The effect of retaliation costs on employee whistleblowing

Jonas Heese
Harvard Business School
jheese@hbs.edu

Gerardo Pérez-Cavazos
Harvard Business School
gperezcavazos@hbs.edu

Abstract

We use large increases in unemployment insurance (UI) benefits to study the effects of retaliation costs on employee whistleblowing. Increases in UI benefits reduce the costs that arise from a job loss, one of the costliest forms of retaliation. We find that increases in UI benefits increase the number of facility-level employee workplace safety complaints filed with the regulator. Furthermore, UI benefit increases also result in more violations and more penalties. The effects are concentrated in firms where retaliation is more likely as measured by weaker employee relations, internal controls, and monitoring. Our findings show the importance of reducing retaliation costs to tap into employees' knowledge of misconduct.

Keywords: Employee Whistleblowing; Retaliation Costs; Labor Unemployment Insurance; Workplace Safety Inspections.

JEL Classifications: M40; M41

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1. Introduction

A large number of regulatory agencies in the United States rely on employee whistleblowers to enforce corporate misconduct regulations.[1] Rank-and-file employees are often the first party to observe signs of corporate wrongdoing and are therefore an important source of information for regulators (e.g., Bowen et al. 2010; Dyck et al. 2010). For example, Dey et al. (2020) examine over 2,000 whistleblower lawsuits and show that approximately 60% of them are filed by rank-and-file employees. Regulatory agencies, however, worry that rank-and-file employees who observe wrongdoing do not always share their information because of the high costs associated with whistleblowing. Although most whistleblower laws include anti-retaliation provisions, prior studies show that employers often retaliate against employees. For instance, Dyck et al. (2010) report that in 82% of the whistleblower cases in their study, the employee was fired, quit under duress, or had their responsibilities altered.[2] Despite the critical role that retaliation costs can play in an employee's decision to report corporate wrongdoing to regulators, the extent to which these costs affect rank-and-file employees' decisions to blow the whistle is unknown. In this study, we aim to document this effect.

Empirically, we exploit large increases in unemployment insurance (UI) benefits at the state level as a proxy for shocks to retaliation costs. We then examine whether increases in UI benefits affect rank-and-file employees' reporting of workplace safety issues at firms' facilities located in treated states.[3] We use this setting because it presents several advantages that help us identify the effects of retaliation costs on employee whistleblowing. First, a common challenge in examining the effect of new regulations (e.g., anti-retaliation provisions) on

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[1] For example, the Department of Defense (DOD), the Department of Health and Human Services (HHS), the Environmental Protection Agency (EPA), and the Securities and Exchange Commission (SEC) rely on whistleblowers to support their enforcement activities.

[2] As Dyck et al. (2010, p.2245) summarize "given these costs, the surprising part is not that most employees do not talk, but that some talk at all."

[3] Facilities include regional offices, manufacturing plants, distribution centers, and mines, among others.

behavior (e.g., employee whistleblowing) is that regulations often emerge as a response to an event, typically include various new provisions, and can be accompanied by increased enforcement (e.g., Leuz and Wysocki 2016). We overcome this challenge by focusing on large increases in UI benefits, which directly capture changes in the costs related to job loss, one of the costliest forms of retaliation. An advantage of this setting is that many states revise their UI benefits infrequently, leading to staggered large increases in benefits that can markedly reduce employees' retaliation costs by increasing their income security in the case of job loss (e.g., Agrawal and Matsa 2013; Dou et al. 2016). Moreover, Agrawal and Matsa (2013) show that state-level increases in UI benefits are unrelated to changes in the economy. In addition, it is unlikely that the state government's decision to increase UI benefits is related to employee whistleblowing on workplace safety issues.

Second, examining the effects of retaliation costs on employee whistleblowing is complicated, because a firm's actual misconduct is typically unobservable, making it difficult to identify whether a shock, such as the passage of an anti-retaliation provision, affects employee reporting of wrongdoing or changes actual wrongdoing. We overcome this issue by using a comprehensive data set on facility-level workplace safety inspections, accidents, and violations from the Occupational Safety and Health Administration (OSHA). This database covers a sample of 63,612 inspections and 120,564 workplace safety violations (resulting in penalties of approximately $413 million) of 43,514 unique facilities of 1,135 unique Compustat firms. These firms include approximately 80% of Fortune 100 and Fortune 500 firms from 2002 to 2017. The data set discloses which inspections are triggered by employee complaints, allowing us to measure employee whistleblowing.[4] The data also reveal which inspections are triggered by actual accidents allowing us to control for changes in firms' compliance with

[4] Throughout the paper, we refer to "inspections triggered by employee complaints" as "employee complaints."

workplace safety regulations. In addition, the data set identifies actual violations that were sanctioned by OSHA, so we can further investigate whether changes in employee whistleblowing also result in changes in violations and penalties.

Third, it is difficult to establish causality between retaliation costs and employee whistleblowing, as other unobservable factors unrelated to retaliation may affect employee whistleblowing.[5] We overcome this challenge by using large state-level increases in UI benefits to capture changes in the costs that local employees bear when they report workplace safety violations at firms' facilities. Following Dou et al. (2016), we examine employee whistleblowing following an increase in UI benefits of at least 10%, because such large increases are likely known to employees and substantially reduce their cost of job loss.[6] The data identify the facility where the inspection occurred. Thus, we can examine changes in whistleblowing at the facility level, allowing us to control for firm-level characteristics.

We find that facility-level employee reporting of workplace safety hazards significantly increases with the increase in UI benefits. An increase in UI benefits of at least 10% generates a 0.47% increase in the number of employee complaints filed with OSHA.[7] In terms of consequences, we find that an increase in UI benefits of at least 10% generates a 0.7% increase in the number of violations and a 4.3% increase in dollar penalties. Two inferences can be drawn from these results. First, additional complaints either have merit or increase regulatory scrutiny of facilities with an increasing number of complaints. Second, additional complaints generally relate to violations that are more severely punished by OSHA.

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[5] For example, the passage of an anti-retaliation provision often affects all firms (or all facilities of a firm) at the same point in time, making it difficult to appropriately control for time (or firm) trends.

[6] In our sample, the average increase in UI benefits following an increase of at least 10% is $1,347.

[7] Note that this is likely a lower bound estimate on the number of complaints, as we can only observe inspections triggered by complaints (not complaints per se) and multiple complaints could result in only one inspection. For example, if three employees complain about the same company in a short amount of time, this is likely to result in only one inspection, not three. It is also possible that a complaint results in no additional inspections.

Our identification strategy is a difference-in-differences methodology that exploits staggered increases in UI benefits over time. The first difference is the change in employee whistleblowing at each facility before and after the change in UI benefits of at least 10%. The control group consists of all other facilities located in states that did not experience a large increase in UI benefits. In particular, the control group includes facilities of the same firm and other firms that are located outside of the treated state. Thus, we are able to control for firm-level shocks that affect employee whistleblowing, as not all facilities of a given firm are located in the same state and hence are not simultaneously affected by an increase in a given state's UI benefits. The change in employee whistleblowing within this control group is the second difference captured in our tests. We estimate the effect of UI benefits on facility-level whistleblowing as the difference in those two differences.

We also include a set of facility-, firm-, state-, and county-level controls to ensure that our results are not driven by facility, firm, or macroeconomic conditions. Furthermore, we include facility fixed effects to control for time-invariant facility characteristics, and we include year fixed effects to control for general time trends. While our identification strategy controls for firm-specific shocks, facility and year fixed effects control for omitted factors at the local, firm, or facility level over time.

While the average effect of UI benefits on employee whistleblowing is positive and economically significant, we conjecture that various factors could attenuate that relation. In particular, we use cross-sectional tests to examine three distinct factors that potentially affect the likelihood of retaliation. First, increases in UI benefits that reduce the costs of job loss are likely less important for employees working in firms with a culture that is less prone to retaliation. We identify such firms by using data from Morgan Stanley Capital International

(MSCI) on firms' employee relations. We find that the effect of increases in UI benefits on employee whistleblowing is concentrated in firms that have weaker employee relations.

Second, firms with weak control systems are less likely to have processes in place to protect employees against retaliation. We use the incidence of internal control weaknesses as a proxy for firms with weak control systems. We expect that increases in UI benefits are likely more important for employee reporting in firms with internal control weaknesses. Consistent with this conjecture, we find that the effect of an increase in UI benefits on employee whistleblowing is concentrated in firms with internal control weaknesses.

Third, we conjecture that increases in UI benefits are likely more important for employees farther away from headquarters, because they are typically less protected from retaliation by their local supervisors. Consequently, we expect that the effect of increases in UI benefits on employee whistleblowing is more pronounced for facilities that are located farther away from headquarters. We find evidence consistent with this conjecture. Overall, the findings from our three cross-sectional tests suggest that UI benefit increases empower employees to report violations in situations that are more prone to retaliation.

A remaining concern is the possibility that changes in state-level UI benefits affect the behavior of employees at the facility. If increases in UI benefits result from poor local economic conditions, firms might focus on output at the expense of workplace safety (e.g., Heese and Pérez-Cavazos 2019), leading to more complaints. We address this possibility in six ways. First, we examine whether there is a contemporaneous change in accidents at the facility level. Second, we examine whether there is a change in inspections triggered by accidents at the facility level.[8] Third, we examine whether OSHA receives more referrals from other sources. Fourth, we examine whether OSHA schedules more inspections. We do not find a change in

[8] OSHA requires companies to notify OSHA of an accident, making accidents a reliable measure of behavior.

contemporaneous accidents or any of these three types of inspections. Fifth, we examine the timing of the increase in whistleblowing (Agrawal and Matsa 2013), finding that the effect on whistleblowing occurs *after* (and not before) UI benefits increase. Sixth, we examine facilities' sales and number of employees when UI benefits increase, and we do not find evidence of changes when benefits increase. These results rule out the notion that unobserved economic conditions affect facilities at the time of changes in UI benefits.

Finally, we examine the persistence of the effect of UI benefit increases on employee whistleblowing and explore additional consequences. We find that the effect disappears after approximately three years. We also find a decrease in accidents in the three- and five-year periods following a UI benefit increase, suggesting that reducing retaliation costs leads to an increase in firm compliance with workplace safety regulations over the long term.

Our study makes two main contributions. First, prior accounting studies show that rank-and-file employees play an important role in informing authorities about corporate misconduct (e.g., Bowen et al. 2010; Dey et al. 2020; Dyck et al. 2010), accelerate enforcement actions (e.g., Call et al. 2017), and deter future violations (e.g., Wilde 2017). The concern, however, is that a considerable amount of misconduct goes unreported because employees fear retaliation from their employer for blowing the whistle. Our study documents the extent to which reducing retaliation costs leads to an increase in employee whistleblowing of workplace safety violations. Our findings also extend to accounting violations, because those violations are often reported by rank-and-file employees and are typically comparable in size.[9]

[9] Salaries of employees who blow the whistle about financial matters and workplace safety are similar. Reviews of court documents show that rank-and-file employees reporting accounting violations earn approximately $50,000. According to Glassdoor, the salary of employees working at factories ranges from approximately $38,000 for a plant worker up to $71,000 for assembly supervisors and plant engineers. In addition, the Association of Certified Fraud Examiners finds that the average asset misappropriation fraud, which represents 89% of financial frauds, costs the organization $114,000 (ACFE 2018). Similarly, Bowen et al. (2010) find that two thirds of SOX-related cases brought by whistleblowers relate to non-severe issues.

Second, workplace safety is a universal concern (Glaeser and Guay 2017).[10] Yet, prior studies show that managers who face pressure to meet earnings benchmarks are willing to sacrifice worker safety (Caskey and Ozel 2017). Our paper contributes to this literature by showing that decreasing retaliation can constrain firms' practice to manage earnings by compromising workplace safety. Relatedly, our findings inform policy-makers and regulators by documenting how they can access employees' knowledge of misconduct via modest increases in compensation for job loss.

2. Background and Related Literature

2.1. Retaliation against Employee Whistleblowers

Employee whistleblowers play a critical role in uncovering corporate misbehavior, such as accounting fraud (e.g., Bowen et al. 2010; Dyck et al. 2010). For example, rank-and-file employees may observe signs of misconduct and could inform the authorities. Prior studies show that rank-and-file employees detect a large portion of accounting violations.[11] For instance, 50% of the whistleblower cases examined by Dyck et al. (2010) were exposed by rank-and-file employees. In a larger sample of 2,000 lawsuits filed by whistleblowers under the False Claims Act (FCA), Dey et al. (2020) show that nearly 60% of the cases are filed by rank-and-file employees. Similarly, the Association of Certified Fraud Examiners (2018) finds that whistleblowers help detect over 40% of corporate misconduct cases.

The value of whistleblowers has led to the enactment of anti-retaliation laws aimed at protecting whistleblowers. For example, Section 806 of the Sarbanes-Oxley Act (SOX) protects employees reporting wrongdoing that violates SEC regulation. OSHA is responsible

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[10] In 2017, 5,147 workers died on the job (BLS 2018), and companies spent more than $60 billion to comply with OSHA regulations (Kniesner and Leeth 2014).
[11] Two case examples are *Donaldson v. Severn Sav. Bank, F.S.B.* and *Deltek, Inc. v. Dep't of Labor*. In both cases, the employee filing the complaint disclosed either earning a salary under $60,000 or not having a college degree.

for investigating retaliation complaints, enforcing provisions from more than 20 whistleblower statutes, such as labor safety and SOX violations.[12] Despite the prevalence of anti-retaliation provisions, empirical evidence shows that whistleblowers are insufficiently protected against retaliation. For example, Earle and Madek (2007) examine 677 complaints filed under SOX and find that 499 were dismissed, 95 were withdrawn, and only 2% of cases were ruled in favor of the employee. Dyck et al. (2010) find that more than 80% of the whistleblowers in their sample were retaliated against by their firm, typically leading to job loss and often to extended periods of unemployment. Similarly, Dey et al. (2020) show that almost 80% of the whistleblowers mention to have suffered retaliation; more than 35% report being fired.

2.2. Economics of Whistleblowing

After observing misconduct, an employee's decision to report it to an external regulator likely depends on her assessment of the expected costs and benefits. Some of the benefits are monetary and are funded from recovered amounts.[13] For example, Dey et al. (2020) find that the expected monetary benefit for a whistleblower is approximately $200,000 ($406,000 for public firms), representing four (eight) times the annual compensation of the median employee working for a publicly traded firm. Additional rewards may have altruistic origins, with whistleblowers perceiving their actions as aimed at correcting wrongdoing that harms the interests of society (e.g., Arnold and Ponemon 1991; Vandekerckhove and Commers 2004).

The main costs of whistleblowing result from retaliation. While retaliation can take many forms, it generally leads to loss of income either through demotions, lack of advancement opportunities, terminations, or legal expenses. Dyck et al. (2010) conclude that whistleblowers

[12] For a detailed summary of the anti-retaliation provisions enforced by OSHA, please see https://www.whistleblowers.gov/sites/wb/files/2019-06/whistleblower_acts-desk_reference.pdf.

[13] For example, qui tam lawsuits filed under the False Claims Act allow whistleblowers (or relators) to receive between 15% and 30% of the recovered amounts. OSHA does not provide monetary rewards for whistleblowing.

face weak incentives and do not gain much from revealing wrongdoing. Survey evidence supports this finding. For instance, in a 1992 survey of 1,500 federal workers, Zingales (2004) finds that 25% of employees reported that they experienced verbal harassment and intimidation, 20% were shunned by coworkers and managers, 18% were assigned to less desirable duties, and 11% were denied a promotion. Similarly, Brickley (2003), who conducted a review of 200 whistleblower complaints filed with the National Whistleblower Center in 2002, finds that 50% of whistleblowers were fired after they reported misconduct, and the other 50% faced various on-the-job harassment and disciplinary actions.

The insights from our setting also extend to accounting violations, as employees face similar economic forces in both settings. For instance, although UI increases appear modest, they represent a sizable portion of rank-and-file employees' salary, which can thus affect their decision to report wrongdoing to the authorities. In addition, although the average workplace safety violation is not large, the vast majority of accounting frauds are comparable in size (ACFE 2018).[14] For example, Bowen et al. (2010) find that two thirds of SOX-related cases brought by whistleblowers relate to non-severe issues labeled as "other allegations," "allegations without detailed information," or "other accounting-related allegations."

3. Data and Setting

3.1. OSHA Workplace Safety and Health Inspections

The Occupational Safety and Health Administration (OSHA) was created in 1970 and is part of the Department of Labor (OSHA 2019a). OSHA is the federal regulator that oversees workplace safety for most private sector employers and their employees in the United States. OSHA is responsible for monitoring the safety and health conditions of approximately 130

[14] According to the Association of Certified Fraud Examiners (2018), 55% of financial frauds cost the firm less than $200,000.

million workers employed across 8 million different worksites. To that end, OSHA promulgates and enforces standards and regulations related to equipment safety, communication of hazards, and training programs, among many other issues.[15] In 2018, OSHA employed approximately 2,100 inspectors and conducted 73,013 inspections (OSHA 2019b).

OSHA inspections can be triggered by accidents, employee complaints, or referrals from other federal, state, or local agencies; they can also be planned. Planned inspections are scheduled based on random selection, according to special emphasis on a specific industry or type of hazard or as a consequence of the violation history of a firm (with a typical facility having less than a 1 in 100 chance of being inspected in a given year). Employees can trigger an inspection by filing a complaint with OSHA (via https://www.osha.gov/pls/osha7/eComplaintForm.html). In their filing, employees must disclose their identity and their employer's name, and they must provide detailed information about the hazard and contact information of witnesses who can corroborate the claim.[16] If OSHA conducts an inspection, it shares the detailed complaint with the employer after removing the whistleblower's name. Although the investigation process is supposed to protect the whistleblower's identity, OSHA receives thousands of retaliation complaints every year.

OSHA distinguishes among three types of violations: serious, willful and repeated, and other-than-serious.[17] Other-than-serious and serious violations involve a penalty between \$8,908 and \$12,471 per violation, whereas willful violations involve a penalty of up to \$124,709 per violation (OSHA 2016a). Beyond these civil penalties, the Occupational Safety

[15] For a list of OSHA standards see https://www.osha.gov/law-regs.html.

[16] Under Section 17(g) of the OSH Act, it is unlawful to make any false statement, representation, or certification in any complaint. Violations can be punished with a fine up to \$10,000, or by imprisonment up to 6 months.

[17] A violation is serious "if there is a substantial probability that death or serious physical harm could result from a condition which exists", a violation is willful and repeated "where an employer has demonstrated […] an intentional disregard […] for employee safety and health", and a violation is other-than-serious "if the accident/incident or illness that would be most likely to result from a hazardous condition would probably not cause death or serious physical harm".

and Health Act also established criminal sanctions for violations resulting in the death of a worker, unauthorized notice of upcoming inspections, or falsifying business records.

Section 11(c) of OSHA prohibits retaliation against employees who complain about unsafe or unhealthful conditions. Employees can file a retaliation complaint with OSHA. If OSHA decides to investigate and finds evidence of retaliation, the firm can be subject to penalties (OSHA 2016b). According to OSHA (2019c), less than 30% of the complaints filed by whistleblowers result in a settlement. The average investigation of a complaint takes nearly 10 months, in part because OSHA's investigative resources are limited (Toris 2019).

We obtain data on OSHA inspections, penalties, and violations from several OSHA datasets available at https://enforcedata.dol.gov/views/data_summary.php. These datasets include information on the type of inspection, the facility where the inspection or violation occurred, and the size of the penalty, among other information. We begin with a total of 1,617,278 inspections conducted by OSHA from 2002 to 2017. We then use the name of the facility inspected to link these facilities to a publicly traded parent firm. We are able to link 70,814 inspections to 1,185 unique Compustat firms. Last, we drop facility-year observations missing control variables. Our final data set consists of 63,612 inspections conducted in 43,514 unique facilities of 1,135 unique firms. Approximately 80% of Fortune 100 and Fortune 500 firms appear in our sample. Table 1, Panel A describes our sample composition.

Panel B provides a breakdown of the number of inspections triggered by employee complaints and total inspections by year. Inspections and complaints in our sample are scattered across time, with 2007 being the year with the highest number of inspections (6.7% of the total) and 2014 the year with the highest number of complaints (7.2% of the total).

– Insert Table 1 here –

3.2. Unemployment Insurance Benefits in the U.S.

The U.S. unemployment insurance system provides temporary income to eligible workers who 1) are involuntarily unemployed and 2) are seeking new employment. While the basic UI framework is common nationwide, each state has the autonomy to set its program's parameters (i.e., eligibility, the duration of benefits, and the amount of benefits paid to unemployed workers). UI benefits provided to eligible workers are based on formulas determined by state law. Typically, a state's benefit formula calculates the earnings realized by the worker in four of the last five quarters and seeks to replace approximately half of those wages through weekly payments, subject to minimum and maximum bounds. Significant variation exists in the level of total maximum benefits across states.[18] In 2017, (the last year in our sample), Florida paid the lowest total maximum benefits ($3,300 per year) and Massachusetts the highest ($33,090 per year).

Several anecdotes illustrate that factors unrelated to economic conditions drive changes in unemployment benefits (see also Agrawal and Matsa 2013). In 2008, Louisiana passed a law that required the state to increase its unemployment benefits if the state's unemployment insurance trust fund balance exceeded $1.4 billion. In 2009, the balance exceeded that threshold, resulting in an increase of weekly unemployment benefits from a maximum of $258 to $284 (Louisiana Budget Project 2009). In 2017, the District of Columbia increased its weekly maximum benefits from $359 to $425 because a coalition of unemployed workers, union representatives, and other advocates petitioned the lawmakers to adjust UI benefits which had stagnated at $359 per week since 2005 (Hagner 2018).[19] There are various ways in which

[18] States finance UI benefits through payroll taxes levied on employers. The firm-level effective tax rates are determined by the unemployment history of the individual firm through a method referred to as experience rating. States have the authority to set their own state-level experience-rating schemes. Consequently, firms with a recent history of layoffs have higher effective unemployment tax rates under this system.

[19] Agrawal and Matsa (2013) present additional examples pertaining to their sample period. For example, New York increased its UI benefits by 36% in 1990, its first increase in five years. The long delay and eventual large adjustment were tied to political haggling over unrelated workers' compensation and other laws (Verhovek 1989).

employees may learn about large increases in UI benefits. For example, large increases in UI benefits are often widely discussed in state politics and are part of politicians' political agendas. Employees might also learn about benefits through local media or social interactions with friends or family members who are receiving benefits.

We gather data about state unemployment insurance laws from the U.S. Department of Labor.[20] In particular, the Department of Labor reports semiannually state-by-state information about the maximum number of benefit weeks payable and weekly benefit amount. Following Agrawal and Matsa (2013) and Dou et al. (2016), we calculate the maximum UI benefits as the product of weekly benefits and number of weeks allowed and use it to measure the generosity of each state's UI benefits.[21] States often keep UI benefits unchanged for multiple years, frequently leading to years that have large increases.

Following Dou et al. (2016), we use the maximum total benefits to identify state-year events that see UI benefit increases of at least 10% while not following an increase greater than 10% in unemployment benefits in the prior year. Table 2 shows the distribution of the 27 events during our sample period. These events are well distributed across states and years. Table 2 also shows the percentage and dollar change around these events. The average percentage change is 16.8% and the average change in dollar amount is $1,347. This magnitude is meaningful, as a large fraction of the U.S. workforce lives paycheck to paycheck.[22]

– Insert Table 2 here –

4. Empirical Methodology

[20] Information on state UI laws can be found at: https://oui.doleta.gov/unemploy/statelaws.asp.

[21] Agrawal and Matsa (2013) show that measuring the generosity of UI systems as the maximum total benefits is appropriate and closely tracks actual benefits paid by states (the elasticity of maximum total benefits to actual compensation payments is approximately 0.9).

[22] Prior work documents a substantial increase in expenditures at payday (e.g., Stephens 2003; Shapiro 2005).

We examine the effect of increases in UI benefits on facility-level employee reporting of workplace safety violations using a difference-in-differences methodology. The basic regression we estimate is as follows:

$$Complaints_{i,j,s,t} = \alpha_0 + \alpha_1 Treatment_{s,t} + Controls + \alpha_j + \alpha_t + \epsilon, \qquad (1)$$

where the dependent variable $Complaints_{i,j,s,t}$ is the natural logarithm of one plus the total number of employee complaints filed with OSHA that firm i incurred related to potential OSHA violations in its facility j, located in state s, during year t. The main explanatory variable $Treatment_{s,t}$ takes the value of 1 for facility j in the year of an increase in state unemployment benefits of at least 10%, and 0 otherwise.

This identification strategy allows us to employ a difference-in-differences methodology exploiting changes in UI benefits over time. To better understand the intuition of our difference-in-differences tests, consider the substantial state-level increase in UI benefits in Massachusetts in 2007, for example. The first difference is the change in employee OSHA complaints in each facility located in Massachusetts before and after the increase in UI benefits. The implicit control group at time *t* consists of all other facilities located in states that did not experience an increase in UI benefits (this control group includes *both* facilities of the same firm that are not located in Massachusetts and facilities of other firms that are not located in Massachusetts). The change in employee OSHA complaints within this control group is the second difference captured in our tests. The effect of increases in UI benefits on facility-level employee reporting is estimated as the difference in those two differences. Figure 1 provides a graphical summary of our research design in the context of the Massachusetts example.

– Insert Figure 1 here –

Our tests include the following facility, firm, state-level, and county-level *Controls*. At the facility level, we control for size (measured as the natural logarithm of facility-level revenues and employees). At the firm level, we control for size (measured as the natural logarithm of a firm's market value), profitability (measured as return on assets), and leverage. At the county level, we control for the unemployment rate to control for the macroeconomic conditions at the facilities' locations. At the state level, we control for the gross domestic product (GDP) growth rate to capture the macroeconomic conditions in the state where the facility is located. Similar to Agrawal and Matsa (2013) and Dou et al. (2016), we control for county-level and state-level factors, as changes in UI benefits can be driven by macroeconomic conditions, which may affect facility-level reporting. Variables are defined in the Appendix.

We include facility fixed effects to control for time-invariant facility characteristics such as the level of workplace safety violations, and we include year fixed effects to control for general time trends such as annual changes in OSHA's budget. While our identification strategy controls for firm-specific shocks, facility and year fixed effects control for omitted factors at the local, firm, or facility level over time. Standard errors are clustered at the firm level, which accounts for potential time-varying correlations in unobserved factors that affect different facilities within the same firm.[23] As described in Section 5.4.3., the findings are robust to more complex fixed-effects structures.

Table 3 provides descriptive statistics on the variables included in our tests. The average facility has 0.097 inspections per year, out of which 0.034 are triggered by employee complaints. The average facility has 0.184 violations and the average penalty is approximately \$629.[24] On average, facilities employ 258 employees and generate \$3.5 million in sales. On average, the facilities belong to firms that have \$33.9 billion in market value, a return on assets

[23] Our results are robust to clustering standard errors at the state level (untabulated).
[24] Note that the average penalty value is based on the full sample of facility-year observations, including a large number of observations without a violation. Conditional on receiving a violation, the average penalty is \$22,503.

of 5%, and leverage of 2.36; they are located in areas with an average GDP growth rate of 3.93% and an unemployment rate of 5.83%.

– Insert Table 3 here –

5. Results

5.1. Main Results

To test our hypothesis, we examine whether the number of employee complaints related to workplace safety violations changes at facilities when UI benefits increase by at least 10%; we report the results in Table 4. To do so, we examine the natural logarithm of one plus the number of employee complaints per facility and year (denoted *Complaints*) using equation (1). If increases in UI benefits increase whistleblowing, we expect that the number of inspections triggered by complaints per facility increase after an increase in UI benefits.

Columns 1-3 in Table 4 report the results from estimating equation (1). The main difference across these models is the inclusion of different *Controls*. Specifically, column 1 does not include any *Controls*. Column 2 includes facility- and firm-specific *Controls*, and column 3 also includes facility-location *Controls*. All models include facility and year fixed effects. In all models, the coefficient associated with the treatment effect is positive and significant (p-value < 0.01). The results are also economically significant. For the average facility, an increase in UI benefits of at least 10% increases the number of employee complaints by approximately 0.47% (based on Table 4, column 3).

These effects are also economically meaningful. To contextualize this magnitude, consider that the average whistleblower who is retaliated against loses approximately the difference between 1.1 years of salary (Dey et al. 2020) and UI benefits. In 2018, the average maximum UI benefits amounted to $11,857 per year, and the average salary for the median employee of a publicly traded firm was $50,603 per year, calculated using median pay from annual proxy statements and number of employees from S&P Global Market Intelligence (Serkez and Francis 2019). Considering the average salary of the median employee and the average UI benefits across the U.S. yields a loss of approximately $43,806. To reduce this loss by 10%, the average whistleblower would require an additional $4,381 in UI benefits. Dividing this figure by $1,347, the average increase in UI benefits for our treatment states, results in a multiplier of 3.3. In other words, we can calculate a back-of-the-envelope estimate of the increase in complaints by multiplying the 0.47% coefficient on UI benefits shown in Table 4 by 3.3. The product of these numbers indicates that reducing retaliation by 10% would result in the number of complaints increasing by 1.5%. It is important to note that this is a back-of-the-envelope calculation, and as such, it involves a number of assumptions. For example, this calculation assumes that we can linearly extrapolate the marginal effects that we document.[25]

While Section 5.4. presents a large set of tests intended to mitigate identification concerns, our control variables, such as those for state- and county-level conditions, have a minimal effect on the estimated association between UI benefits and employee reporting. Consequently, an omitted variable that explains the findings would have to be uncorrelated with our control variables and have a consistent effect over time (despite changes in facility-, firm-, county-, or state-level conditions).

– Insert Table 4 here –

5.2. Violations and Penalties Following Employee Complaints

A question that arises from our primary findings is whether these additional employee complaints result in additional violations and penalties and hence have consequences for firms.

[25] An alternative approach to assess the economic significance of our findings is to calculate the incremental adjusted R-squared for the model in Table 4, column 3. We find that the incremental R-squared of *Treatment* is similar to the incremental R-squared of *Firm-Level* and *County-Level Controls*, and half of the *Year Fixed Effects* (untabulated). When assessing this magnitude, it is important to note that *Facility-Level Controls* and *Facility Fixed Effects* are likely to capture elements of retaliation costs, reducing the explanatory power of *Treatment*.

On one hand, it is possible that the reduction in costs leads disgruntled employees to file meritless allegations that waste the resources of regulators and accused firms alike (Bok 1980; Gobert and Punch 2000), and therefore do not lead to additional violations and penalties. On the other hand, if the additional complaints have merit or raise regulatory scrutiny of facilities with an increasing number of complaints, we would expect to observe an increase in violations and penalties.[26] To examine these two possible effects, we test whether increases in UI benefits also result in increases in the dollar penalties paid for workplace safety violations as well as increases in the number of workplace safety violations.

As shown in Table 5, we find a positive and significant coefficient on *Treatment* using either the natural logarithm of one plus the dollar penalties (column 1) or the natural logarithm of one plus the total number of violations (column 2) as the dependent variables. These findings indicate that the additional employee complaints lead to additional violations and penalties. In particular, we find that the number of violations increases by a similar magnitude as the number of complaints (0.70%, compared to 0.47%), and we also find that penalties increase by 4.3% in response to a UI benefit increase, suggesting that the additional violations are more serious.

We present back-of-the-envelope calculations to help better illustrate the economic magnitude of these effects. As calculated in the previous section, UI benefits would need to increase by a factor of 3.3 to reduce the cost of job loss by 10% for the median employee. Under this scenario, OSHA violations and penalties would then increase by approximately 2.3% and 14.0% respectively.[27] From the perspective of a facility, this would represent an increase in the expected annual penalties of $579 (the average penalty per violation is $22,503, but, on average, only 0.184 violations occur per year). To put this magnitude into context, we

[26] Our tests cannot distinguish which of these two mechanisms, i.e., merit or regulatory scrutiny, primarily explains our results.
[27] We estimate these factors by multiplying 3.3 times 0.0070 and 0.0432 respectively.

scale it by the average annual profit of a facility, \$299,710. This translates into a 0.2% reduction in annual profit for the average facility.[28] When interpreting these magnitudes, it is important to consider that these tests capture only the legal penalties associated with violations, but do not capture other costs incurred by firms, such as litigation and reputational costs.[29] Thus, our tests provide a lower-bound estimate of the cost a firm incurs. The magnitude of the effect also provides a likely explanation for firms' decisions to retaliate despite anti-retaliation provisions: the benefits of retaliation can be significant.

– Insert Table 5 here –

5.3. Cross-Sectional Tests

In this section, we examine a number of cross-sectional predictions derived from our main hypothesis. More specifically, the cost of retaliation can be decomposed into the product of the *likelihood of retaliation* times the *loss from retaliation*. While our treatment variable captures a reduction in the magnitude of the loss, it does not affect the likelihood of retaliation. We expect that firm- or facility-level variation in (1) employee relations, (2) quality of the firm's control systems, and (3) distance to headquarters captures differences in the likelihood of retaliation, affecting the intensity of our treatment. For example, we expect that firms with stronger employee relations are less likely to retaliate against employees, lessening the effect of UI increases on employee whistleblowing.[30]

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[28] We calculate the average profit of a facility by using the average sales for the facilities in our sample (\$3,526,000) and the average net margin of their parent companies during our sample period (8.5%).

[29] In addition to regulatory fines, companies face other costs from workplace safety violations such as litigation, workers' wage demands, the ability to win contracts, investor perceptions, and the ability to attract and retain skilled employees (Caskey and Ozel 2017). Injury frequency and severity increase workers' compensation insurance premiums. Additionally, companies can face wage differentials to compensate employees for injury risk (Viscusi 2010; Wei 2007), and injuries can impede firms' ability to win contracts that specify a minimum safety record as a prequalification condition (e.g., U.S. Army 2012, §4). These additional costs can be substantial. Karpoff et al. (2008) estimate that the reputational loss exceeds legal penalties by a factor of 7.5.

[30] The tension in these tests stems from the possibility that variation in our cross-sectional variables also captures variation in the loss from retaliation. For example, if firms with weaker control systems are more likely to block future career opportunities for whistleblowers, it is possible that UI benefit increases do not affect their employees'

5.3.1. Strong Employee Relations

Increases in UI benefits that reduce the costs of job loss in the event of retaliation are likely less important for employees working in firms with a culture that is less prone to retaliation. To identify such firms, we use the Environmental, Social, and Governance (ESG) statistics compiled by MSCI. The database contains indicators that identify positive and negative employee relations practices. Negative employee relation practices, for example, include union relations concerns, workforce health and safety concerns, supply chain controversies, child labor controversies, labor management controversies, and restrictions of employee rights. *Strong Employee Relations* takes the value of 1 if the number of employee strengths is greater than or equal to the number of employee concerns for a given firm, and 0 otherwise. The sample for these tests is smaller as data on firms' employee relations are missing for some firms. We expect that the effect of UI benefit increases on employee whistleblowing is stronger for firms with weaker employee relations.

We split our sample into firms with weaker and stronger employee relations and run our primary test separately for these two subsamples. As shown in Table 6, Panel A, the coefficient on *Treatment* is positive and significant for firms with weaker employee relations but is not significant for firms with stronger employee relations. The difference in the coefficients across the two subsamples is also statistically significant. These results are consistent with our expectation that the treatment intensity is higher for firms that are more likely to have a culture prone to retaliation.

5.3.2. Control Systems

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decisions to blow the whistle. Conversely, if firms with stronger control systems do not engage in severe retaliatory practices, employees might, at the margin, be more likely to blow the whistle after UI benefits increase.

Second, firms with weak control systems are less likely to have processes in place to protect employees against retaliation. We use the incidence of internal control weaknesses as our measure to identify firms with weak control systems. We expect that UI benefit increases are likely more important for whistleblowing in firms with internal control weaknesses.

To examine this conjecture, we define *Internal Control Weakness* as an indicator equal to 1 in the year of an internal control weakness, and 0 otherwise. These tests are limited to the period 2004 to 2017, as data on internal control weaknesses became available only as of 2004. As shown in Table 6, Panel B, the coefficient on *Treatment* is positive and significant only in the subset of firms with an internal control weakness. The difference in the coefficients across the two subsamples is also statistically significant. Thus, consistent with our conjecture, we find that the effect of UI benefit increases on employee whistleblowing is concentrated in firms with internal control weaknesses.

5.3.3. Distance to Headquarters

In this section, we examine whether the effect of an increase in UI benefits on employee misconduct is lower when within-firm monitoring is stronger. Thus, in contrast to the previous cross-sectional tests, this test exploits within-firm as opposed to across-firm differences. To measure within-firm monitoring, we calculate the geographical distance between headquarters and each facility. This measure is motivated by research that shows that larger geographical distance is associated with information disadvantages (e.g., Audretsch and Feldman 1996; Heese and Pérez-Cavazos 2019; Ivkovich and Weisbenner 2005). Employees located farther away from headquarters are less protected from retaliation by their local supervisors. Hence, we expect that the effect of UI benefit increases on employee whistleblowing will be stronger for the facilities located farther away from headquarters.

We separately run our primary model for facilities with a larger (above median) and lower (below median) *Distance to Headquarters*. As shown in Table 6, Panel C, the coefficient on *Treatment* is positive and significant only in the subset of facilities with a larger distance from headquarters. The difference in the coefficients across the two subsamples is also statistically significant. These results suggest that the effect of UI benefits on employee whistleblowing is concentrated in facilities located farther away from headquarters, as distance reduces monitoring by headquarters.

– Insert Table 6 here –

5.4. Further Tests

We address two potential concerns. First, unobservable companywide changes may affect the incidence of facility-level employee complaints. Specifically, UI benefit increases could increase actual misconduct that we capture through increased employee whistleblowing. Second, it is possible that local economic conditions lead states to change UI benefits and result in more facility-level misconduct. In this case, the relation between UI benefits and facility-level employee complaints could be spurious. We provide a number of tests to mitigate these concerns and examine robustness.

5.4.1. Accidents and Other Inspections

A concern with our findings is the possibility that changes in UI benefits affect the actual behavior of employees and firms. For example, increases in state-level UI benefits could be the result of poor local economic conditions. In response to such conditions, employees and local managers could focus on output at the expense of workplace safety (e.g., Heese and Pérez-Cavazos 2019), potentially explaining a higher level of complaints. Hence, our previous tests would simply capture the actual change in underlying behavior. To rule out this alternative, we examine whether the number of accidents and the number of inspections triggered by accidents and other stakeholders are influenced by UI benefits. In particular, we examine whether UI benefits affect the number of accidents, inspections triggered by accidents, referrals from other parties, or planned inspections by OSHA. If employees increase misconduct or firms make changes to their operations in response to UI benefit increases, these changes should result in more accidents. Empirically, we measure the number of accidents using three different accident rates reported by OSHA and via the number of inspections triggered by accidents.

Accident data come from the OSHA workplace safety survey that has been publicly released for the period 2002 to 2011 (Caskey and Ozel 2017).[31] We obtain these data from Caskey and Ozel. As shown in Table 7, columns 1-3, we do not find a change in the *Total Case Rate* (*TCR*), the *Days Away, Restricted, or Transferred Rate* (*DART*), or the *Days Away from Work Injury and Illness Rate* (*DAFWII*) in response to UI benefit increases of at least 10%.[32] Next, we use investigations triggered by accidents as a proxy for accidents, which allows us to capture actual accidents over the full time period of our study.[33] Table 7, column 4 reports the results from rerunning equation (1), using *Accidents*, the natural logarithm of one plus the number of inspections triggered by accidents as the dependent variable. As shown in Table 7, column 4, the coefficient on *Treatment* is positive and insignificant, indicating that UI benefits do not affect inspections triggered by accidents. These results mitigate the concern that UI benefit increases simply increase actual misconduct.

To mitigate this concern more thoroughly, we also examine whether UI benefits affect inspections triggered by referrals or planned inspections. If increases in UI benefits increase misconduct, we would expect to find an increase in inspections triggered by referrals from

[31] Firms are required to report accidents to OSHA. OSHA enforces these requirements via penalties and audits.
[32] For a detailed explanation of these rates, please see https://www.bls.gov/iif/osheval.htm.
[33] Both databases are likely to have some degree of underreporting of accidents. For example, Caskey and Ozel (2017) point out that a summary of the 1996 to 1998 audits indicate that about 20% of establishments had recordkeeping errors in the OSHA workplace safety survey, to which OSHA responded in 2001 with revised guidelines and practices to improve recordkeeping (Federal Register number 66:5916-6135).

other federal, state, or local agencies. Similarly, OSHA might increase its number of planned inspections if it expects increases in UI benefits to increase misconduct. Table 7, columns 5 and 6 report the results from rerunning equation (1), using either *Referrals*, the natural logarithm of one plus the number of inspections triggered by referrals, or *Planned Inspections*, the natural logarithm of one plus the number of planned inspections, as dependent variables. As shown in Table 7, columns 5 and 6, the coefficient on *Treatment* is insignificant, indicating that UI benefits do not affect inspections triggered by referrals or planned inspections, further mitigating this concern.

– Insert Table 7 here –

5.4.2. Timing

Next, we examine the timing of the relation between changes in UI benefits and employee whistleblowing. If unobservable state-level economic conditions have some persistence, they might affect the relation between UI benefits and employee reporting. Similar to Agrawal and Matsa (2013) and Dou et al. (2016), we examine this possibility by including controls for lead and lag *Treatments* in our primary model.

As shown in Table 8, column 1, including these additional variables does not attenuate the estimated association between contemporaneous UI benefits and employee reporting. In addition, the coefficients on lead and lag *Treatments* are small in magnitude and statistically insignificant. These results indicate that UI benefits are associated with subsequent increases in employee reporting and not vice versa, mitigating the likelihood of omitted variable bias.

5.4.3. Different Fixed Effects Structures

As described above, our primary analyses include facility and year fixed effects. In this section, we examine whether our results are robust to more complex fixed-effects structures. First, we add interactions between industry fixed effects and year fixed effects to control for industry-specific shifts over time. As shown in column 2 of Table 8, our results are robust to the inclusion of industry-year fixed effects. Second, we add firm-year fixed effects to control for changes in firms' efforts to affect employee reporting over time. Companywide changes, such as the introduction of new policies can affect facility-level whistleblowing. Although it is unlikely that companywide changes are correlated with state-level UI benefits, this test helps us rule out this alternative explanation. As shown in Table 8, column 3, our results hold.

5.4.4. Facility-Level Sales and Employees

Similar to Agrawal and Matsa (2013), we also examine the effects of UI benefits increases on facilities' sales (Table 8, column 4) and number of employees (Table 8, column 5). As shown in columns 4 and 5, the coefficient on *Treatment* is not statistically significant, indicating that increases in UI benefits have no significant relation to facility's sales or number of employees. These results help rule out the concern that unobserved economic conditions are driving both UI benefits and complaints.

– Insert Table 8 here –

5.4.5. Falsification Tests

To further rule out that our main findings may be driven by a general time trend or other events unrelated to UI benefits at the state level, we run a falsification test in which we assign the UI benefits from a random state to each facility by drawing from a uniform distribution and then estimating our main model. We repeat this randomization procedure 1,000 times using *Complaints*, *Penalties,* and *Violations* as our dependent variables.

Table 9 reports the results. We report both the average β_1 coefficients of estimating the model with the random data 1,000 times and the $\hat{\beta}_1$ coefficients estimated using the actual data. Using the three dependent variables, the coefficients based on the random data are close to zero and differ statistically and economically from the results based on the actual data. These results rule out the possibility of a general time trend or other concurrent events driving our results.

– Insert Table 9 here –

5.4.6. Persistence and Accident Incidence Rate

In this section, we examine the persistence of the effect of UI benefit increases on employee reporting and explore additional consequences. Our primary research design examines the effect of UI benefit increases on employee whistleblowing in the year following the increase. Over an extended period, firms could take into account changes to employees' whistleblowing, and change their behavior accordingly. For example, if the firm observes more complaints filed with the regulator, the firm may adapt by increasing compliance with workplace safety regulations, thereby reducing accidents. Consequently, in the following periods employees might not observe as many issues to report.

To shed light on these possibilities, we conduct two tests. First, we explore the persistence of our main results by repeating our main analysis by setting *Treatment* to 1 for each of the subsequent 3 or 5 years. As shown in Table 10, panel A, we find a positive and significant coefficient on *Treatment* using a three-year treatment window (see column 1) and a positive but statistically insignificant coefficient on *Treatment* using a five-year treatment window (see column 2). These findings suggest that the effect of UI benefits on employee complaints is present for approximately three years.

Second, we explore whether firms change their behavior following an increase in UI benefits. Specifically, we examine whether increases in UI benefits reduce the number of accidents during a longer treatment window. The intuition for these tests is that fewer accidents suggests that the firm increases its compliance with workplace safety regulations. As shown in Table 10, panel B, we find a significant decrease in *TCR*, *DART*, or *DAFWII* in the three-year and five-year periods following UI benefit increases. The magnitudes are also economically meaningful. For example, *TCR* decreases by 0.053 cases per 100 employees during the three years following a UI benefit increase. Compared to the unconditional facility-level incidence rate of 0.231 cases per 100 employees, this represents a reduction of approximately 23% in workplace safety incidents. These findings suggest that reducing retaliation costs increases firms' compliance with workplace safety regulations over the longer term.

– Insert Table 10 here –

5.4.7. Alternative Research Designs

We also run tests using alternative research designs. First, we use the continuous variable *Benefit*, defined as the natural logarithm of the maximum total benefits in year *t* calculated as the product of the maximum weekly benefit amount and the maximum duration allowed in year *t*, instead of *Treatment*. As shown in Table 11, column 1, we find a positive and significant coefficient on *Benefit*. In contrast to our primary tests, the economic magnitude and statistical significance is lower, perhaps because employees are unlikely to respond to small increases in UI benefits. To further explore this possibility, we rerun our primary tests and include $Treatment_{5\text{-}10\%}$, which takes the value of 1 for the year after an increase in UI benefits by between 5 and 10%, in addition to *Treatment*. The baseline in these tests are increases in UI benefits smaller than 5%. As shown in Table 11, column 2, the coefficient on $Treatment_{5\text{-}10\%}$ is not significant, while the coefficient on *Treatment* is. The results suggest that employees do not change their reporting behavior in response to small increases in UI benefits.

Second, the first five years following an increase in UI benefits of at least 10% might not be appropriate control observations. Thus, we run an additional test in which we exclude observations in years *t+2* to *t+5* following a UI benefit increase. As shown in Table 11, column 3, we find a positive and statistically significant coefficient on *Treatment* using this alternative research design. Finally, we also estimate a model using the change in complaints between *t* and *t+1* as dependent variable. As shown in Table 11, column 4, we find a positive and statistically significant coefficient on *Treatment* using this alternative research design.

– Insert Table 11 here –

6. Conclusions

This paper examines the effects of reducing retaliation costs on facility-level employee whistleblowing of workplace safety hazards. Using state-level increases in UI benefits to capture changes in retaliation costs borne by rank-and-file employees, we quantify the magnitude of the effect. We find that an increase in benefits by at least 10% increases the number of facility-level inspections triggered by employee complaints by 0.47%. We further document that UI benefit increases also result in more violations and penalties, indicating that the additional tips either have merit or raise regulatory scrutiny of the accused facilities. Findings from our cross-sectional tests suggest that UI benefit increases empower employees to report violations in firms that are more prone to retaliation.

Our study contributes to the growing literature on employee whistleblowing. Prior studies document the benefits of employee whistleblowing, while highlighting that an obstacle in obtaining those benefits is that employees are often reluctant to share their information with the authorities because of the fear of retaliation. Our study documents the extent to which income loss affects an employee's decision to blow the whistle. Moreover, we show that, on the margin, the additional tips that are generated due to reduced retaliation costs are valuable, as they result in higher penalties and more violations.

Relatedly, our findings have important implications for policy-makers. Enforcing regulations and curtailing corporate misconduct is an endeavor that requires information that is often in the hands of employees. Documenting the effects of reducing retaliation costs borne by employee whistleblowers as well as firms' incentives to retaliate are important steps towards the implementation of effective programs that generate valuable tips. Considering that whistleblowers in our setting face similar economic forces as employees in other settings, our study shows how regulators can access rank-and-file employees' knowledge of misconduct via modest increases in compensation for job loss. Finally, it is worth emphasizing that our study focuses on the behavior of rank-and-file employees in response to an increase in UI benefits. It is unlikely that UI benefit increases of this dollar magnitude influence upper managers' decisions to report wrongdoing. However, given the critical role that rank-and-file employees can play in detecting corporate misconduct, it is important to understand how retaliation costs shape their whistleblowing decisions.

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Appendix. Variable Definitions

The following variables are constructed using data from OSHA [OSHA], data on facilities from Dun and Bradstreet [D&B], Compustat [C], data on county and state characteristics from the Bureau of Labor Statistics [BLS], data on internal control weaknesses from Audit Analytics [AA], and data on employee concerns from the Environmental, Social, and Governance (ESG) statistics compiled by Morgan Stanley Capital International [MSCI].

A. Variables of Interest

Complaints	The log of one plus the total number of employee complaints per facility and year. [OSHA]
Penalties	The log of one plus the total penalties for misconduct per facility and year. [OSHA]
Violations	The log of one plus the number of violations per facility and year. [OSHA]
Treatment	Indicator variable that is set to 1 for the year after an increase in UI benefits by at least 10%. [BLS]
Strong Employee Relations	Indicator variable that is set to 1 if the number of employee strengths is equal or larger than the number of employee concerns as reported in the Environmental, Social, and Governance (ESG) statistics compiled by Morgan Stanley Capital International, and 0 otherwise. [MSCI]
Internal Control Weakness	Indicator variable that is set to 1 in the year of an internal control weakness, and 0 otherwise. [AA]
Distance to Headquarters	Indicator variable that is set to 1 if the geographical distance between the firm's headquarters and the facility is above the median, and 0 otherwise. [C + D&B]
TCR	The Total Case Rate (TCR) is computed as follows: ((number of deaths + days away from work + job transfers or restrictions + other recordable cases) x 200,000) / employee hours worked. The 200,000 hours in the formula represents the equivalent of 100 employees working 40 hours per week, 50 weeks per year, and provides the standard base for the incidence rates. [OSHA]
DART	The Days Away, Restricted, or Transferred Rate (DART) is computed as follows: ((days away from work + job transfers or restrictions) x 200,000) / employee hours worked. [OSHA]
DAFWII	The Days Away from Work Injury and Illness Rate (DAFWII) is computed as follows: ((days away from work) x 200,000) / employee hours worked. [OSHA]
Accidents	The log of one plus the total number of inspections triggered by accidents per facility and year. [OSHA]
Referrals	The log of one plus the total number of inspections triggered by referrals per facility and year. [OSHA]
Planned Inspections	The log of one plus the total number of planned inspections per facility and year. [OSHA]

B. Controls

Employees Facility	The log of the number of employees per facility. [D&B]
Sales Facility	The log of sales per facility (in thousands of dollars). [D&B]

Size	The log of the market value of equity (in millions of dollars). [C]
Leverage	The ratio of total liabilities to total equity. [C]
ROA	Net income scaled by total assets. [C]
GDP Change	The change in GDP per state. [BLS]
Unemployment Rate	The unemployment rate per county. [BLS]

Figure 1. Research Design

This figure provides an example from our sample to better illustrate how we code the *Treatment* variable for our analyses. Consider a facility located in Massachusetts. In 2007, Massachusetts increased its UI benefits by more than 10%. This increase is coded as a "treatment" of the facility located in Massachusetts in 2008 (all other years are not treated). Other facilities of the same firm located elsewhere, as well as facilities of firms not located in Massachusetts form the control group. Increases in UI benefits occur at different points in time for our sample firms, affecting the time series of the *Treatment* variable. Each 0/1 coded cell (emphasized in bold) represents a facility-year observation included in our analysis.

Facility	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Facility located in Massachusetts	**0**	**0**	**0**	**0**	**0**	**0**	**1**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**
Facility ***not*** located in Massachusetts, but part of the same parent company	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**
Facility ***not*** located in Massachusetts with different parent company	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**

Table 1. Sample

Panel A. Sample Composition

This table presents the sample composition for the period 2002-2017.

	Number of Inspections (1)	Number of Firms (2)
All Inspections	1,617,278	
Less: Private and not linked companies	(1,546,464)	
Subtotal	70,814	1,185
Less: Missing controls	(7,202)	(50)
Final sample	63,612	1,135

Panel B. Sample Composition by Year

This table presents the distribution of employee complaints and inspections in our sample for the period 2002-2017 by year.

Year	Number of Employee Complaints	% of Total	Number of Inspections	% of Total
2002	1,559	6.9%	4,069	6.4%
2003	1,596	7.1%	4,102	6.4%
2004	1,469	6.5%	4,066	6.4%
2005	1,503	6.7%	4,104	6.5%
2006	1,341	5.9%	3,971	6.2%
2007	1,320	5.9%	4,279	6.7%
2008	1,270	5.6%	4,252	6.7%
2009	1,256	5.6%	4,137	6.5%
2010	1,452	6.4%	4,229	6.6%
2011	1,326	5.9%	3,863	6.1%
2012	1,493	6.6%	3,889	6.1%
2013	1,451	6.4%	4,029	6.3%
2014	1,615	7.2%	4,078	6.4%
2015	1,381	6.1%	3,985	6.3%
2016	1,330	5.9%	3,506	5.5%
2017	1,191	5.3%	3,053	4.8%
Total	22,553	100%	63,612	100%

Table 2. Treatment Years by State

This table reports the years from 2002 to 2017 in which a state saw an increase in state unemployment benefits of at least 10%.

State	Treatment Year	% Change in Unemployment Benefit	$ Change in Unemployment Benefit
Alabama	2003	10.5%	$520
Arizona	2005	17.1%	$910
Arkansas	2006	10.7%	$962
California	2002	43.5%	$2,600
D.C.	2006	16.2%	$1,300
D.C.	2017	18.4%	$1,716
Kentucky	2004	10.9%	$936
Louisiana	2009	10.1%	$676
Maryland	2003	10.7%	$780
Maryland	2008	11.8%	$1,040
Massachusetts	2007	10.8%	$2,520
Michigan	2003	20.7%	$1,612
Missouri	2008	14.3%	$1,040
Missouri	2017	53.8%	$2,240
Montana	2004	11.7%	$982
New Hampshire	2003	12.4%	$1,066
New Hampshire	2008	14.8%	$1,430
New Mexico	2004	26.4%	$1,898
New Mexico	2008	17.9%	$1,794
North Dakota	2013	10.5%	$1,404
Oklahoma	2008	14.6%	$1,300
Oklahoma	2014	14.0%	$1,404
Tennessee	2010	18.2%	$1,300
Vermont	2003	12.5%	$1,014
Virginia	2003	18.7%	$1,300
Washington	2002	12.5%	$1,650
Wyoming	2008	10.9%	$988
Average		16.8%	$1,347

Table 3. Summary Statistics Facilities

This table reports the summary statistics on an annual basis of the variables used in our analyses. All variables are defined in the Appendix.

	Facility-Years (N = 656,609)				
Variable	Mean	Std.	Min.	Median	Max.
Number of Employee Complaints	0.034	0.287	0	0	32
Number of Total Inspections	0.097	0.481	0	0	41
Number of Violations	0.184	2.125	0	0	848
Penalties (in $)	629	50,047	0	0	32,550,000
Benefits (in $)	11,069	3,695	3,300	11,024	33,090
Employees Facility	258	1,002	1	100	155,200
Sales Facility (in thousands)	3,526	18,505	0.010	48	485,668
Size (in millions)	33,974	59,878	74	8,933	790,050
Leverage	2.36	6.672	0.016	1.589	8.406
ROA	0.050	0.109	–0.237	0.053	0.237
GDP Change (in %)	3.93	2.91	–5.30	4	10.7
Unemployment Rate (in %)	5.83	3.06	0	5.5	14.4

Table 4. UI Benefits Increases and Facility-Level Reporting

This table reports the estimation results from linear regressions of the following form:

$$Complaints_{i,j,s,t} = \beta_0 + \beta_1\ Treatment_{s,t} + \phi\ Controls + \varepsilon$$

The dependent variable is the natural logarithm of the number of workplace safety complaints filed by employees per facility and year. Column 1 reports results without *Controls*. Column 2 reports results with facility-level and firm-level *Controls*. Column 3 reports results with facility-level, firm-level, and county-level *Controls*. All models include facility and year fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits of at least 10%. All variables are defined in the Appendix, and the sample spans the period 2002-2017. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Complaints	
Variables	(1)	(2)	(3)
Treatment	0.0044***	0.0045***	0.0047***
	(0.0012)	(0.0012)	(0.0012)
Employees Facility		0.0024***	0.0024***
		(0.0003)	(0.0003)
Sales Facility		0.0001	0.0001
		(0.0003)	(0.0003)
Size		0.0017**	0.0017**
		(0.0007)	(0.0007)
Leverage		–0.0000***	–0.0000***
		(0.0000)	(0.0000)
ROA		–0.0004	–0.0002
		(0.0027)	(0.0027)
GDP Change			–0.0003***
			(0.0001)
Unemployment Rate			0.0001
			(0.0002)
Facility FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Clustered by	Firm	Firm	Firm
Adj. R-squared	0.083	0.083	0.083
Observations	656,609	656,609	656,609

Table 5. Penalties and Violations

This table reports the estimation results from linear regressions of the following form:

$$Penalties\ or\ Violations_{i,j,s,t} = \beta_0 + \beta_1\ Treatment_{s,t} + \phi\ Controls + \varepsilon$$

The dependent variable is either the natural logarithm of the penalty amounts resulting from workplace safety violations per facility and year (column 1) or the natural logarithm of the number of violations resulting from workplace safety violations per facility and year (column 2). All models include facility and year fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits of at least 10%. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. All variables are defined in the Appendix, and the sample spans the period 2002-2017. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Penalties	Violations
Variables	(1)	(2)
Treatment	0.0432***	0.0070***
	(0.0109)	(0.0025)
Controls	Yes	Yes
Facility FE	Yes	Yes
Year FE	Yes	Yes
Clustered by	Firm	Firm
Adj. R-squared	0.044	0.047
Observations	656,609	656,609

Table 6. Cross-Sectional Tests

Panel A. Employee Relations

This table analyzes cross-sectional variation in the results of Table 4. The sample is partitioned based on *Strong Employee Relations*, which is equal to 1 if the number of employee strengths is equal or larger than the number of employee concerns as reported in the MSCI database, and 0 otherwise. The sample for these tests is smaller as data as data on employee strengths and weaknesses are not available for some firms. The dependent variable is the number of workplace safety complaints filed by employees per facility and year. *Yes* (*No*) refers to observations where *Strong Employee Relations* is equal to 1 (0). All models include facility and year fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits by at least 10%. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. All variables are defined in the Appendix, and the sample spans the period 2002-2017. Standard errors are clustered at the facility level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Complaints	
		Strong Employee Relations	
		Yes	No
Variables		(1)	(2)
Treatment	α	0.0024	0.0045*
		(0.0018)	(0.0025)
Controls		Yes	Yes
Facility FE		Yes	Yes
Year FE		Yes	Yes
Clustered by		Firm	Firm
R-square		0.125	0.073
Observations		231,075	166,267
		$\alpha_{Yes} < \alpha_{No}$	
p-value		0.031	

Panel B. Internal Control Weakness

This table analyzes cross-sectional variation in the results of Table 4. The sample is partitioned based on *Internal Control Weakness*, which is equal to 1 in the year of an internal control weakness, and 0 otherwise. The dependent variable is the number of workplace safety complaints filed by employees per facility and year. *Yes* (*No*) refers to observations where *Internal Control Weakness* is equal to 1 (0). All models include facility and year fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits by at least 10%. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. All variables are defined in the Appendix, and the sample spans the period 2004-2017, as data on internal control weaknesses are available only as of 2004. Standard errors are clustered at the facility level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Complaints	
		Internal Control Weakness	
		Yes	No
Variables		(1)	(2)
Treatment	α	0.0149*	0.0008
		(0.0084)	(0.0016)
Controls		Yes	Yes
Facility FE		Yes	Yes
Year FE		Yes	Yes
Clustered by		Firm	Firm
R-square		0.086	0.081
Observations		10,810	570,048
		$\alpha_{Yes} > \alpha_{No}$	
p-value		0.092	

Panel C. Distance to Headquarters

This table analyzes cross-sectional variation in the results of Table 4. The sample is partitioned based on *Distance to Headquarters*, which is equal to 1 if the average distance between a firm's headquarters and a facility is larger than the median value, and 0 otherwise. The dependent variable is the number of workplace safety complaints filed by employees per facility and year. *High* (*Low*) refers to observations where *Distance to Headquarters* is equal to 1 (0). All models include facility and year fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits of at least 10%. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. All variables are defined in the Appendix, and the sample spans the period 2002-2017. Standard errors are clustered at the facility level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable		Complaints	
		Distance to Headquarters	
		High	Low
Variables		(1)	(2)
Treatment	α	0.0074***	0.0016
		(0.0020)	(0.0015)
Controls		Yes	Yes
Facility FE		Yes	Yes
Year FE		Yes	Yes
Clustered by		Firm	Firm
R-square		0.056	0.105
Observations		333,408	323,201

	$\alpha_{High} > \alpha_{Low}$
p-value	0.014

Table 7. Accidents and Other Inspections

This table reports the estimation results from linear regressions of the following form:

$$Accidents\ /\ Other\ Inspections_{i,j,s,t} = \beta_0 + \beta_1\ Treatment_{s,t} + \phi\ Controls + \varepsilon$$

The dependent variable is either different types of accident rates per facility and year (columns 1-3) or different types of *Other Inspections* per facility and year (columns 4-6). In column 1, *Accidents* is *TCR* (*Total Case Rate*), which captures a firm's injury rate per 100 workers per year. In column 2, *Accidents* is *DART* (Days Away, Restricted, or Transferred Rate), which captures the rate of days away from work per 100 workers per year. In column 3, *Accidents* is *DAFWII* (*Days Away from Work Injury and Illness Rate*), which captures the rate of days away from work because of injuries and illnesses per 100 workers per year. In column 4, *Other Inspections* is the natural logarithm of the number of inspections triggered by accidents. In column 5, *Other Inspections* is the natural logarithm of the number of inspections triggered by referrals. In column 6, *Other Inspections* is the natural logarithm of the number of planned inspections. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment_Rate*. The tests in columns 1-3 are limited to the period 2002-2011, as data on accidents at the facility level are not available after 2011. All models include facility and year fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits of at least 10%. All variables are defined in the Appendix. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Accidents			Other Inspections		
Variables	TCR (1)	DART (2)	DAFWII (3)	Accidents (4)	Referrals (5)	Planned Inspections (6)
Treatment	–0.034	–0.028	–0.008	0.0002	0.0009	0.0002
	(0.036)	(0.023)	(0.012)	(0.0006)	(0.0007)	(0.0012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered by	Firm	Firm	Firm	Firm	Firm	Firm
Adj. R-squared	0.384	0.388	0.357	0.053	0.039	0.047
Observations	399,506	399,506	399,506	656,609	656,609	656,609

Table 8. Macroeconomic Conditions

This table reports the estimation results from linear regressions of the following form:

$$Complaints_{i,j,s,t} \,/\, Sales\ Facility_{i,j,s,t} \,/\, Employees\ Facility_{i,j,s,t} = \beta_0 + \beta_1\ Treatment_{s,t} + \phi\ Controls + \varepsilon$$

In columns 1-3, the dependent variable is the natural logarithm of the number of workplace safety complaints filed by employees per facility and year. In column 4, the dependent variable is the natural logarithm of facility sales, and in column 5, the dependent variable is the natural logarithm of facility employees. Column 1 reports results including the lead and lag of *Treatment*. Column 2 reports results with year fixed effects interacted with industry fixed effects. Column 3 reports results with year fixed effects interacted with firm fixed effects. All models include facility and year fixed effects. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits of at least 10%. In columns 1-3, *Controls* include *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate* (in column 3 the firm-level *Controls Size*, *Leverage*, and *ROA* are absorbed by the firm-year fixed effects). In column 4, *Controls* include *Employees Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. In column 5, *Controls* include *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. All variables are defined in the Appendix, and the sample spans the period 2002-2017. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Complaints			Sales Facility	Employees Facility
Variables	(1)	(2)	(3)	(4)	(5)
Treatment	0.0050***	0.0046***	0.0042***	–0.0016	–0.0053
	(0.0013)	(0.0012)	(0.0013)	(0.0078)	(0.0138)
$Treatment_{t-1}$	0.0011				
	(0.0010)				
$Treatment_{t+1}$	0.0001				
	(0.0014)				
Controls	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Year FE x Industry FE	No	Yes	No	No	No
Year FE x Firm FE	No	No	Yes	No	No
Clustered by	Firm	Firm	Firm	Firm	Firm
Adj. R-squared	0.083	0.084	0.085	0.681	0.728
Observations	656,609	656,609	656,609	656,609	656,609

Table 9. Falsification Tests

This table presents falsification tests on the number of facility-level complaints, violations, and penalties in response to changes to UI benefits. The table reports the comparison between the coefficients β_1 obtained from two randomization procedures based on the following equation:

$$Y_{i,j,s,t} = \beta_0 + \beta_1\ Treatment_{s,t} + \phi\ Controls + \varepsilon.$$

The dependent variable the number of inspections triggered by a complaint, the penalty amount, or the number of violations. Our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits of at least 10%. The randomization procedure is as follows: we use a uniform distribution to randomize the state in which each facility is located. The randomization procedure takes 1,000 random draws of the randomized element. *p*-values (in brackets) reflect the probability that the coefficient estimated using the randomized data (β_1) is greater than the coefficient estimated using the actual data based on Table 4, column 3 ($\widehat{\beta_{1,complaints}}$ = 0.0047), Table 5, column 1 ($\widehat{\beta_{1,penalties}}$ = 0.0432), and Table 5, column 2 ($\widehat{\beta_{1,violations}}$ = 0.0070). *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. Variables are defined in the Appendix.

Dependent variable	$\widehat{\beta_1}$ *Actual data*	β_1 *Random data*	H_o: $\beta_1 > \widehat{\beta_1}$ [*p*-value]
(1) Complaints	0.0047	0.00005	[0.005]
(2) Penalties	0.0432	–0.00081	[0.011]
(3) Violations	0.0070	–0.000075	[0.066]

Table 10. Persistence and Accident Incidence Rate

Panel A. Persistence

This table examines the persistence of the results presented in Table 4 using different treatment effects. The dependent variable is the natural logarithm of the number of workplace safety complaints filed by employees per facility and year. In column 1, *Treatment* takes the value of 1 for the three years after an increase in UI benefits of at least 10%. In column 2, *Treatment* takes the value of 1 for the five years after an increase in UI benefits of at least 10%. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. All variables are defined in the Appendix, and the sample spans the period 2002-2017. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Complaints	
Variables	(1)	(2)
$Treatment_{3\_Years}$	0.0013*	
	(0.0007)	
$Treatment_{5\_Years}$		0.0005
		(0.0006)
Controls	Yes	Yes
Facility FE	Yes	Yes
Year FE	Yes	Yes
Clustered by	Firm	Firm
Adj. R-squared	0.083	0.083
Observations	656,609	656,609

Panel B. Accident Incidence Rate

This table reports the estimation results from linear regressions of the following form:

$$Accidents_{i,j,s,t} = \beta_0 + \beta_1\ Treatment_{s,t} + \phi\ Controls + \varepsilon$$

The dependent variable is different types of accident rates per facility and year. In columns 1 and 4, *Accidents* is *TCR* (*Total Case Rate*), which captures a firm's injury rate per 100 workers per year. In columns 2 and 5, *Accidents* is *DART* (Days Away, Restricted, or Transferred Rate), which captures the rate of days away from work per 100 workers per year. In columns 3 and 6, *Accidents* is *DAFWII* (*Days Away from Work Injury and Illness Rate*), which captures the rate of days away from work because of injuries and illnesses per 100 workers per year. In columns 1 to 3, $Treatment_{3\_Years}$ takes the value of 1 for the three years after an increase in UI benefits by at least 10%. In columns 4 to 6, $Treatment_{5\_Years}$ takes the value of 1 for the five years after an increase in UI benefits by at least 10%. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment_Rate*. The tests are limited to the period 2002-2011, as data on accidents at the facility level are not available after 2011. All models include facility and year fixed effects. Variables are defined in the Appendix. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Accidents					
Variables	TCR (1)	DART (2)	DAFWII (3)	TCR (4)	DART (5)	DAFWII (6)
$Treatment_{3\_Years}$	–0.053**	–0.037**	–0.013**			
	(0.023)	(0.015)	(0.006)			
$Treatment_{5\_Years}$				–0.045**	–0.030**	–0.009**
				(0.021)	(0.014)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered by	Firm	Firm	Firm	Firm	Firm	Firm
Adj. R-squared	0.384	0.388	0.357	0.384	0.388	0.357
Observations	399,506	399,506	399,506	399,506	399,506	399,506

Table 11. Alternative Research Designs

This table presents robustness tests of the results presented in Table 4 using alternative research designs. In column 1, our main explanatory variable is *Benefit*, which is the natural logarithm of the maximum UI benefits in state *s* in year *t*. In column 2, our main explanatory variable is *Treatment*, which takes the value of 1 for the year after an increase in UI benefits of at least 10%. We also include *Treatment*$_{5\text{-}10\%}$, which takes the value of 1 for the year after an increase in UI benefits of between 5 and 10%. The baseline in these tests is increases smaller than 5%. In column 3, *Treatment* takes the value of 1 for the year after an increase in UI benefits of at least 10%, missing in the five years following such an increase, and 0 otherwise. In columns 1-3, the dependent variable is the natural logarithm of the number of workplace safety complaints filed by employees per facility and year. In column 4, the dependent variable is the change in complaints between *t* and *t+1*. *Treatment* takes the value of 1 for the year after an increase in UI benefits of at least 10%, and 0 otherwise. *Controls* included are *Employees Facility*, *Sales Facility*, *Size*, *Leverage*, *ROA*, *GDP Change*, and *Unemployment Rate*. In column 4, *Controls* are expressed in terms of changes. Columns 1-3 include year and facility fixed effects; column 4 includes year fixed effects. All variables are defined in the Appendix, and the sample spans the period 2002-2017. Standard errors are clustered at the firm level. Standard errors are reported below the coefficients. *, **, *** indicate significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Dependent Variable	Complaints			
Variables	(1)	(2)	(3)	(4)
Benefit	0.0007*			
	(0.0004)			
Treatment$_{5\text{-}10\%}$		0.0005		
		(0.0007)		
Treatment		0.0047***		
		(0.0012)		
Treatment$_{Exclude_5_Years}$			0.0046***	
			(0.0014)	
Treatment				0.0044*
				(0.0026)
Controls	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes
Clustered by	Firm	Firm	Firm	Firm
Adj. R-squared	0.069	0.083	0.084	0.000
Observations	656,609	656,609	503,005	656,609