Do Corporate Taxes Affect Workplace Safety?

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Abstract

We examine how workplace injury rates change when firms are subject to a corporate tax shock. We find that tax increases lead to a significance increase in reported injuries, but tax decreases have no similar effect. Our difference-in-differences empirical strategy relies on staggered state-level corporate tax changes that exploits spatial discontinuity in treatment and control establishments located in contiguous border counties within the same firm. The results are strongest in industries with low union bargaining power, for firms with high marginal tax rates, poor safety culture scores, firms that barely meet or beat analysts' earnings forecasts and firms that hire seasonal workers. Our results suggest that tax increases lead to real effects at the expense of employees, with no similar benefit accruing for tax cuts.

JEL Classification: G30; H25; H32; J28; K32

Keywords: workplace safety, employee welfare, corporate taxes, natural experiments

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1. Introduction

A government's taxation policy has been shown to directly influence many facets of economic activity, e.g., individuals' consumption and savings choices (Atkinson, 1971; Dammon and Spatt, 2001), firms' employment (Ljungqvist and Smolyansky, 2016), investment and leverage decisions, among others (Dammon and Senbet, 1988; Fama and French, 1998; Dammon and Spatt, 2001; Djankov et al., 2010; Doidge and Dyck, 2015). Given the importance of tax policy, it is not surprising that it captures attention and stirs much debate among economists, politicians and the general public. For instance, in December 2017, President Trump's tax plan became law. In the months leading up to and following its passage, this garnished tremendous media attention and controversy. At the core of the debate, is the benefit of the tax cut to the average American worker. For example, in a recent interview published in *The Economist*, Republican Senator Marco Rubio, a member of Trump's own political party suggests, '…there's no evidence whatsoever that the money's been massively poured back into the American worker.'[1] In this study, we examine the impact of tax policy changes on an understudied, yet topic of great importance to the average American worker—their safety in the workplace.

Besides the obvious social welfare issues related to worker safety, the economic consequences to US productivity losses from safety-related incidents are huge. For instance, it is estimated that injuries in the workplace cost the US economy over $250 billion annually. To put this in perspective, Leigh (2011) indicates that this exceeds the cumulative cost of all cancers.[2] Thus, it is not surprising that firms invest substantial sums in safety-related technology and safety training programs to improve labor productivity.[3] Additionally, having a poor safety record could lead to other undesired consequences such as the inability to hire and retain workers, loss of business

[1] https://www.economist.com/news/united-states/21741146-florida-senator-thinks-reheating-reaganomics-dead-end-marco-rubio-offers-his

[2] Leigh (2011) reports medical cost estimates of $67 billion (27% of the total) and indirect costs were almost $183 billion (73%). Though most of the direct costs are covered under workers' compensation insurance, injury and illness frequency and severity is used as a key input in the calculation of workers' compensation insurance premiums. According to the Liberty Mutual Workplace Safety Index, firms' total direct workers' compensation costs for the top ten injury types, including insurance, claims, and claims processing were as high as $59.6 billion in 2014.

[3] We had discussions with a regional CFO of a multi-billion dollar publicly-traded manufacturing firm regarding their safety program. He indicated that safety was of utmost importance at his organization. They not only invest in software, training materials, program concepts, but they invest heavily in human capital with dedicated safety personnel. For instance, they have a Safety Director that oversees the entire organization with additional safety groups at each Business Unit level.

partners, lawsuits, regulatory fines, negative publicity, and ultimately a lower firm value (Cohn and Wardlaw, 2016).[4]

We propose two opposing hypotheses as explanations for the potential impact of tax policy changes on worker safety. First, there are several reasons why a tax hike (cut) could lead to a deterioration (improvement) in workplace safety. An increase in taxes reduces firms' after-tax profits, thus, to maintain the same level of profits a firm may choose to cut investments in workplace safety. Different from conventional types of corporate investment (e.g., R&D, PPE, etc.), investments in workplace safety are opaque and difficulty to track, making them particularly vulnerable to cut when managers face resource constraints. For instance, Caskey and Ozel (2017) find that workplace injuries increase for firms that meet or just beat analysts' forecasts suggesting that managers engage in real activities management when under pressure because these actions are difficult to detect. Likewise, Cohn, Nestoriak and Wardlaw (2017) find a significant improvement in injury rates when firms are purchased by PE firms. They argue that going private alleviates short-term pressure to perform and firms can instead focus on operational efficiencies. Alternatively, taxes might affect worker safety through the leverage channel. Heider and Ljungqvist (2015) document that firms increase their use of debt following a tax increase. A rise in debt triggers more short-term interest payments. Thus, firms may cut long-term investment in workplace safety to meet these higher short-term debt obligations.[5]

An alternative view focuses on the risk-taking channel. Ljungqvist, Zhang, and Zuo (2017) show that firms reduce risk-taking when faced with tax increases, since higher taxes lower expected profits more for riskier projects compared to safer ones. This is because the government shares a firm's upside gain, but not the downside risk. Consistent with this view, Mukherjee, Singh and Zaldokas (2017) find that increases in taxes reduces innovation productivity at firms. In addition, Hausken and Zhuang (2016) develop a theory to examine the tradeoff between safety and production. They show that a higher tax rate leads the firm to exert greater safety effort hence lowering injury rate. This is because increasing corporate taxes is similar to increasing unit production costs and thus inducing the firm to decrease its production efforts and shift focus to

[4] For instance, in April 2017, Tyson Foods promised to improve its working conditions at it processing plants after negative press attention and pressure from consumer advocates. https://www.bloomberg.com/news/articles/2017-04-26/tyson-commits-to-better-conditions-for-workers-after-criticism.

[5] Firms spend an economically large amount to comply with OSHA safety requirements. According to the U.S. Bureau of Economic Analysis, companies spent between $52 and $66 billion complying with OSHA regulations in 2010 (Kniesner and Leeth, 2014), which is equivalent to about 5% of domestic corporate profits.

increasing safety quality. Therefore, this view predicts that an increase in the tax rate will lead to improvements in worker safety.

Empirically, pinning down the economic consequences of changes in tax policy is challenging for several reasons. First, changes in tax policy are unlikely to be random, instead induced by fundamental country- and state-level economic conditions. Consequently, observed relations between changes in tax policy and *ex post* worker safety outcomes could be driven by unobserved omitted variables that affect both the tax policy changes and firms' investment in workplace safety. Second, even if a tax change was random, we do not know how worker safety would have progressed had the tax change not occurred. In other words, we do not observe a counterfactual outcome making it difficult to quantify the effect of changes in tax policy.

To deal with these empirical challenges, we follow Heider and Ljungqvist (2015) and employ a difference-in-differences (DiD) framework that relies on changes in U.S. state-level corporate income taxes. While changes in federal corporate tax rates occur infrequently, we can identify many changes in state-level corporate taxes over time. For instance, during the 2002-2011 time period, we make use of 33 changes in state corporate income tax rates. Of particular importance, these tax changes are staggered over time across states such that in any given year, some states are impacted with a tax change (treated observations) while others are not (control observations), which allows us to implement a difference-in-differences framework. This approach implicitly accounts for potentially confounding effects, which would not be possible if we were to focus on changes in federal tax rates.

Our empirical strategy is designed to mitigate confounding effects, particularly unobservable characteristics that have the potential to drive any relation between taxes and safety making causal inference difficult. In particular, our setting allows us to exploit the spatial discontinuity created by state tax policy. Since a state's tax rule stops at its border, its immediate neighbors share similar economic environments, but are unaffected by a change in tax policy. Such a geographic discontinuity in tax policy makes neighboring states reasonable counterfactuals. Therefore, our control firms are selected from a treated state's neighbors. Taking this even further, we refine the DiD approach by identifying treatment and counterfactuals that are located in contiguous *counties* on either side of a state border. As we compare economic outcomes in groups of neighboring counties straddling a state border, we significantly mitigate any bias resulting from unobserved local variation in economic conditions that might correlate with the tax change.

It is important to note that firms pay state taxes where they conduct business as opposed to where their headquarters are located. Our micro-data of work-related injury rates is at the *establishment* level such that we can compare safety outcomes not only across firms, but within firms as many firms in our sample have plants spread across the US. Thus, in one of our tests, we can identify a set of establishments within the same firm, but located in a state that does not experience a tax change making it an ideal counterfactual. Therefore, it allows us to difference out potential firm-level omitted variables that are time variant.

We obtain establishment-level workplace safety data from the Occupational Safety Health Administration (OSHA), which regulates workplace safety for most private sector employers in the U.S. Under the OSHA Data Initiative Program (ODI) that was active during our 2002 to 2011 sample period, OSHA surveyed private-sector establishments and collected data on reportable injuries and illnesses attributable to work-related activities. Our final sample of corporate income tax changes contains 13 tax increases and 20 tax cuts in 22 states affecting a total of 1,447 establishment-level observations.

We document an asymmetric effect of corporate tax changes on workplace safety. We find workplace injury rates increase when firms experience a tax increase, but tax cuts have no similar impact. Compared to establishments located in states that are not subject to tax changes, establishments in states with a tax increase experience a 6% increase in injury rates in the subsequent year. As we refine the DiD test to make identification tighter by focusing on neighboring states, contiguous counties that straddle a state border, or control establishments of the same firm as the treated one, our results remain robust. For example, compared to control establishments that are located in contiguous boarder counties, injury rates rise by 9.4% in treated establishments that are subject to a tax increase.

Finally, we examine some cross-sectional implications of our results. We conjecture that incentives play an important role in determining managements' willingness to compromise safety measures. Specifically, we explore several characteristics that are likely to influence management incentives: bargaining power of labor, firms' marginal tax rates, firms' safety culture, pressure to meet/beat analyst earnings forecasts and whether or not the establishment employs temporary workers. The intuition for labor's bargaining power is that when it is high, management may have less flexibility to react to a negative cash flow shock by taking resources away from safety investments. Likewise, when firms have a low marginal tax rate, the sensitivity to tax changes should be minimal. Such firms would be less likely to make cuts to safety compared to firms that have a high marginal tax rate. Firms that have a strong safety culture should be less likely to sacrifice employee welfare since they value it within their organization. Caskey and Ozel (2017) find higher injury rates for firms that just barely meet or beat earnings forecasts. Thus, when subject to a negative cash flow shock, we conjecture that the firms that are trying to meet their earnings benchmarks would be most likely to sacrifice safety. Finally, costs of safety training for temporary workers are higher than those for regular workers. Therefore, we hypothesize that firms with temporary workers are more willing to compromise safety investments when they experience a tax increase. Consistent with these notions, we find the adverse effect of tax hikes on worker safety is greater in industries with lower employee bargaining and in firms with lower safety cultures, higher marginal tax rates, for those that barely meet/beat EPS numbers and for firms that hire temporary workers.

Our paper contributes to at least two distinct strands of research. First, we add to the nascent literature on worker safety. Cohn and Wardlaw (2016) find evidence that worker safety is related to financial constraints whereas, as just discussed, Caskey and Ozel (2017) find safety is compromised for firms that just barely meet or beat earnings forecasts. These two papers suggest that when managers are pressured or constrained, they cut investments in safety because such investments are discretionary and opaque, which makes them vulnerable to cuts. Christensen et al. (2017) show that when regulations required mine owners to disclose injury rates in their financials (even though they were publicly available beforehand), injury rates significantly decline. They argue that the forced disclosure raises public awareness and induces management to make improvements. Bradley, Mao, and Zhang (2018) show that injury rates are negatively related to analyst following suggesting that analysts play a disciplinary role. Our paper adds to this literature by showing how a plausible exogenous cash flow shock leads to real activities management. Our paper is particularly important because governments use tax policy to offer incentives and disincentives for certain actions (i.e., investment). Our evidence suggests that when firms face a tax increase, a subtle and negative unintended consequence is that employee welfare is compromised.

Our paper also belongs to a growing literature that investigates the efficacy of tax policy on various corporate decisions, particularly in line with studies that employ difference-in-differences estimators exploiting changes in corporate income taxes at the state-level. Giroud and Rauh (2017) focus on changes in business activity of multi-state firms responding to changes in state corporate taxes. Heider and Ljungqvist (2015) study changes in firms' leverage following changes in state corporate tax rates. Ljungqvist, Zhang, and Zuo (2017) investigate how firms alter their risk-taking policy responding to tax changes. Similarly, Mukherjee, Singh, and Žaldokas (2017) tackle the question whether corporate taxes hinder innovation. Our study focuses on the effect of corporate taxes on employee welfare, adding a new and important aspect to this emerging literature. Further, we add to the debate whether tax policy has the desired outcomes.

It is important to note that a significant advantage of our paper is the granularity of our data. We observe safety measures at the *establishment* level, which we can precisely match to the states that experience a tax change (and controls that do not). The aforementioned studies suffer from the shortcoming that data on corporate decisions (e.g., leverage, innovation, etc.) at the establishment level are not available. Thus, corporate outcome variables are measured at the *firm* level, whereas tax rate shocks are mapped to states where the firms' headquarters reside (Heider and Ljungqvist 2015; Ljungqvist, Zhang, and Zuo 2017; Mukherjee, Singh, and Žaldokas 2017). States tax corporate activities within their borders, and the state where a firm was incorporated or headquartered is irrelevant for state tax policy. As a result, the identification of a treatment effect cannot be determined precisely in studies using firm-level outcomes as their treatment variable. Since we employ micro-data of work-related injury rates at the establishment level, these features of the data allow us to obtain a cleaner and sharper treatment effect.[6]

The rest of the paper is organized as follows. Section 2 describes state corporate income taxes and our identification strategy, and section 3 presents the data and summary statistics. Section 4 presents our main empirical tests and section 5 discusses some cross-sectional implications of our results. Section 6 concludes.

2. State Corporate Income Taxes and Identification Strategy

In this section, we describe state corporate income taxes and our identification strategy. Specifically, section 2.1 describes state corporate income tax policy and section 2.2 discusses our empirical framework.

2.1. State Corporate Income Taxes

Importantly, the state where a firm was incorporated (which is often Delaware) or headquartered is irrelevant for state tax policy. Instead, firms are taxed in every state where they have sales, employees, or properties. Therefore, a state corporate income tax change in state A will

[6] In untabulated results, we find that tax rate changes in headquartered state have no impact on establishment level worker safety.

affect all taxable corporate business activities of a firm in state A, and only in state A, regardless of where the firm is incorporated or headquartered. Thus, changes in state corporate income taxes offers a clean shock to study how such changes may influence business decisions in the state.

Each state maintains its own corporate tax policy. In 1969, eight states (FL, NH, NV, OH, SD, TX, WA and WY) did not tax corporations. By 2013, only three of these states continued that policy (NV, SD and WY). Most states tax corporate profits, but three (OH, TX, and WA) assess revenue. State taxes are a substantial part of firms' overall tax burden. For instance, Heider and Ljungqvist (2015) document that state taxes represent 21% (13.7%) of total taxes in an average (median) publicly traded U.S. firm during 1989 to 2011. We use their data on the changes of state-level corporate income tax rates (from Appendices A and B). Consistent with their paper and others, we focus on changes in the top statutory marginal tax rate. This approach is appropriate since states either impose a flat corporate tax rate or charge the top rate on fairly low levels of income (for example, $25,000).[7]

Figure 1 in Ljungqvist and Smolyansky (2016) shows that average state corporate tax rates are quite stable over our sample period of 1996−2011. The variation in tax rates among states has tightened moderately over time, from a 4% difference between the 20th and 80th percentile in 1996 to a 3.45% difference in 2011. We identify 13 tax increases in 10 states and 20 tax cuts in 12 states during this time frame. In a given year, on average 2.55% of states experience a rise in tax rates (=13/(10 years*51 states including DC)) and 3.92% of states face tax cuts (=20/(10*51)). As shown in Table 1, the mean tax increase amounts to 80 basis points, a 10.7% increase from the prior year. The mean tax cut is 105 basis points, a 11.34% reduction from the prior year.

A concern with studies on fiscal policy is whether changes in tax rates are anticipated. If firms determine their corporate policies based on the taxes they expect to incur in the future, observed responses to an actual tax change may not reflect the causal effect of taxes on their decisions. Hennessy and Strebulaev (2015) stress that a necessary and sufficient condition for correct inference about causal effects is that the policy variable (tax rates) follows a random walk. Ljungqvist and Smolyansky (2016) find empirical support they do. In robustness tests, we exclude firms that are located in states whose tax rate changes are predictable.

[7] In 2010, the highest corporate income tax bracket was set at $100,000 in 38 of the 44 states that charge a corporate income tax.

For state level corporate taxes, income a firm earns in a state is computed by multiplying the firm's total taxable income by an apportionment formula.[8] Apportionment formulae vary across states. As of 2012, thirteen states adopt a single-factor formula based on sales, allocating a firm's profit to the state based on the proportion of the firm's in-state sales to its total sales. A three-factor formula is used by thirty-three states, applying a weighted average of the proportions of sales, employees, and properties.[9] Because we use the DiD approach in our analysis, variations in apportionment formulae across states can be differenced away, hence will not confound our results. However apportionment rules can potentially affect our results if state tax rate changes coincide with changes in apportionment rules in the state. Fortunately, Ljungqvist and Smolyansky (2016) show that only five of the 140 tax increases and five of the 131 tax cuts in their sample overlap with changes in state apportionment rules.[10]

2.2. Identification Strategy

Our goal is to quantify the causal effects of corporate income taxes on workplace safety. Per our discussion above, we face two identification challenges. One is that tax changes are plausibly driven by unobserved changes in economic conditions that also affect corporate investment decisions in workplace safety. An OLS regression of ex post worker safety on changes in tax policy hence would likely lead to biased estimates because of this omitted variable problem. The other challenge is that even if a tax change truly was random, we do not observe a counterfactual. That is, how would worker safety progressed without the tax change? There might be a simultaneous regulation or policy changes pertaining to worker safety at the state or federal level, thus the outcome "response" may not be a consequence of the tax change, but rather caused by these changes.

Our empirical strategy aims to overcome these two key challenges by employing staggered changes in state corporate income tax rates over time, which offers a set of plausible counterfactuals. Furthermore, narrowing in on treated and control groups on contiguous counties spanning a state border allows us to hold variation in unobserved economic factors constant. Our approach is essentially a standard difference-in-differences method that takes advantage of a spatial discontinuity in state tax policy for identification purposes. Focusing on border counties improves our identification strategy twofold. First, despite that changes in state taxes are likely to be endogenous

[8] Note that firms do not apportion their costs among states, which reduces incentives to engage in transfer pricing.
[9] Among these 33 states, 22 states overweight the sales factor.
[10] We eliminate these and find the results are robust.

with respect to the state, they are conceivably exogenous from the viewpoint of individual counties straddling a state border. Second, the economic environment is more homogeneous in a narrow geographic region spanning either side of the state border than between two states. Greater similarity in economic factors allows us to difference away the confounding effects of unobserved local economic conditions.

We use an example to demonstrate these advantages. Suppose that economic conditions weaken in one state. To meet budget shortfalls, the state raises corporate income taxes. Likewise, firms cut investments in workplace safety because of the poor economic fundamentals. As a result, firms in treated and control states will have different trends of worker safety irrespective of treatment, i.e., the control state cannot offer a plausible counterfactual. This violates the parallel-trends assumption required for identification in a difference-in-differences test. To circumvent this issue, we assume parallel trends in pairs of counties contiguous to the state border. This assumption is valid providing border counties face similar variation in local economic conditions. As such we employ counties on one side of the state border to establish a counterfactual outcome for treated counties on the other side.

3. Sample selection, variable construction, and summary statistics

In this section, we describe the sample selection process and how each variable is constructed, followed by summary statistics. Section 3.1 describes how we select the sample. Section 3.2 provides a description of how our variables are constructed. Section 3.3 presents some summary statistics.

3.1. Sample Selection

Our data on workplace injuries are obtained from the Occupational Safety Health Administration (OSHA). Under the OSHA Data Initiative Program (ODI), OSHA surveys private-sector establishments to collect data on reportable work-related injuries and acute illnesses attributable to work-related activities from 1996 to 2011.[11] OSHA surveyed about 80,000 private-sector establishments every year. All establishments under OSHA jurisdiction with 11 or more employees were required to maintain a log recording injuries and illnesses available to OSHA

[11] OSHA discontinued ODI in 2011 because of funding cuts.

inspectors, unless OSHA exempted the industry due to a past history of low accident rates.[12] For each establishment, OSHA records injury and illness data, along with the establishment name, location, SIC code, number of employees, number of hours worked, and indicator variables for whether or not the establishment experiences unusual events such as strikes, facility shutdown, or natural disasters. We restrict our sample to a period of 2002 through 2011 since OSHA simplified and changed its recording criteria for injuries and illnesses and the coverage of industries in 2002, thus values are not comparable before versus after the changes. We manually match each establishment from OSHA to firms in the Compustat annual file based on names. Following Cohn and Wardlaw (2016) and Caskey and Ozel (2017), we exclude financials (SIC code between 6000 and 6999) and regulated firms (SIC code between 4900 and 4999). Each firm in Compustat might have multiple establishments, which leaves us with a primary sample of 44,384 establishment-year observations with 16,130 unique establishments and 3,149 unique firms. Each firm on average has 3.53 establishments in a year.

State corporate income tax changes are obtained from Heider and Ljungqvist (2015). Gross state product (GSP) comes from the U.S. Bureau of Economic Analysis. State unemployment rate is from the U.S. Bureau of Labor Statistics. We obtain firm financial statement information from Compustat, stock return data from CRSP, union membership and collective bargaining agreement coverage from Unionstats and simulated marginal tax rates from Blouin, Core, and Guay (2010).[13]

3.2. Variable construction

Following OSHA's definition, we employ four measures of workplace injury. *Total Case Rate* (*TCR*) is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000. *Total Case* is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases. *Injury Rates with Days Away, Restricted, or Transferred* (*DART*) is the number of injuries and illnesses that result in days away from work or with job restriction or transfer, divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000. *Injury Rates with Days Away from Work* (*DAFWII*) is the number of injuries and illnesses that

[12] The ODI program did not cover employers subject to safety regulation from other federal agencies, such as the Mine Safety and Health Administration or the Federal Aviation Administration.
[13] This data can be found at www.unionstats.com.

result in days away from work, divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000.

In our regressions, we follow Cohn and Wardlaw (2016) and Caskey and Ozel (2017) to control for a set of firm- and establishment-level characteristics that could affect changes in workplace safety. The firm-level control variables include *Ln(Assets)* (logarithm of book value of total assets), *Leverage* (total short-term and long-term debt divided by total assets), *PPE/Assets* (net property, plant, and equipment divided by total assets), *Sales/Assets* (total sales divided by total assets), *CAPEX/Assets* (the ratio of capital expenditure to total assets), *Market-to-Book* (market value of assets divided by book value of assets), *FCF/Assets* (the ratio of total free cash flows to total assets), *Cash/Assets* (the ratio of cash balances to total assets), and *Dividends/Assets* (the ratio of cash dividends to total assets). We also control for establishment-level variables, including *Ln(Number of Employee)* (the logarithm of total number of employees working in a given establishment during the year), and *Hours Per Employee* (total number of annual hours worked divided by the number of employees in a given establishment). We further control for state-level variables, including *State GSP Growth* (real gross state product (GSP)), and *State Unemployment Rate* (state unemployment rate). Detailed variable definitions are provided in Appendix A.

3.3. Summary statistics

Table 1 presents summary statistics based on the final sample of 14,802 establishment-year observations during 2002-2011. In terms of workplace safety measures, mean and median values of *TCR* is 9.65 and 8.31, respectively; the median of *Total Case* is 12 per establishment-year. These are comparable to values shown in Caskey and Ozel (2017). The mean number of employees per establishment is 345, and mean number of each employee working hours is 1,852 per year. These numbers are comparable to those reported in Cohn and Wardlaw (2016). As for firm-level characteristics, mean and median *Assets* is $16.89 and $10.75 billion, respectively; mean *Market-to-Book* ratio is 1.77; average *Leverage* is 0.26; *PPE* is on average 41% of the total assets; average capital expenditure is about 6.3% of total assets; an average firm pays 1.6% of total assets as dividends to shareholders. Overall, these firm-level variables are also consistent with those reported in Cohn and Wardlaw (2016) and Caskey and Ozel (2017). In terms of state-level variables, our median *State GSP Growth Rate* is 2.1% and median *State Unemployment Rate* is 5.4%, which are comparable to the median of 2.9% *State GSP Growth Rate* and median of 5.4% *State Unemployment Rate* reported in Heider and Ljungqvist (2015).

Insert Table 1

4. Empirical results

Following Heider and Ljungqvist (2015) and Mukherjee et al. (2017), we our empirical technique is a difference-in-differences approach to investigate how changes in states' corporate income tax rates affect firms' workplace injury rate. This method allows us to control for the time-invariant, firm-specific omitted variables as well as time-varying industry trends and nationwide shocks.

4.1 Injury rate around tax changes: Univariate results

We begin our analysis by plotting the sample mean change in injury rate to assess the impact of tax changes on workplace safety. Figure 1 displays the annual change in total case rate (*⊿TCR*) and annual change in total cases (*Total Case*) during 2 years before and 2 years after state corporate tax increases (Panel A) and tax cuts (Panel B) in year 0. The treated group contains establishments located in a state that experiences an increase (or a cut) in corporate income tax rate. The control group contains establishments located in a state that does not experience any change in corporate tax rate.

Insert Figure 1

As shown in Panel A, the treated group experiences a drastic rise in the change in both *TCR* and *Total Case* in the year following tax increases, though *TCR* reverts partially in the second year post-tax hikes. In both figures, the control firms' injury rates stay constant.[14] In Panel B, neither measure of injury rate exhibits much change in the years following tax cuts. Similar to Panel A, control firms show no change in injury rates.

4.2 Baseline regressions

The results in Figure 1 might be confounded either by contemporaneous changes in firms' financial conditions that are unrelated to tax changes, or by fluctuation in state-level conditions that cause changes in both tax and workplace injury. Next, we estimate the following multivariate

[14] We observe a mild drop in injury rate in the year tax rises in treated firms, suggesting that the parallel-trends assumption might be violated. Hence we will further test pre-trends in multiple regressions in the later section.

regression using the difference-in-differences approach to control for observed variation in firm-, establishment-, and state-level conditions.[15]

$$\Delta TCR_{e,i,s,t+1} = \beta_1 * Tax\_Increase_{s,t} + \beta_2 * Tax\_Decrease_{s,t} + \gamma_1 * \Delta W_{i,t} + \gamma_2 * \Delta X_{e,t} + \gamma_3 * \Delta Z_{s,t} + \alpha_t + \varepsilon_{e,i,s,t+1.} \quad (1)$$

where *e*, *i*, *s*, *t* index establishments, firms, states, and years. The unit of analysis is an establishment-year. Δ is the first difference operator, and the dependent variable $\Delta TCR_{e,i,s,t+1}$ is the leading change of total case rate (*TCR*) from year *t* to year *t*+1. The variables of interest are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables that equal one if a state experiences an increase or decrease in corporate tax rates in year *t*, respectively. Following Cohn and Wardlaw (2016) and Caskey and Ozel (2017), we control for a set of firm-characteristics ($\Delta W_{i,t}$) that could affect changes in workplace safety. These include *ΔLn(Assets)*, *ΔLeverage*, *ΔPPE/Assets*, *ΔSales/Assets*, *ΔCAPEX/Assets*, *ΔMarket-to-Book*, *ΔFCF/Assets, ΔCash/Assets*, and *ΔDividends/Assets*. We also control for establishment-level characteristics ($\Delta X_{e,t)}$), including *ΔLn(Number of Employee)* and *ΔHours Per Employee*. We further control for state-level variables ($\Delta Z_{s,t}$), including *ΔState GSP Growth* and *ΔState Unemployment Rate*.

First-differencing in Equation (1) removes unobserved firm-specific effects incurred in the corresponding levels equation. Unlike a levels specification with firm fixed effects, it can easily accommodate repeated treatments (a firm experiences several tax increases or tax cuts over time), treatment reversals (a tax increase followed by a tax cut later, or vice versa) or asymmetry in firms' responses to tax changes. In equation (1), for any change in corporate income tax in state *s* at time *t*, the control states are all those states that were not subject to a tax rate change at that time (though we will also consider finer control sets).

Insert Table 2

The baseline regressions results are reported in Table 2. In column (1), the coefficient estimate on $Tax\_Increase_{s,t}$ is positive and statistically significant, suggesting that following a tax hike, establishments experience greater injury rates. The result is also economically large. Compared to establishments located in states that are not subject to tax changes, establishments in states with tax increases incur an increase in injury rate of 0.648 in the subsequent year. Compared to the average pre-treatment *TCR* of 10.873, a 0.648 rise in *TCR* is equivalent to an increase of 5.96%.[16] To remove

[15] Taking differences removes unobserved establishment-specific fixed effects.
[16] The average pre-treatment *TCR* is 10.873, which is equivalent to an 5.96% (=0.648 /10.873) increase in *TCR*.

unobserved time-variant omitted variables, we further control for year fixed effects in column (2), the coefficient estimate on *Tax_Increase*$_{s,t}$ remains positive and significant.

Following Heider and Ljungqvist (2015) and Mukherjee et al. (2017), we include SIC2 industry-year fixed effects which remove unobserved time-varying industry shocks. Such a regression allows for covariates that vary at the firm- or state-level over time. For example, we can control for time-varying factors at the state level that may be correlated with changes in both state taxes and workplace safety, while firm-level covariates control for other firm-level variables correlated with firm workplace safety policy. Including industry-year fixed effects allows us to compare treated and control groups within the same industry at the same time. As shown in column (3), the result remains significant.

In columns (4)-(6), we provide the same regressions in models (1)-(3), but introduce *Tax_Decrease*$_{s,t}$. None of the coefficient estimates are significant indicating that workplace safety does not change following cuts in state corporate income taxes. When we include both *Tax_Increase*$_{s,t}$ and *Tax_Decrease*$_{s,t}$ in the same regression in columns (7)-(9), the coefficient estimates on *Tax_Increase*$_{s,t}$ remain significantly positive, whereas those of *Tax_Decrease*$_{s,t}$ continue to be insignificant.

Overall, we find an asymmetric effect of tax changes on workplace safety. Increases in state corporate income tax is positively related to workplace safety, however tax cuts have no significant effect on injury rate. This asymmetry in the response variable to tax cuts is consistent with the leverage effect documented in Heider and Ljungqvist (2015). They find that firms increase debt following a tax rise, but do not change leverage following a tax cut. Our evidence mirrors theirs.

4.3 Robustness Tests

In this section, we conduct a battery of robustness checks to ensure our baseline results are valid. First, we estimate regressions using three alternative measures of workplace injury rates provided by OSHA, including *Total Case* (sum of injuries and illnesses that result in days away from work or transfer and other recordable cases), *DART* (number of injuries and illnesses with days away from work and with job restriction or transfer divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000), and *DAFWII* (number of injuries and illnesses with days away from work divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000). Results are reported in Appendix Table B1. Overall, we find similar results as those reported in Table 2.

It is possible that some investments to worker safety take time for observable changes to materialize. For instance, a renovation at the establishment, new machinery or technology may take more than a year to see the full potential on safety measures. We thus investigate the longer term effect of tax changes on safety. In Appendix Table B2, we examine how tax increases and tax cuts affect all four measurements of injury rates in leading years *t+2* and *t+3*. We find that the tax increases or decreases mostly have no significant effect on changes in injury rate in years *t+2* or *t+3*.[17]

We also investigate whether the main results are sensitive to the inclusion of different fixed effects and clustering using different standard errors. The results are reported in Appendix Table B3. Our results remain robust after controlling for industry-year fixed effects, state fixed effects, and clustering standard errors at the year or at the year-state level.

Finally, our baseline specifications identify tax changes using dummy variables. This has the benefit of ease of economic interpretation, but it does not consider the magnitude of tax changes. To capture the size of tax changes, in Appendix Table B4 we use a continuous measure which is simply the change in the tax rate from time *t*-1 to time *t*. The results are robust.

4.4 Identification of counterfactuals

In this section, we address some concerns of the difference-in-differences method such as the estimators are biased because of pre-trends, tax changes may have been predicted, and the observed changes in workplace safety are driven by local economic shocks rather than tax changes.

Our baseline results presented in Table 2 are based on all establishments of U.S. publicly-traded firms across the entire country and using all state tax changes. We recognize two potential challenges to causal inference. First, both tax changes and firm investment in workplace safety may be driven by unobserved changes in local economic conditions causing incorrect estimation of the tax effect. Second, if tax changes were expected, firms could adjust their investment policy before tax changes occur, leading to biased coefficient estimates. In this section, we conduct additional tests to address these concerns.

Insert Table 3

4.4.1. Neighboring states

[17] We find slight statistical significance in year 3 where in column (3) and a tax decrease has a positive coefficient and column (6) a tax increase has a *negative* coefficient.

To address the omitted variable problem (the first issue discussed), we attempt to control for local economic conditions in several ways. These tests are presented in Table 3. First, in Panel A, we drop all distant control establishments, identifying controls that are located in a neighboring state (either a tax-rise or tax-cut state). By doing so, both the treated and control groups are arguably exposed to similar local economic conditions, alleviating the concerns that any unobservable geographical heterogeneity affects workplace safety. Our sample is substantially reduced by close to 40% (5,895 establishment-year observations). However, we find similar results as our baseline case. Tax increases leads to a significant rise in all four measures of injury rates. Judging from the coefficient estimate on $Tax\_Increase_{s,t}$ in column (1) of Table 3 Panel A and the corresponding specification in column (9) of Table 2, the economic magnitude of the effect of a tax increase is remarkably similar when we narrow the control sample to neighboring states. We continue to observe that workplace injury rates do not respond to tax cuts.

4.4.2. Contiguous border counties

While identifying control firms in neighboring states is cleaner than using potential controls in different parts of the country, one might still argue that the economic conditions vary substantially among establishments in contiguous states, especially for firms located at the opposite ends of two large states. For instance, local economic conditions for an establishment residing in Miami, FL are likely different than one located in Atlanta, GA, which is approximately 700 miles away. Therefore, we next design a cleaner test by focusing on treated and control establishments that are located in contiguous counties on either side of a state border. Such county-pairs likely share similar local economic conditions. This spatial discontinuity allows us to control for unobservable characteristics while isolating the impact of different tax treatments. The results are reported in Panel B. All four measures of injury rates load positively on tax increases, but tax cuts are not significant in this setting confirming the baseline models. The coefficient estimates on $Tax\_Increase_{s,t}$ are larger than our baseline estimates and those in Panel A.

4.4.3 Treated and counterfactuals within same firm

The standard DiD approach applied above allows us to difference out the potential effect of time invariant firm-specific omitted variables, however it is unable to control for firm-specific omitted variables that are time variant. A big advantage of the OSHA data is that the unit of observation is at the establishment level and, as discussed, firms pay state tax where their establishments are located rather than where their firm headquarters reside. Thus, we can exploit this unique feature of the data to also difference out time variant firm-specific omitted variables.

For this test, we restrict control establishments to those belonging to the same firm of the treated establishments. That is, for each treated establishment the counterfactual matches are establishments of the same firm located in different states that does not experience a tax change. For instance, Tyson Foods has an establishment in AR that experienced a tax decrease in 2005. Tyson Foods also has establishments in many states such as AL, GA, MO, NC, TN and others that were not subject to a tax rate change in 2005, which serve as control establishments. Such pairings control for any firm-level policy on workplace safety, which allows us to focus on the change in establishment-level safety policy subject to the tax shock. As shown in Panel C, again we find an asymmetric effect of tax changes on injury rates. Tax increases lead to a significant increase in injury rates, but tax cuts have little effect.

4.4.4 Same firm, neighboring state and county

In the previous section, we exploited the fact that establishments and tax changes are at the state-level and therefore allows us to control for time variant firm-specific unobservable characteristics. However, it could be argued that we need to simultaneously control for local economic conditions for the arguments provided earlier. We do so in Panels D and E. We start with the controls in Panel D and further require them to be located in neighboring states. In the case of Tyson Foods, for treated establishments in AR, we restrict the control group to only those located in the three neighboring states, including MO, TN, and TX. In Panel E, we take the controls in Panel D and further restrict them to reside in contiguous counties on the other side of a state border as the treated establishment. For example, Tyson Foods has a treated establishment located in Benton County, AR. We identify a control establishment of Tyson Foods residing in McDonald County, MO, which borders Benton County, AR. By doing so, we control for both potential confounds in local economic conditions and time-variant firm-specific omitted variables. Despite the small sample sizes, our results generally remain qualitatively the same. The coefficient in column is positive, albeit not statistically significant. However, of all the specifications reported in Panels D and E, 7 of 8 are significant. Overall our results do not appear to be driven by unobservable local economic shocks, firm heterogeneity, or time-variant firm-level omitted variables.

4.5 Anticipated tax changes

If state tax rate changes are anticipated before the new tax rates are implemented, the measured treatment responses to realized tax rate changes may not capture causal effects if the response is predictable (Hennessy and Strebulaev, 2015). Heider and Ljungqvist (2017) perform a series of tests to determine whether corporate tax rates follow a random walk (i.e., are unpredictable). Indeed, when considered independently, they find each state's tax policy changes are unanticipated. However, they recognize that the tax policy of a state may be conditional on the tax policy of its neighbors. Thus, they take into account cross-sectional dependency in tax rates across states. Based on these findings, we exclude establishments located in Connecticut or Massachusetts in Panel A and we exclude establishments located in New England (i.e. Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, or Vermont) in Panel B. The results are reported in Table 4. Eliminating establishments located in these states does not impact our results.

Insert Table 4

An alternative way of addressing the anticipation effect is the "narrative approach" of Romer and Romer (2010). Following the findings in Ljungqvist, Zhang, and Zuo (2017), we exclude establishments located in Colorado, Connecticut, Minnesota, or New York in Panel C. In all panels across all four measurements of injury rates, we find robust and consistent evidence that tax increases lead to a rise in injury rates while tax cuts lead to no change of injury rates in the subsequent year.

4.6. Pre-trends and reversals

Difference-in-difference estimation relies on a key assumption: parallel-trends. While Figure 1 shows that treated firms tend to display a downward trend before the shock, control firms are relatively stable. To empirically validate the parallel-trends assumption, we follow Heider and Ljungqvist's (2015) setup and include a series of indicator variables that represent leading and lagging tax increases and tax cuts. $Tax\_Increase_{s,t-2}$, $Tax\_Increase_{s,t-1}$, $Tax\_Increase_{s,t}$, $Tax\_Increase_{s,t+1}$, and $Tax\_Increase_{s,t+2}$ are dummy variables that equal one if a state increased its corporate tax rate in lagging year *t-2, t-1,* contemporaneous year *t*, and leading years *t+1, t+2*, respectively. A series of *Tax_Decrease* dummy variables are defined similarly. We include industry and year fixed effects to remove unobservable industry shocks.

All the coefficient estimates on *Tax_Increase* pre- year *t* are statistically insignificant indicating that parallel trends assumption is satisfied for tax increases. It implies that both treated and control establishments experience similar injury rate trend prior to the tax hikes.

To examine reversals, note that we focus on year *t+1* and leading year *t+2*. All the coefficient estimates on *Tax_Increase* in the year that tax changes occur and subsequent one year (years *t*+1 and *t+2*) are statistically insignificant, suggesting no evidence of reversal. The coefficient estimates on *Tax_Decrease* are mostly insignificant in year *t+1* and *t+2* as well, though there is some weak evidence that *Total Case* and *DART* increase responding to tax cuts in the contemporaneous year.

Insert Table 5

4.7 Anxiety and distracted workers

Although our tests in section 4.4 identify counterfactuals that are in very close proximity and likely to experience very similar economic conditions, it is plausible that when states increase corporate tax rates, it simultaneously adopts policies that negatively impact average workers, such as raising other taxes. If this is the case, workers in the treated state may feel more anxious or distracted (compared to workers in the adjoining state), which in turn causes them to lose focus and suffer more injuries at work. In an attempt to address this potential confounding issue, we collect data on state-level alcohol consumption and automobile accidents. There is a vast literature linking alcohol consumption and anxiety levels (i.e, Buckner, Eggleston, and Schmidt, 2006). Further, more distracted drivers would be more likely to be involved in automobile accidents. If workers in treated states experience an increase in anxiety levels (or are more distracted), we would expect to see a corresponding increase in state-wide alcohol consumption levels or automobile accidents.

We collect data on state level alcohol consumption rates from http://www.healthdata.org/us-health/data-download and state level automobile accident rates from https://www-fars.nhtsa.dot.gov/States/StatesCrashesAndAllVictims.aspx. In Appendix B5, we provide two tests. First, we include the change in alcohol consumption rates and changes in automobile accident rates as additional controls in our baseline specifications. As Panel A shows, the results are robust. In Panel B, we investigate if a tax increase (or decrease) has an impact on alcohol consumption or auto accidents in that state. Similar to Table 3, we conduct DiD tests but using *ΔAlcohol* and *ΔAuto Accident* as the dependent variables. We find state corporate tax changes does not cause a change in alcohol consumption or auto accident rates.

Overall, the results from section 4 provide convincing evidence that a shock to tax rates has real effects on worker safety. This only appears to be the case in the event of a tax increase where reported injury rates increase. We do not see a symmetric effect when establishments experience a tax decrease. In the next section, we examine some cross-sectional implications of our results.

5. Cross-sectional differences in tax policy changes and worker safety

Collectively, the evidence points to deteriorating work conditions for the average firm when subjected to a tax increase. However, some firms should be more restricted in their ability to cut safety investments whereas others may be more likely to do so. We conjecture that incentives play a role in firms' flexibility or willingness to make changes to safety protocol. For instance, the bargaining power of labor may impact safety conditions. If the bargaining power of the workforce is high, management has less flexibility to make significant cuts that threaten employee welfare. Similarly, heterogeneity in incentives may cause some firms to be more willing to jeopardize safety conditions. We focus on four other firm characteristics: marginal tax rates, whether a firm meet or barely beat earnings estimates, corporate safety culture, and whether a firm hires seasonal workers. We discuss the motivation and investigate these hypotheses below.

5.1 Labor

The primary function of labor unions is to increase the welfare of employees primarily through wages and work conditions. A vast literature exists on the ability of unions to bargain for employees from the wage perspective. While earlier studies and the conventional view is that significant wage gaps between unionized and nonunionized employees exist (i.e., Freeman and Medoff, 1984), more recent research questions this view (i.e., DiNardo and Lee, 2004). Likewise, the conventional view is that labor unions protect workers by bargaining for safer worker conditions, but the empirical literature is limited on whether or not labor unions indeed improve working conditions, particularly in the US.[18]

We conjecture that establishments with strong union protection would be more immune to any economic shocks, including tax changes, than those with weak union protections. To investigate this proposition, we divide the sample into two groups: High vs. Low *Union Membership* group. An establishment belongs to High (Low) *Union Membership* group if its industry *Union Membership* is above (below or equal to) the sample median in a given year. The results are reported in Table 6 columns (1) and (2).

[18] In a study of coal mine safety, Morantz (2013) finds that unions are associated with a decline in traumatic and fatal injuries, but an increase in total and non-traumatic injuries. Weil (1992) finds that in construction workplaces, unionized sites have higher probabilities of inspection and face greater scrutiny than non-unionized sites.

Insert Table 6

As predicted, in the subsample with high *Union Membership,* injury rates do not change when an establishment is subjected to a tax increase. In contrast, injury rates increase significantly in the year following tax increases for the subsample of establishments with low *Union Membership*. A Wald test significantly rejects the null hypothesis that the coefficient estimates on $Tax\_Increase_{s,t}$ across High vs. Low *Union Membership* groups are not different. On the other hand, consistent with the results throughout this paper, injury rates appear not responding to tax cuts in either group.

In columns (3)-(4), we employ an alternative measure of union power. We obtain the percentage of employees that are covered by collective bargaining agreement each year in each industry from Unionstats. An establishment belongs to High (Low) *Bargain Agreement Coverage* group if its industry level *Bargain Agreement Coverage* is above (below or equal to) the sample median in a year.

Not surprisingly, we find similar results as those in columns (1) and (2). Establishments in industries with low bargaining power see a rise in injuries when there is a tax increase, but those in high bargaining power industries do not. Likewise, neither group responds to a tax cut.

5.2. Marginal tax rates

Firms have different exposure to a tax change depending on their profitability. For instance, if a firm is unprofitable, it will face little exposure to the change in income taxes. A firm that is highly profitable will face the opposite. Here we employ marginal tax rates (MTR) to measure firms' exposures to tax changes. We divide the sample into two groups: High vs. Low *Marginal Tax Rat*e. An establishment belongs to High (Low) *Marginal Tax Rate* group if its firm's *Marginal Tax Rate* is above (below or equal to) the sample median in a year. The results are reported in Table 7 columns (1) and (2).

We find that injury rates significantly increase following a tax increase in firms with high MTR, but do not change following tax cuts. On the other hand, firms subject to low MTR, injury rates do not respond to tax changes. Thus, consistent with the view that most changes in state tax occur in the top tax rates, we observe the strongest effects in firms with high marginal tax rates.

Insert Table 7

5.3. Earnings expectations

As discussed previously, Caskey and Ozel (2017) find significantly higher injury rates for firms that just barely beat or meet consensus earnings forecasts compared to firms that easily beat/miss these estimates. They argue that management makes short-term cuts to safety costs to report earnings in line with their consensus benchmark. We examine if the effect of tax policy is related to benchmark management by identifying firms that barely meet or beat forecasts.

We define benchmark managers following the prior literature (Burgstahler and Dichev, 1997; Caskey and Ozel, 2017) as firms meets or beats analysts' consensus earnings forecasts by two cents or less. Consistent with the results in Caskey and Ozel (2017), columns (3) and (4) show that firms that barely meet or beat earnings estimates experience a significant increase in injury rates when subjected to a tax increase. In contrast there is no significant change in injury rate responding to tax hikes in those firms that miss or beat earnings forecast by a wide margin. Thus, this finding confirms the view that when management is pressured to cater to Wall Street estimates, and is hit with a negative cash flow shock, safety is compromised.

5.4 Corporate safety culture

We employ the KLD database, provided by MSCI ESG Research, to measure a firm's overall safety environment. There exists an emerging literature on corporate social responsibility that almost exclusively relies on the KLD index to proxy for a firm's stance on corporate social responsibility (CSR).[19] There are seven qualitative areas addressed by KLD: community, corporate governance, diversity, employee relations, environment, human rights, and product. Each section has sub-categories that can be rated positively as a strength or negatively as a concern. We focus on two variables: "Health and Safety Strength" and "Health and Safety Concerns," to capture firms' corporate culture on safety. KLD assigns 0/1 rating for both variables. We construct a composite index, *Safety Index*, by taking the workplace safety strength rating and subtracting the workplace safety weakness rating provided by KLD. Firms with a *Safety Index* that equals to -1 are defined as those with a low safety rating. The remaining are defined as having a high safety rating. We conjecture that firms that rate high in this category are less likely to compromise employee welfare in the event of a tax hike because safety is valued in these firms' corporate culture. Columns (5) and (6) confirm this conjecture. Indeed, firms that are rated highly in safety index do not experience a significant rise in injury rates when they are hit with a tax hike. On the other hand, firms that rate poorly along this dimension do.

[19] For instance, see Bae, Kang, and Wan (2011), Deng, Kang, and Low (2013), and Di Giuli and Kostovetsky (2014) for studies using the KLD index.

5.5 Seasonal workers

It is a common belief that seasonal or temporary workers are more prone to injuries (Smith et al., 2010). Various reasons for this association have been given such as temporary workers are unfamiliar with the workplace and safety protocol and/or firms are unwilling to devote the resources to properly train temporary workers because of the costs involved. Therefore, we expect that establishments hiring seasonal workers are more sensitive to tax hikes than those not hiring seasonal workers. Fortunately, OSHA provides data on whether or not an establishment hires temporary workers. We investigate whether an establishment hires seasonal workers has a different effect of tax increases on workplace safety. Columns (7) and (8) show that establishments with or without seasonal workers both experience a significant rise in injury rates when hit with a tax increase, but establishments with seasonal workers experience a significantly larger increase.

6. Conclusion

There is arguably no single political issue that is more polarizing and generates as much attention as taxes. In this paper, we focus on changes in tax policy and worker safety. To our knowledge, we are the first to do so. This is a particularly important topic because worker safety directly or indirectly (through family or peers) impacts a significant proportion of the American workforce.

Our main punchline is that tax increases lead to worsening injury rates whereas tax cuts do not have a similar effect. We perform a battery of tests to ensure this result is robust. Our identification strategy relies on a difference-in-differences estimator where we make use of staggered state income tax policies allowing us to identify a plausible robust counterfactual. Specifically, for an establishment experiencing a tax change in a given state, we identify a control establishment in the same firm straddling a contiguous county border (in a different state not subject to a shock). This setup allows us to control for unobserved confounds such as local economic conditions or firm policies.

In addition to our main tests, we provide some complementary cross-sectional evidence. We conjecture that firm-level incentives likely impact firms' willingness to make adjustments to safety investment when faced with a cash flow shock. Consistent with this line of thinking, we find that firms operating in industries with low union membership or less bargain agreement coverage (both of which are proxies for employee rights and bargaining power) are more susceptible to an increase in injury rates when the tax rate increases. Likewise, we find that firms with higher marginal tax rates (those that are more likely to be impacted by a tax increase) and those that rate low on measures of safety culture experience a greater increase in injury rates when subject to a tax increase. We find firms that are pressured to make their numbers (meet/beat EPS estimates) show higher post-tax injury rates. Finally, we show that firms that hire seasonal workers are more sensitive to tax increases. This evidence shows how incentives and characteristics can influence real activities management.

Our paper can lend to the debate and our understanding regarding the efficacy of tax policy. The asymmetric finding (safety deteriorates when tax increase, but does not improve for tax cut) does not support the argument that businesses invest tax cuts back in their business. We acknowledge that our paper does not address typical corporate investments such as PPE or R&D. Nonetheless, investments in worker safety are indeed important and impact a very large constituency. And precisely because they are not the typical investments, disclosure is not mandated making them at risk to cuts.

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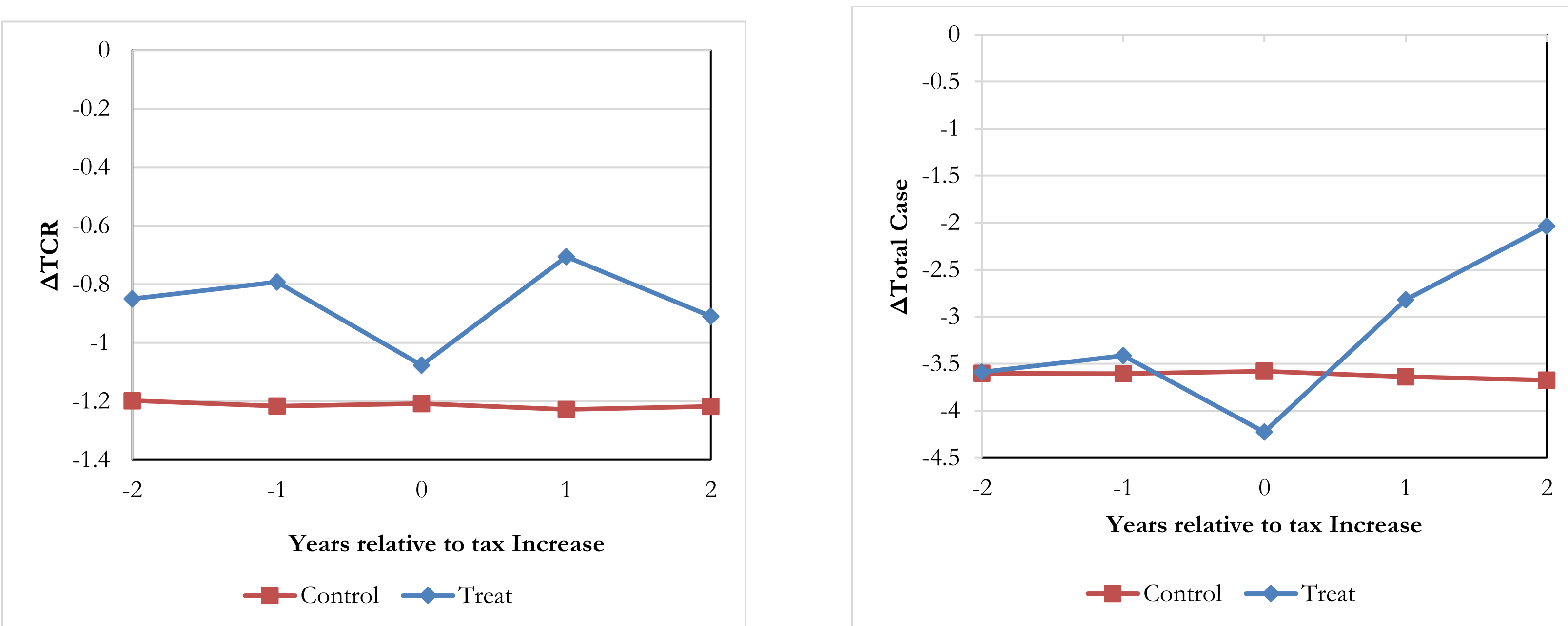
Figure 1

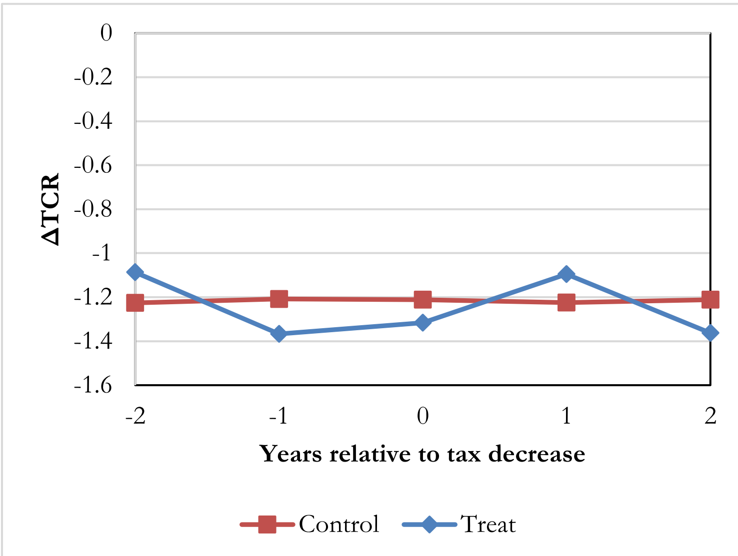
Injury Rate and Corporate Taxes: Pre-Trends and Post-Trends.

The figures plot annual change of total case rate (*ΔTCR*) and change of total case (*ΔTotal Case)* during 2 years before and 2 years after state corporate tax increase (Panel A) and tax decrease (Panel B) in year 0. *TCR* is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000. *Total Case* is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases. Treated group contains establishments located in a state that experiences an increase (or decrease) in corporate tax rate. Control group contains establishments located in a state that does not experience any change in corporate tax rate.

Panel A. Tax Increase



Panel B. Tax Decrease



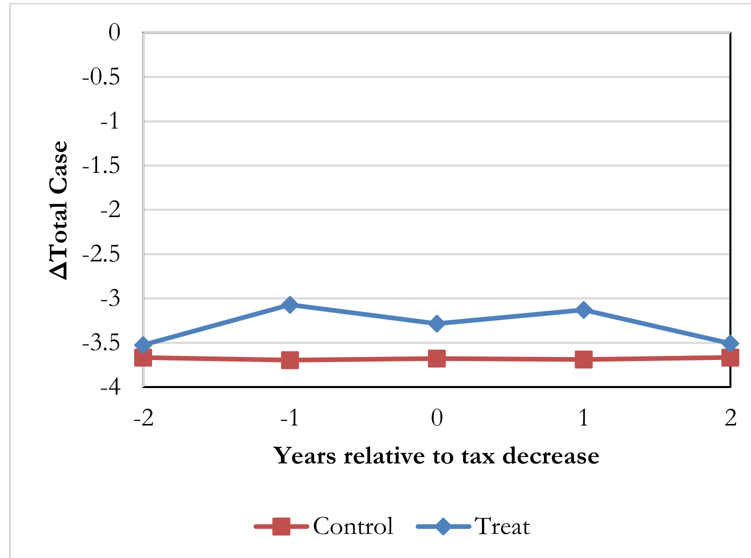


Table 1
Summary Statistics.

This table presents summary statistics of our sample. The sample consists of establishments from Occupational Safety and Health Administration (OSHA) during 2002-2011 that belong to non-financial and non-utility firms. Workplace safety related variables are defined as following: *TCR* is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000. *DART* is the number of injuries and illnesses that result in days away from work or with job restriction or transfer, divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000. *DAFWII* is the number of injuries and illnesses that result in days away from work, divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000. *Total Case* is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases. *ΔTCR, ΔTotal Case*, *ΔDART*, and *ΔDAFWII* are changes in *TCR, Total Case, DART, and DAFWII*, respectively. Variables that are on firm level are defined as following: *Assets* is total assets (in $billions) at a fiscal year end; *Sales* is total sales (in $billions) in a fiscal year; *Leverage* is total short-term and long-term debt divided by total assets at a fiscal year end; *Market-to-Book* is market value of assets divided by book value of assets at a fiscal year end; *PPE/Assets* is net property, plant, and equipment divided by total assets; *Sales/Assets* is total sales divided by total assets; *CAPEX/Assets* is capital expenditure divided by total assets; *FCF/Assets* is total free cash flows divided by total assets; *Cash/Assets* is cash and short-term investments divided by total assets; *Dividends/Assets* is total cash dividends paid to common shares divided by total assets. Variables that are on establishment-level are defined as following: *Number of Employee* (in 000s) is total number of employees working in a given establishment during the year; *Hours Per Employee* (in 000s) is total number of annual hours worked in a given establishment divided by the number of employees. Variables that are on state-level are defined as following: *Real GSP Growth Rate* is the growth rate of real Gross State Product; *State Unemployment Rate* is the unemployment rate in a state.

Variable	N	Mean	P25	Median	P75	Standard Deviation
TCR	14,802	9.650	3.911	8.311	13.590	7.676
Total Case	14,802	30.620	5.000	12.000	24.000	95.200
DART	14,802	6.453	1.979	5.321	9.214	5.814
DAFWII	14,802	2.764	0.280	1.599	3.723	3.669
ΔTCR	14,802	-1.216	-3.907	-0.767	1.477	5.658
ΔTotal Case	14,802	-3.662	-6.000	-1.000	2.000	14.900
ΔDART	14,802	-0.967	-3.095	-0.457	1.093	4.542
ΔDAFWII	14,802	-0.456	-1.504	-0.025	0.616	2.742
Change of Tax Rate (tax increases) (%)	13	0.800	0.500	0.750	1.150	0.282
Change of Tax Rate (tax decreases) (%)	20	-1.046	-1.150	-0.500	-0.250	1.382
Number of Employee (in 000s)	14,802	0.345	0.090	0.152	0.305	0.806
Hours Per Employee (in 000s)	14,802	1.852	1.612	1.860	2.066	0.561
Assets (in $billions)	14,802	16.890	1.585	10.750	31.880	16.020
Sales (in $billions)	14,802	42.580	1.989	13.790	43.240	79.700
PPE/Assets	14,802	0.413	0.252	0.423	0.573	0.194
Sales/Assets	14,802	1.503	1.021	1.431	1.851	0.730
CAPEX/Assets	14,802	0.063	0.031	0.055	0.090	0.042
FCF/Assets	14,802	0.063	0.027	0.059	0.114	0.086
Cash/Assets	14,802	0.085	0.029	0.050	0.114	0.095
Dividends/Assets	14,802	0.016	0.000	0.010	0.021	0.031
Leverage	14,802	0.262	0.143	0.242	0.337	0.183
Market-to-Book	14,802	1.774	1.107	1.562	2.428	0.878
State GSP Growth Rate	14,802	0.018	0.006	0.021	0.032	0.024
State Unemployment Rate	14,802	5.753	4.783	5.408	6.300	1.618

Table 2

State-level Corporate Taxes and Workplace Safety: Baseline Results.

This table presents the regression results of how workplace injury rates changes in establishments respond to changes in corporate income taxes in the states where the establishments locate. The sample consists of establishments from Occupational Safety and Health Administration (OSHA) during 2002-2011 that belong to non-financial and non-utility firms. For all specifications, the dependent variable is ΔTCR_{t+1} (change of total case rate (*TCR*) from year t to year t+1, where *TCR* is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000). Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state experiences an increase or decrease in corporate tax rate in year t. Definitions of control variables are reported in the Appendix A. The unit of analysis is an establishment-year. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}
$Tax\_Increase_{s,t}$	0.648**	0.673**	0.453**				0.650**	0.672**	0.453**
	(0.034)	(0.027)	(0.038)				(0.033)	(0.027)	(0.038)
$Tax\_Decrease_{s,t}$				0.040	-0.040	0.079	0.059	-0.020	0.081
				(0.878)	(0.873)	(0.731)	(0.818)	(0.934)	(0.719)
$\Delta Ln(Assets)_t$	-0.239	0.135	0.425	-0.221	0.148	0.419	-0.237	0.135	0.425
	(0.469)	(0.678)	(0.273)	(0.500)	(0.648)	(0.282)	(0.473)	(0.680)	(0.273)
$\Delta Leverage_t$	-0.012	-0.236	1.052	-0.045	-0.260	1.061	-0.016	-0.235	1.051
	(0.985)	(0.720)	(0.160)	(0.943)	(0.689)	(0.157)	(0.980)	(0.721)	(0.160)
$\Delta PPE/Assets_t$	-2.994*	-1.463	-0.991	-2.898*	-1.368	-0.993	-2.995*	-1.462	-0.989
	(0.069)	(0.409)	(0.581)	(0.078)	(0.442)	(0.581)	(0.069)	(0.409)	(0.582)
$\Delta Sales/Assets_t$	0.146	0.044	0.362	0.146	0.049	0.351	0.143	0.045	0.360
	(0.669)	(0.905)	(0.383)	(0.673)	(0.896)	(0.399)	(0.679)	(0.904)	(0.386)
$\Delta CAPEX/Assets_t$	2.171	-0.622	3.309	1.983	-0.808	3.272	2.168	-0.624	3.297
	(0.541)	(0.867)	(0.387)	(0.570)	(0.825)	(0.390)	(0.542)	(0.867)	(0.390)
$\Delta Market\text{-}to\text{-}Book_t$	0.101	-0.004	-0.035	0.089	-0.019	-0.033	0.102	-0.005	-0.034
	(0.622)	(0.984)	(0.908)	(0.679)	(0.930)	(0.913)	(0.618)	(0.982)	(0.911)
$\Delta FCF/Assets_t$	-1.950	-1.851	-0.884	-1.924	-1.814	-0.882	-1.954	-1.849	-0.889
	(0.207)	(0.227)	(0.585)	(0.221)	(0.244)	(0.587)	(0.206)	(0.227)	(0.583)

ΔCash/Assets$_t$	2.109	2.457*	1.020	2.115	2.490*	0.996	2.103	2.460*	1.012
	(0.132)	(0.086)	(0.496)	(0.133)	(0.083)	(0.506)	(0.134)	(0.087)	(0.501)
ΔDividends/Assets$_t$	-6.470	7.658	-2.385	-6.516	7.579	-2.341	-6.459	7.662	-2.410
	(0.489)	(0.464)	(0.812)	(0.484)	(0.466)	(0.815)	(0.489)	(0.463)	(0.810)
ΔLn(Number of Employee)$_t$	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ΔHours Per Employee$_t$	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ΔState GSP Growth$_t$	-6.006*	-5.446	0.090	-5.197	-4.721	0.652	-5.934	-5.471	0.182
	(0.099)	(0.139)	(0.969)	(0.199)	(0.247)	(0.777)	(0.119)	(0.153)	(0.938)
ΔState Unemployment Rate$_t$	0.028	-0.024	-0.155	0.056	0.004	-0.126	0.029	-0.024	-0.153
	(0.579)	(0.655)	(0.233)	(0.271)	(0.933)	(0.319)	(0.578)	(0.656)	(0.242)
Year Fixed Effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Industry-Year FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802
Adjusted R2	0.015	0.019	0.074	0.014	0.018	0.073	0.015	0.019	0.073

Table 3

Potential Confounds: Effects of Local Economic Condition.

This table presents the regression results to address potential confounds – unobserved changes in local economic conditions that affect the changes in both state corporate tax rate and firms' investments in workplace safety. See Table 2 for a description of the sample. In Panel A, we restrict control establishments to those located in neighboring states, thus excluding distant states. In Panel B, we restrict the sample consisting of treated and control establishments in contiguous counties on either side of a state border. In Panel C, we restrict the control establishments to those belonging to the same firm of the treated establishments. In Panel D, we start with the controls in Panel C and further require them to reside in neighboring states. In Panel E, we start with the controls in Panel C and further require them to be located in contiguous counties on other side of a state border compared to the treated one. The dependent variables are changes in four measures of workplace injury: *ΔTCR, ΔTotal Case*, *ΔDART*, and *ΔDAFWII* in columns (1)-(4), respectively. Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state increased or decreased its corporate tax rate in year t. Definitions of variables are in the Appendix A. The unit of analysis is an establishment-year. State-, firm- and establishment-specific variables are included however not report for brevity. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Neighboring States

Dependent Variable	(1) ΔTCR_{t+1}	(2) $\Delta Total\ Case_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t}$	0.442*	1.067*	0.670***	0.217*
	(0.054)	(0.053)	(0.001)	(0.051)
$Tax\_Decrease_{s,t}$	-0.095	0.233	0.054	0.144
	(0.704)	(0.645)	(0.809)	(0.216)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	5,895	5,895	5,895	5,895
Adjusted R^2	0.064	0.180	0.082	0.038

Panel B: Contiguous Boarder Counties

Dependent Variable	(1) ΔTCR_{t+1}	(2) $\Delta Total\ Case_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t}$	1.021*	3.255*	1.985***	0.655*
	(0.091)	(0.079)	(0.001)	(0.065)
$Tax\_Decrease_{s,t}$	-0.403	1.024	-0.464	-0.118
	(0.515)	(0.626)	(0.293)	(0.731)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
County-Pair-Year FE	YES	YES	YES	YES
Observations	1,586	1,586	1,586	1,586
Adjusted R^2	0.556	0.556	0.536	0.552

Panel C: Treated and Untreated Establishments in the Same Firm

Dependent Variable	(1) ΔTCR_{t+1}	(2) $\Delta Total\ Case_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t}$	0.535*	1.983*	0.597***	0.373**
	(0.073)	(0.080)	(0.001)	(0.012)
$Tax\_Decrease_{s,t}$	0.197	0.852	0.349	0.204
	(0.514)	(0.368)	(0.143)	(0.174)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	9,183	9,183	9,183	9,183
Adjusted R^2	0.094	0.228	0.107	0.038

Panel D: Treated and Untreated Establishments in the Same Firm and Neighboring States

Dependent Variable	(1) ΔTCR_{t+1}	(2) $\Delta Total\ Case_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t}$	0.507**	1.866**	0.595**	0.391**
	(0.047)	(0.019)	(0.018)	(0.011)
$Tax\_Decrease_{s,t}$	0.245	1.227	0.272	0.259
	(0.383)	(0.197)	(0.234)	(0.165)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	4,449	4,449	4,449	4,449
Adjusted R^2	0.095	0.134	0.105	0.056

Panel E: Treated and Untreated Establishments in the Same Firm and Contiguous Boarder Counties

Dependent Variable	(1) ΔTCR_{t+1}	(2) $\Delta Total\ Case_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t}$	0.124	2.544*	0.883*	0.842**
	(0.843)	(0.057)	(0.069)	(0.020)
$Tax\_Decrease_{s,t}$	1.262	0.272	-0.424	-0.063
	(0.126)	(0.945)	(0.317)	(0.810)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
County-Pair-Year FE	YES	YES	YES	YES
Observations	1,185	1,185	1,185	1,185
Adjusted R^2	0.284	0.400	0.303	0.561

Table 4

Excluding Anticipated Tax Change.

This table presents the regression results by excluding establishments located in states subject to anticipated changes in tax rates. See Table 2 for a description of the sample. Ljungqvist and Smolyansky (2016) examine whether state tax rates follow a Martingale (i.e., changes in tax rates are unpredictable). Based on their findings, we exclude establishments located in Connecticut or Massachusetts in Panel A; we exclude establishments located in New England (i.e. Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, or Vermont) in Panel B; An alternative way of addressing the anticipation effect is the "narrative approach" of Romer and Romer (2010). Following the findings in Heider and Ljungqvist (2015), we exclude establishments located in Colorado, Connecticut, Minnesota, or New York in Panel C. The dependent variables are changes in four measures of workplace injury: *ΔTCR, ΔTotal Case*, *ΔDART*, and *ΔDAFWII* in columns (1)-(4), respectively. Definitions of variables are in the Appendix A. The unit of analysis is an establishment-year. State-, firm- and establishment-specific variables are included however not report for brevity. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Excluding Connecticut, Massachusetts (CT, MA)

Dependent Variable	(1) *ΔTCR* $_{t+1}$	(2) *ΔTotal Case* $_{t+1}$	(3) *ΔDART* $_{t+1}$	(4) *ΔDAFWII* $_{t+1}$
Tax_Increase$_{s,t}$	0.535***	1.210**	0.665***	0.276**
	(0.008)	(0.036)	(0.000)	(0.016)
Tax_Decrease$_{s,t}$	0.019	0.068	0.093	0.205
	(0.935)	(0.893)	(0.653)	(0.110)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	14,109	14,109	14,109	14,109
Adjusted R2	0.073	0.166	0.085	0.028

Panel B: Excluding New England (CT, ME, MA, NH, RI, VT)

Dependent Variable	(1) *ΔTCR* $_{t+1}$	(2) *ΔTotal Case* $_{t+1}$	(3) *ΔDART* $_{t+1}$	(4) *ΔDAFWII* $_{t+1}$
Tax_Increase$_{s,t}$	0.529***	1.267**	0.656***	0.279**
	(0.010)	(0.031)	(0.000)	(0.016)
Tax_Decrease$_{s,t}$	0.010	-0.007	0.064	0.176
	(0.966)	(0.989)	(0.761)	(0.171)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	13,820	13,820	13,820	13,820
Adjusted R2	0.066	0.086	0.083	0.025

Panel C: Excluding Colorado, Connecticut, Minnesota, or New York (CO, CT, MN, NY)

Dependent Variable	(1) *ΔTCR* $_{t+1}$	(2) *ΔTotal Case* $_{t+1}$	(3) *ΔDART* $_{t+1}$	(4) *ΔDAFWII* $_{t+1}$
Tax_Increase$_{s,t}$	0.536***	1.234**	0.645***	0.284**
	(0.007)	(0.038)	(0.000)	(0.015)
Tax_Decrease$_{s,t}$	-0.072	-0.022	0.020	0.162
	(0.757)	(0.968)	(0.924)	(0.227)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	13,379	13,379	13,379	13,379
Adjusted R2	0.065	0.089	0.082	0.023

Table 5

Testing for Pre-Trends and Reverse Causality.

To assess possible pre-trends and reverse causality, we include three lags and up to one lead in the baseline regression with industry and year fixed effects. See Table 2 for a description of the sample. The dependent variables are changes in four measures of workplace injury in year $t+1$: ΔTCR_{t+1}, $\Delta Total\ Case_{t+1}$, $\Delta DART_{t+1}$, and $\Delta DAFWII_{t+1}$ in columns (1)-(4), respectively. We include a set of leading and lagging indicator variables of tax rate changes: $Tax\_Increase_{s,t-2}$, $Tax\_Increase_{s,t-1}$, $Tax\_Increase_{s,t}$, $Tax\_Increase_{s,t+1}$, and $Tax\_Increase_{s,t+2}$ are dummy variables that equal one if a state increased its corporate tax rate in lagging year *t-2, t-1, t,* contemporaneous year $t+1$, and leading year $t+2$, respectively. Tax_Decrease dummy variables are defined similarly. We include the same set of state-, firm- and establishment-specific variables as those in Table 2, however not report below for brevity. Definitions of variables are in the Appendix A. The unit of analysis is an establishment-year. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	(1) ΔTCR_{t+1}	(2) $\Delta Total\ Case_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t-2}$	0.026	-0.075	-0.126	0.043
	(0.940)	(0.928)	(0.529)	(0.718)
$Tax\_Increase_{s,t-1}$	0.210	0.662	0.246	-0.063
	(0.525)	(0.194)	(0.275)	(0.523)
$Tax\_Increase_{s,t}$	0.611**	0.892*	0.838***	0.337**
	(0.019)	(0.066)	(0.000)	(0.010)
$Tax\_Increase_{s,t+1}$	0.020	-0.074	-0.160	0.032
	(0.953)	(0.929)	(0.396)	(0.802)
$Tax\_Increase_{s,t+2}$	-0.034	-0.193	-0.234	-0.148
	(0.915)	(0.857)	(0.180)	(0.480)
$Tax\_Decrease_{s,t-2}$	0.563*	0.471	0.330	0.186
	(0.060)	(0.422)	(0.139)	(0.157)
$Tax\_Decrease_{s,t-1}$	0.136	0.376	0.134	0.049
	(0.551)	(0.401)	(0.311)	(0.696)
$Tax\_Decrease_{s,t}$	0.102	0.146	0.194	0.215*
	(0.661)	(0.723)	(0.372)	(0.070)
$Tax\_Decrease_{s,t+1}$	-0.063	0.305	0.082	0.007
	(0.707)	(0.251)	(0.666)	(0.961)
$Tax\_Decrease_{s,t+2}$	-0.252	-0.235	-0.123	-0.215*
	(0.124)	(0.461)	(0.409)	(0.079)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	14,802	14,802	14,802	14,802
Adjusted R2	0.067	0.162	0.077	0.026

Table 6

Union Power and Labor Intensity.

This table presents the results on how union power and labor intensity affect the treatment effect. See Table 2 for a description of the sample. For all specifications, the dependent variable is ΔTCR_{t+1}, the change of total case rate *TCR* from year t to year t+1. Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state increased or decreased its corporate tax rate in year t. In columns (1) and (2), we divide the sample into two groups: High vs. Low *Union Membership* group. An establishment belongs to High (Low) *Union Membership* group if its industry *Union Membership* is above (below or equal to) the sample median in a year. In columns (3) and (4), we divide the sample into two groups: High vs. Low *Bargain Agreement Coverage* group. An establishment belongs to High (Low) *Bargain Agreement Coverage* group if its industry *Bargain Agreement Coverage* is above (below or equal to) the sample median in a year. In columns (5) and (6), we divide the sample into two groups: High (Low) *Labor Intensity* group. A firm belongs to High (Low) *Labor Intensity* group if its *Labor Intensity* is above (below or equal to) the sample median in a year. Definitions of control variables are in the Appendix A. The unit of analysis is an establishment-year. State-, firm- and establishment-specific variables are included however not report for brevity. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. Wald tests are conducted to test the null hypothesis that the coefficient estimates on *Tax_Increase* are not different between the two subsamples. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	*Union Membership*		*Bargain Agreement Coverage*		*Labor Intensity*	
	(1)	(2)	(3)	(4)	(5)	(6)
	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}
	High	Low	High	Low	High	Low
Tax_Increase$_{s,t}$ (β_1)	0.063	0.918***	0.018	0.956***	0.133	0.846***
	(0.863)	(0.006)	(0.960)	(0.002)	(0.764)	(0.008)
Tax_Decrease$_{s,t}$ (β_2)	-0.058	0.081	-0.039	0.090	0.172	-0.089
	(0.828)	(0.799)	(0.878)	(0.762)	(0.449)	(0.835)
Wald Test on (β_1)	χ^2=4.31**		χ^2=6.81***		χ^2=2.58	
	(0.024)		(0.001)		(0.108)	
Control Variables	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES
Observations	6,013	6,906	6,081	6,838	6,331	6,588
Adjusted R2	0.032	0.129	0.036	0.128	0.041	0.124

Table 7

Other Differential Treatment Effects.

This table presents the results on how marginal tax rate, and meeting or beating analyst earnings forecast, and corporate safety culture rating affect the treatment effect. See Table 2 for a description of the sample. For all specifications, the dependent variable is ΔTCR_{t+1}, the change of total case rate *TCR* from year t to year t+1. Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state increased or decreased its corporate tax rate in year t. In columns (1) and (2), we divide the sample into two groups: High vs. Low *Marginal Tax Rat*e group. A firm belongs to High (Low) *Marginal Tax Rate* group if its *Marginal Tax Rate* is above (below or equal to) the sample median in a year. In columns (3) and (4), we divide the sample into two groups: Yes vs. No *Meet or Beat Earning Expectation*. A firm belongs to Yes group if it a firm meets or beats analysts' consensus earnings forecasts by two cents or less; otherwise, it belongs to No group. In columns (5) and (6), we divide the sample into two groups: High vs. Low *Safety Culture Rating*. We construct a composite index, *Safety Index*, by taking the workplace safety strength rating then subtracting the workplace safety weakness rating provided by KLD database. A firm with a *Safety Index* that equals to -1 are defined as those with Low *Safety Culture Rating*; otherwise, a firm defined as with High *Safety Culture Rating*. In columns (7) and (8), we divide the sample based on whether or not an establishment has employed seasonal workers in a year. Definitions of control variables are in the Appendix A. The unit of analysis is an establishment-year. State-, firm- and establishment-specific variables are included however not report for brevity. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. Wald tests are conducted to test the null hypothesis that the coefficient estimates on *Tax_Increase* are not different between the two subsamples. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Marginal Tax Rate		*Meet or Beat Earning Expectation*		*Safety Culture Rating*		*Seasonal Workers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}
	High	Low	Yes	No	High	Low	Yes	No
$Tax\_Increase_{s,t}$ (β_1)	1.072***	0.037	1.583**	0.328	-0.948	0.910***	2.227**	0.396*
	(0.008)	(0.901)	(0.022)	(0.237)	(0.221)	(0.002)	(0.019)	(0.058)
$Tax\_Decrease_{s,t}$ (β_2)	0.024	0.027	0.958	-0.247	0.414	-0.207	-1.111	0.086
	(0.915)	(0.950)	(0.101)	(0.293)	(0.258)	(0.500)	(0.592)	(0.700)
Wald Test on (β_1)	χ^2=3.37*		χ^2=2.73*		χ^2=6.66***		χ^2=5.53**	
	(0.066)		(0.098)		(0.001)		(0.019)	
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	5,877	7,042	2,920	11,882	7,765	3,796	539	14,263
Adjusted R2	0.126	0.062	0.048	0.076	0.055	0.159	0.031	0.076

Appendix A
Variable Definitions.

Variables	Definitions
State Variables	
Tax_Increase	An indicator variable equaling one if a state increased its corporate tax rate in year t
Tax_Decrease	An indicator variable equaling one if a state decreased its corporate tax rate in year t
⊿State GSP Growth	Change in Real gross state product (GSP) growth rate
⊿State Unemployment Rate	Change in the state unemployment rate
⊿Alcohol	Change in alcohol consumption for each state in each year, which is the age-standardized prevalence of drinking by both males and females, available at http://www.healthdata.org/us-health/data-download.
⊿Auto Accident	Change in auto accident rate for each state in each year, which is the fatality rate per 100 million vehicle miles traveled, including all accidents for both males and females, available at https://www-fars.nhtsa.dot.gov/States/StatesCrashesAndAllVictims.aspx.
Establishment-Specific Variables	
TCR	Sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000.
DART	The number of injuries and illnesses that result in days away from work or with job restriction or transfer, divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000
DAFWII	The number of injuries and illnesses that result in days away from work, divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000
Total Case	Sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases.
⊿Number of Employee	Change in total number of employees working in a given establishment during the year.
⊿Hours Per Employee	Change in total number of annual hours worked in a given establishment divided by the number of employees.
Firm-Specific Variables	
⊿Assets	Change in book value of total assets measured at the end of each fiscal year.

∆Leverage	Change in firm's total short-term and long-term debt divided by total assets.
∆PPE/Assets	Change in firm's net property, plant, and equipment divided by total assets.
∆Sales/Assets	Change in firm's total sales divided by total assets.
∆CAPEX/Assets	Change in firm's capital expenditure divided by total assets.
∆Market-to-Book	Change in firm's market value of assets divided by book value of assets. Market value of assets equals the sum of market value of equity, book value of total liabilities, and liquidation value of preferred stock minus deferred tax liabilities.
∆FCF/Assets	Change in firm's total free cash flows divided by total assets, which equals (oibdq-xint-txdi-capx)/at.
∆Cash/Assets	Change in firm's cash and short-term investments divided by total assets.
∆Dividends/Assets	Change in firm's total cash dividends paid to common shares divided by total assets.
Union Membership	Percentage of employees that are union memberships, by each year and each industry. Data source: http://www.unionstats.com/
Bargain Agreement Coverage	Percentage of employees that are covered by collective bargaining agreement, by each year and each industry. Data source: http://www.unionstats.com/
Marginal Tax Rate	Simulated marginal tax rates (after interest expense) from Blouin, Core, and Guay (2010)
Labor Intensity	Following Agrawal and Matsa (2013), labor intensity is calculated as the ratio of labor and pension expense to sales in Compustat.
Meet or Beat Earning Expectation	Indicator variable that equals one if a firm meets or beats analysts' consensus earnings forecasts by two cents or less, and zero otherwise. The consensus earnings forecast is computed based on each analyst's latest forecast issued between 180 to 4 days before earnings announcement

Appendix Table B1

Alternative Measurements of Injury Rate.

This table presents the regression results of how workplace injury rates changes in establishments responding to changes in corporate income taxes in the states where the establishments locate, using alternative measurements of injury rates. See Table 2 for a description of the sample. In Panel A, the dependent variable is $\Delta Total\ Case_{t+1}$ (i.e. the change of *Total Case* from year t to year t+1); in Panel B, the dependent variable is $\Delta DART_{t+1}$ (i.e. the change of *DART* from year t to year t+1); in Panel C, the dependent variable is $\Delta DAFWII_{t+1}$ (i.e. the change of *DAFWII* from year t to year t+1). Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state increased or decreased its corporate tax rate in year t. We include the same set of state-, firm- and establishment-specific variables as those in Table 2, however not report below for brevity. Definitions of variables are in the Appendix A. The unit of analysis is an establishment-year. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: *ΔTotal Case*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$	$\Delta Total\ Case_{t+1}$
$Tax\_Increase_{s,t}$	0.907**	0.897*	0.860*				0.927**	0.907*	0.861*
	(0.045)	(0.066)	(0.079)				(0.039)	(0.062)	(0.078)
$Tax\_Decrease_{s,t}$				0.572	0.277	0.155	0.599	0.304	0.161
				(0.274)	(0.599)	(0.740)	(0.253)	(0.563)	(0.729)
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Industry-Year FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802
Adjusted R2	0.120	0.139	0.163	0.120	0.139	0.164	0.120	0.139	0.120

Panel B: $\Delta DART$

Dependent Variable	(1) $\Delta DART_{t+1}$	(2) $\Delta DART_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DART_{t+1}$	(5) $\Delta DART_{t+1}$	(6) $\Delta DART_{t+1}$	(7) $\Delta DART_{t+1}$	(8) $\Delta DART_{t+1}$	(9) $\Delta DART_{t+1}$
$Tax\_Increase_{s,t}$	0.515**	0.546**	0.628***				0.519**	0.548**	0.629***
	(0.013)	(0.013)	(0.000)				(0.012)	(0.012)	(0.000)
$Tax\_Decrease_{s,t}$				0.103	0.042	0.129	0.118	0.058	0.133
				(0.602)	(0.820)	(0.522)	(0.540)	(0.747)	(0.500)
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Industry-Year FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802
Adjusted R2	0.017	0.022	0.084	0.016	0.021	0.017	0.017	0.022	0.084

Panel C: $\Delta DAFWII$

Dependent Variable	(1) $\Delta DAFWII_{t+1}$	(2) $\Delta DAFWII_{t+1}$	(3) $\Delta DAFWII_{t+1}$	(4) $\Delta DAFWII_{t+1}$	(5) $\Delta DAFWII_{t+1}$	(6) $\Delta DAFWII_{t+1}$	(7) $\Delta DAFWII_{t+1}$	(8) $\Delta DAFWII_{t+1}$	(9) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t}$	0.227**	0.247**	0.223**				0.233**	0.253**	0.225**
	(0.023)	(0.014)	(0.039)				(0.019)	(0.012)	(0.039)
$Tax\_Decrease_{s,t}$				0.177	0.145	0.165	0.184	0.152	0.167
				(0.192)	(0.158)	(0.134)	(0.177)	(0.136)	(0.129)
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Industry-Year FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802
Adjusted R2	0.006	0.010	0.027	0.007	0.010	0.028	0.006	0.010	0.028

Appendix Table B2

Long-term Effect on Injury Rate.

This table presents the regression results of how long-term workplace injury rates changes in establishments responding to changes in corporate income taxes in the states where the establishments locate. See Table 2 for a description of the sample. The dependent variables are changes in four measures of workplace injury: *⊿TCR* in columns (1)-(2), *⊿Total Case* in columns (3)-(4), *⊿DART* in columns (5)-(6), and *⊿DAFWII* in columns (7)-(8), respectively. Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state increased or decreased its corporate tax rate in year t. We include the same set of state-, firm- and establishment-specific variables as those in Table 2, however not report below for brevity. Definitions of variables are in the Appendix A. The unit of analysis is an establishment-year. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	(1) $⊿TCR_{t+2}$	(2) $⊿TCR_{t+3}$	(3) $⊿Total\ Case_{t+2}$	(4) $⊿Total\ Case_{t+3}$	(5) $⊿DART_{t+2}$	(6) $⊿DART_{t+3}$	(7) $DAFWII_{t+2}$	(8) $DAFWII_{t+3}$
$Tax\_Increase_{s,t}$	0.134	-0.265	0.786	0.817	0.185	-0.327**	-0.109	0.038
	(0.666)	(0.101)	(0.159)	(0.388)	(0.390)	(0.038)	(0.308)	(0.818)
$Tax\_Decrease_{s,t}$	0.072	0.521*	0.413	-0.733	0.090	0.263	0.002	0.173
	(0.714)	(0.079)	(0.361)	(0.709)	(0.478)	(0.247)	(0.985)	(0.140)
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	14,802	14,802	14,802	14,802	14,802	14,802	14,802	14,802
Adjusted R2	0.073	0.055	0.163	0.179	0.083	0.071	0.027	0.021

Appendix Table B3

Alternative Clustering and State FE.

This table presents the results of OLS regressions that estimate the relationship between state-level corporate taxes and workplace safety. See Table 2 for a description of the sample. For all specifications, the dependent variable is ΔTCR_{t+1} (change of total case rate (*TCR*) from year t to year t+1, where *TCR* is the sum of deaths and all injuries and illnesses that result in days away from work or with job restriction or transfer, and other recordable cases divided by the number of hours worked by all employees in a given establishment-year, then multiplied by 200,000). Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state increased or decreased its corporate tax rate in year t. We include the same set of state-, firm- and establishment-specific variables as those in Table 2, however not report below for brevity. In columns (1) and (2), we include industry-year fixed effects. P-values based on robust standard errors clustered at year, and at year and state level are reported in parentheses under the corresponding estimated coefficients in columns (1) and (2), respectively. In columns (3) and (4), we include state fixed effects. P-values based on robust standard errors clustered at year, and at year and state level are reported in parentheses under the corresponding estimated coefficients in columns (3) and (4), respectively Definitions of variables are in the Appendix A. The unit of analysis is an establishment-year. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}	ΔTCR_{t+1}
$Tax\_Increase_{s,t}$	0.453***	0.453**	0.567***	0.567*
	(0.004)	(0.027)	(0.006)	(0.053)
$Tax\_Decrease_{s,t}$	0.081	0.081	0.051	0.051
	(0.725)	(0.676)	(0.785)	(0.807)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	NO	NO
State FE	NO	NO	YES	YES
Cluster SE	Year	Year_State	Year	Year_State
Observations	14,802	14,802	14,802	14,802
Adjusted R2	0.074	0.074	0.016	0.016

Appendix Table B4

Tax Rate Change

This table presents the results of OLS regressions that estimate the relationship between state-level corporate taxes and workplace safety. See Table 2 for a description of the sample. The dependent variables are changes in four measures of workplace injury: *⊿TCR, ⊿Total Case*, *⊿DART*, and *⊿DAFWII* in columns (1)-(4), respectively. Independent variables are $⊿Tax\_Increase\_Rate_{s,t}$ and $⊿Tax\_Decrease\_Rate_{s,t}$, which are continuous variables that measures the percentage point change of a state's increased or decreased corporate tax rate in year t. We include the same set of state-, firm- and establishment-specific variables as those in Table 2, however not report below for brevity. We include the same set of state-, firm- and establishment-specific variables as those in Table 2, however not report below for brevity. Definitions of variables are in the Appendix A. The unit of analysis is an establishment-year. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable	$⊿TCR_{t+1}$	$⊿Total\ Case_{t+1}$	$⊿DART_{t+1}$	$⊿DAFWII_{t+1}$
$⊿Tax\_Increase\_Rate_{s,t}$	0.832*	7.154**	0.817*	0.738**
	(0.065)	(0.029)	(0.097)	(0.015)
$⊿Tax\_Decrease\_Rate_{s,t}$	-0.008	0.094	0.028	-0.001
	(0.930)	(0.901)	(0.762)	(0.976)
Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	13,922	13,922	13,922	13,922
Adjusted R2	0.056	0.200	0.073	0.020

Appendix Table B5

Alcohol Consumption and Auto Accident Rate

This table presents the results on alcohol consumption and auto accident. See Table 2 for a description of the sample. In Panel A, we estimate the relationship between state-level corporate taxes and workplace safety, with alcohol consumption and auto accident rate as additional control variable. The dependent variables are changes in four measures of workplace injury: *ΔTCR, ΔTotal Case*, *ΔDART*, and *ΔDAFWII* in columns (1)-(4), respectively. We include the same set of state-, firm- and establishment-specific variables as those in Table 2, however not report below for brevity. In addition to the control variables in Table 2, we add two more control variables: changes in state-level auto accident rate and changes in county-level alcohol consumption (*ΔAlcohol* and *ΔAuto Accident*). In Panel B, the dependent variables are *ΔAlcohol* and *ΔAuto Accident* in column (1) and column (2), respectively. Independent variables are $Tax\_Increase_{s,t}$ and $Tax\_Decrease_{s,t}$, which are indicator variables equaling one if a state increased or decreased its corporate tax rate in year t. Definitions of variables are reported in the Appendix A. The unit of analysis is an establishment-year. P-values based on robust standard errors clustered at state level are reported in parentheses under the corresponding estimated coefficients. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Additional Control Variables

Dependent Variable	(1) ΔTCR_{t+1}	(2) $\Delta Total\ Case_{t+1}$	(3) $\Delta DART_{t+1}$	(4) $\Delta DAFWII_{t+1}$
$Tax\_Increase_{s,t}$	0.454**	0.869*	0.627***	0.225**
	(0.037)	(0.082)	(0.000)	(0.037)
$Tax\_Decrease_{s,t}$	0.064	0.174	0.071	0.126
	(0.764)	(0.671)	(0.731)	(0.273)
Control Variables	YES	YES	YES	YES
Additional Control Variables	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
Observations	14,802	14,802	14,802	14,802
Adjusted R2	0.074	0.163	0.084	0.028

Panel B: How Alcohol Consumption and Auto Accident Respond to Tax Changes

Dependent Variable	(1) $\Delta Alcohol_{t+1}$	(2) $\Delta Auto\ Accident_{t+1}$
$Tax\_Increase_{s,t}$	0.041	-0.001
	(0.672)	(0.960)
$Tax\_Decrease_{s,t}$	0.446	0.050
	(0.199)	(0.101)
Control Variables	YES	YES
Industry-Year FE	YES	YES
Observations	14,802	14,802
Adjusted R2	0.438	0.166