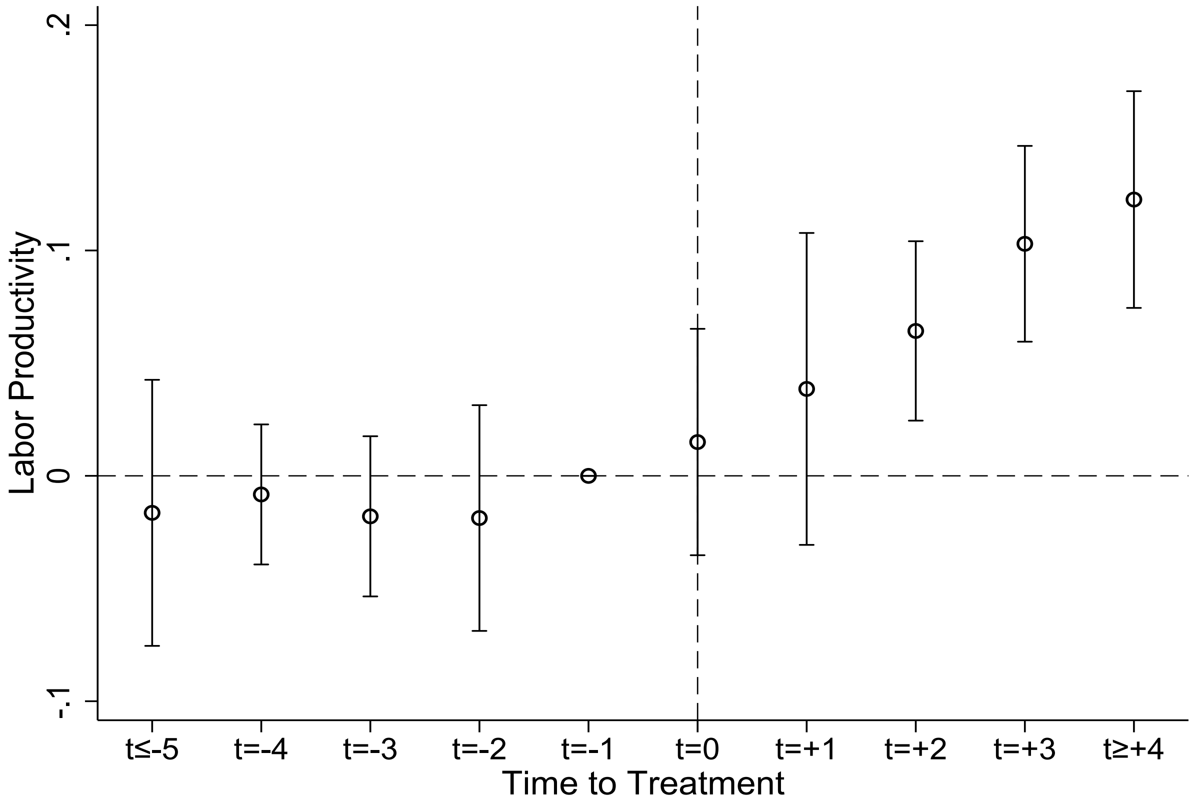

Figure 2: Dynamic Effects with CCM Data: ROA

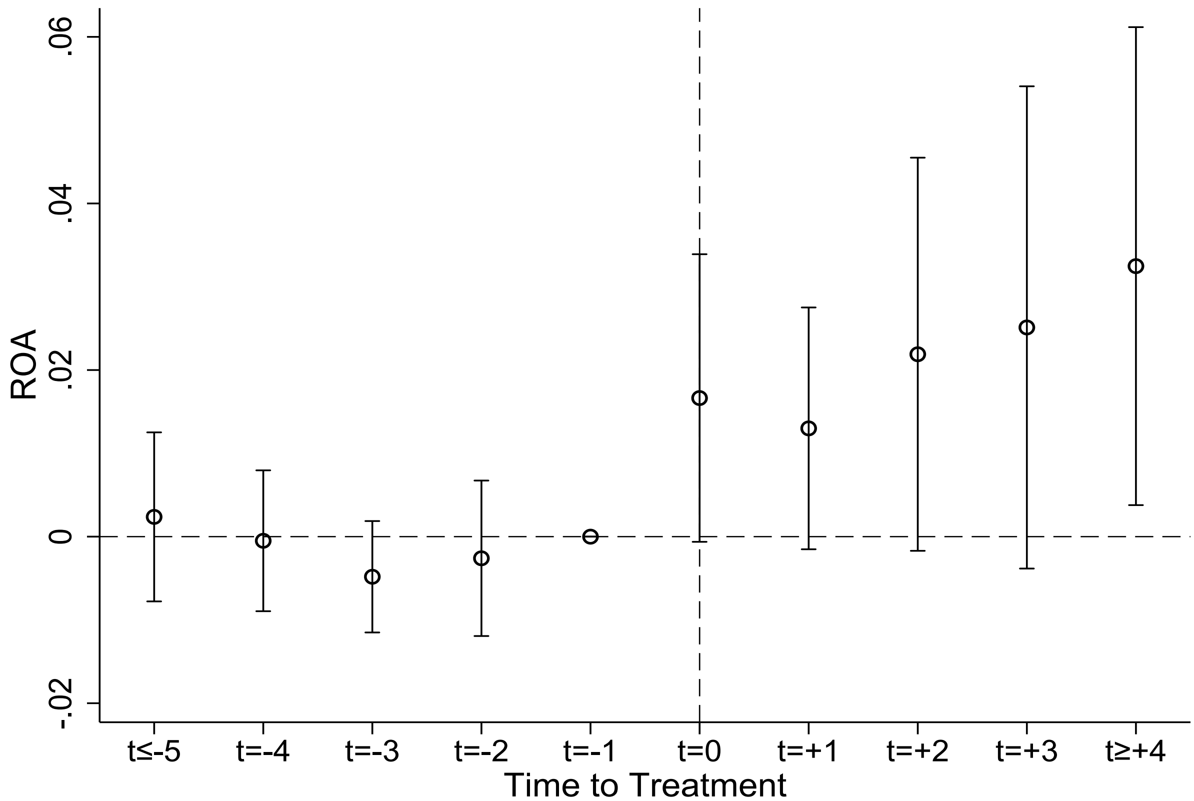

Figure 3: Labor Productivity: Establishment-Level Data ($\geq$ 50 Employees)

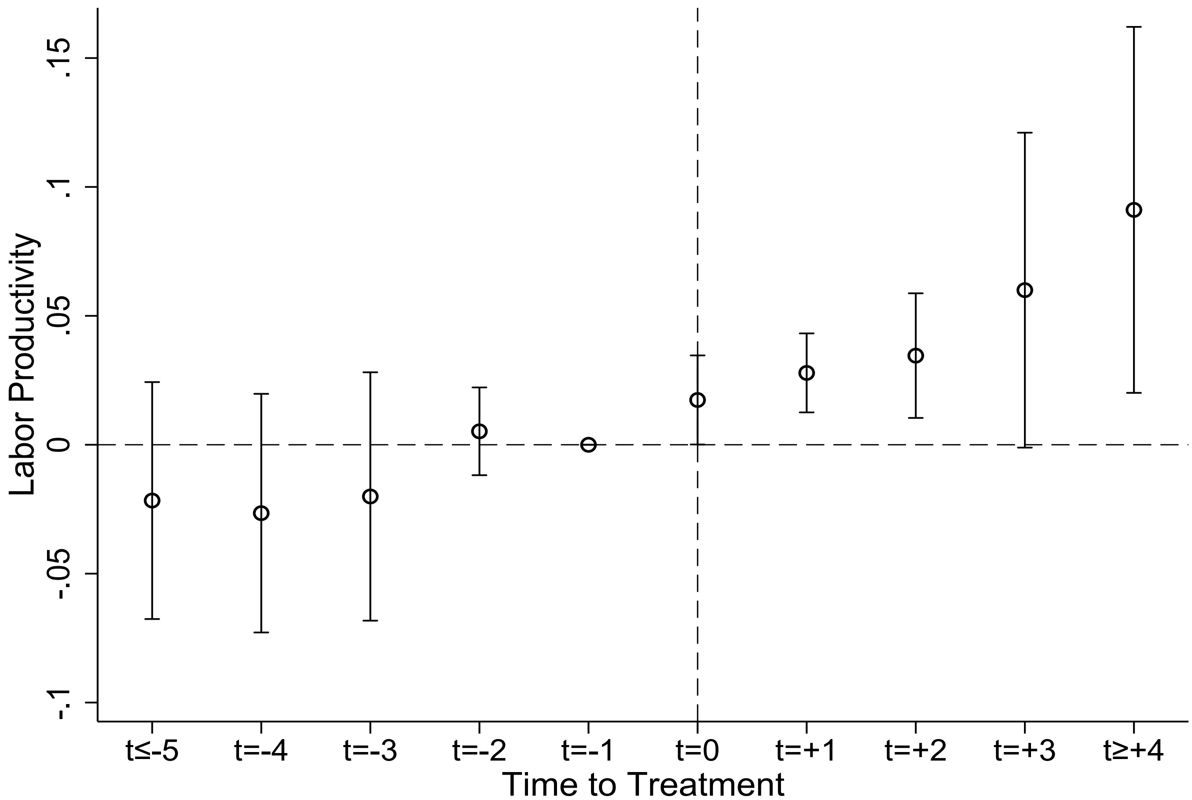

Figure 4: Labor Productivity: Establishment-Level Data
($\geq$ 18 Employees)