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Matthew S. Johnson, Daniel Schwab, and Patrick Koval

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Department of Economics and Accounting

College of the Holy Cross

Box 45A

Worcester, Massachusetts 01610

(508) 793-3362 (phone)

(508) 793-3708 (fax)

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Legal Protection Against Retaliatory Firing Improves Workplace Safety*

Matthew S. Johnson
Duke University

Daniel Schwab
College of the Holy Cross

Patrick Koval
Opioid Prevention and Education Network
Michigan State University

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Abstract

Workplace safety policies are designed to ensure that employers internalize the costs of injuries, but employers can undermine these policies with threats of dismissal. We show that states' adoption of the public policy exception to at-will employment—an exception forbidding employers from firing workers for filing workers' compensation claims or for whistleblowing—led to a substantial reduction in injuries. The widespread adoption of the public policy exception explains 14 percent of the decline in fatal injury rates between 1979 and 1994. Statutory protections from retaliatory firing also improved safety, but only when employers faced sufficiently strong penalties for violating them.

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*Emails: matthew.johnson@duke.edu; dschwab@holycross.edu; patrickkoval@gmail.com. We thank Terri Gerstein, Rebecca Givan, Kevin Lang, Michael Lipsitz, Seth Sanders, Robert Schwab, Emily Spieler, and David Weil for helpful comments, as well as seminar attendees at College of the Holy Cross. Alison Pei and Caroline Randall provided excellent research assistance.

1 Introduction

Workplace injuries and illnesses impose a substantial economic burden; in the United States alone, their direct costs (e.g., medical care) and indirect costs (e.g., lost productivity) total roughly $250 billion each year (Leigh, 2011). Laws exist to ensure that employers internalize some of these costs. For example, the price of workers' compensation insurance, which firms are required to hold, is a function of their workers' claim history (Moore and Viscusi, 1989; Ruser, 1991). Firms are also required to comply with government safety regulations, and—to the extent that such regulations effectively address workplace hazards—regulatory enforcement ensures that firms face financial penalties for maintaining hazardous workplaces.

Historically, though, the doctrine of at-will employment has effectively allowed employers to treat employment relationships as outside the bounds of these laws. Under this doctrine, which has been the default law governing employment contracts since the late nineteenth century, an employer can dismiss a worker for any reason (that is, without having to establish "just cause"). Thus, in the absence of legal exceptions (which we discuss below), employers are permitted to discharge at-will employees in retaliation for filing workers' compensation claims or reporting violations of safety regulations to federal agencies—even though workers have a legal right to take these actions. Such retaliation is not uncommon: the Occupational Safety and Health Administration (OSHA) receives thousands of complaints about whistleblower retaliation each year (Weatherford, 2013). In the global COVID-19 pandemic of 2020, there have been numerous reports of employers firing workers in retaliation for speaking up about safety concerns.[1] If the threat of dismissal reduces workers' willingness to file for workers' compensation after injury, or to report unsafe or illegal activity, then employers may not internalize as many of the costs of workplace injuries as laws and policies intend.

In the 1970s, however, the relationship between at-will employment and workplace safety began to change in some states. The most salient reason for this change was an expansion of

[1] Examples include reports of Amazon firing warehouse workers (Day, 2020) and hospitals firing nurses (Carville et al., 2020) and doctors (Allen, 2020).

the common-law *public policy exception* to at-will employment, which forbids employers from discharging employees who are either following or refusing to violate public policy.[2] First adopted in 1959 in California to protect dismissals for employees refusing to commit perjury, the public policy exception expanded to also protect dismissals for filing a workers' compensation claim in 1973 and whistleblowing in 1981. In addition, during this period many states passed statutory protections (that is, laws passed by state legislatures) against dismissals for workers' compensation filing or whistleblowing.

We investigate how these legal protections against retaliatory discharge affected the likelihood that a worker would be injured on the job in the first place. It is plausible that such protections could improve workplace safety. If injured workers are more likely to file for workers' compensation, or if all workers are more likely to complain about unsafe conditions, when they know they cannot be fired for doing so, then these legal protections raise employers' incentives to mitigate workplace injury hazards, leading to fewer future injuries.[3] At the same time, such protection could theoretically have the opposite effect and *increase* injuries: by lowering *workers'* effective cost of filing for workers' compensation, the exception could reduce workers' cost of getting injured, incentivizing them to take less care on the job (Krueger, 1990).

To examine this relationship empirically, we leverage staggered adoption of the common-law public policy exception to at-will employment across states between the 1970s and 1990s. Additionally, we estimate the effects of *statutory* dismissal protections using newly quantified data on states' adoptions of statutory protections against dismissals for workers' compensation filing and whistleblowing. We examine the effects of these protections on rates of work-related injuries and illnesses with a difference-in-difference design, using multiple historical sources to measure injuries.

We find that common-law protections against retaliation for workers' compensation and whistle-

[2]The public policy exception is one of three exceptions to at-will employment adopted in the United States, the other two being the implied contract and good faith exceptions, which we briefly describe in Section 2.2.1.

[3]More broadly, exceptions to at-will employment could enhance workplace safety for other reasons. For example, safety is the result of relationship-specific investments, which are likely to be underprovided in at-will employment when turnover is inefficiently high (MacLeod and Nakavachara, 2007).

blowing led to a substantial improvement in workplace safety. States' adoption of the public policy exception led to an 11–13 percent reduction in workplace injury and illness rates. This estimate is statistically significant and remains stable with the use of alternative specifications, additional control variables, and different data sources to measure injuries. Event study estimates reveal that this effect shows up immediately—the year following adoption—and persists for several years.

To put this magnitude in context, fatal workplace injury rates in the United States declined 41 percent between 1980 and 1994. Considering the change in the share of the workforce that was covered by the public policy exception over this same period, our estimates imply that the widespread adoption of the public policy exception explains roughly 14 percent of this overall decline.

In contrast to the common law public policy exception, we find less consistent evidence that statutory protections improved safety. We find suggestive evidence that statutory protection for whistleblowing improved safety: the point estimates indicate a negative effect on injuries, but the magnitude and statistical significance are somewhat sensitive to specification. We find no evidence that the workers' compensation statutory protection had any safety effect: our point estimates are all essentially zero and never statistically significant. Given the institutional details of these statutory protections, these more muted average effects of statutory protections are not necessarily surprising: the typical statutory protection yielded lower expected costs to employers than did the public policy exception to at-will employment, and their scale and stringency varied widely across states. Heterogeneity analysis supports this interpretation: while statutory protections had a small effect on average, they led to a larger reduction in injuries in states in which the protections levied larger penalties on employers that violated them.

These results suggest that the public policy exception to at-will employment raised employers' effective costs of workplace injuries and illnesses, incentivizing employers to take costly actions to reduce them. However, other mechanisms are also possible. We conduct two analyses to assess whether ours is the likely mechanism at play. First, we find that the public policy exception led to improved compliance with safety and health regulations–a salient example of employers' inputs into abating workplace hazards. Second, we assess whether the effects of the exception are heterogeneous based on factors that we expect would affect the degree to which the public policy exception raised employers' costs of workplace injuries.

To this end, we assess whether the safety effect of the public policy exception depends on the presence of labor unions. As we discuss in Section 6.3, much evidence indicates that unions lower the cost to individual workers of filing for workers' compensation, such as by collecting information about relevant laws and policies (Weil, 1996) as well as providing direct support for the filing of claims (Hirsch et al., 1997). Thus, to the extent that the public policy exception raised the threat that a worker would file for workers' compensation following an injury, this threat would have been more credible among unionized workers.[4]

Consistent with this logic, we find that the public policy exception led to a substantially larger reduction in injuries among unionized employers than among non-unionized employers. Bolstering this evidence of the role of unions, we find that the public policy exception had a substantially smaller effect on injuries in states that had adopted right-to-work (RTW) laws—which have been shown to substantially weaken the power of unions—as of the start of our sample period.

Our results extend a robust literature on the effects of exceptions to at-will employment (or wrongful discharge laws) on labor markets. Prior studies have examined the effects of these exceptions on employment and wages (Autor et al., 2006), productivity (Autor et al., 2007), innovation (Acharya et al., 2013), and investments in relationship-specific assets (MacLeod and Nakavachara, 2007), among other outcomes. We contribute to this literature by investigating a previously unexplored effect of these exceptions and revealing two sources of heterogeneity in their effects.

Our results also add to a wide literature on the economic determinants of workplace safety and health (surveyed in Ruser and Butler, 2010). Our paper is particularly relevant to the literature on the effects of workers' compensation, surveyed in Kniesner and Leeth (2014). Prior work has examined the effects of insurance premiums (Moore and Viscusi, 1989), experience rating (Ruser, 1991; Thomason and Pozzebon, 2002), and benefit levels (Fishback, 1987; Krueger, 1990). We

[4]It is also possible that the public policy exception would be redundant in unionized firms since unions already include "just clause" provisions in contract negotiations. We discuss this alternative possibility in Section 6.3.

show that common-law protections that make it less risky for workers to blow the whistle or file for workers' compensation improved workplace safety—with premiums, benefit levels, and other factors held constant.

2 Background

Even though employers have some private incentives to limit workplace injuries, the institution of at-will employment, policy distortions, and labor market frictions likely attenuate such incentives. As a result, legal restrictions on retaliatory firing—which have been adopted in various forms across a subset of states—could motivate employers to invest more in worker safety, thus reducing the occurrence of work-related injuries.[5]

2.1 Workers' Compensation, Safety Regulations, and Employers' Costs of Injuries

Existing public policies and labor market competition, in theory, ensure that employers face incentives to mitigate workplace injuries. However, much evidence suggests that these disciplinary forces are more muted than might be expected.

One such policy is the workers' compensation system. Passed in the United States in the early 1900s, this system insures workers against income risks in the event of a job-related injury. The premiums that most employers pay into the workers' compensation system are "experience-rated," meaning that they depend on the employer's history of prior claims. Furthermore, employers pay deductibles under most plans, ensuring that they pay at least a portion of total injury costs. Because these features raise employers' costs when they experience more injuries, they provide incentive for employers to improve worker safety.[6]

Other public policies enable workers to file safety and health complaints with the federal

[5]For more details surrounding the discussion we present in this section, see Appendix A.

[6]Prior studies have demonstrated, as theory would predict, that experience rating (Ruser, 1991; Bruce and Atkins, 1993; Thomason and Pozzebon, 2002), higher premiums (Moore and Viscusi, 1989), and higher deductibles (Shields et al., 1999) all lead to fewer workplace injuries. See Kniesner and Leeth (2014) for an overview of the literature examining the incentive effects of different aspects of the workers' compensation system.

government and to serve as whistleblowers. Section 11(c) of the Occupational Safety and Health Act of 1970 gives workers the right to file complaints with the Occupational Safety and Health Administration (OSHA) when they feel exposed to a serious hazard or that their employer is violating safety and health regulations. OSHA, the federal agency charged with ensuring "safe and healthful working conditions" (OSHA, 2020), also enforces various whistleblower laws that, in principle, protect workers from discharge for blowing the whistle in domains ranging from asbestos removal to consumer protection. In theory, workers' ability to complain or blow the whistle motivates companies to proactively improve safety (Weil and Pyles, 2005).

However, evidence suggests that employers do not fully internalize the costs of injuries the way that these policies intend. First, up to half of eligible injuries do not get filed for workers' compensation (Shannon and Lowe, 2002; Biddle and Roberts, 2003; Fan et al., 2006; Groenewold and Baron, 2013). One reason that would certainly deter injured workers from filing for workers' compensation is if they perceive a threat of retaliation from their employer for doing so (Spieler and Burton Jr., 2012). One recent study found that 20 percent of workers reported fearing that they could lose their job if they filed a workers' compensation claim (Edisis, 2017). Another found that 50 percent of low-wage workers in New York City, Los Angeles, and Chicago reported being instructed not to file a workers' compensation claim or were fired for doing so (Bernhardt et al., 2009).

Similar barriers and fear of retaliation limit workers' ability to take advantage of their rights to complain or to blow the whistle. Punishment for violating Section 11(c) is essentially nonexistent: employers face no fines if they violate it. Furthermore, a high burden of proof and other barriers make it difficult for workers to file a whistleblower complaint to OSHA (Weatherford, 2013). A 1990 report found that fewer than 10 percent of OSHA's own inspectors said that workers could definitely exercise their rights to complain without fear of employer retaliation (Government Accountability Office, 1990).

How would these policy distortions affect the provision of workplace safety? Injuries are generally more costly for employers when the injured worker files for workers' compensation, since most claims raise the employer's future premium and entails paying a deductible. [7] Similarly, maintaining a hazardous workplace is more costly for employers if their workers complain or blow the whistle to OSHA, since these actions can lead to regulatory fines and bad publicity (Johnson, 2020). If contracts are incomplete, an employer has the incentive and ability to fire an at-will worker in retaliation for filing a workers' compensation claim or for blowing the whistle. Workers, fearing a threat of dismissal, will be less likely to undertake these actions. Injuries are thus less costly for employers in expectation, reducing employers' incentives to make investments to reduce them. As a result, laws that limit employers' ability to retaliate against workers for filing workers' compensation claims or blowing the whistle could raise employers' investments in worker safety.[8]

However, even if such policy distortions are present, in theory features of the labor market already incentivize employers to limit injuries. In a competitive labor market, workers demand higher wages to work in riskier jobs (Rosen, 1986); indeed, much empirical evidence in the compensating differential literature confirms that workers earn higher wages for undertaking riskier jobs (Viscusi and Aldy, 2003; Kniesner et al., 2012; Lee and Taylor, 2019; Lavetti, 2020). However, growing evidence that most labor markets are characterized by imperfect competition suggests that this market discipline might be more muted than implied in canonical models. Monopsonistic competition is pervasive (Manning, 2011; Dube et al., 2020), arising from explicit sources like employer concentration (Azar et al., 2020) but also broader factors like idiosyncratic worker preferences (Lamadon et al., 2022). Imperfect competition affects both the *level* of (wage and non-wage) compensation (Prager and Schmitt, 2021; Dube et al., 2018), but it also attenuates the *price* of injury risk with respect to wages (Lavetti and Schmutte, 2018). Workers also have imperfect information about injury risk and other job attributes (Viscusi and Moore, 1991; Conlon et al., 2018). Thus,

[7]The exception to this statement is that particularly small firms pay a flat workers' compensation that is not experience rated. However, such firms employ a small share of the overall workforce (Ruser, 1985).

[8]Acharya et al. (2013) show theoretically that incomplete contracts create a similar hold-up problem for employee innovation effort: employers cannot commit to not armtwist employees who contributed considerable effort to valuable innovation for a larger share of the ex-post surplus. As a result, innovation effort is inefficiently low in at-will employment relationships, and laws that limit employers' ability to engage in such retaliation raise employees' innovative effort.

while the labor market undoubtedly ensures that employers have some private incentive to minimize injury risk, imperfect competition attenuates this incentive.

Finally, even if one abstracts from policy distortions and imperfect competition, workplace safety still might be inefficiently low under at-will employment. Investments in workplace safety include capital expenditures like updating machinery, but they also include relationship-specific assets like worker training and developing familiarity with processes, equipment, and "culture" (Williamson et al., 1975). If contracts are incomplete and cannot be conditioned on the level of relationship-specific investment, then parties will under-invest in these relationships under at-will employment (MacLeod and Nakavachara, 2007).

Policy distortions likely mute the extent to which workers' compensation and whistleblower opportunities incentivize employers to improve safety in at-will employment relationships. Furthermore, imperfect competition and incomplete contracts attenuate the extent to which the labor market disciplines employers' provision of safety. Legal protections against employer retaliation, by raising employers' expected costs of injuries and by incentivizing investment in relationship-specific assets, could thus improve workplace safety.

2.2 Legal Restrictions on Retaliatory Firing

Over the last few decades, various states have adopted two types of limits on employers' ability to retaliate against workers for filing for workers' compensation or blowing the whistle.

2.2.1 The Public Policy Exception to At-Will Employment

Common law is adopted through precedent that arises from the decisions of particular court cases. Since the late nineteenth century, US courts have generally interpreted the employer-employee relationship to be one of equal power for both parties; the resulting "at-will" doctrine concluded that any employment contract could be terminated at any time by either party. However, beginning with the Industrial Revolution, judges began to recognize "wrongful discharge laws," or exceptions to this interpretation that reflected recognition of power disparities between employers and employees.

The at-will exception most relevant to the current paper is the *public policy exception.* First adopted in California in 1959, the public policy exception prohibits the dismissal of an employee who is either following or refusing to violate well-established public policy. The exception initially protected a relatively narrow set of actions, including serving on a jury or refusing to commit perjury. Legal cases in 1973 and 1981 expanded the exception to protect workers against retaliation for filing workers' compensation claims and for whistleblowing, respectively. In 1970, California was the sole adopter of the public policy exception. By 1980, 15 states had adopted the exception, and this number grew to 42 by 1990. Figure 1 shows the number of states that had adopted the exception each year since 1970.

Once adopted, the public policy exception represented a salient change in the legal environment for employers. The exception is a tort-based action, which means that employees can sue for not just compensatory damages (e.g. back pay, attorney's fees), but also punitive damages. Because punitive damages are meant to "punish" the employer, the level of such damages can be considerable (Edelman et al., 1992).

Along with the public policy exception are two other recognized common-law exceptions to at-will employment, which are not the focus of our paper. The *implied contract exception* prevents the dismissal of an employee if the dismissal is in violation of a written or verbal statement that implies a contract has been established. The *good faith exception* establishes a covenant of good faith and fair dealing in all employer-employee relationships, effectively requiring that all dismissals be made with just cause, although in practice is mainly applied in cases related to the timing of a dismissal.

Prior studies indicate that passage of the public policy exception was not driven by underlying political or economic trends (Autor et al., 2007; DeNicco, 2015). Instead, common-law exceptions are a function of cases specific to a particular employment relationship or occupation, and the willingness of sitting judges to hear them. Bird and Smythe (2008) find that neither economic nor political factors had any meaningful or significant predictive power for if and when a state adopted the public policy exception; the authors conclude that "it seems likely, therefore, that judges usually base their decisions on legal authorities rather than policy considerations or economic conditions"

(Bird and Smythe, 2008).

2.2.2 Statutory Workplace Safety Protections

Whereas common law is adopted through court precedent, states' statutory law is encoded into legislation passed by state legislatures. Many states have adopted statutory protections for filing for workers' compensation and for whistleblowing. While these protections protect a similar scope of worker actions as the common-law public policy exception, legal scholars have argued that most statutory protections are less likely to be an effective deterrent for employers (Sinzdak, 2008).

Thirty-five states have enacted whistleblower statutes that forbid employers from terminating employees for reporting unlawful activity within the firm, such as non-compliance with safety and health regulations. However, most statutes impose burdens that make it difficult for individuals to initiate a claim (see Appendix A for details). Furthermore, the damages that claimants can pursue are heterogeneous across states: in most states claimants are limited to pursuing compensatory damages, and only a few allow for punitive damages. These limitations are in contrast to the pubic policy exception, which offers a clear, uniform course of action for employees to file suits for both compensatory and punitive damages (Edelman et al., 1992).

Additionally, 35 states have enacted statutes to prohibit dismissal in response to an employee's filing for workers' compensation. Punishments prescribed by these statutes vary widely, but they tend to be even lower than those under the whistleblower statutes (see Appendix A for details.)

We describe each state's whistleblower and workers' compensation statutes in Tables B.1 and B.2. Figure 1 shows how many states had adopted these statutes each year.

Given the relatively low and variable levels of potential damages available to claimants under statutory protections for whistleblowing and filing workers' compensation, we expect them to have a) less of an average effect on injuries than the common-law public policy exception, which poses a stronger and more consistent means of preventing retaliatory discharge, and b) a larger effect in states that enable claimants to pursue punitive damages.

3 Data

To undertake our analysis we need data on (1) the years in which states adopted protections against retaliation and (2) measures of workplace injuries. We present summary statistics for these measures in Table 1 and describe them in this section.

3.1 Adoption of the Public Policy Exception and Statutory Protections

To measure adoption of the public policy exception, we use data from Autor et al. (2006) on precedent-setting legal decisions that signify the adoption years of each of three exceptions to at-will employment. We create a variable equal to 0 if a state has not adopted the exception, to 1 in the years the state adopted the exception, and to a fraction between 0 and 1 in the year of adoption depending on the month it occurred. We create analogous variables for the good faith and implied contract exceptions to at-will employment, which serve as controls.

We use a combination of sources to measure the adoption of statutory protections for workers' compensation filing and whistleblowing. We obtained workers' compensation statutory codes from Littler's Workers' Compensation Retaliation Survey (Altman et al., 2012), which we cross-referenced with state law codes available through LexisNexis as well as additional law review journals to confirm passage years. We conducted similar cross-referencing searches for whistleblower statutes using statute codes and passage years provided by Callahan and Dworkin (2000) and statutes collected by The Employment Law Group P.C. and the National Conference of State Legislatures. Using these data, we construct a panel from 1970 to 2005 of state-year observations for adoption of both the workers' compensation and whistleblower statutory protections.[9]

3.2 Measuring Workplace Injuries and Illnesses

We use two distinct sources to measure work-related injuries and illnesses over the period in which legal protections against retaliatory dismissal were enacted.

Our first measure is the occurrence of OSHA inspections triggered by a serious workplace injury.

[9]Unlike the public policy exception, we do not observe the month of adoption for statutory protections. Thus, we code our statutory protection variables as indicators equal to one starting the year of adoption.

Many employers are required to comply with hundreds of OSHA regulatory standards, which range from maintenance of specific capital equipment to more general restrictions that workers not be exposed to certain hazards. OSHA has direct jurisdiction in 29 states; the remaining 21 states have received approval to operate their own state-run safety and health programs.[10] Inspections are OSHA's primary tool for monitoring compliance with these standards. OSHA inspections can be initiated for multiple reasons. Most relevant to our paper, in the event that a workplace experiences a worker fatality or hospitalization of three or more workers, the employer is required by law to report it to OSHA, and OSHA is required to inspect the workplace. Thus, the occurrence of such an inspection indicates that a serious injury took place.

The majority of OSHA's inspections occur for reasons other than a serious injury. "Programmed" inspections, which focus on particular industries or hazards, are initiated for reasons exogenous to events at a particular workplace.[11] These inspections are pursuant to National Emphasis Programs (NEPs), which focus on nationwide priorities, or Local Emphasis Programs (LEPs), which focus on regional priorities. Because programmed inspections target workplaces only in a particular industry or that are likely to have a specific hazard (e.g. based on their location or production technology), no workplace-specific factors (such as recent injuries) influence the occurrence of a programmed inspection.[12] Furthermore, conditional on the criteria on which NEPs or LEPs are based (e.g. industry or region), OSHA often allocated inspections among establishments meeting these criteria using random assignment (Lee and Taylor, 2019).

We identify the occurrence of OSHA inspections using OSHA's Integrated Management Information System (IMIS), a database that contains detailed information on every OSHA inspection

[10]Figure D.1 shows which states are under OSHA's jurisdiction.

[11]Inspections can also be triggered by a complaint (from an employee or member of the public) alleging safety and health hazards, or a "referral" (an allegation of hazards made by an inspector, a government agency, or the media).

[12]The one exception to this statement is OSHA's Site-Specific Targeting program, which was begun in 1998 to focus programmed inspections on establishments that had recently experienced high injury rates. However, this program is not highly relevant to our study, as the bulk of our sample period is prior to 1998.

conducted since the late 1970s.[13] Key variables include the date the inspection was opened, the reason for the inspection (injury, complaint, referral, programmed, other), and facility characteristics (name, address, industry, number of employees present, whether the employees are represented by a union, etc.).

While states under OSHA's jurisdiction began reporting to the IMIS database in 1979, the 21 states outside of OSHA's jurisdiction did not begin reporting to IMIS until the late 1980s, with complete records starting in 1992. Thus, we restrict our analysis of the data to inspections that occurred beginning in 1979 in states under federal OSHA jurisdiction and inspections that occurred beginning in 1992 in states outside federal OSHA jurisdiction. We collapse the inspection data to the state-year level to obtain the number of inspections in each category occurring each year in every state (in robustness checks, we collapse the data at the more refined state-sector-year level).

As a secondary measure of workplace injuries and illnesses, we digitized historical records of annual work-related fatalities from the National Safety Council (NSC) in its annual publication *Accident Facts* for the years 1970 through 2000. These reports included tables on the principal classes of accidental deaths by state. Most relevant to this paper, these classes include fatal workplace injuries, which the NSC collected from state industrial commissions. States reported these data to NSC on a voluntary basis; as a result, some states did not report in certain years.

Each of our two measures of workplace injuries has both advantages and disadvantages relative to the other. One advantage of the OSHA data is that this information reflects both fatal and serious nonfatal injuries, thereby covering a larger share of work-related injuries than the NSC data. Additionally, at least for the states under federal OSHA jurisdiction, the OSHA data provide a balanced panel of injury rates over several decades. On the other hand, the NSC data are based on actual injury reports (rather than on government inspections that are the result of an injury), the reporting begins in 1970 (nine years before any OSHA data), and fatal injuries are reported with high accuracy (Morantz, 2013). A downside is that because states voluntarily reported their data to

[13]We downloaded the data from OSHA's website in July 2014, available at http://ogesdw.dol.gov/views/data_summary.php.

the NSC, data availability is intermittent.

With these comparisons in mind, a benefit of having both the OSHA and NSC measures is that we can assess the effects of legal protections on safety using two independent measures of injuries that cover a different distribution of states over time. Figure D.2 plots the annual coverage of the NSC and OSHA data from 1970 through 2005.

3.3 Constructing Injury and Illness Rates

To measure work-related injury and illness *rates*, we obtain total employment in each state-year from Current Employment Statistics, published by the Bureau of Labor Statistics. Finally, in some analyses, we measure injury rates separately for unionized and non-unionized workers. To do so, we obtain data on the share of private-sector workers that are unionized by state-year from the website unionstats.com, with data based on Hirsch and Macpherson (2003).[14] We multiply the total employment by the percentage unionized to obtain the number of unionized and non-unionized workers in each state-year.

4 Empirical Strategy

Our goal is to test how legal restrictions against an employer's ability to retaliate against a worker for filing a workers' compensation claim or for reporting illegal conditions to the government affect workplace safety. We focus on the common-law public policy exception as our primary measure of such legal protection in developing our empirical strategy, but we use the same strategy to examine the effects of statutory protections against retaliation.

To identify our intended effect, we must address several sources of endogeneity. First, states that

[14] unionstats.com reports data on overall state-level unionization rates starting in 1964, but the earliest year it reports data on unionization rates separately for the private and public sector is 1983. Because our OSHA data begin in 1979, we impute states' private-sector unionization rates for the years 1979–1982. For each state, we compute the percent difference between the private sector and overall unionization rate in each year 1983–2016. We then regress this percent difference on a state-specific time trend. We use the fitted line to predict the 1979–1982 ratio of private to overall unionization. Finally, we multiply this predicted ratio by the overall unionization rate to get our estimate of each state's private sector unionization rate for the years 1979–1982.

adopt the public policy exception (or statutory protections) might have time-invariant characteristics that lead them to experience more or fewer injuries than non-adopting states for unrelated reasons (e.g., states that adopt the exception might tend to have an overall legal environment more favorable to workers). Second, trends in adoption of the public policy exception and in workplace safety changed over time. We include state and year fixed effects to account for these concerns.

There may still be remaining differences between adopting and non-adopting states. First, prior studies have shown that adoption of common-law exceptions has strong regional variation; for example, Southern states lagged behind other regions in adopting the good faith exception (Autor et al., 2006), and injury rates might vary across regions for other reasons, such as differences in industry mix, worker characteristics, or temperature. We follow prior studies in this literature and include a fixed effect for each year interacted with the nine US Census divisions. Finally, injury rates in states that adopted the public policy exception could be on a different trajectory than those in states that never adopted it.[15] We address this final concern by including state-specific linear time trends in some specifications.

In light of the preceding considerations, we follow several prior studies in the literature (e.g., Autor et al., 2006, 2007; MacLeod and Nakavachara, 2007; Acharya et al., 2013) to leverage the staggered adoption of the public policy exception across states to estimate its effect on safety with a difference-in-difference design. Our main specification is as follows:

$$\ln y_{st} = \beta \cdot PP_{st} + \delta_s + \varphi_s I_s \cdot t + \tau_{dt} + X_{st}\gamma + \epsilon_{st}, \tag{1}$$

where y_{st} is the dependent variable observed for state s in year t. Our primary dependent variable is the log injury rate per 1,000 workers. In our main specifications, we measure the injury rate per 1,000 workers as the number of OSHA inspections triggered by a serious workplace injury divided by total state employment (in thousands). In an alternative specification, we use the number of workplace deaths recorded by the NSC in place of OSHA injury inspections.

[15]This could be the case, for example, if states with especially fast declines in injury rates are less likely to have employment disputes brought forth that lead to a court decision on the public policy exception.

The explanatory variable of interest, PP_{st}, is the indicator equal to 1 if a state has adopted the public policy exception to at-will employment. The term δ_s is a state fixed effect, τ_{dt} represents a division-year fixed effect, and $I_s \cdot t$ represents state-specific linear time trends (with I_s representing state dummies). In some specifications, we include the following additional control variables in X_{st} : indicators that a state has adopted the good faith and implied contract exceptions to at-will employment, the state unemployment rate (because workplace accidents might be influenced by macroeconomic conditions (Boone et al., 2011)), the political party of the governor (as a proxy for the political climate, which could affect workplace safety), and the log of the number of OSHA programmed inspections per thousand workers in the state-year (to account for potential variation in OSHA's overall inspection intensity). In all regressions, we cluster standard errors by state to allow for heteroskedasticity and arbitrary correlation of the error term within a state.

As described above, the years that we include in our sample depend on the dataset used. When the dependent variable is measured using OSHA inspections, we include data beginning in 1979 for states without state-run OSHA offices and beginning in 1992 for states with state-run OSHA offices. When we use NSC data to measure the dependent variable, we begin the sample period in 1970. We end our sample period for all states in 2005; we choose this end date because the latest adoption of the public policy exception occurred in 1990, and the latest statutory protection was adopted in 2003 (we consider alternative end years in robustness checks). We weight observations by each state's share of the national labor force in 1979 when measuring injuries with OSHA data, and by the share in 1970 when measuring injuries with NSC data.

Our identifying assumption is $E(\epsilon_{it} PP_{st} | \delta_s, I_s \cdot t, \tau_{dt}, X_{st}\gamma) = 0$; that is, conditional on state fixed effects, region-year fixed effects, state-specific trends, and additional controls, adoption of the public policy exception is exogenous to injuries. This assumption would be violated if, for example, adoption of legal restrictions on at-will employment is correlated with states' underlying (unobserved) economic or political trends. However, given the institutional background described earlier, this is unlikely to be a concern in practice. Because judges cannot make a precedent-setting decision unless an appropriate case is available, the adoption of a particular exception is difficult to predict and can be due to largely idiosyncratic factors, which limits the possibility of behavior that could introduce endogeneity. We note that endogeneity is potentially a more salient concern when considering the adoption of statutory protections, but evidence that we present later suggests this concern is unimportant in practice.

Finally, to ensure that our estimates are not dependent on any arbitrary choices in our specification, we report several variations on Equation 1, such as regressions that omit division-year fixed effects and/or omit state-specific linear trends.

5 The Safety Effects of Legal Protections Against Retaliatory Firing

We present our estimates of the effect of the public policy exception to at-will employment and the two statutory protections on workplace injury rates in Section 5.1. We assess the sensitivity of these estimates to alternative modeling choices and data sources, and to potential bias arising from heterogeneous treatment effects, in Section 5.2.

5.1 Baseline Estimates

Table 2 presents our main estimates of the effects of various legal protections against retaliatory firing on workplace injury and illness rates, measuring injuries and illnesses with the OSHA data. Columns 1–3 reports estimates from a regression with state and year fixed effects only, for our three forms of protection. The coefficient reported in Column 1 indicates that adoption of the public policy exception to at-will employment led to 12.8 percent fewer injuries per worker ($\hat{\beta} = -0.137, exp(-0.137) - 1 = -0.128$), and the estimate is highly significant ($p = 0.006$). Adoption of the statutory protection for whistleblowing (Column 2) led to an estimated 8.3 percent fewer injuries ($\hat{\beta} = -0.087, p = 0.023$). Adoption of the statutory protection for filing workers' compensation (Column 3) had no detectable effect on safety ($\hat{\beta} = -0.032, p = 0.47$).

In the remaining columns, we successively consider various concerns with the specification in Columns 1–3. In Column 4, we include each of the three protections in the same regression. In Column 5 we additionally control for states' adoption of the other two common-law exceptions to at-will employment—good faith and implied contract—as well as the three additional controls described above. In Column 6 we replace year with division-year fixed effects, and in Column 7 we also include state-specific linear trends. The coefficient on the public policy exception remains statistically significant at least at the ten percent level, and the magnitude is stable across these specifications.

The coefficient on the whistleblower statutory protection attenuates slightly in magnitude to -0.072 in Columns 4 and 5, loses statistical significance in Column 6 with division-year fixed effects, but then slightly *increases* in magnitude in Column 7 with the inclusion of state-specific trends ($\hat{\beta} = -0.114$, $p = 0.076$). The coefficient on the workers' compensation statute remains tiny and statistically insignificant across all columns.[16]

The difference-in-difference specification in Equation 1 imposes the assumption that the effect of legal protection against retaliatory firing on workplace injuries is constant over time. Furthermore, the estimates will be misleading if states that adopted legal protections were experiencing differential trends in injuries in the years prior to adoption.

To assess these concerns, we consider the dynamic impacts of the adoption of the three legal protections against retaliation using the following event study specification:

$$\ln y_{st} = \sum_{i=-5}^{5} \beta_i \cdot PPE_{st}^{i} + \beta_6 PPE_{st}^{6+} + \beta_{-6} PPE_{st}^{-6+} + \delta_s + \varphi_s I_s \cdot t + \tau_{dt} + X_{dt} + \epsilon_{st}, \qquad (2)$$

where PPE_{st}^{i} is equal to 1 if the state adopted the public policy exception exactly i years ago, PPE_{st}^{6+} equals 1 if the public policy exception was adopted six or more years in the past, and PPE_{st}^{-6+} equals 1 if the public policy exception was adopted six or more years in the future. The rest of the regression is the same as the main regression (Equation 1), with state-year controls, state-specific

[16]While not the focus of our study, the coefficients on the other two wrongful discharge laws are of interest. The coefficient for the good faith exception is large in magnitude and strongly statistically significant in the baseline specification with state and year fixed effects in Column 5, but it attenuates by 50% and loses statistical significance with division-year and state trends in Column 7. The point estimate for the implied contract exception is small in magnitude and not statistically significant.

linear trends, and division-year fixed effects included. We estimate analogous models for adoption of the two statutory protections.

Figure 2a plots the coefficients on the leads and lags of adoption of the public policy exception. We normalize the coefficient on PPE_{st}^{-1} to be zero.The coefficients on the lead terms are all close to zero, indicating that no important pre-trends in injury rates existed in the years prior to adoption of the public policy exception. The lag coefficients indicate a moderate decline in injuries in the year that the exception was adopted, and then a larger decline in the year following passage and persisting for at least five years.

We report analogous event study results for the whistleblower statutory protection in Figure 2b. The coefficients on the lead terms are all close to zero; after the passage of a whistleblower protection statute, the coefficients become negative, albeit most terms individually are not significantly different from zero. Figure D.3 presents the event study for the workers' compensation statute: corroborating the regression results, there is no evidence of a change in injuries following its adoption.

5.2 Robustness Tests on Baseline Estimates

Our estimates are stable to alternative datasets and specification choices, are corroborated by placebo and falsification tests, and are robust to potential bias from heterogeneous treatment effects.

5.2.1 Alternative Data Source to Measure Injuries

The prior section revealed that the estimated effect of the public policy exception on workplace injuries is highly robust to different choices of fixed effects, state trends, and inclusion of control variables when we measure injury rates using the OSHA data. Our estimates are also insensitive to using an alternative measure of workplace injuries. Rather than use the number of OSHA inspections triggered by a serious accident to measure the number of injuries in a state-year, we use the number of fatal workplace injuries from the NSC (described above). As with the OSHA measure, our dependent variable is the natural log of the rate of fatal workplace injuries per 1,000 workers. Our specification is otherwise the same as Equation 1, except that we weight observations by each state's 1970 (rather than 1979) employment since the NSC data begins in 1970.

Table 3 presents estimates from nearly identical regressions to those in Table 2, but the dependent variable is now NSC's fatal injury rate rather than OSHA's accident inspection rate. In Column 1, the simplest specification with state and year fixed effects and no other controls, the coefficient on *Public Policy Exception* is -0.133 ($p < .01$), which is essentially identical to the analogous estimate with the OSHA data in Table 2 (-0.137); this similarity is all the more remarkable given that the time period and set of states are quite different in these two tables. Across the remaining columns, the coefficient on the public policy exception remains stable and similar to estimates obtained with the OSHA data.[17] Across all specifications, the coefficients on the *Whistleblower statute* are negative, but they are smaller in magnitude than our estimates with the OSHA data and never statistically significant. As with the OSHA data, the coefficients on the *Workers' Compensation statute* are uniformly small in magnitude and nowhere near significant.

Section C.1 presents event studies for the safety effect of all three policies with NSC data, analogous to those we describe above using the OSHA data. These event studies are noiser than those using the OSHA data, but they broadly support the regression results reported in Table 3.

5.2.2 Alternative Specification Choices

In Section C.2 of the online appendix we show that our results are robust to sensible changes in the sample including: ending our sample period in earlier years, estimating a "donut regression" (excluding the years immediately before, after, and during a change in public policy exception adoption), and restricting the sample to those states under the federal OSHA purview. Our results also hold up against other specification choices, including separating the sample into manufacturing and non-manufacturing, using alternate transformations of the dependent variable, and estimating Poisson or negative binomial regressions rather than our linear model.

[17] We exclude state unemployment as a control because the data begin only in 1976 (whereas the NSC data begin in 1970).

5.2.3 Falsification Tests

We present several falsification tests, which are discussed fully in Section C.2.1 in the online appendix. First, we test for the possibility that the public policy exception itself impacts the rate of OSHA inspections unrelated to injuries. Second, we examine whether there is a relationship between the public policy exception and non-work-related fatal injuries. Evidence of a relationship in either case would suggest that our main findings may be spurious, but we find no such evidence.

5.2.4 Alternative Inference Using Placebo Adoption

Section C.3 presents a complementary statistical approach to our main analysis where we generate alternative p-values using "randomization inference" (Young, 2019). The implied p-value is quite similar to the p-value we obtain from our main specification using standard inference techniques.

5.2.5 Assessing Potential Bias from Heterogeneous Treatment Effects

Recent studies have advanced our understanding of the performance of difference-in-difference estimators in settings where effects of a policy might be heterogeneous either across time or across states (de Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Athey and Imbens, 2022; Callaway and Sant'Anna, 2021). We probe the sensitivity of our estimated effects of the public policy exception on safety with two of these new methods. First, de Chaisemartin and d'Haultfoeuille (2020) illustrate that the standard two-way difference-in-difference estimator can be biased, and even have the wrong sign, if treatment effects are sufficiently heterogeneous; furthermore, the authors provide an alternative estimator that is robust to heterogeneity. Second, Callaway and Sant'Anna (2021) develop an estimator of average treatment effects in a difference in difference design that is robust to heterogeneous treatment effects. In Appendix C.4 we describe these two approaches in more detail, how we implement them and show that our estimates are robust to heterogeneous treatment effects using either approach.

5.3 Interpreting and Comparing the Magnitudes of our Baseline Estimates

The estimates in Section 5.1 consistently imply that the public policy exception to at-will employment led to an economically meaningful improvement in workplace safety. Consider that, over roughly our sample period, overall fatal workplace injury and illness rates in the United States declined from 7.5 per 100,000 workers in 1980 to 4.4 per 100,000 workers in 1994, a decline of 41 percent (Centers for Disease Control, 1998).[18] Over this same time period, we calculate that the share of US employment in a state that had adopted the public policy exception rose 45 percentage points, from 39 percent to 84 percent. Using the median estimate across the specifications in Table 3 (which is -0.132), our results imply that adoption of the public policy exception accounts for 13.6 percent of the overall decline in fatal injury rates over this period.[19]

In contrast, we find less convincing evidence that the two statutory protections against retaliatory firing improved safety. Our estimates tend to imply that the whistleblower statutory protection led to a reduction in injury rates, but the magnitude is smaller than the public policy exception, and the estimate is more sensitive to specification choices. We find no evidence that the workers' compensation statute had any effect on safety, on average. Given the institutional detail described in Sections 2 and A.2.2, it is not necessarily surprising that the statutory protections would have a weaker average effect on safety than the public policy exception: the penalty for employers who failed to abide by the whistleblower statute was lower on average than the pubic policy exception, average penalties under the workers' compensation statute were even lower, and penalties for both varied widely across states. Consistent with these legal protections raising employers' costs of

[18]Note that while the sample period in our regressions includes 1979–2005, the 1980–1994 span of the data from the Centers for Disease Control (1998) was the best overlap we could find for panel data on fatal workplace injuries in the United States. One reason we cannot include years beyond 1994 is that in 1995, the system for measuring fatal workplace injuries changed from the National Traumatic Occupational Fatalities surveillance system, under the National Institute for Occupational Safety and Health, to the Bureau of Labor Statistics Census of Fatal Occupational Injuries. Because these two systems used different methods, their numbers are not directly comparable.

[19]The calculation is $-0.124 * 0.45/(-0.41) = 13.6$ percent, where $-0.124 = exp(-0.132) - 1$.

maintaining hazardous workplaces, we would expect that the protections with the largest penalties for employers—the public policy exception—would spur the largest improvement in safety.

While this logic implies that the *average* safety effect of the statutory protections would be lower than the public-policy exception, it also implies that the statutes' effect should be *heterogeneous* depending on the damages available to one filing a claim. One of the most salient differences between the public policy exception and the two statutory protections is the availability of punitive damages which—unlike compensatory damages or reinstatement requirements—have no upper bound and can be quite substantial. Whereas the public policy exception uniformly allowed a plaintiff to sue for punitive damages, only a handful of statutory protections included them.

We test for such heterogeneity in Section C.5 of the online appendix. The results imply that statutes that include punitive damages do—as predicted—reduce workplace injuries more than statutes without punitive damages.

6 Why Does Legal Protection Against Retaliation Improve Safety?

The above results reveal that legal protection against retaliatory firing for filing workers' compensation or whistleblowing substantially improved workplace safety. The discussion in Section 2.1 outlined one mechanism that could drive this effect: by raising employers' expected costs of workplace injuries, such legal protections encourage employers to increase inputs into safety. The results we have shown are consistent with this mechanism. Statutory protections only improved safety when they enabled employees to sue for punitive damages ($i.e.$,, when employers' penalty for violating them was sufficiently strong). The public policy exception to at-will employment, which provided a clear, uniform course of action for employees to sue for punitive damages, led to economically meaningful and persistent improvements in workplace safety.

In this section, we further examine whether this mechanism plausibly drives our results. We first consider an alternative explanation: that this relationship does not correspond to workplaces getting safer but rather reflects compositional effects. We then test whether the public policy exception affected a direct measure of employers' inputs into safety: compliance with safety regulations.

Finally, we conduct a suggestive test of whether the exception led to a larger improvement in safety when workers would be more likely to take advantage of it. We focus this analysis on the public policy exception, since the evidence in Section 5.1 revealed that the effect of the two statutory protections was low on average and highly heterogeneous across states.

6.1 Did the Public Policy Exception Change the Workforce Composition?

If the public policy exception is more costly for firms in hazardous sectors, then adoption of the public policy exception might lead to fewer injuries not because workplaces become safer but because workers move to safer sectors. We test for such compositional changes in Table D.1. We mimic Table 2, except that our dependent variable is the share of state-level annual employment in manufacturing. We see no evidence that the public policy exception shifted the composition of employment in or out of manufacturing. The point estimates on the public policy exception are all tiny in magnitude, mostly insignificant, and inconsistently signed across specifications. It appears unlikely that shifts in worker composition across sectors meaningfully influence our results.[20]

6.2 The Effect of the Public Policy Exception on Compliance with Safety Regulations

By reducing workers' risks to filing for workers' compensation or blowing the whistle on their employer's safety hazards, we argue that the public policy exception would have incentivized employers to increase their inputs into safety. One salient example of employers' inputs into safety is their compliance with government safety and health regulations. Thus, we examine whether non-compliance with OSHA regulations decreased following the adoption of the public policy exception. We estimate a modified version of the regression model in Equation 1:

$$Noncompliance_{jsit} = \beta_1 PP_{st} + \delta_s + \varphi_s I_s \cdot t + \tau_{dt} + \phi_{jt} X_{jsit}\gamma + \epsilon_{jsit} \tag{3}$$

Here, our dependent variable is a measure of non-compliance detected during an inspection

[20]A related issue is the public policy exception might lead to losses in employment for particular groups of workers with higher ex ante risk of injury, for example by raising employment costs of production workers. However, prior studies have found little to no effect of the public policy exception on employment levels or flows (Autor et al., 2007).

of workplace j in state s, in industry i, conducted in year t. PP, and the sets of fixed effects are as described above, except that we also include industry-year fixed effects (ϕ_{jt}) to account for time-varying shocks across industries.[21] We control for a vector of workplace-specific controls in X, including the log number of employees reported present during the inspection and if the workers were represented by a labor union, as well as time-varying state-level controls such as adoption of the statutory protections and other exceptions to at-will employment.

We quantify non-compliance as the sum of the "gravity" assigned to all violations detected in an inspection. OSHA assigns a "gravity" score to each violation based on the perceived severity of the injury that could result from the violation and the probability that such an injury would occur; the gravity of a violation ranges from 0 to 10, with 10 being most severe. We preserve the IMIS dataset at the inspection level and calculate the total gravity across all violations assessed to each inspection. Because the total gravity exhibits substantial skew but also contains a fairly large number of zeroes, we use its inverse hyperbolic sine transformation.[22]

Because violations (and associated gravity) are only observed conditional on an inspection occurring, we restrict the estimation of Equation 3 to programmed inspections. As described in Section 3.2, the occurrence of programmed inspections is exogenous to events at a particular workplace, conditional on industry and location; results in Section C.2 also showed their occurrence was orthogonal to adoption of the public policy exception. In our main regressions, we additionally drop inspections in the construction sector. We do this because construction makes up the vast majority of programmed inspections (over two-thirds), and this industry disproportionately uses short-term work contracts that make wrongful discharge laws less applicable. Transactions between construction firms and contractors are typically arms-length, short-term arrangements (Cox and Thompson, 1997), and likely as a result construction workers are more likely to be contingent or alternative work arrangements[23] in which wrongful discharge laws are not applicable.

[21] We operationalize "industry" 1-digit SIC industry codes. Our estimates are not materially changed by using other aggregations of industry codes.

[22] We obtain similar results using different measures of non-compliance, such as the number of violations assessed.

[23] In all years, the BLS Contingent and Alternative Employer Arrangements Survey reveals that the use of contingent

We report our estimates without the construction sector in Table 4. In Column 1 we report results from a specification with state and year fixed effects only. The point estimate on the public policy exception is negative, but small in magnitude and statistically insignificant. The estimate is largely unchanged controlling for the other legal protections in Column 2. When we additionally include division-year fixed effects in Column 3, the point estimate increases in magnitude to -0.108 and becomes marginally significant ($p = .084$). Finally, when we additionally include state-specific trends in Column 4, the estimate increases even more in magnitude and statistical significance. We estimate that adoption of the public policy exception led to a 13.5% ($\hat{\beta} = -0.145, p = 0.03$) reduction in the gravity assessed among non-construction inspections. We report results in Table D.2 from regressions that include the construction sector; the estimated effect of the public policy exception is qualitatively similar but, as expected, attenuates in magnitude and significance.[24]

Compliance with government safety regulations is a salient example of employers' inputs into workplace safety. The evidence in this section thus supports the hypothesis that the public policy exception increased employers' inputs into safety, which might have led to fewer workplace injuries.

6.3 Heterogeneous Effects of the Public Policy Exception: The Role of Labor Unions

We have argued that the public policy exception raised employers' expected costs of workplace injuries due to limitations in existing policies. As described in Section 2.1, barriers and fear of retaliation limit workers' ability to take advantage of the workers' compensation system and to blow the whistle about safety hazards. By providing protection to workers against retaliation for filing for workers' compensation, or for complaining to the government, a forward-looking employer would realize that future injuries will come with higher expected costs, and thus the employer has more incentive to make investments to reduce the likelihood of future injuries.

work is more prevalent in construction than in other industries. See, for example, the 1999 survey, available here: https://www.bls.gov/news.release/history/conemp_12211999.txt.

[24]The estimates in Table D.2 suggest that adoption of the whistleblower statute actually led to *more* gravity of violations, but the estimates are sensitive to specification and flip sign in our main results that drop the construction sector.

However, the extent to which legal protection against retaliation would raise employers' costs of injuries is likely to be unevenly distributed. All else equal, a change in legal protection will have a larger impact on employers' expected costs of injuries when (a) workers face lower additional barriers to filing for workers' compensation or blowing the whistle, and (b) workers are more likely to become aware of the legal change.

Much evidence reveals that both of these conditions are more likely to be true for workers represented by a labor union. Unions lower the costs of acquiring information about how to file for workers' compensation, as well as about policies that affect the benefits and costs of doing so; such information might be too costly for individual workers to acquire themselves (Weil, 1996). Unionized workers are more likely to know how to obtain workers' compensation than non-union workers, and unions help workers through the often complex process of filing a claim (Hirsch et al., 1997), for example, through in-house claims management (Thomason and Pozzebon, 2002). Unionized workers are more responsive to policies that affect the benefits from filing for workers' compensation such as waiting periods and benefit levels (Hirsch et al., 1997). In a similar vein, unions lower the costs for individual workers to file complaints with OSHA, both by facilitating the process and by lowering information costs to do so (Weil and Pyles, 2005).

More generally, labor unions have been shown to enhance the implementation and effects of a variety of public policies and laws centered around working conditions. Mandated health and safety committees led to larger increases in OSHA enforcement for unionized workplaces than for non-unionized workplaces (Weil, 1999), and publicizing facilities' OSHA violations led to larger improvements in compliance in areas with high union density than in areas with low union density (Johnson, 2020). In other contexts, unions augment collective bargaining by using legal enforcement mechanisms such as calling on enforcement agencies or by directly taking up legal cases on behalf of workers (Colling, 2006). Such qualities have led others to argue that "unions are an instrument in translating statute and case law into changed employment practice" (Dickens, 2002). In other words, even though unions might reasonably be considered a substitute for protections codified in employment law, they actually often serve as a *complement* to them.

For all these reasons, it is plausible that legal protections against dismissal for workers' compensation filing or whistleblowing would lead to a larger increase in employers' expected costs of injuries—and thus a larger reduction in injuries—among unionized workplaces relative to non-unionized workplaces.

On the other hand, other arguments imply that legal protection against dismissal would have a *smaller* safety effect in unionized workplaces. Unions often include "just cause" provisions in their collective bargaining agreements, which already put restrictions on employers' ability to discharge workers in ways that overlap with (and subsume) the public policy exception. Given this redundancy, the public policy exception might have less potential to improve safety in unionized workplaces.

However, while just cause provisions would in theory render the public policy exception less useful for unionized workers, the historical context suggests a more nuanced interpretation. During the 1980s (the period when many states adopted the public policy exception), legal scholars considered the concept of "just cause" to be ambiguous, nebulous, and not well-understood (Abrams and Nolan, 1985). By defining one explicit condition that constituted "just cause," the public policy exception could have provided a concrete guidepost for unions to exercise the rights that their contracts in theory provided. Indeed, many unions include provisions in collective bargaining agreements to enforce federal, state, and local statutes; while seemingly redundant, such provisions are useful because it is easier for workers to enforce a contract provision through a grievance procedure than to navigate statutory or common law procedures.[25]

We examine whether union presence moderates the safety effect of the public policy exception in two ways. First, we directly test whether the public policy exception affected injury rates differentially at unionized and non-unionized workplaces. We use the variable in the OSHA IMIS dataset indicating whether a union was present during the inspection to create annual state-level measures of injury counts for unionized and non-unionized workplaces. To construct these separate

[25]For example, the faculty labor union at Rutgers University required in their collective bargaining agreement that the university provide lactation spaces "in accordance with the law:" see `https://www.rutgersaaup.org/wp-content/uploads/securepdfs/2020/05/AAUP-AFT-FT-Agreement-2018-2022.pdf`.

injury *rates*, we divide these counts by state-level annual unionized and non-unionized employment. (We describe in Section 3.3 how we measure unionized and non-unionized employment.)

The second way that we test the moderating role of union presence is by examining whether the safety effect of the public policy exception differs in states that had previously enacted right-to-work (RTW) laws. These laws allow workers to decline to pay union dues even if they are covered by a collective bargaining agreement, leading to free-rider problems. They have been shown to decrease union organizing (Eren and Ozbeklik, 2016), decrease union membership (Ellwood and Fine, 1987), and limit unions' bargaining strength (Ichniowski and Zax, 1991). These laws are also correlated with other "pro-business" policies that disproportionately benefit employers over workers (Holmes, 1998), potentially capturing other characteristics that limit workers' ability to leverage other favorable legal changes. We identify 16 states that had adopted RTW laws by 1979 using data from National Conference of State Legislatures (nd). Table D.3 displays summary statistics of our primary dependent and independent variables separately for RTW and non-RTW states.

We present the results in Table 5. Column 1 reports estimates from our simplest model with state and year fixed effects, similar to the estimate in Column 1 of Table 2, with two important changes. First, the union of observation is now at the *state–year–union-status*, where "union-status" equals either *unionized* or *non-unionized* workplace. Second, we include an interaction of the public policy exception variable with an indicator equal to 1 for unionized workplaces. Third, because unionized and non-unionized workplaces differ in unobservable ways and may exhibit differential trends, we interact each of our set of fixed effects with a dummy for unionized workplaces.[26] The main effect of the public policy exception implies that the exception led to fewer injuries at non-unionized workplaces, but the estimate is statistically insignificant ($p = .13$). The interaction term (-0.101, $p = 0.047$) reveals that the public policy exception led to a substantially larger reduction in injuries among unionized workplaces; combining the two implies a safety effect for unionized workplaces of 20 percent ($-0.127 + -0.101 = -0.228$, $exp(-0.228) - 1 = -0.203$) ($p = 0.049$).

[26]Including these fixed effects without the interaction does not qualitatively change the point estimate, but—unsurprisingly—substantially increases the standard error.

In Columns 2–4, we include region-year fixed effects (Column 2), state-specific trends (Column 3), and additional controls (Column 4). The moderating role of unions persists, and with state-specific trends actually increases: in column 4, the main effect for non-union workplaces is essentially zero ($\hat{\beta} = -0.011$, $p = .87$), and the interaction term increases in magnitude to -0.251, $p = .017$).

In Columns 5–8, we instead interact the public policy exception with our other measure of union presence: an indicator that a state had *not* passed RTW laws before 1979. Since states with RTW laws differ from those without RTW laws, we also include year fixed effects interacted with a no-RTW-law dummy.[27] This model implies a similar moderating effect of union presence as Columns 1–4. Across all columns, the main effect of the public policy exception implies that the safety effect of the public policy exception was, if anything, *positive* in RTW states, though the estimate is always small and nowhere near statistically significant. On the other hand, in all specifications, the interaction term reveals that the public policy exception's effect on injuries is much more negative in non-RTW states. Considering the most saturated model in Column 8, combining the main effect and interaction terms implies a safety effect in non-RTW states of 20 percent ($0.147 + -0.325 = -0.222$), $exp(-0.222) - 1 = -0.20$ ($p < .01$).

While the public policy exception in principle protected all workers from retaliation for filing workers' compensation or whistleblowing, it would have raised employers' costs of injuries only if it actually made workers more inclined to take these actions. Unionized workers would arguably be more responsive to a legal change that lowered the costs of filing workers' compensation or blowing the whistle, making it plausible that the public policy exception raised employers' costs of injuries more in unionized workplaces. The results in this section support this premise. These results do not definitively establish the mechanism through which the public policy exception improved workplace safety, but they are consistent with the hypothesis that the exception improved safety by raising (some) employers' implicit costs of injuries.

[27]This inclusion has essentially no effect on the estimates.

7 Conclusion

Laws and public policies exist—through the workers' compensation system and government safety regulations—to ensure that employers face incentives to limit workplace injuries and illnesses. However, at-will employment relationships were historically effectively treated as outside the bounds of these laws. This changed with the adoption of various legal protections that forbade employers from retaliating against workers for filing a workers' compensation claim or reporting illegal conditions to government agencies. We found that states' adoption of the public policy exception to at-will employment—the most salient such legal protection—led to sustained and economically meaningful reductions in workplace injuries. Statutory protections for these actions, which tended to offer smaller damages for claimants, had a smaller effect on safety. Collectively, these results imply that employers might not have been internalizing the costs of workplace injuries as existing policies had intended.

An important caveat is that we cannot definitively say that the safety improvements spurred by the public policy exception raised welfare. However, given the enormous economic burden of workplace injuries, the social benefits of the reduction in injuries were undoubtedly substantial. Additionally, no existing evidence suggests that the public policy exception meaningfully raised employers' costs: the public policy exception had no effect on employment (MacLeod and Nakavachara, 2007) or on establishment entry or productivity (Autor et al., 2007). It thus seems unlikely that safety improvements spurred by the public policy exception were welfare diminishing. Moreover, our results reveal that a sentiment embodied by prior literature, that "the public policy exception is not generally thought to impose substantial constraints on employer behavior" (Autor et al., 2007, pg. F192), overlooked an important element of employer behavior: investments in workplace safety.

Our results inform understanding of how safety is provided in the labor market. If the level of workplace injury risk is determined in a frictionless, competitive labor market, then employers fully internalize the costs of injuries via the wage premiums they must pay workers to accept risky jobs. In this scenario, protections against dismissal for filing a workers' compensation claim or for whistleblowing have little scope to improve safety; furthermore, any improvements in safety would impose large costs on firms and/or workers. However, imperfect competition in labor markets—which much research reveals is pervasive—attenuates this market discipline. Additionally, theoretical arguments that at-will employment leads firms to under-invest in relationship-specific assets like workplace safety imply that injuries might be inefficiently high absent dismissal protections. These departures from perfect competition illustrate how the legal protections against dismissal that we study could improve safety without meaningfully affecting firms' costs.

While we find that the public policy exception adopted via the courts improved workplace safety, statutory protections with similar intents on average had either a smaller effect (whistleblower protections) or no measurable effect (workers' compensation protections). We argue that the disparity between common-law and statutory protections is not surprising, given the variance in remedies available from statutes, which were always equal to or lesser than those available under court-adopted equivalents. Furthermore, remedies available through the courts, especially with the potential for larger punitive damages, add an additional party—namely lawyers—that would be motivated to provide additional encouragement and information to workers regarding the process of seeking redress, increasing the likelihood of employees exercising such rights. Supporting this rationale, we find that statutes with harsher penalties did in fact deter a significant number of injuries.

At the same time, we found that the public policy exception appeared to improve safety to a much greater extent in unionized workplaces, or among workplaces located in states without policies that diminish unions' power. Given the many sources of support that unions provide workers to file for workers' compensation or complain to OSHA, non-unionized workers may have not had the support, information, or power to leverage the legal protections against unjust dismissal.

The role of union presence might explain why today—even after 43 states have adopted the public policy exception—there remain widespread reports of employers retaliating against workers for actions like filing for workers' compensation (Bernhardt et al., 2009). Given that the share of workers covered by a labor union is much lower today than it was in our sample period comprising the 1980s and 1990s, fewer workers today might be able to take advantage of legal protections offered by the public policy exception than when these laws were initially passed.

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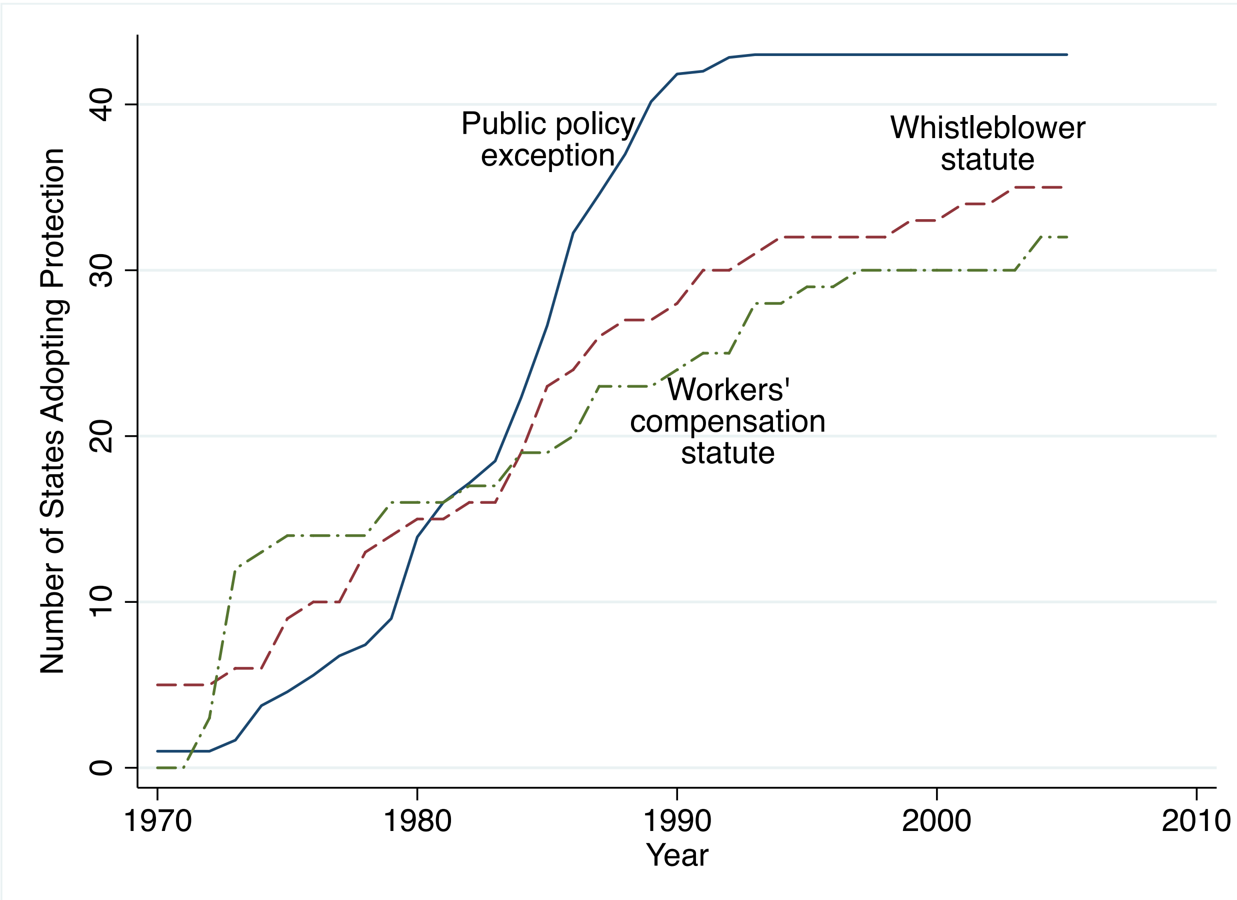
8 Tables and Figures

Figure 1: States' Adoption of Public Policy Exception and Workers' Compensation and Whistleblower Statutes Over Time



Note: This figure shows the number of states adopting the public policy exception, workers' compensation statutes, and whistleblower statutes over time. Please see text for sources.

Table 1: Summary Statistics

	mean	sd
Accident inspections per 1000 employees (OSHA)	0.031	0.059
Deaths per 1000 employees (NSC)	0.060	0.047
Public policy exception	0.548	0.494
Workers' comp. anti-retaliation statute	0.449	0.497
Whistleblower protection statute	0.419	0.494

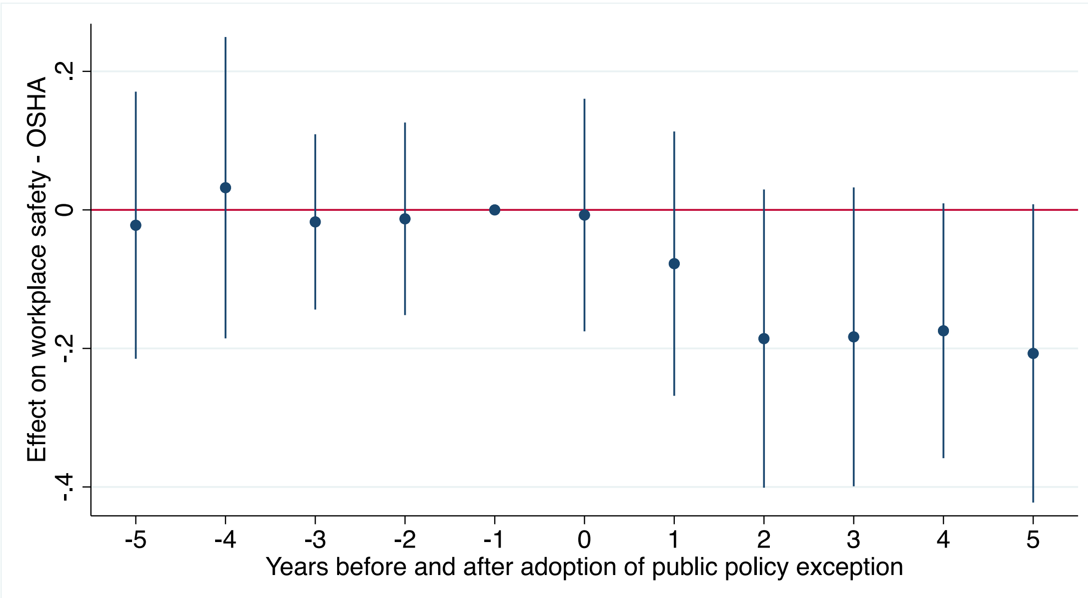
Note: Table shows summary statistics for our key dependent and explanatory variables. Please see text for further details and sources.

Table 2: The Effect of the Public Policy Exception and Statutory Protections on Workplace Injuries (Data from OSHA)

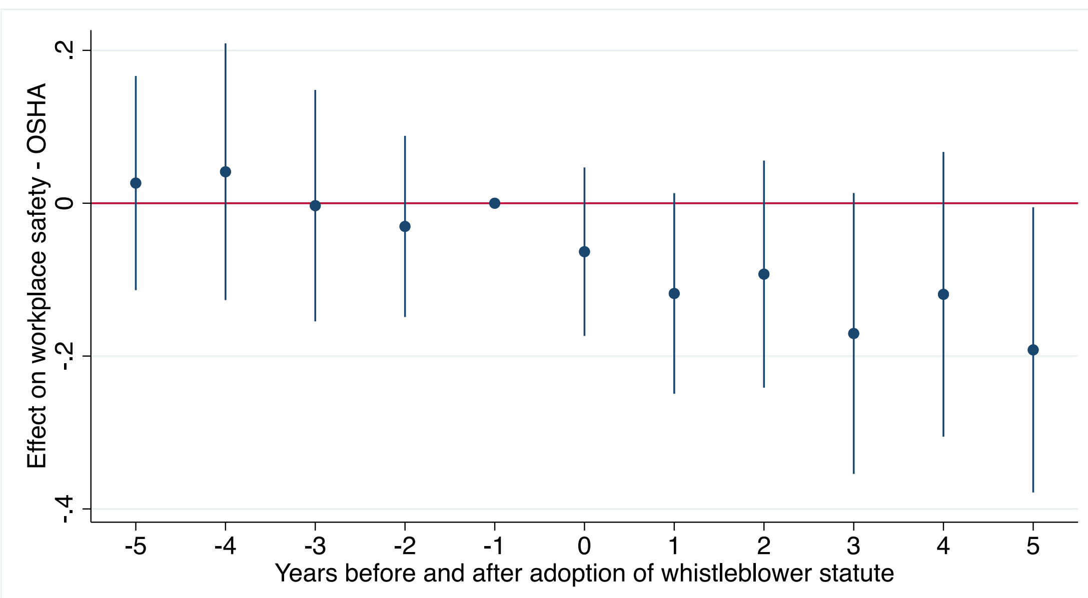
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	-0.137***			-0.123***	-0.120***	-0.095**	-0.112*
	(0.046)			(0.046)	(0.044)	(0.046)	(0.062)
Whistleblower statute		-0.087**		-0.072*	-0.072**	-0.056	-0.114*
		(0.037)		(0.039)	(0.034)	(0.053)	(0.062)
Workers' comp statute			-0.032	-0.003	0.027	0.077	0.014
			(0.044)	(0.034)	(0.025)	(0.056)	(0.086)
Good faith exception					-0.203***	-0.176**	-0.095
					(0.041)	(0.077)	(0.136)
Implied contract exception					-0.024	-0.029	-0.003
					(0.047)	(0.062)	(0.091)
State unemployment rate					-0.034***	-0.038***	-0.023*
					(0.012)	(0.011)	(0.014)
Democratic governor					-0.024	-0.037	-0.024
					(0.039)	(0.028)	(0.026)
Prog. inspection rate					0.020	0.054	-0.013
					(0.038)	(0.045)	(0.037)
Observations	1077	1077	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

Note: The dependent variable is the natural log of the number of OSHA accident inspections divided by the state labor force (in thousands). The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Figure 2: Event Studies: Effects of the Public Policy Exception and Whistleblower Statute on Workplace Injuries (Data from OSHA)



(a) Effect of the Public Policy Exception on Workplace Injuries



(b) Effect of the Whistleblower Statute on Workplace Injuries

Note: The plot shows the effect of the public policy exception and whistleblower statute on workplace injuries (data from OSHA) before and after adoption, based on Equation 2. Coefficients on the left side of the plot indicate leads, and coefficients on the right side indicate lags. Included in the regression but not displayed are coefficient terms for having adopted the relevant policy six or more years in the past or six or more years in the future; see text for details. The dots represent the point estimates, and the vertical bars show 95 percent confidence intervals.

Table 3: The Effect of the Public Policy Exception and Statutory Protections on Fatal Workplace Injuries (Data from NSC)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	-0.133***			-0.143***	-0.132**	-0.108**	-0.109*
	(0.047)			(0.046)	(0.057)	(0.046)	(0.055)
Whistleblower statute		-0.029		-0.057	-0.057	-0.030	-0.061
		(0.062)		(0.061)	(0.053)	(0.044)	(0.049)
Workers' comp statute			-0.040	-0.039	-0.018	0.015	0.117
			(0.071)	(0.060)	(0.056)	(0.060)	(0.079)
Good faith exception					-0.150	0.029	-0.120
					(0.122)	(0.162)	(0.154)
Implied contract exception					-0.024	-0.038	-0.035
					(0.077)	(0.081)	(0.071)
Democratic governor					-0.061	0.003	0.002
					(0.042)	(0.031)	(0.029)
Prog. inspection rate					-0.023	-0.005	-0.011
					(0.024)	(0.016)	(0.013)
Observations	911	911	911	911	911	911	911
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

Note: The dependent variable is the natural log of the number of workplace death rate divided by the state labor force (in thousands). The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table 4: The Effects of the Public Policy Exception on Compliance with OSHA Regulations

	(1)	(2)	(3)	(4)
Pubic policy exception	-0.042	-0.050	-0.108*	-0.145**
	(0.063)	(0.067)	(0.061)	(0.065)
Whistleblower statute		-0.074	-0.046	-0.133
		(0.079)	(0.076)	(0.084)
Workers' comp statute		0.099	-0.013	0.007
		(0.106)	(0.070)	(0.065)
Good faith exception		0.069	-0.006	0.020
		(0.118)	(0.105)	(0.052)
Implied contract exception		0.073	0.080	-0.078
		(0.049)	(0.050)	(0.059)
Log # workers present	0.040***	0.040***	0.039***	0.038***
	(0.010)	(0.010)	(0.009)	(0.009)
Union present	0.032**	0.031**	0.036***	0.037***
	(0.013)	(0.013)	(0.011)	(0.011)
Observations	317250	317250	317250	317250
Mean Dep Var (in levels)	0.808	0.808	0.808	0.808
Sector-state and sector-year FE	Yes	Yes	Yes	Yes
Industry year FE	Yes	Yes	Yes	Yes
Division-year FE	No	No	Yes	Yes
State trend	No	No	No	Yes

Note: The dependent variable is *asinh(gravity)*, the inverse hyperbolic sine of the sum of the gravity assigned to all violations detected in an inspection, where "gravity" is a score ranging from 0–10 based on OSHA's assessment of the severity of the violation. Construction sector is omitted; see text for details. Robust standard errors clustered by state shown in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table 5: Heterogeneous Effects of the Public Policy Exception on Workplace Injuries, Based on Union Presence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public policy exception	-0.127	-0.069*	-0.042	-0.011	0.047	0.025	0.129	0.147
	(0.078)	(0.037)	(0.064)	(0.068)	(0.094)	(0.081)	(0.111)	(0.122)
Unionized workplaces=1 × Public policy exception	-0.101**	-0.081*	-0.251**	-0.251**				
	(0.050)	(0.047)	(0.102)	(0.102)				
No Right-to-Work law=1 × Public policy exception					-0.245**	-0.186*	-0.323**	-0.325**
					(0.111)	(0.102)	(0.148)	(0.139)
Prog. inspection rate				-0.018				-0.013
				(0.041)				(0.041)
Whistleblower statute				-0.092				-0.108
				(0.067)				(0.069)
Workers' comp statute				0.096				0.040
				(0.122)				(0.129)
Observations	2154	2154	2154	2154	1050	1050	1050	1050
Mean Dep Var	-3.483	-3.483	-3.483	-3.483	-3.746	-3.746	-3.746	-3.746
State and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division-year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State trend	No	No	Yes	Yes	No	No	Yes	Yes
Additional Controls	No	No	No	Yes	No	No	No	Yes

Note: In Columns 1–4, the unit of observation is a state-union-status–year (where "union status" is a dummy variable that distinguishes unionized and non-unionized workplaces). We measure the accident rate at unionized workplaces as the natural log of the number of OSHA accident inspections in which a labor union was present, divided by the number of unionized workers in the state-year in thousands. We measure the accident rate at non-unionized workplaces analogously. In Columns 5–8, *No RTW law* is a dummy equal to 1 if a state had not adopted right-to-work (RTW) laws as of 1979. In Column 1–4, each of the fixed effects noted in the table footer is interacted with the dummy for workplace unionization. The specifications in Column 5–8 also controls for year fixed effects interacted with the no-RTW-law dummy. Columns 4 and 8 include the same controls as in Column 7 of Table 2. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses.

A Background

In this Appendix we provide details and expanded information that supplements Section 2.

Even though employers have some private incentives to limit workplace injuries, the institution of at-will employment, policy distortions, and labor market frictions likely attenuate such incentives. As a result, legal restrictions on retaliatory firing—which have been adopted in various forms across a subset of states—might motivate employers to invest more in worker safety, thus reducing the occurrence of work-related injuries.

A.1 Workers' Compensation, Safety Regulations, and Employers' Costs of Injuries

Certain existing public policies are designed to ensure that employers face incentives to mitigate workplace injuries; moreover, labor market competition in theory makes it costly for employers to maintain dangerous workplaces. However, much evidence suggests that the disciplinary forces of these policies and market competition are more muted than might be expected.

One such policy is the workers' compensation system. Passed in the United States in the early 1900s, the workers' compensation system insures workers against income risks in the event of a job-related injury. Employers pay premiums into the system, and workers who sustain an injury are guaranteed a portion of their earnings as compensation if they file a claim. Workers' compensation is one of the largest social insurance programs in the United States: in 2017, employers' costs (primarily consisting of premiums and deductibles) totaled \$97 billion, and benefits paid to workers totaled \$62 billion (Weiss et al., 2019). In comparison, unemployment benefits paid in 2017 totaled less than half of this amount, at \$30 billion.[28] The premiums that employers pay into the workers' compensation system are "experience-rated," meaning that they depend on the employer's history of prior claims. Furthermore, employers must pay deductibles under most plans, ensuring that they pay at least a portion of total injury costs.[29]

[28] Source: Bureau of Labor Statistics Unemployment Insurance Data, https://oui.doleta.gov/unemploy/DataDashboard.asp, accessed April 2020.

[29] Prior studies have demonstrated that experience rating (Ruser, 1991; Bruce and Atkins, 1993;

A second public policy that seeks to ensure that employers internalize the costs of injuries is though laws that enable workers to file safety and health complaints with the federal government and to serve as whistleblowers. Section 11(c) of the Occupational Safety and Health Act of 1970 gives workers the right to file complaints with the Occupational Safety and Health Administration (OSHA) when they feel exposed to a serious hazard or that their employer is violating safety and health regulations. When OSHA—the federal agency charged with setting and enforcing workplace safety standards—receives a worker complaint, OSHA typically inspects the establishment in question and issues financial penalties for each violation of safety regulations that the inspector detects. Many prior studies have found that OSHA inspections effectively reduce injury rates (Haviland et al., 2012; Levine et al., 2012). Furthermore, OSHA enforces various whistleblower laws that, in principle, protect workers from discharge for serving as a whistleblower in domains ranging from asbestos removal to consumer protection.

However, much evidence suggests that employers do not fully internalize the costs of injuries the way that these policies intend. First, up to half of eligible injuries do not get filed for workers' compensation (Shannon and Lowe, 2002; Biddle and Roberts, 2003; Fan et al., 2006; Groenewold and Baron, 2013). There are many reasons that an injured worker might not file for workers' compensation, such as a lack knowledge of the system (Azaroff et al., 2013) or of how to navigate the process to file a claim (Weil, 1996). One reason that would certainly deter injured workers from filing for workers' compensation is if they perceive a threat of retaliation from their employer for doing so (Spieler and Burton Jr., 2012). The legal case in 1973 that initially changed the public policy exception to at-will employment, discussed below, involved an instance of such retaliation. Such retaliation is not a relic of the past: one recent study found that 20 percent of workers reported fearing that they could lose their job if they filed a workers' compensation claim (Edisis, 2017). Another study found that only 8 percent of low-wage workers in New York City, Los Angeles, and

Thomason and Pozzebon, 2002), higher premiums (Moore and Viscusi, 1989), and higher deductibles (Shields et al., 1999) all lead to fewer workplace injuries. See Kniesner and Leeth (2014) for an overview of the literature examining the incentive effects of different aspects of the workers' compensation system.

Chicago who experienced a serious work-related injury filed a workers' compensation claim; 50 percent of them reported being instructed not to file a claim or were fired for doing so (Bernhardt et al., 2009).

Similar barriers and fear of retaliation likely limit workers' ability to take advantage of their rights to complain or to blow the whistle. Many have observed that protections under OSHA's Section 11(c) are weak at best. Punishment for violating Section 11(c) is essentially nonexistent: employers face no fines if they violate it. Furthermore, several barriers make it difficult for workers to file a whistleblower complaint: they must file the complaint within 30 days of the event, and they face a particularly high burden of proof for the complaint to be deemed valid, among other factors (Weatherford, 2013). OSHA pursues litigation in an extremely small minority of the thousands of complaints of retaliation it receives each year (Weatherford, 2013). It is no surprise, then, that a 1990 report found that fewer than 10 percent of OSHA inspectors said that workers could definitely exercise their rights under Section 11(c) of the Occupational Safety and Health Act without fear of employer retaliation (Government Accountability Office, 1990).

How would these policy distortions affect the provision of workplace safety? Injuries are more costly for employers when the injured worker files for workers' compensation, since every claim raises the employer's future premium and requires it to pay into a deductible. Similarly, maintaining a hazardous workplace is more costly for employers if their workers complain or blow the whistle to OSHA since these actions can lead to regulatory fines and bad publicity (Johnson, 2020). If contracts are incomplete, an employer cannot commit to not firing an at-will worker in retaliation for filing a workers' compensation claim or for blowing the whistle. Workers, fearing a threat of dismissal, will be less likely to undertake these actions. Injuries are thus less costly for employers in expectation, reducing employers' incentives to make investments in reducing them. As a result, laws that limit employers' ability to retaliate against workers for filing workers' compensation claims or blowing the whistle could raise employers' investments in worker safety.[30]

[30] Acharya et al. (2013) show theoretically that incomplete contracts create a similar hold-up problem for employee innovation effort: employers cannot commit to not arm-twist employees who contributed considerable effort to valuable

Even if the above logic regarding policy distortion is true, features of the labor market already theoretically incentivize employers to limit workplace injuries. In a competitive labor market, workers demand higher wages to work in riskier jobs (Rosen, 1986); indeed, much empirical evidence confirms that workers earn higher wages for undertaking riskier jobs, all else equal (Viscusi and Aldy, 2003; Kniesner et al., 2012; Lee and Taylor, 2019). However, substantial and growing evidence that labor markets are characterized by imperfect competition suggests that this market discipline might be more muted than implied in canonical models. There is evidence that monopsonistic competition is pervasive (Manning, 2011; Dube et al., 2020), arising from explicit sources such as employer concentration (Azar et al., 2020) but also broader factors like idiosyncratic worker preferences (Lamadon et al., 2019), and that the degree of monopsony power affects wages (Prager and Schmitt, 2021; Dube et al., 2018). Imperfect competition affects not just the *level* of compensation (both wages and non-wage compensation like injury risk), but it also attenuates the *price* of injury risk with respect to wages (Lavetti and Schmutte, 2018). Apart from imperfect competition, workers also have imperfect information about injury risk and other non-wage attributes of jobs (Viscusi and Moore, 1991; Conlon et al., 2018). Thus, while the labor market undoubtedly ensures that employers have some private incentive to minimize injury risk, imperfect competition and imperfect information attenuate this incentive relative to a perfectly competitive benchmark.

Finally, even if the labor market were competitive, and one abstracts from the policy distortions above, other theory suggests that workplace safety still might be under-provided in a competitive labor market under at-will employment. Investments in workplace safety include capital expenditures like updating and upkeeping machinery, but they also include relationship-specific assets like worker training, creating a "culture" of safety, and developing familiarity with processes and equipment (Williamson et al., 1975). If contracts are incomplete and in particular cannot be conditioned on the level of relationship-specific investment, then parties will under-invest in these relationships under at-will employment (MacLeod and Nakavachara, 2007). Because laws that limit retaliatory firing

innovation for a larger share of the ex-post surplus. As a result, innovation effort is inefficiently low in at-will employment relationships, and laws that limit employers' ability to engage in such retaliation raise employees' innovative effort.

raise the standards for dismissal, they cause employers to be more careful monitoring employees, raising incentives for making relationship-specific investments such as in safety (MacLeod and Nakavachara, 2007).

Policy distortions likely mute the extent to which workers' compensation and whistleblower opportunities incentivize employers to improve safety in at-will employment relationships. Furthermore, imperfect competition and incomplete contracts attenuate the extent to which the labor market disciplines employers' provision of safety. It is thus plausible that legal protections against employer retaliation for such behavior, by raising employers' expected costs of work-related injuries, could improve workplace safety.

A.2 Legal Restrictions on Retaliatory Firing

Over the last few decades, various states have adopted two types of limits on employers' ability to retaliate against workers for filing for workers' compensation or blowing the whistle. In this section, we provide brief institutional background about these two types of limits, in turn.

A.2.1 Common-Law Protections for Workers' Compensation Filing and Whistleblowing: the Public Policy Exception to At-Will Employment

Common law is adopted through precedent that can arise in the decisions of particular court cases. Since the late nineteenth century, US courts have generally interpreted the employer-employee relationship to be one of equal power for both parties; as a result, the "at-will" employment doctrine concluded that any employment contract should be considered an at-will agreement that could be terminated at any time by either party.

However, beginning with the Industrial Revolution, policymakers and judges began to make adjustments to this interpretation reflecting increasing recognition of disparities in power between employers and employees. The integral nature of employment to a person's livelihood, as well as the lack of recourse for retaliatory or unjust dismissal, was argued to necessitate exceptions to this doctrine of at-will employment (Muhl, 2001). Three major exceptions to at-will employment have been recognized by courts.

The public policy exception prohibits the dismissal of an employee who is either following or refusing to violate well-established public policy. California was the first state to adopt this exception, following the ruling in *Petermann v. International Brotherhood of Teamsters*[31] in 1959, in which the plaintiff filed suit after termination for refusal to falsely testify on behalf of the employer.

Since its first adoption in 1959, subsequent court decisions have widened the scope of actions covered by the public policy exception. One of the earliest expansions of the exception was the inclusion of filing for workers' compensation, beginning in Indiana with *Frampton v. Central Indiana Gas Co.* in 1973. Dorothy Frampton, the plaintiff, brought suit against her former employer, which had discharged her in retaliation for filing a workers' compensation claim after she was injured on the job. Central Indiana Gas, the employer, claimed a right to terminate Frampton without cause since she was an at-will employee. Though the court agreed that "under ordinary circumstances an employee at will may be discharged without cause," it held that courts should recognize an exception to this general rule when the employee is discharged "solely for exercising a statutorily conferred right." The court allowed the plaintiff to proceed with a tort action against the employer for compensatory and punitive damages.

A second expansion of the public policy exception included whistleblowing, beginning in Illinois with the 1981 case of *Palmateer v. International Harvesting Co.* Ray Palmateer, the plaintiff, alleged that after working for his employer for 16 years, he was dismissed after providing information to law enforcement on an employee who was potentially in violation of the law and agreeing to cooperate in any subsequent investigation or trial. The court ruled that cooperation with law enforcement should not be dissuaded by the threat of dismissal; by doing so, the court placed whistleblowing within the protection of the public policy exception.

Employers in states that had not adopted the public policy exception faced much lower risk from retaliating against workers in response to injury-related incidents. One legal case is illustrative. In the 1986 case *Evans v. Bibb Company* (178 Ga. App. 139 (1986)), a textile worker in Georgia brought suit against his former employer after being discharged in retaliation for filing a workers'

[31] 174 Cal. App. 2d184, 344 P.2d 25 (1959)

compensation claim. Though the worker argued he was wrongfully discharged for pursuing his rights under the Georgia Workers' Compensation Act, the court sided with the employer. Because Georgia had not adopted the public policy exception, the court decided that the employer, with or without cause and regardless of its motives, may discharge the employee without liability.[32]

Despite its initial adoption in 1959 in California, the public policy exception was not widely adopted until the 1970s and 1980s. In 1970, California was the sole adopter. By 1980, 15 states had adopted the exception, and this number grew to 42 by 1990. It currently exists in 43 states, and no state has ever revised the exception after adopting it. Figure 1 plots the evolution of states' adoption of the public policy exception over time.

Along with the public policy exception are two other recognized common-law exceptions to at-will employment, which are not the focus of our paper. The *implied contract* exception prevents the dismissal of an employee if the dismissal is in violation of a written or verbal statement that implies a contract has been established. The *good faith* exception establishes a covenant of good faith and fair dealing into all employer-employee relationships, effectively requiring that all dismissals be made with just cause. The implied contract and good faith exceptions have been adopted in 41 and 11 states, respectively.

Once adopted, the public policy exception represented a salient change in the legal environment for employers. As described in Edelman et al. (1992), numerous articles were written in legal, personnel, and management journals warning employers of the potentially profound change to the employment relationship spurred by the public policy exception (as well as the two other exceptions to at-will employment). For example, a 1984 article written by two lawyers in *Management Review* stated "the explosion of wrongful discharge litigation presents an important challenge for managers" (Edelman et al., 1992). One plausible reason that legal and personnel professions went to such great lengths is that the public policy exception was a tort-based action, which meant that employees

[32]Similarly, in the 1991 case *Grant v. Butler* (590 So 2d 254), a plaintiff in Alabama reported hazardous working conditions to OSHA and was subsequently fired. The court refused to create a public policy exception and dropped the case, arguing that such protections should be left to legislative means (Bird, 2004).

could sue for not just compensatory damages (e.g. back pay, attorney's fees), but also punitive damages. Because punitive damages are meant to "punish" the employer, the level of such damages can be considerable (Edelman et al., 1992).

As has been argued in prior studies, passage of the public policy exception (as well as the implied contract and good faith exceptions) was unlikely to be driven by underlying political or economic trends (Autor et al., 2007; DeNicco, 2015). Instead, common-law exceptions are a function of relevant cases available to be heard and the willingness of sitting judges to hear them. Such cases are specific to a particular employment relationship or occupation, but the consequences of the legal decision affect the state's labor laws more broadly. Additionally, as Acharya et al. (2013) argue with respect to productivity, the effect on workplace injury and illness rates is most likely not of concern to judges when they are deciding whether to adopt exceptions to at-will employment. Rather, judges' rulings are most likely to focus on the ability of employees to fight retaliatory or malicious dismissal (MacLeod and Nakavachara, 2007; Acharya et al., 2013). Consistent with this mode of judicial decision-making, Bird and Smythe (2008) find that neither economic nor political factors had any meaningful or significant predictive power for if and when a state adopted the public policy exception; the authors conclude that "it seems likely, therefore, that judges usually base their decisions on legal authorities rather than policy considerations or economic conditions" (Bird and Smythe, 2008).

A.2.2 Statutory Workplace Safety Protections

In contrast to common law, states' statutory law is encoded within legislation passed by state legislatures. Many states have adopted statutory protections for filing for workers' compensation and for whistleblowing. While these protections are similar to the common-law public policy exception in terms of the scope of protected worker actions, for reasons described in this section legal scholars have argued that these statutory protections are less likely to be an effective deterrent for employers (Sinzdak, 2008).

Thirty-five states have enacted whistleblower statutes that forbid employers from terminating employees for reporting ongoing forbidden or criminal activity within the firm, such as non-compliance with safety and health regulations. However, whereas the pubic policy exception uniformly allows a plaintiff to seek both compensatory and punitive damages (Edelman et al., 1992), most whistleblower statutes offer far more limited damages. Only two states (MT and NJ) allow for punitive damages, and roughly half of states with a statute only allow the plaintiff to sue for reinstatement and back-pay, which is unlikely to be a meaningful deterrent to employers. Among the rest, some offer compensatory damages, others attorney's fees. Furthermore, whereas the public policy exception offers a clear, uniform course of action for employees to file suits, statutes often place strict and confusing requirements within the procedures for reporting, rendering it difficult to report without jeopardizing one's own position.[33] Given the varied circumstances that can lead to an employee observing wrongdoing, these restrictions inevitably impose a burden on whistleblowers, reducing their capacity—and likely their willingness—to report wrongdoing. In Table B.1, we provide details about each state's whistleblower statute, including the damages offered, the remedy mechanism, and whether the employee or the state assumes the costs.

Additionally, 35 states have enacted statutes to prohibit retaliatory dismissal in response to an employee's filing for workers' compensation.[34] Punishments prescribed by these statutes vary widely, but they tend to be even lower than those under the whistleblower statutes; we describe these statutes in Table B.2. Some states offer no damages at all; others offer only reinstatement or back-pay (but not both).[35]

Common-law dismissal protections nearly always supersede any corresponding statutory protection, further diminishing the potency of statutes. If a state had enacted a statutory protection prior to

[33]For example, states place varied and confusing restrictions on to whom an employee may report. Some states require that whistleblowers report to government agencies to be guaranteed protection, while others require the report be made to a supervisor within the company itself (Sinzdak, 2008).

[34]26 states have passed both statutes, nine states have only passed the workers' compensation statutes, and nine others have passed only the whistleblower statute.

[35]For example, Kentucky's statute enables a worker to recover all damages but does not reference reinstatement as a potential remedy. In contrast, Hawaii's statute has no references to recovery of damages but does explicitly require reinstatement.

the courts' adoption of the common-law protection, the court always has the authority to augment the statute by prescribing additional penalties.[36] In contrast, we find no cases of statutory protections that expand on the powers already given through a previously adopted public policy exception.

For all these reasons, the public policy exception poses a stronger and more consistent means of preventing retaliatory discharge than do statutory protections. It is perhaps no surprise, then, that legal scholars have argued that these statutory protections are unlikely to be an effective deterrent for employers (Sinzdak, 2008). Thus, we expect them to have less of an effect on injuries than the common-law public policy exception.

[36]For example, in the Tennessee case *Hodges v. S.C. Toof & Co.* (1992), the court ruled that a statute prohibiting retaliatory dismissal for serving jury duty had insufficient remedies; as a result, the court added a private cause of action to sue for damages in addition to the penalties in the statute.

B Description of States' Whistleblower and Workers' Compensation Statutory Protections

Table B.1: State Whistleblower Protection Statutes

State	Statute	Year	Remedies	Remedy Mechanism	Assumption of Costs
AL					
AK	Alaska Stat. 18.60.089	1973	Reinstatement; Back pay	Judicial courts; State sues	State
AZ	Ariz. Rev. Stat. 23-425	1972	Reinstatement; Back pay	Judicial courts; State sues	State
AR					
CA	Cal. Lab. Code 6310	1973	1973: Forbidden but no remedies; 1985: Reinstatement; Back pay; Attorney's fees	Administrative	Employee
CO					
CT	§31-51m	1982	Reinstatement; Back pay; Attorney's fees	Judicial courts; Employee sues	Employee
DE	19 Del. C. §1703	2004	Reinstatement; Back pay; Compensatory damages; Attorney's fees	Judicial courts; Employee sues	Employee
FL	§448.102	1991	Reinstatement; Back pay	Judicial courts; Employee sues	Employee
GA					
HI	§378-61 et seq.	1987	Reinstatement; Back pay; Compensatory damages; Attorney's fees; Fine - $500 to $5,000	Judicial courts; Employee sues	Employee
ID					

IL	740 ILCS 174/5	2004	Reinstatement; Back pay; Compensatory damages; Attorney's fees	Judicial courts; Employee sues	Employee
IN					
IA					
KS					
KY	Ky. Rev. Stat. Ann. §338.121	1972	Reinstatement; Back pay	Administrative	Employee
LA	§23:964	1997	Reinstatement; Back pay; Compensatory damages; Attorney's fees	Judicial courts; Employee sues	Employee
ME	Me. Rev. Stat. Ann. Tit 26 §570	1979	Reinstatement; Back pay	Judicial courts; State sues	State
MD	§09.20.01 et seq.	1973	Reinstatement; Back pay	Administrative	Employee
MA					
MI	Mich. Comp. Laws 408.1065	1974	Reinstatement; Back pay	Administrative	Employee
MN	Minn. Stat. 182.654	1973	Reinstatement; Back pay; Attorney's fees	Administrative	Employee
MS					
MO					
MT	Mont. Code Ann. 39-2-905	1987	Back pay - up to 4 years; Punitive damages	Judicial courts; Employee sues	Employee

NE	Neb. Rev. Stat. 48-443	1993	Reinstatement; Back pay	No mechanism outlined	
NV	Nev. Rev. Stat. 618-445	1973	1973:; Forbidden but no remedies; 1975:; Reinstatement; Back pay	Judicial courts; State sues	State
NH	N.H. Rev. Stat. T. XXIII §275-E	1987	Reinstatement; Back pay; Attorney's fees	Judicial courts; Employee sues	Employee
NJ	§34:19-1 et seq.	1986	Reinstatement; Back pay; Attorney's fees; Punitive damages; Fine up to $20,000	Judicial courts; Employee sues	Employee
NM	N.M. Stat. 50-9-25	1975	Reinstatement; Back pay	Judicial courts; State sues	State
NY	Labor Law §740	1984	Reinstatement; Back pay; Attorney's fees	Judicial courts; Employee sues	Employee
NC					
ND	§34-01-20	1993	Reinstatement; Back pay - up to 2 years; Attorney's fees	Judicial courts; Employee sues	Employee
OH	R.C. 4113.51	1990	Reinstatement; Back pay; Attorney's fees	Judicial courts; Employee sues	Employee
OK					
OR	Or. Rev. Stat. Ann. 654.062	1973	Reinstatement; Back pay	Judicial courts or administrative	Employee or state
PA					
RI	R.I. Gen. Laws Ann. §28-50 et seq.	1995	Reinstatement; Back pay; Compensatory damages; Attorney's fees	Judicial courts; Employee sues	Employee

SC	S.C. Code Ann. 41.15.510	1973	Reinstatement; Back pay	Judicial courts; State sues	State
SD					
TN	50-1-304	1972	Reinstatement; Back pay	Judicial courts; State sues	State
TX	Tex. Labor Code Ann §411.082-083	1993	Reinstatement; Back pay	Judicial courts; Employee sues	Employee
UT	Utah Code Ann. §34A-6-203	1973	Reinstatement; Back pay	Administrative	Employee
VT	21 V.S.A. §231.	1973	Reinstatement; Back pay	Judicial courts; State sues	State
VA	Va. Code §40.1-51.2:1	1979	Reinstatement; Back pay	Commissioner or employer sues	Employee or state
WA					
WV					
WI					
WY	Wyo. Stat. Ann 27-11-109	1984	Fine - \$5,000 - \$70,000		

Note: This table provides information on state whistleblower protection statutes. See text for details.

Table B.2: State Workers' Compensation Statutes

State	Statute	Year	Remedies	Remedy Mechanism	Assumption of Costs
AL	Alabama Code Section 25-5-11.1.	1984	No remedies listed		
AK	AS 23.30.247(a)	1988	Civil action; no limit on damages listed	Judicial courts; Employee sues	Employee
AZ	ARS 23-1501(3)(c)(iii)	1987	Increase workers' compensation benefits by 25% or by $500, whichever is greater	Administrative	Employee
AR	A.C.A. §11-9-107.	1968	1968: Misdemeanor; 1987: Employee can recover attorney's fees; 1993: Upgraded to felony		
CA	Cal. Lab. Code §132a	1941	1941: Misdemeanor; 1972: Increase in workers' compensation benefits by 50%; 1987: Reinstatement; Back pay	Administrative	Employee
CO					
CT	Act. Conn. Gen. Stat. §31-290a(a).	1984	Reinstatement; Back pay; Compensatory damages; Attorney's fees; Punitive damages	Either judicial courts or administrative	Employee or state
DE	Del. Code Ann. tit. 19 §2365.	1994	Reinstatement; Back pay; Attorney's fees; Fine - $500 - $3,000	Judicial courts; Employee sues	Employee

FL	Fla. Stat. §440.205.	1979	No remedies listed		
GA					
HI	Haw. Rev. Stat. §386-142; Haw. Rev. Stat. §378-32(2).	1978	Reinstatement		
ID					
IL	820 ILCS 305/4(h).	1975	No remedies listed		
IN					
IA					
KS					
KY	K.R.S. §342.197.	1984	Compensatory damages; Attorney's fees	Judicial courts; Employee sues	Employee
LA	La. Rev. Stat. §23:1361(A), (B).	1980	Back pay - up to 1 year; Attorney's fees	Administrative	Employee
ME	Me. Rev. Stat. 39-A §353.	1991	Reinstatement; Back pay; Attorney's fees	Administrative	Employee
MD	M.D. Code Lab. & Empl., §9-1105.	1957	Misdemeanor		

MA	Mass. Gen. Laws ch. 152 §75B(2)	1985	Reinstatement; Back pay; Attorney's fees	Judicial courts; Employee sues	Employee
MI	MCL §418.301(11).	1985	No remedies listed		
MN	Minn. Stat. §176.82.	1975	Reinstatement; Back pay; Attorney's fees; Punitive damages - up to 3x workers' comp benefits	Judicial courts; Employee sues	Employee
MS					
MO	R.S. Mo. §287.780.	1925	1925: Misdemeanor - one week to one year in jail; Fine - \$50 - \$500; 1979: Civil action; No limit on damages listed	Judicial courts; Employee sues	Employee
MT	Section 39-71-317; 39-2-905	1987	Reinstatement if employee recovers within two years; Back pay - up to 4 years	Administrative	Employee
NE					
NV					
NH					
NJ	N.J.S.A. §34:15 39.1.	1968	Reinstatement; Back pay; Fine \$100 - \$1000; Up to 60 days in jail	Administrative	Employee
NM	N.M. Stat. §52-1-28.2(A)	1990	Reinstatement; Fine - up to \$5,000	Administrative	Employee
NY	NY Workers Compensation Law §120	1973	Reinstatement; Back pay; Attorney's fees; Fine - \$100 - \$500	Administrative	Employee

NC	NC Gen. Stat. §95-241 et seq.	1991	Reinstatement; Back pay; Attorney's fees; Punitive damages	Commissioner or employer sues	Employee or state
ND	N.D. Cent. Code §65-05-37.	2003	Civil action; no limit on damages listed; Attorney's fees	Judicial courts; Employee sues	Employee
OH	R.C. §4123.90	1978	Reinstatement; Back pay; Attorney's fees	Judicial courts; Employee sues	Employee
OK	85 Ok. St. §5	1976	1976: No remedies listed; 1986: Reinstatement; Compensatory damages; Punitive damages - up to $100,000; 2011: Repealed	Judicial courts; Employee sues	Employee
OR	ORS §659A.040(1).	2001	Reinstatement; Back pay - up to 2 years; Attorney's fees	Either judicial courts or administrative	Employee or state
PA					
RI					
SC	S.C. Code Ann. §41-1-80.	1986	Reinstatement; Back pay	Judicial courts; Employee sues	Employee
SD	S.D. Codified Laws §62-1-16.	1999	Civil action; no limit on damages listed	Judicial courts; Employee sues	Employee
TN					
TX	Texas Labor Code §451.001.	1993	Reinstatment; Civil action; no limit on damages listed	Judicial courts; Employee sues	Employee
UT					

VT	21 V.S.A. §710	1985	1985: No remedies listed; 2013: Reinstatement; Back pay; Civil penalties - no explicit limit	Administrative	Employee
VA	Va. Code §65.2-308.	1982	Reinstatement; Back pay; Compensatory damages; Attorney's fees	Judicial courts; Employee sues	Employee
WA	RCW §51.48.025	1985	Reinstatement; Back pay	Commissioner or employer sues	Employee or state
WV	W. Va. Code §23-5A-1.	1978	No remedies listed		
WI	Wis. Stat. §102.35.	1975	Reinstatement; Back pay; Fine - \$50 - \$500	Administrative	Employee
WY					

Note: This table provides information on state workers' compensation statutes. See text for details.

C Additional Sensitivity Analysis

This section presents a variety of tests of sensitivity of the main results of the paper.

C.1 Additional Event Studies

In the main body of the paper we present two event studies. The first (Figure 2a) considers the effect of the public policy exception and the second (Figure 2b) considers instead whistleblower protection statutes, and both measure workplace accidents using data from OSHA. This section shows four analogous event studies which all follow closely the methodology used in Figures 2a and 2b but vary the policy being considered and the data source for workplace safety.

Figure D.3 considers the effect of workers' compensation statutes on workplace injuries as measured with the OSHA data. The figure shows no evidence that adoption of a workers' compensation statute affected injuries, which is consistent with the null effect we find in Table 2.

Figure D.4 reproduces Figure 2a but uses NSC data on workplace deaths rather than OSHA data on accident inspections. The pre-trends bounce between positive and negative and are never statistically significant. The coefficients in the post-period are nearly all at least slightly negative and one is statistically significant and negative. While noisier than the event study with the OSHA data, this graph corroborates the robustly negative effects of the PPE on fatal injuries found in in Table 3.

Figure D.5 shows an unclear relationship between the adoption of whistleblower statutes and workplace deaths reported to NSC, which is consistent with the weak connection shown in Table 3.

Figure D.6 shows an event study with workers' compensation and the NSC dataset. There is no clear change in injuries in the post period, which is entirely consistent with the lack of relationship found in Table 3.

C.2 Sensitivity to Alternative Specification Choices

Our estimates are quite stable to alternative choices of our sample, unit of observation, and transformations of the dependent variable.

Changes to the sample window: An important consideration with our results is that, while most of the variation in the adoption of the public policy exception (and, to a lesser extent, the statutes) was in the 1970s and 1980s, we include up to the year 2005 in our regressions in Table 2. Including a "post-period" that is so long, and that differs in length across states, could influence our estimates in counter-intuitive ways, especially in the presence of dynamic or heterogeneous treatment effects (Goodman-Bacon, 2021). We assess the sensitivity of our estimates to this sample period by re-estimating the effects of the public policy exception, progressively choosing an earlier sample end year. For the sake of brevity, we conduct this exercise only for the public policy exception using the most saturated regression model (corresponding to Column 7 of Table 2) . We collect our estimates from this exercise in Figure D.7. Our estimates are quite stable to choosing earlier sample end points.

A related concern is that while our sample period begins in 1979, for those 21 states outside of OSHA's jurisdiction we only observe OSHA-reported injury data beginning in 1992, which is after the majority of public policy exceptions had taken place. In Table D.4 we replicate the estimates in Table 2, except that we restrict the sample to the 29 states under OSHA's jurisdiction. The estimates are essentially unchanged.

A distinct concern regarding the sample window is that the "treatment effect" of legal protection in the year of adoption and in later years could be quite different, for example due to delays in firms learning about the law change. Similarly, if firms somehow anticipate that a law change is about to occur, injury rates could change in the year prior to adoption. While such concerns are largely addressed with the event study specification reported in Figure 2a, we also replicate our main results in Table 2 but exclude the year immediately preceding, of, and immediately following adoption of the three legal protections. We report the results in Table D.5. The estimates are, if anything, slightly larger in magnitude than our baseline estimates.

Changing the unit of observation: Because we collapse our OSHA-observed injury data to the state-year level, this aggregation could mask heterogeneous effects across sectors. We run a specification similar to Equation 1, except that the unit of observation is a state-sector-year, with sectors partitioned into "manufacturing" and "non-manufacturing." We amend Equation 1 by including sector-state and sector-year fixed effects instead of just state fixed effects. The estimates, reported in Table D.6, closely align with our baseline estimates.

We also explicitly test for any sectoral heterogeneity in the effect of legal protection against retaliation in Table D.7, where we separately estimate their effects for manufacturing and non-manufacturing sectors. No clear pattern of heterogeneity emerges: in some specifications the point estimates are larger for manufacturing, in others the reverse is true, and the differences are never statistically significant.

Alternative Transformations of Injuries: In our specifications thus far reported, our dependent variable is the log of the rate of injuries per worker and we use a linear model. We consider other sensible choices. In Tables D.8 and D.9, we still use a linear model but switch the dependent variable to be the log of the *number* of injuries, and we control for the log of employment. In Tables D.10 and D.11, we instead estimate a Poisson regression and a negative binomial regression with the dependent variable equal to the number of injuries.[37] Our estimates are remarkably unchanged across these various choices.

C.2.1 Falsification Tests

Whether we measure workplace injuries using the counts of serious injury inspections conducted by OSHA, or the number of fatal work-related injuries reported to NSC, we find consistent evidence that the public policy exception meaningfully reduced injuries. These results might be spurious if we found that the public policy exception (or other legal protections) led to changes in types of OSHA inspections unrelated to injuries, or to fatal injuries that are not work-related. We find no such evidence.

In Table D.12, we replicate Table 2, except that our dependent variable is the log of the rate of OSHA *programmed* inspections per worker. Recall from Section 3.2 that programmed inspections are determined based on national or local OSHA priorities and thus are unrelated to events occurring at any particular workplace. Their occurrence, then, should be insensitive to changes in legal protection against retaliatory firing. Table D.12 supports this intuition. Across all

[37]We do not include results from Poisson or negative binomial regressions with both region-year fixed effects and state-specific trends, as this model was unable to converge.

specifications, the coefficient on the *Public Policy Exception* is statistically insignificant, and it flips sign depending on specification. We do obtain large and statistically significant negative coefficients on the *Whistleblower statute* with state and year fixed effects, but the point estimates attenuate in magnitude and are no longer significant with the inclusion of region-year and state-specific trends in Columns 6 and 7, respectively. We report corresponding event study estimates for the number of programmed inspections in Figures D.8 and D.9. Corroborating the regression estimates, these figures show show no change in the occurrence of programmed inspections before or after the adoption of either the public policy exception or the whistleblower statute.

In Table D.13, we replicate Table 3, except that our dependent variable is the log of the rate of non-work fatal injuries in NSC per individual in the population.[38] The purpose of this falsification test is to test whether there is some omitted variable that is affecting overall accidents and is correlated with the public policy exception or either of the statutory protections. The results in Table D.13 reveal no indication of a negative relationship between any of the policies and non-work fatal injuries (there is a very weakly positive relationship in two specifications), which provides reassurance that the main results are not being driven by an omitted variable.

C.3 Alternative Inference Using Placebo Adoption

As a complement to our baseline approach for inference (clustering heteroskedasticity-robust standard errors by state), we can generate alternative p-values on our estimates using an approach sometimes called "randomization inference" (Young, 2019). We only implement this procedure for the public policy exception, and for the specification with state and year fixed effects and no controls, to avoid confusion with how we treat the other policies as controls. We randomly assign an adoption of a "placebo" pubic policy exception, based on the empirical distribution of actual adoption across states. Specifically, we note the number of states that adopt the public policy exception in each year, and we assign that same number of a random subset of states (without replacement) to adopt

[38]Non-work injuries are defined as the sum of injuries in the home and non-automobile injuries in public places. Automobile injuries cannot be included because the NSC dataset does distinguish between work and non-work automobile injuries.

a placebo exception in that year. We estimate Equation 1 using this placebo PPE instead of the actual PPE, and we save the coefficient on the placebo PPE. We repeat this process 10,000 times. We use where the estimate of the actual PPE (in Column 1 of Table 2) falls in this distribution of placebo $PPEs$ to construct our p-value.

Figure D.10 plots the distribution of coefficients on the placebo PPE. As expected, this distribution is centered at zero. The dotted vertical line, representing our coefficient on the actual PPE, is far in the tail of this distribution. Out of 10,000 iterations, 106 have a coefficient with an absolute value that is greater than 0.137 (the absolute value of the coefficient on actual PPE, implying a p-value of 0.016, which is quite similar to our p-value of 0.006 using our more standard inference.

C.4 Assessing the Robustness of Our Main Estimates to Heterogeneous Treatment Effects

Recent papers have illustrated that the standard two-way fixed effects estimator can be biased when the effect of a policy is heterogeneous over time or across geographical areas (Goodman-Bacon, 2021; Athey and Imbens, 2022). In this section we assess whether heterogeneity poses a threat to identification in our estimates of the safety effect of the public policy exception using methods advanced by two recent papers: de Chaisemartin and d'Haultfoeuille (2020) (hereafter referred to as CH) and Callaway and Sant'Anna (2021) (hereafter CS).

C.4.1 de Chaisemartin and d'Haultfoeuille (2020)

CH provides a diagnostic test for whether heterogeneity across time or states is likely to bias the difference-in-difference estimator. The paper also provides a modified version of the conventional difference-in-difference estimator which is robust to heterogeneity that a researcher can use when the diagnostic indicates that heterogeneity may be an important source of bias.

The diagnostic is based on the weights that the conventional difference-in-difference estimator assigns to each observation. The researcher first computes the ratio of the standard deviation of these weights to the absolute value of the difference-in-difference estimator; this ratio reveals the minimum value of the standard deviation of the average treatment effects across observations under which the average treatment on the treated (ATT) may actually have the opposite sign than the difference-in-difference coefficient. Second, the researcher calculates the proportion of weights that are negative. If the ratio is close to zero, and if many weights are negative, the standard difference-in-difference estimator is likely to be biased. In this case, CH provide an alternative estimator.

We undertake this diagnostic test with the *did_multipleGT* Stata package that the authors helpfully provide, and we apply it to our dataset and fixed effects specification corresponding to Column 7 of Table 2.[39] In our case, 315 out of 753 ATT's have a negative weight; however, the ratio of our difference-in-difference estimator (-0.123) over the standard deviation of these weights (0.0053) is large (23.6). Combined, this diagnostic suggests heterogeneity in treatment effects is unlikely to meaningfully bias our estimates, and the chance that the "true" average treatment effect is opposite sign is essentially zero.

Even though the diagnostic from CH suggests that bias from heterogeneity is not a substantive concern in our context, for the sake of completeness we present results using CH's heterogeneity-robust estimator in Figure D.11. This figure is analogous to Figure 2a, with the same dependent variable (natural log of the injury rate), independent variables of interest (lags of the adoption of the public policy exception), fixed effects (state and division-year) and linear state trend. The key difference is that we do not include any leads of the adoption of the exception; this is because CH's estimator *assumes* common pre-trends; given that the point estimates of all leads (illustrated in Figure 2a were all essentially zero, this assumption appears valid in our setting.

The figure shows that the CH estimator yields a remarkably similar estimate of the effect of the public policy exception on workplace injuries. The point estimates on each lag is very similar to those in Figure 2a; the estimate is small in the year of adoption, but in the following years the estimate is negative and sizable (implying a reduction in injuries of roughly 18 percent). In sum,

[39]One limitation of the CH diagnostic in our context is that the Stata command that CH provide to calculate the weights (*twowayfeweights*) does not allow the researcher to weight observations, even though we weight our state-year observations by each state's share of the national population in 1979.

there is no evidence that heterogeneity across either time or states is biasing our main results.

C.4.2 Callaway and Sant'Anna (2021) Estimator

Callaway and Sant'Anna (2021) (CS) propose an alternative estimator to estimate average effects of a treatment that is adopted in a staggered fashion across groups. CS first considers "group-time average treatment effects:" the average treatment effect for group g and time t, where groups g are defined by the time period when units are first treated. The authors then provide steps to aggregate these many group-time effects into a single summary measure, such as the average treatment effect on the treated (ATT). Inference is obtained via bootstrapping. We implemented this estimator using the authors' *did* R package; we refer readers to Callaway and Sant'Anna (2021) for more details.

In Table D.14, we compare our estimates of effect of the public policy exception on injuries (with the OSHA data) that we obtain with our baseline two-way fixed effects approach versus the Callaway and Sant'Anna (2021) estimator. We compare the estimates using a) state and year fixed effects only (Columns 1 and 4), state and division-year fixed effects (Columns 2 and 5), and state and division-year and state-specific trends (Columns 3 and 6). In all cases, the Callaway and Sant'Anna (2021) estimator yields a point estimate that is slightly *larger* and more precise than our baseline estimate. These results further illustrate that our estimates are robust to heterogeneous treatment effects, and if anything might slightly *under-estimate* the effect of the public policy exception.

C.5 Heterogeneous Effects of Statutory Protections Based on Availability of Punitive Damanges

As described in Section 5.3, we expect that whistleblower and workers' compensation statutes are more likely to promote workplace safety when they include stricter penalties like punitive damages. To test this hypothesis, we create variables indicating whether a state adopted a whistleblower statute without punitive damages *Whistleblower, no punitive* or with them *Whistleblower, punitive*, as well as analogous variables for the workers' compensation protection.[40]

[40] Among the states in our sample underlying the regressions in Table 2, 15 adopted the whistleblower staute during the sample period, and two of these (New Jersey and Montana) included punitive damages. In that same set of states, 12 adopted the workers' compensation statute, of which four (Connecticut, North Dakota, South Dakota, and Texas) included punitive damages. These numbers do not include a few states that had adopted these statutes outside of our

We test for such heterogeneity in Table D.15. We report regressions in a similar organization to Table 2, except that we omit controls for any of the common-law exceptions and state-level controls, since including an interaction might risk overfitting the data in the presence of such controls. (Our results are not qualitatively different if we include them.) Column 1 separately estimates the effect of whistleblower statutes with and without punitive damages, in a specification that includes state and year fixed effects only. Adoption of a whistleblower statute without punitive damages leads to a 7.8% reduction in the injury rate ($\hat{\beta} = -0.081$, $p = 0.035$); adoption of the statue *with* punitive damages leads to an even larger reduction in injuries of 13.2% ($\hat{\beta} = -0.142$, $p = 0.003$), and these estimates are statistically significantly different from each other ($p = 0.016$). Column 2 reveals similar heterogeneity in the workers' compensation statute: adoption of a workers' compensation statute without punitive damages has no detectable effect on the injury rate ($\hat{\beta} = 0.047$, $p = 0.461$); adoption of the statue *with* punitive damages leads to 7.9% reduction in injuries ($\hat{\beta} = -0.082$, $p = 0.021$), and these estimates are statistically significantly different from each other ($p = 0.035$).

We probe the reliability of these estimates of heterogeneity in the remaining columns with successively more demanding tests. In Column 3, we keep the same set of fixed effects but include both types of both statutes in the same regression. The estimates for the whistleblower statute are essentially unchanged and remain statistically significantly different from each other; the qualitative pattern for the workers' compensation statute holds, but the estimates become statistically indistinguishable from each other. In Column 4, we include division-year rather than year fixed effects. The patterns largely hold for the whistleblower statute, and the point estimate on the statute with punitive damages *increases* in magnitude, but the estimates are slightly less precise (the p-value on the difference between the estimates is 0.116). The estimates for the two types of workers' compensation statutes attenuate even more in magnitude and are quite imprecise. Finally, in Column 5 we additionally include state-specific linear trends. This last specification is extremely demanding on the data, particularly with such a small set of states adopting statutes with punitive damages, and it risks introducing bias (particularly in this small sample) when it is difficult to separate pre-existing

sample period.

trends from dynamic treatment effects (Wolfers, 2006). In this specification, the pattern reverses for the whistleblower statutes: the point estimate on adoption of the statute without punitive damages doubles in magnitude to -0.17 ($p = 0.025$), that on the statute with punitive damages flips sign and loses significance. We caution against interpreting these last results, given the concerns just highlighted.

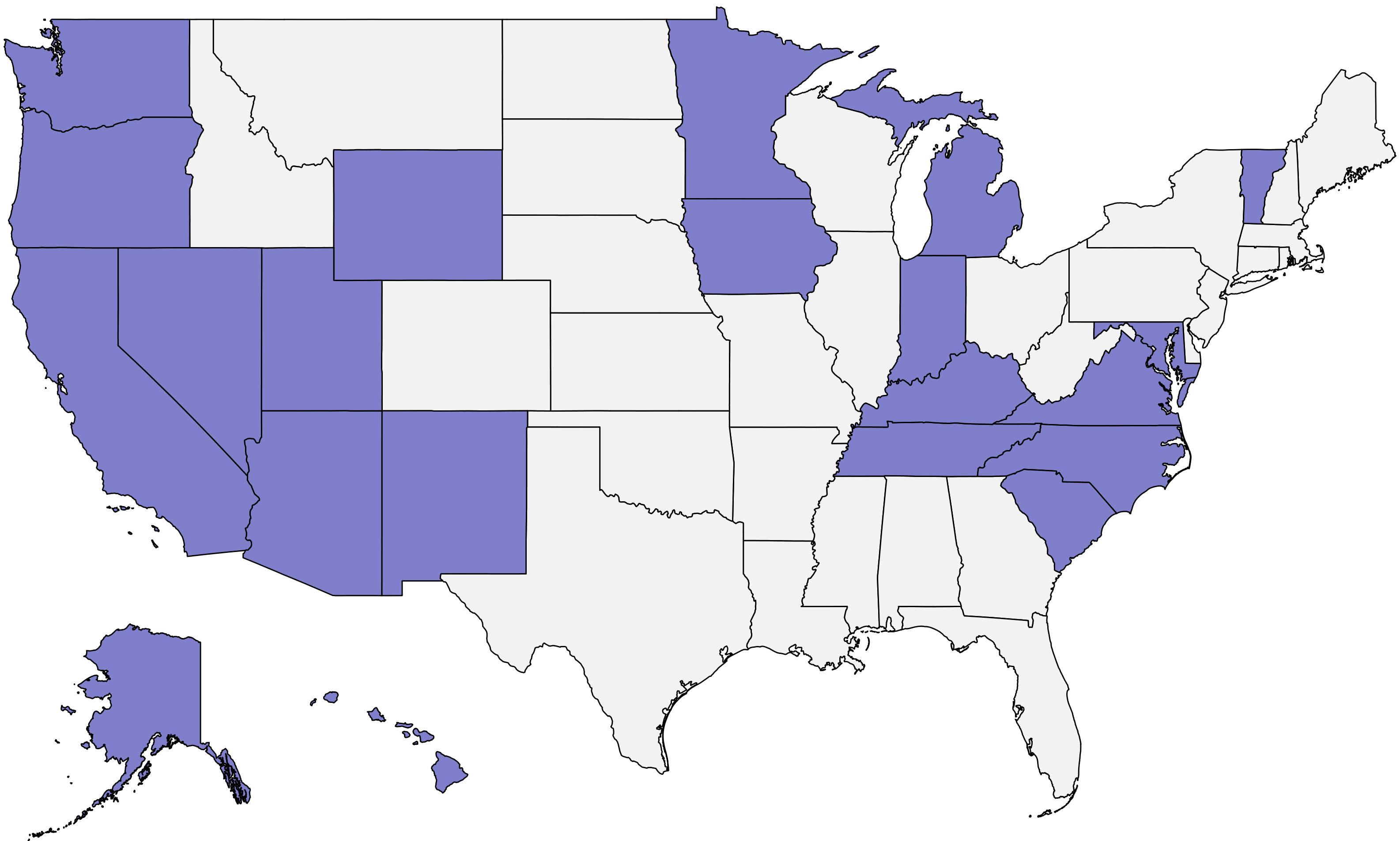
Turning to NSC data, there are not sufficient changes during our sample period in which states have passed whistleblower statutes with punitive damages to analyze possible heterogeneity in whistleblower statutes. Column 1 of Table D.16 shows a regression where the independent variable is workers' compensation statutes, with and without punitive damages, with only state and year fixed effects. Statutes with punitive damages cause a 15.5% reduction in injuries ($\hat{\beta} = -0.168, p = 0.006$), compared to a much smaller reduction of 0.25% for statutes without punitive damages ($\hat{\beta} = -0.025, p = 0.734$); however, the difference is not quite statistically significant ($p = 0.124$).

In Column 2 we replace year fixed effects with division-year fixed effects. The coefficient on statute with punitive damages is negative and a larger magnitude compared to the statute without punitive damages ($\hat{\beta} = -0.051$ compared to $\hat{\beta} = 0.025$), but neither coefficient is statistically significant, and the difference is also not statistically significant ($p = 0.399$).

Column 3 adds state-specific linear trends. Here the whistleblower statute with punitive damages is statistically significant at the 10 percent level, and coefficient is larger in magnitude compared to the coefficient on the statute without punitive damages and a different sign ($\hat{\beta} = -0.062$ compared to $\hat{\beta} = 0.109$). They are significantly different at the 10 percent level ($p = 0.0501$).

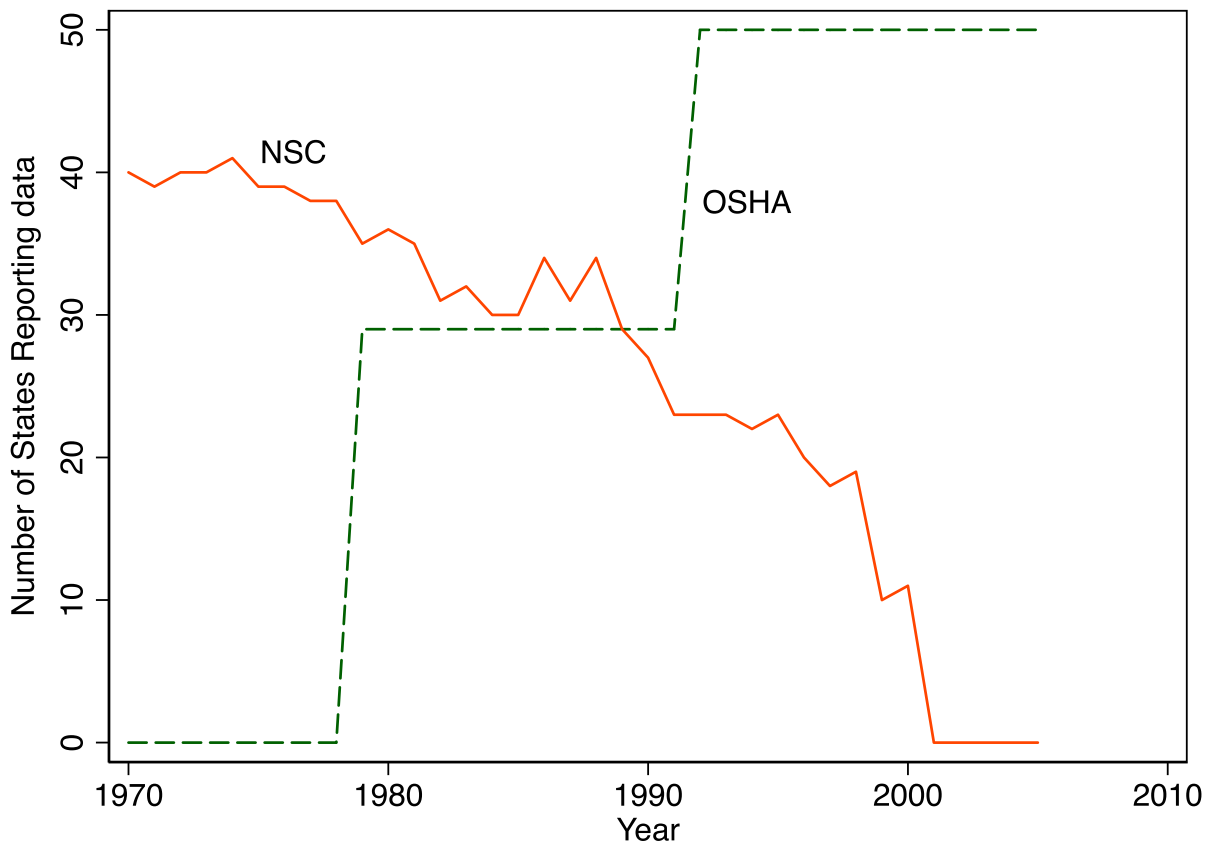
D Appendix Tables and Figures

Figure D.1: States Under OSHA's Jurisdiction



Note: Private-sector establishments in the 29 states in light gray are under federal OSHA jurisdiction; the 21 states in blue have their own OSHA-approved state-run occupational safety and health plans. Source: https://www.osha.gov/dcsp/osp/.

Figure D.2: Number of States Reporting Workplace Accidents and Deaths to OSHA and NSC, by Year



Note: This figure shows the number of states in our dataset reporting to our two sources of injury data each year from 1970 through 2005. The first source is Occupational Safety and Health Administration (OSHA) inspections triggered by a serious injury, collected from OSHA's Integrated Management Information System. The second source is data on fatal injuries collected from the National Safety Council (NSC).

Table D.1: The Effect of the Public Policy Exception and Whistleblower Statute on the Share of Employment in Manufacturing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	0.013			0.012*	0.013	-0.013***	-0.002
	(0.008)			(0.007)	(0.008)	(0.004)	(0.002)
Whistleblower statute		0.002		0.000	0.001	0.004	-0.000
		(0.006)		(0.007)	(0.007)	(0.004)	(0.002)
Workers' comp statute			0.004	0.003	0.006	0.001	-0.003
			(0.011)	(0.011)	(0.010)	(0.006)	(0.003)
Good faith exception					-0.005	-0.012*	0.002
					(0.016)	(0.006)	(0.002)
Implied contract exception					-0.008	0.001	0.000
					(0.007)	(0.004)	(0.003)
State unemployment rate					-0.001	-0.003**	-0.002***
					(0.001)	(0.001)	(0.000)
Democratic governor					-0.002	-0.006***	-0.000
					(0.002)	(0.002)	(0.001)
Observations	1077	1077	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes
Mean Dep Var	0.176	0.176	0.176	0.176	0.176	0.176	0.176

Note: These regressions follow Table 2, but the dependent variable is the share of employment in manufacturing. The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.2: The Effects of the Public Policy Exception on Compliance with OSHA Regulations: All Sectors

	(1)	(2)	(3)	(4)
Pubic policy exception	-0.041	-0.053	-0.046	-0.075**
	(0.121)	(0.104)	(0.056)	(0.037)
Whistleblower statute		0.107	0.138**	0.021
		(0.088)	(0.066)	(0.045)
Workers' comp statute		0.000	-0.029	0.005
		(0.096)	(0.066)	(0.042)
Good faith exception		0.035	-0.013	-0.040
		(0.117)	(0.088)	(0.037)
Implied contract exception		0.015	0.015	-0.089***
		(0.064)	(0.031)	(0.032)
Log # workers present	0.053***	0.053***	0.053***	0.052***
	(0.009)	(0.009)	(0.009)	(0.009)
Union present	-0.061**	-0.061**	-0.047**	-0.050**
	(0.024)	(0.023)	(0.021)	(0.022)
Observations	893486	893486	893486	893486
Mean Dep Var (in levels)	0.693	0.693	0.693	0.693
Sector-state and sector-year FE	Yes	Yes	Yes	Yes
Industry year FE	Yes	Yes	Yes	Yes
Division-year FE	No	No	Yes	Yes
State trend	No	No	No	Yes

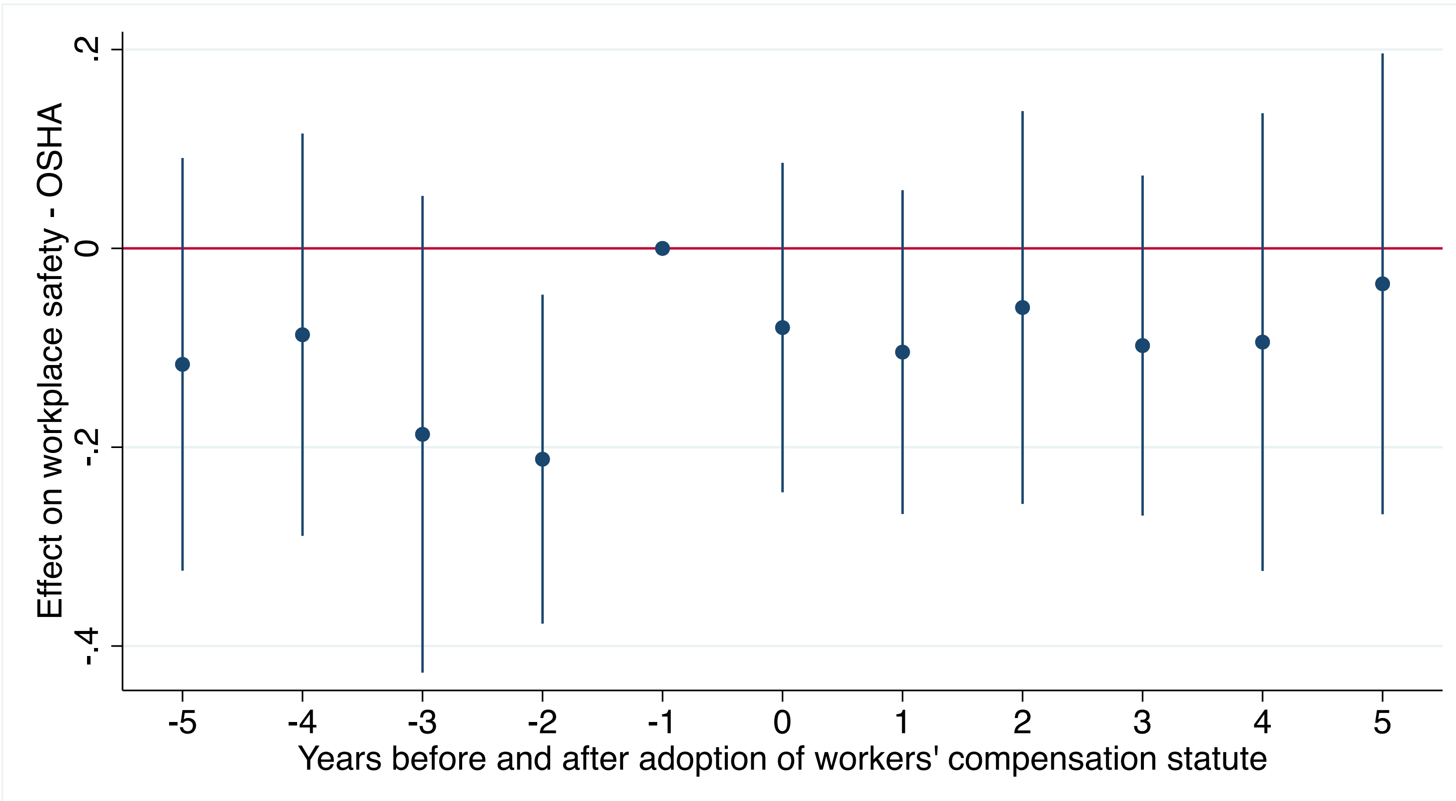
Note: *asinh(gravity)* is the inverse hyperbolic sine of the sum of the gravity assigned to all violations detected in an inspection, where "gravity" is a score ranging from 0–10 based on OSHA's assessment of the severity of the violation. All sectors are included; see text for details. Robust standard errors clustered by state shown in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.3: Summary Statistics Split by Right-to-Work Status

	Not RTW	RTW	Total
Complaint inspections per 1000 employees (OSHA)	0.741	0.614	0.695
	(1.874)	(1.674)	(1.805)
Accident inspections per 1000 employees (OSHA)	0.0733	0.0894	0.0792
	(0.159)	(0.218)	(0.183)
Deaths per 1000 employees (NSC)	0.0537	0.0750	0.0615
	(0.0444)	(0.0470)	(0.0465)
Public policy exception	0.441	0.282	0.383
	(0.497)	(0.450)	(0.486)
Workers' comp. anti-retaliation statute	0.459	0.194	0.362
	(0.498)	(0.396)	(0.481)
Whistleblower protection statute	0.337	0.388	0.356
	(0.473)	(0.488)	(0.479)

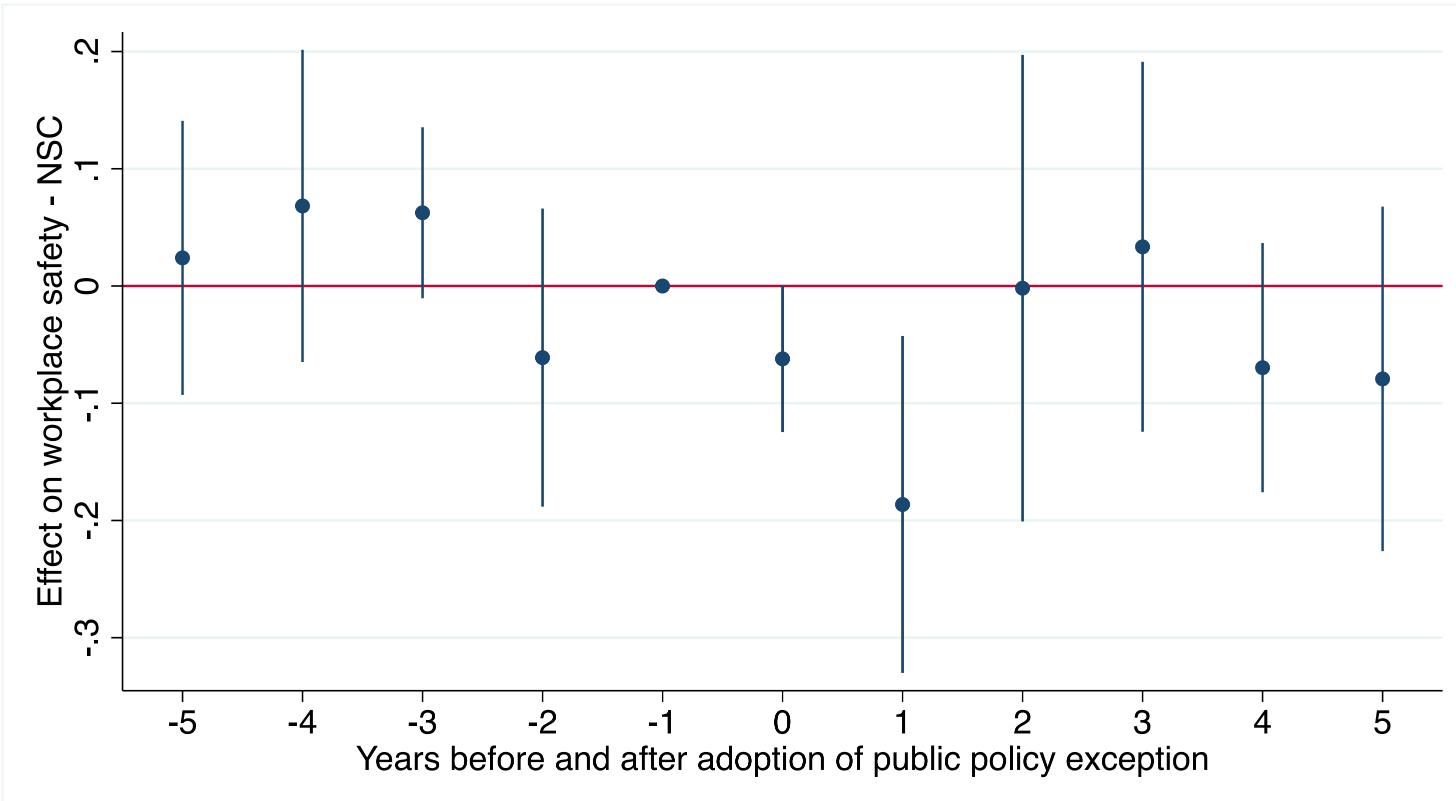
Note: Table shows mean and (in parentheses) standard deviation for our key dependent and explanatory variables, broken down by whether a state has passed a Right-to-Work law. Complaint inspections refer to inspections by OSHA that were initiated by a complaint by an employee. Please see text for further details and sources.

Figure D.3: Event Study: Effect of Workers' Compensation on Workplace Injuries (Data from OSHA)



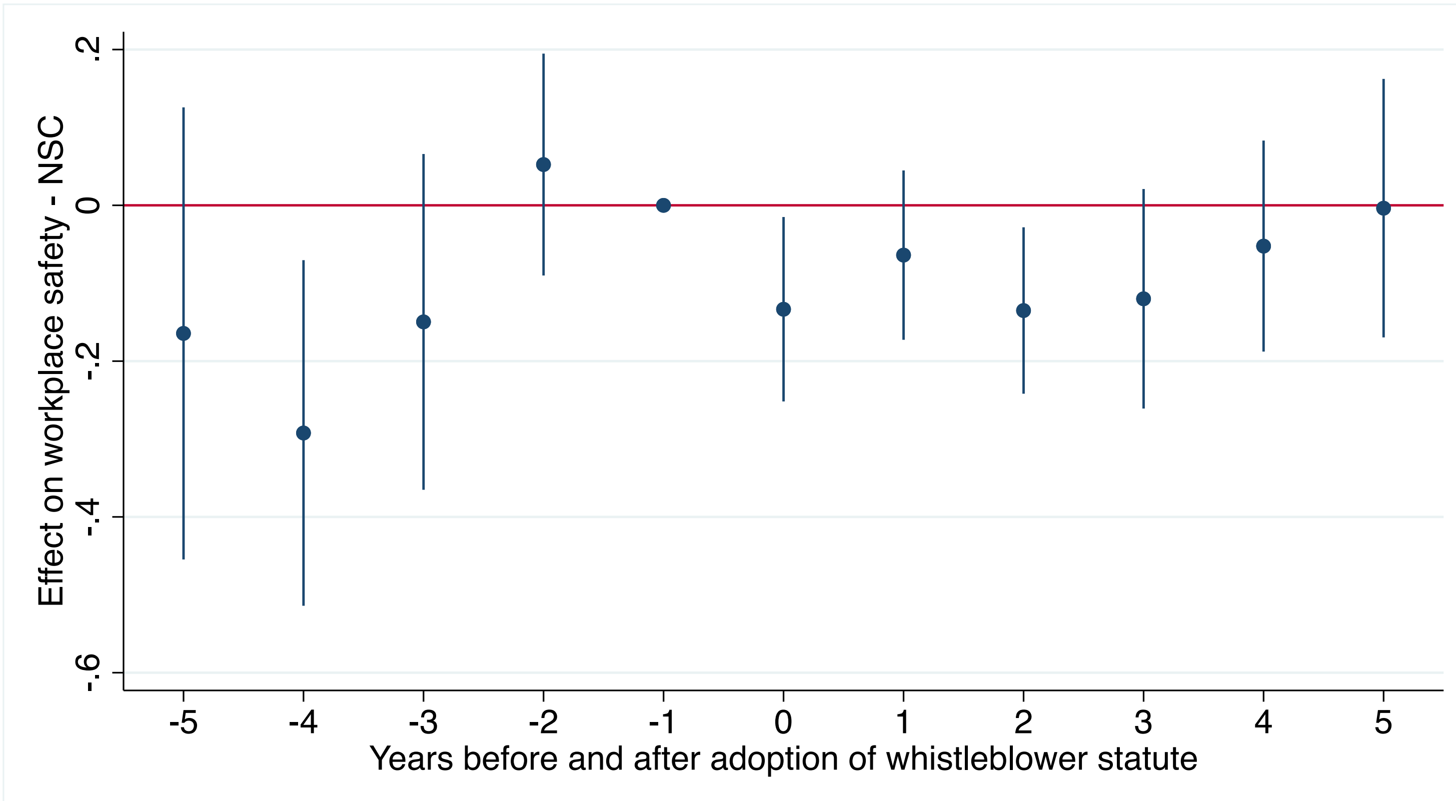
Note: The plot shows the effect of workers' compensation statutes on workplace injuries (data from OSHA) before and after adoption, based on Equation 2. Coefficients on the left side of the plot indicate leads, and coefficients on the right side indicate lags. Included in the regression but not displayed are coefficient terms for having passed a workers' compensation statute six or more years in the past or six or more years in the future; see text for details. The dots represent the point estimates, and the vertical bars show 95 percent confidence intervals.

Figure D.4: Event Study: Effect of the Public Policy Exception on Workplace Fatalities (Data from NSC)



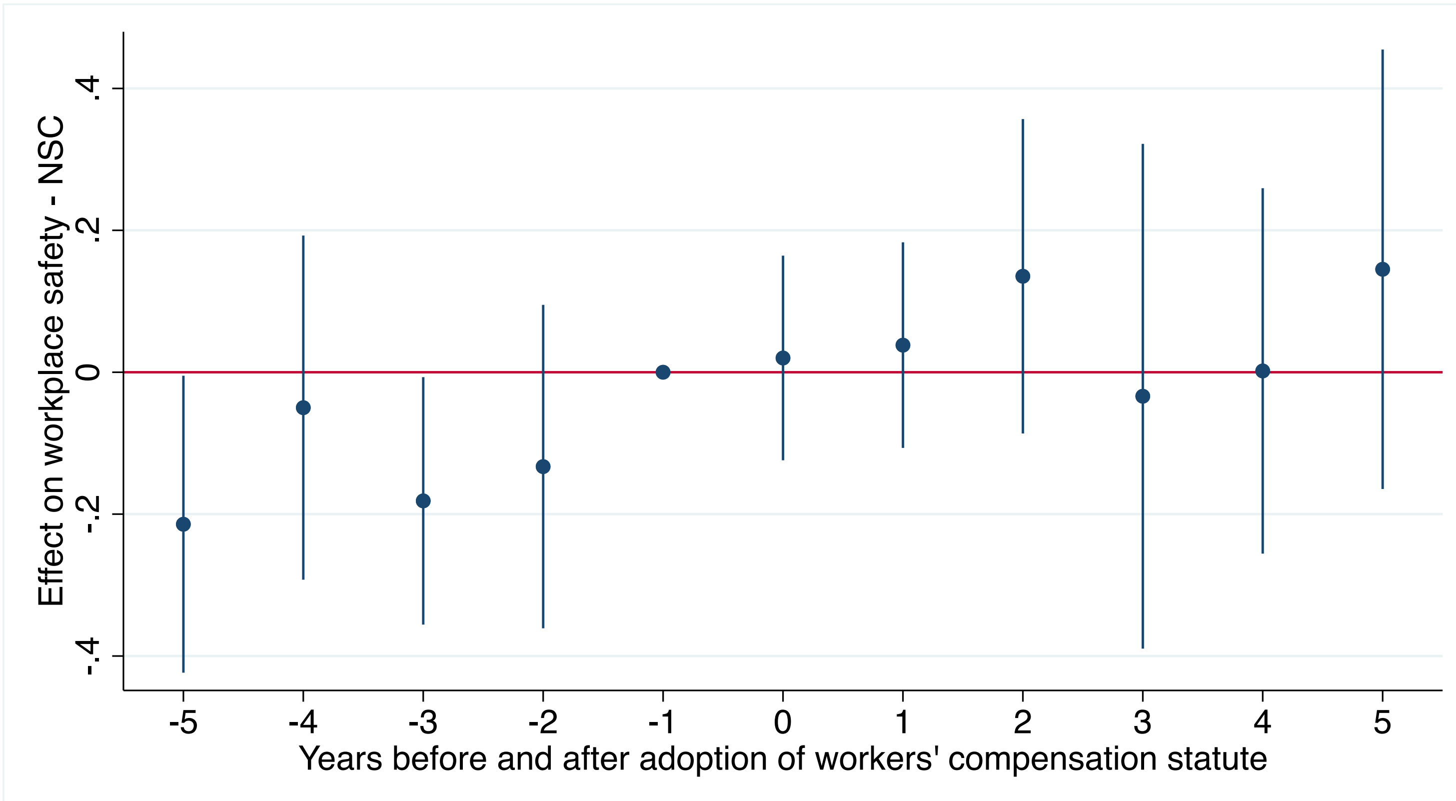
Note: The plot shows the effect of the public policy exception on workplace fatalities (data from NSC) before and after adoption, based on Equation 2. Coefficients on the left side of the plot indicate leads, and coefficients on the right side indicate lags. Included in the regression but not displayed are coefficient terms for having adopted the public policy exception six or more years in the past or six or more years in the future; see text for details. The dots represent the point estimates, and the vertical bars show 95 percent confidence intervals.

Figure D.5: Event Study: Effect of Whistleblower Statute on Workplace Fatalities (Data from NSC)



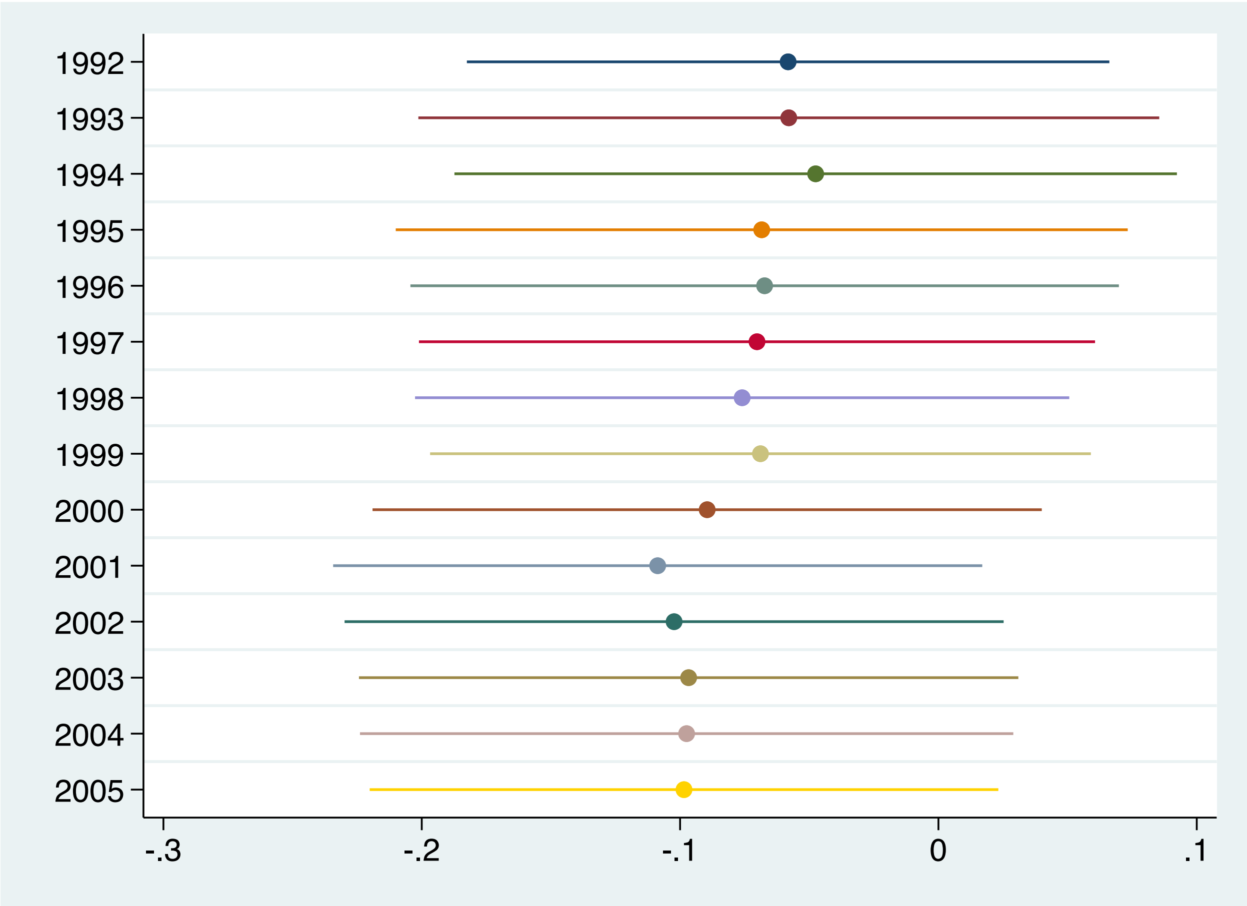
Note: The plot shows the effect of a whistleblower protection statute on workplace fatalities (data from NSC) before and after adoption, based on Equation 2. Coefficients on the left side of the plot indicate leads, and coefficients on the right side indicate lags. Included in the regression but not displayed are coefficient terms for having passing a whistleblower protection statute six or more years in the past or six or more years in the future; see text for details. The dots represent the point estimates, and the vertical bars show 95 percent confidence intervals.

Figure D.6: Event Study: Effect of Workers' Compensation Statute on Workplace Fatalities (Data from NSC)



Note: The plot shows the effect of the workers' compensation protection statutes on workplace fatalities (data from NSC) before and after adoption, based on Equation 2. Coefficients on the left side of the plot indicate leads, and coefficients on the right side indicate lags. Included in the regression but not displayed are coefficient terms for having passed a workers' compensation statute six or more years in the past or six or more years in the future; see text for details. The dots represent the point estimates, and the vertical bars show 95 percent confidence intervals.

Figure D.7: The Effect of the Public Policy Exception on Workplace Injuries (Data from OSHA): Modifying the End Date of the Analysis



Note: This figure shows the coefficients from regressions mirroring the main specification (Table 2, Column 7), but with different end dates. The main analysis uses an end date of 2005, while this figure shows a range of end dates from 1992 to 2005. Each row indicates one regression, with the dot representing a point estimate for the coefficient on public policy exception, and the error bar representing a 95 percent confidence interval.

Table D.4: The Effect of the Public Policy Exception and Whistleblower Statute on Workplace Injuries (only federal OSHA)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	-0.137***			-0.121**	-0.115**	-0.080*	-0.097
	(0.047)			(0.047)	(0.044)	(0.044)	(0.066)
Whistleblower statute		-0.100**		-0.083**	-0.088**	-0.108**	-0.134**
		(0.036)		(0.039)	(0.034)	(0.041)	(0.064)
Workers' comp statute			-0.047	-0.014	0.014	0.086	0.019
			(0.046)	(0.036)	(0.029)	(0.065)	(0.102)
Good faith exception					-0.183***	-0.139*	-0.058
					(0.034)	(0.076)	(0.151)
Implied contract exception					-0.021	-0.024	0.014
					(0.042)	(0.061)	(0.091)
State unemployment rate					-0.020**	-0.030***	-0.022
					(0.009)	(0.011)	(0.014)
Democratic governor					0.009	-0.033	-0.019
					(0.024)	(0.025)	(0.029)
Prog. inspection rate					-0.018	0.012	0.018
					(0.032)	(0.044)	(0.039)
Observations	783	783	783	783	783	783	783
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

Note: These regressions follow Table 2 but limits the sample to states under federal OSHA. The dependent variable is the accident rate, measured as the natural log of the number of OSHA accident inspections divided by the state labor force (in thousands). The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state are in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.5: Donut Regression: The Effect of the Public Policy Exception on Workplace Injuries (Data from OSHA)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	-0.152**			-0.138**	-0.140***	-0.122**	-0.187**
	(0.059)			(0.058)	(0.050)	(0.050)	(0.071)
Whistleblower statute		-0.095**		-0.082*	-0.090**	-0.086*	-0.133**
		(0.042)		(0.041)	(0.036)	(0.044)	(0.062)
Workers' comp statute			-0.018	0.013	0.040	0.060	-0.014
			(0.050)	(0.036)	(0.025)	(0.051)	(0.072)
Good faith exception					-0.244***	-0.242***	-0.213*
					(0.053)	(0.056)	(0.114)
Implied contract exception					-0.011	-0.034	-0.027
					(0.048)	(0.066)	(0.092)
State unemployment rate					-0.032***	-0.032**	-0.016
					(0.011)	(0.014)	(0.018)
Democratic governor					-0.019	-0.033	-0.026
					(0.035)	(0.028)	(0.027)
Prog. inspection rate					0.007	0.040	-0.005
					(0.033)	(0.039)	(0.042)
Observations	967	967	967	967	967	967	967
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

Note: These regressions follow Table 2, but omit the state-year observations where the public policy exception is adopted, as well as the year before and after adoption. The dependent variable is the natural log of the number of OSHA accident inspections divided by the state labor force (in thousands). The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.6: The Effect of the Public Policy Exception Workplace Injuries: Manufacturing vs. Other Sectors (Data from OSHA)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	-0.129**			-0.107*	-0.103**	-0.066	-0.123*
	(0.058)			(0.058)	(0.051)	(0.053)	(0.064)
Whistleblower statute		-0.115**		-0.094*	-0.086*	-0.062	-0.137**
		(0.045)		(0.050)	(0.046)	(0.063)	(0.067)
Workers' comp statute			-0.065	-0.033	0.005	0.091	0.016
			(0.053)	(0.054)	(0.040)	(0.078)	(0.124)
Good faith exception					-0.297***	-0.268**	-0.091
					(0.074)	(0.107)	(0.152)
Implied contract exception					-0.021	-0.026	-0.006
					(0.060)	(0.073)	(0.095)
State unemployment rate					-0.040***	-0.050***	-0.036**
					(0.013)	(0.013)	(0.014)
Democratic governor					-0.024	-0.037	-0.022
					(0.045)	(0.034)	(0.029)
Prog. inspection rate					0.045	0.076	0.010
					(0.032)	(0.045)	(0.025)
Observations	2154	2154	2154	2154	2154	2154	2154
Sector-state and sector-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

Note: The dependent variable is the natural log of the number of OSHA accident inspections divided by the state labor force (in thousands), at the sector-state level, where sector is manufacturing or other. The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.7: The Effect of the Public Policy Exception and Whistleblower Statute on Workplace Injuries (Data from OSHA): Split by Manufacturing or Other

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public policy exception	-0.089	-0.170***			-0.033	-0.153***	-0.190*	-0.068
	(0.079)	(0.056)			(0.077)	(0.047)	(0.095)	(0.064)
Whistleblower statute			-0.162**	-0.070	-0.121	-0.064*	-0.190*	-0.088
			(0.074)	(0.042)	(0.078)	(0.033)	(0.099)	(0.059)
Workers' comp statute					-0.019	0.034	0.074	-0.030
					(0.084)	(0.034)	(0.245)	(0.082)
Good faith exception					-0.531***	-0.110**	-0.172	0.009
					(0.167)	(0.054)	(0.222)	(0.155)
Implied contract exception					-0.030	-0.008	-0.064	0.022
					(0.089)	(0.046)	(0.139)	(0.085)
State unemployment rate					-0.038**	-0.040***	-0.042	-0.027
					(0.018)	(0.012)	(0.026)	(0.016)
Democratic governor					-0.030	-0.021	-0.011	-0.033
					(0.066)	(0.031)	(0.050)	(0.030)
Prog. inspection rate					0.063	-0.007	-0.021	0.017
					(0.038)	(0.033)	(0.029)	(0.037)
Observations	1077	1077	1077	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes	Yes
Division-year FE	No	No	No	No	No	No	Yes	Yes
Sector	Manu.	Non-m.	Manu.	Non-m.	Manu.	Non-m.	Manu.	Non-m.

Note: These regressions follow Table 2 but split the sample into manufacturing and non-manufacturing, with injury data from OSHA. The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.8: Employment Entering Flexibly with Workplace Injuries (Data from OSHA)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	-0.139***			-0.126***	-0.119***	-0.100**	-0.114*
	(0.045)			(0.046)	(0.043)	(0.047)	(0.063)
Log of employment	0.975***	0.980***	0.939***	1.018***	0.906***	1.011***	0.418
	(0.207)	(0.228)	(0.224)	(0.213)	(0.212)	(0.287)	(0.613)
Whistleblower statute		-0.092**		-0.078*	-0.075**	-0.057	-0.121*
		(0.037)		(0.040)	(0.033)	(0.053)	(0.060)
Workers' comp statute			-0.029	-0.002	0.036	0.072	0.009
			(0.043)	(0.035)	(0.028)	(0.057)	(0.084)
Good faith exception					-0.193***	-0.166**	-0.101
					(0.037)	(0.080)	(0.145)
Implied contract exception					-0.033	-0.034	0.005
					(0.048)	(0.065)	(0.089)
State unemployment rate					-0.036***	-0.040***	-0.033**
					(0.011)	(0.012)	(0.015)
Democratic governor					-0.024	-0.037	-0.025
					(0.039)	(0.029)	(0.026)
Prog. inspection rate					0.016	0.054	-0.018
					(0.041)	(0.047)	(0.037)
Observations	1077	1077	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

Note: These regressions follow Table 2 but allow employment to enter into the regression flexibly, with injury data from OSHA. The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.9: Employment Entering Flexibly with Fatal Workplace Injuries (Data from NSC)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	-0.101*			-0.115**	-0.098*	-0.107**	-0.126**
	(0.058)			(0.051)	(0.058)	(0.052)	(0.048)
Log of employment	0.553*	0.513*	0.523*	0.543*	0.531*	0.794**	1.144
	(0.299)	(0.302)	(0.300)	(0.299)	(0.285)	(0.320)	(0.748)
Whistleblower statute		-0.078		-0.095	-0.086	-0.029	-0.070
		(0.059)		(0.061)	(0.054)	(0.046)	(0.050)
Workers' comp statute			-0.043	-0.037	-0.009	-0.003	0.113
			(0.071)	(0.057)	(0.053)	(0.063)	(0.079)
Good faith exception					-0.173	-0.007	-0.130
					(0.106)	(0.137)	(0.146)
Implied contract exception					-0.067	-0.051	-0.037
					(0.078)	(0.087)	(0.075)
Democratic governor					-0.079	0.008	0.009
					(0.052)	(0.032)	(0.027)
Prog. inspection rate					-0.024	-0.009	-0.012
					(0.023)	(0.016)	(0.014)
Observations	911	911	911	911	911	911	911
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

Note: These regressions follow Table 3 but allow employment to enter into the regression flexibly, with injury data from NSC. The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.10: The Effect of the Public Policy Exception and Whistleblower Statute on Workplace Injuries (Data from OSHA): Poisson Regression

	(1)	(2)	(3)	(4)	(5)	(6)
Accident rate (OSHA)						
Public policy exception	-0.139***			-0.132***	-0.104*	-0.104*
	(0.050)			(0.051)	(0.054)	(0.054)
Whistleblower statute		-0.074*		-0.064	-0.056	-0.056
		(0.042)		(0.041)	(0.039)	(0.039)
Workers' comp statute			-0.010	0.015	0.063**	0.063**
			(0.051)	(0.042)	(0.031)	(0.031)
Good faith exception					-0.235***	-0.235***
					(0.037)	(0.037)
Implied contract exception					-0.018	-0.018
					(0.045)	(0.045)
State unemployment rate					-0.320***	-0.320***
					(0.111)	(0.111)
Democratic governor					-0.024	-0.024
					(0.042)	(0.042)
Prog. inspection rate					-0.345	-0.345
					(0.386)	(0.386)
Observations	1077	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No
Division-year FE	No	No	No	No	No	Yes

Note: This table follows Table 2 but uses a Poisson regression rather than ordinary least squares. The dependent variable is the number of accidents, and employment enters as an exposure variable. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.11: The Effect of the Public Policy Exception and Whistleblower Statute on Workplace Injuries (Data from OSHA): Negative Binomial Regression

	(1)	(2)	(3)	(4)	(5)	(6)
Accident rate (OSHA)						
Public policy exception	-0.115*			-0.112*	-0.105*	-0.105*
	(0.059)			(0.059)	(0.054)	(0.054)
Whistleblower statute		-0.037		-0.031	-0.030	-0.030
		(0.040)		(0.042)	(0.038)	(0.038)
Workers' comp statute			0.005	0.009	0.043	0.043
			(0.052)	(0.050)	(0.042)	(0.042)
Good faith exception					-0.234***	-0.234***
					(0.039)	(0.039)
Implied contract exception					-0.025	-0.025
					(0.045)	(0.045)
State unemployment rate					-0.173***	-0.173***
					(0.061)	(0.061)
Democratic governor					-0.024	-0.024
					(0.034)	(0.034)
Prog. inspection rate					-0.106	-0.106
					(0.240)	(0.240)
Observations	1077	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No
Division-year FE	No	No	No	No	No	Yes

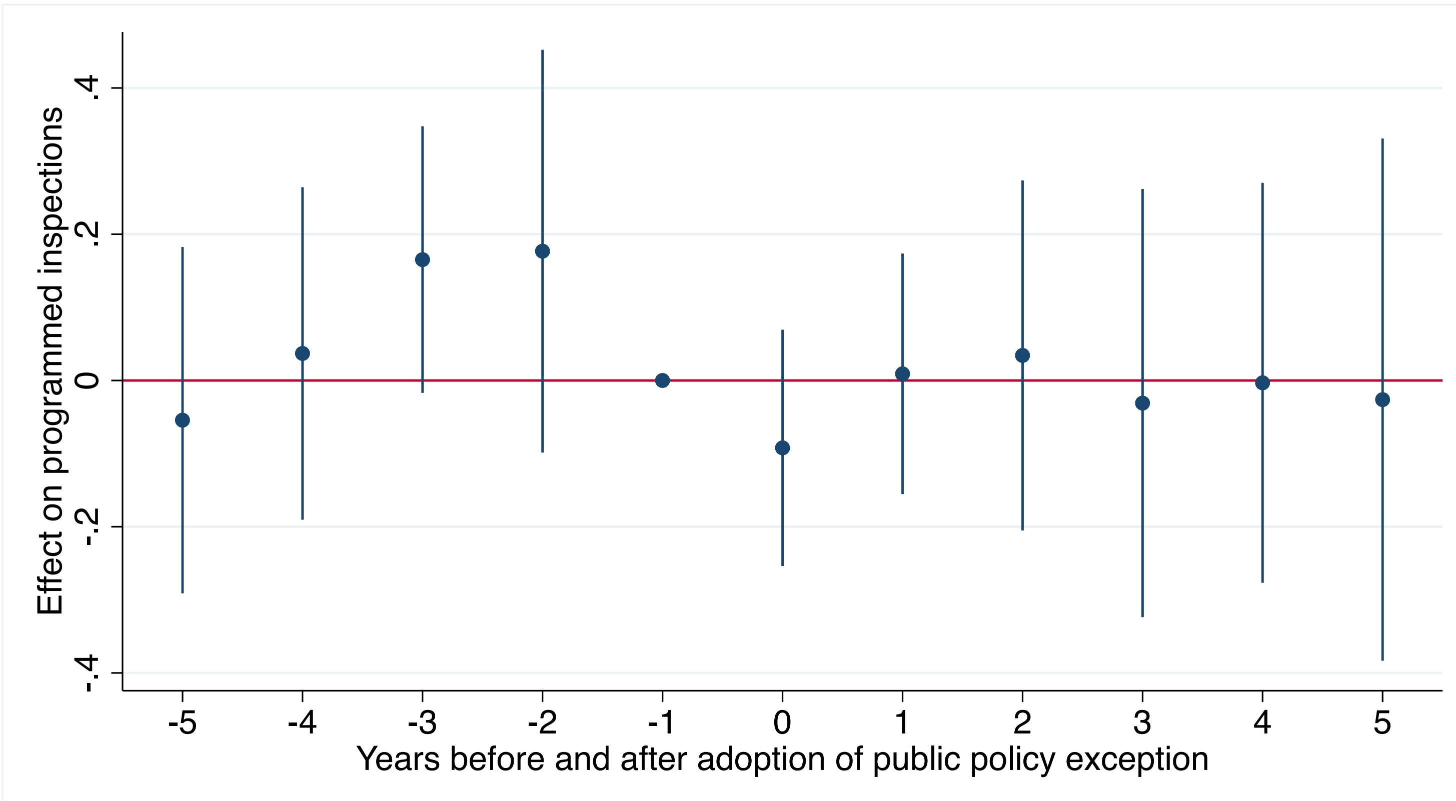
Note: This table follows Table 2 but uses a negative binomial regression. The dependent variable is the number of accidents, and employment is an exposure variable. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.12: The Effect of the Public Policy Exception and Whistleblower Statute on the Programmed Inspection Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	0.060			0.102	0.103	-0.039	-0.163
	(0.118)			(0.104)	(0.098)	(0.089)	(0.108)
Whistleblower statute		-0.223***		-0.244***	-0.226***	-0.121	-0.024
		(0.080)		(0.080)	(0.081)	(0.083)	(0.072)
Workers' comp statute			-0.011	0.053	0.046	0.286**	0.232*
			(0.109)	(0.101)	(0.094)	(0.110)	(0.128)
Good faith exception					-0.004	-0.037	-0.105
					(0.179)	(0.153)	(0.257)
Implied contract exception					-0.088	-0.271*	-0.273**
					(0.114)	(0.143)	(0.134)
State unemployment rate					0.044***	-0.007	0.065*
					(0.016)	(0.027)	(0.035)
Democratic governor					-0.008	-0.059	-0.047
					(0.045)	(0.047)	(0.041)
Observations	1077	1077	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

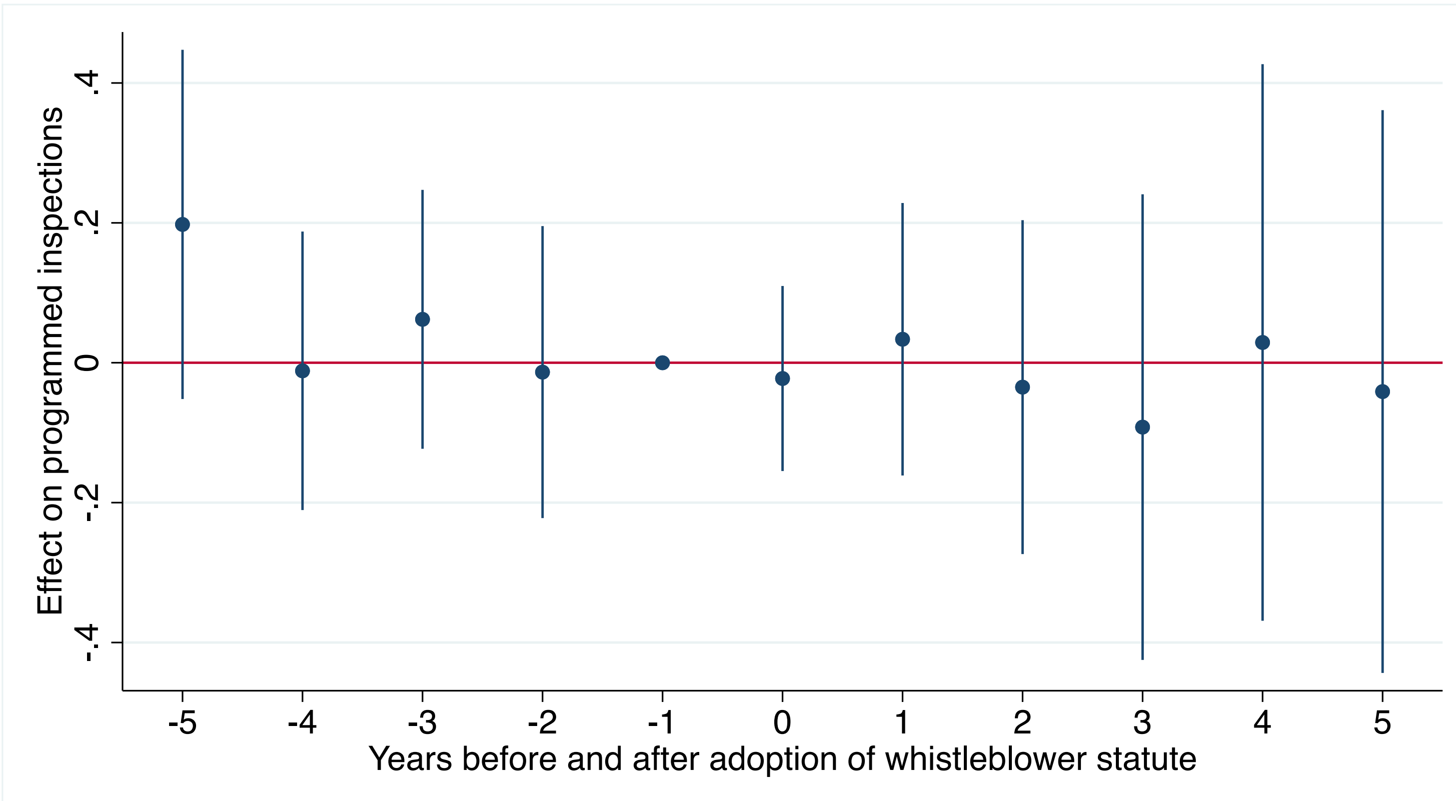
Note: These regressions follow Table 2, but use a dependent variable of the programmed inspection rate. The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state are in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Figure D.8: Event Study: Effect of the Public Policy Exception on the Programmed Inspection Rate (Data from OSHA)



Note: The plot shows the effect of the public policy exception on the programmed inspection rate (data from OSHA) before and after adoption. Coefficients on the left side of the plot indicate leads, and coefficients on the right side indicate lags. Included in the regression but not displayed are coefficient terms for having adopted the public policy exception six or more years in the past or six or more years in the future; see text for details. The dots represent the point estimates, and the vertical bars show 95 percent confidence intervals.

Figure D.9: Event Study: Effect of the Whisteblower Statute on the Programmed Inspection Rate (Data from OSHA)



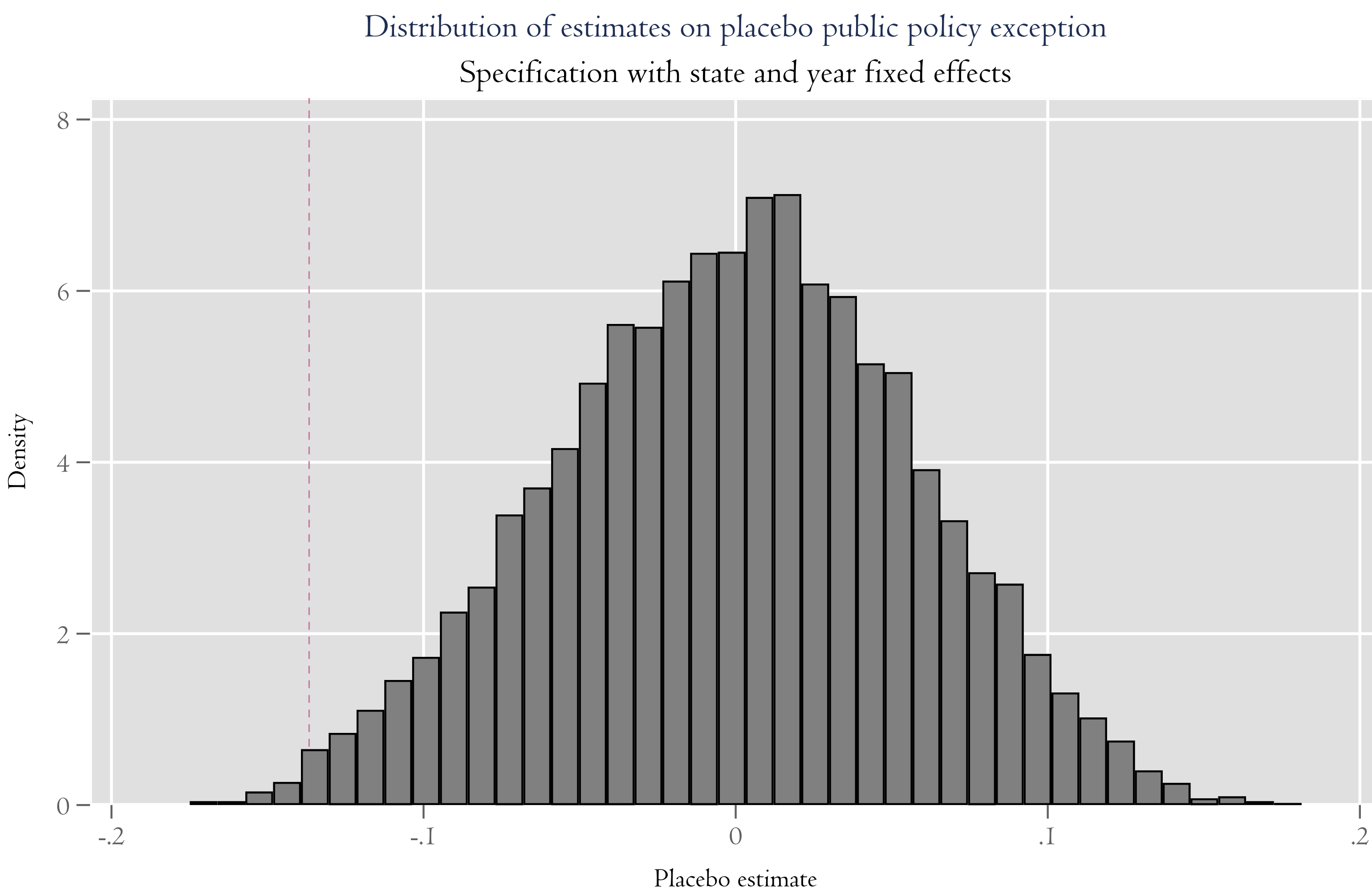
Note: The plot shows the effect of the whistleblower statute on the programmed inspection rate (data from OSHA) before and after adoption. Coefficients on the left side of the plot indicate leads, and coefficients on the right side indicate lags. Included in the regression but not displayed are coefficient terms for having adopted the public policy exception six or more years in the past or six or more years in the future; see text for details. The dots represent the point estimates, and the vertical bars show 95 percent confidence intervals.

Table D.13: Falsification Test: The Effect of the Public Policy Exception and Whistleblower Statute on Fatal Non-Workplace Injuries (Data from NSC)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public policy exception	0.051*			0.050*	0.040	0.034	-0.040
	(0.027)			(0.027)	(0.025)	(0.029)	(0.028)
Whistleblower statute		-0.014		-0.003	-0.002	-0.012	-0.013
		(0.026)		(0.023)	(0.025)	(0.053)	(0.034)
Workers' comp statute			-0.026	-0.026	-0.032	-0.037	-0.016
			(0.026)	(0.024)	(0.023)	(0.032)	(0.021)
Good faith exception					0.048	0.052	-0.056
					(0.034)	(0.063)	(0.049)
Implied contract exception					0.022	0.010	0.044
					(0.025)	(0.029)	(0.027)
Democratic governor					-0.026	0.006	0.019
					(0.016)	(0.020)	(0.017)
Prog. inspection rate					0.012	0.014*	0.005
					(0.007)	(0.008)	(0.006)
Observations	911	911	911	911	911	911	911
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes
Division-year FE	No	No	No	No	No	Yes	Yes

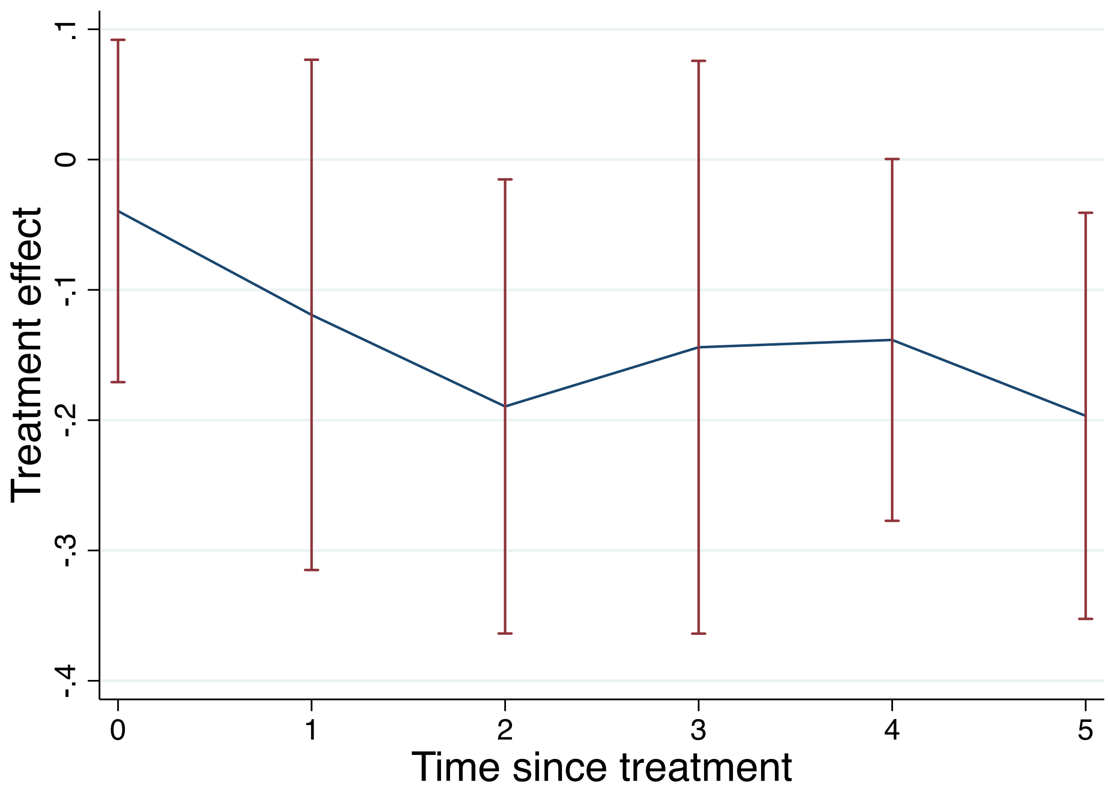
Note: These regressions follow Table 3, but the dependent variable is the natural log of of the number of non-workplace death rate divided by the state labor force (in thousands). The primary independent variables are dummy variables for whether a state has adopted the public policy exception or whistleblower or workers' compensation statutes. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Figure D.10: Randomization Inference on the Effect of the Public Policy Exception on Injuries



Note: The figure plots a histogram of the coefficient on a fake public policy exception across 10,000 iterations. In each iteration, we assign a random subset of states (without replacement) to adopt a placebo public policy exception in each year, with a probability that equals the share of states that actually adopted the exception in that year. We run a regression akin to Equation 1, except that we swap the placebo public policy exception in for the real one. We save the coefficient $\hat{\beta}_1$ from this regression. We repeat the process 10,000 times. The vertical dashed line represents the estimate of the effect on injuries of the actual public policy exception, obtained from Column 1 of Table 2.

Figure D.11: Event Study: Effect of the Public Policy Exception on Workplace Injuries with Heterogeneity-Robust Estimator (Data from OSHA)



The plot shows the effect of adoption of the public policy exception on workplace safety using a heterogeneity-robust estimator from de Chaisemartin and d'Haultfoeuille (2020). Coefficients show the effect size in the years after adoption of the public policy exception and are from a regression that includes state and division-year fixed effects and state-specific linear trends. Vertical bars show 95 percent confidence intervals.

Table D.14: Effect of the Public Policy Exception on Injuries: Two-way Fixed Effects versus Callaway and Sant'Anna (2021) Estimator

	TWFE			Group-Time ATT		
	(1)	(2)	(3)	(4)	(5)	(6)
Public Policy Exception	-0.137***	-0.112***	-0.125*	-0.190***	-0.162***	-0.160***
	(0.046)	(0.042)	(0.064)	(0.041)	(0.053)	(0.059)
Observations	1,077	1,077	1,077	1,077	1,077	1,077
State fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	Yes	No	No	Yes
Division-year fixed effect	No	Yes	Yes	No	Yes	Yes

Note: The dependent variable is the accident rate, measured as the natural log of the number of OSHA accident inspections divided by the state labor force (in thousands). The treatment is a dummy variable for whether a state has adopted the public policy exception. Column (1) under 'TWFE' reports the coefficient on a post-treatment dummy variable from a two-way fixed effects regression. Column (4) under "Group-Time ATT" reports the weighted average (by group size) group-time average treatment effects based on the method from Callaway and Sant'Anna (2021), where a group is defined by the year when the state is first treated. The estimates use the doubly robust estimator discussed in Callaway and Sant'Anna (2021). The additional control variables included in Columns 2, 3, 5, 6 are described in the text. Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.15: The Heterogenous Effect of Statutes with and without Punitive Damages on Workplace Injuries (Data from OSHA)

	(1)	(2)	(3)	(4)	(5)
Whistleblower, punitive	-0.142***		-0.142***	-0.220**	0.047
	(0.046)		(0.045)	(0.091)	(0.133)
Whistleblower, no punitive	-0.081**		-0.067	-0.080	-0.170**
	(0.037)		(0.043)	(0.062)	(0.073)
Workers' comp, punitive		-0.082**	-0.044	0.024	0.069
		(0.034)	(0.029)	(0.064)	(0.100)
Workers' comp, no punitive		0.047	0.032	0.059	-0.014
		(0.063)	(0.065)	(0.087)	(0.105)
Observations	1077	1077	1077	1077	1077
State FE	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	Yes
Division-year FE	No	No	No	Yes	Yes
F-test WB	0.016		0.000	0.116	0.100
F-test WC		0.035	0.292	0.737	0.535

Note: These regressions consider statutes with and without punitive damages. The dependent variable is the natural log of the number of OSHA accident inspections divided by the state labor force (in thousands). Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D.16: The Heterogenous Effect of Statutes with and without Punitive Damages on Workplace Injuries (Data from NSC)

	(1)	(2)	(3)
Workers' comp, punitive	-0.168***	-0.051	-0.062*
	(0.058)	(0.046)	(0.032)
Workers' comp, no punitive	-0.025	0.025	0.109
	(0.073)	(0.075)	(0.079)
Observations	911	911	911
State FE	Yes	Yes	Yes
State trend	No	No	Yes
Division-year FE	No	Yes	Yes
F-test WC	0.124	0.399	0.050

Note: These regressions consider statutes with and without punitive damages. The dependent variable is the natural log of workplace deaths divided by the state labor force (in thousands). Regressions are weighted by state employment in the first year of the sample, with robust standard errors clustered by state in parentheses. * $p < .1$. ** $p < .05$. *** $p < .01$.

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