Asymmetric Effects on Fatality Rates of Changes in Workers' Compensation Laws

Elissa Philip Gentry
Florida State University

W. Kip Viscusi

Asymmetric Effects on Fatality Rates of Changes in Workers' Compensation Laws

Elissa Philip Gentry, *Institute for Advanced Study in Toulouse, University of Toulouse Capitole, and Florida State University College of Law*, and W. Kip Viscusi, *Vanderbilt University*

Send correspondence to: W. Kip Viscusi, University Distinguished Professor of Law, Economics, and Management, Vanderbilt University, 131 21st Avenue South, Nashville, TN 37203, USA; Tel: 615-343-7715; Fax: 615-322-6631; Email: kip.viscusi@vanderbilt.edu.

With irreversible investments in safety, changes in workers' compensation laws should affect employer incentives asymmetrically: increases in workers' compensation generosity should cause employers to invest more in safety, but comparable decreases might not cause them to disinvest in existing precautionary programs or equipment. Although maximum weekly benefits caps have been fairly stable, state laws have expanded or restricted workers' compensation on multiple other dimensions. State laws may impose new requirements regarding burdens of proof, access to medical care, and the duration of benefits. This article estimates the effect of changes in these more comprehensive measures of workers' compensation laws on workplace safety. Using confidential, restricted data from the Census of Fatal Occupational Injuries, the article finds that increases in workers' compensation generosity lead to a significant decrease in fatality rates, while decreases in workers' compensation generosity do not significantly increase fatality rates. (*JEL*: I18, J28,81,83, K32)

1. Introduction

In the inherent conflict between employer and worker interests, workers' compensation provides a compromise: employers guarantee compensation

Support from ANR-Labex IAST is gratefully acknowledged.

American Law and Economics Review
doi:10.1093/aler/ahz007
Advance Access publication on July 11, 2019

for injured workers in exchange for workers waiving their right to sue the employer after an injury. Given the difficulties and costs involved in initiating a lawsuit against a company and the potential adverse effects on the employment relationship, this compromise ideally compensates workers for damages stemming from job-related injuries with minimal cost. Workers' compensation, however, also serves an additional function: given the no-fault payout for injury and the linkage of workers' compensation premiums to firms' accident histories, workers' compensation benefits provide an incentive for companies to create safer workplaces.

Each state can adjust its workers' compensation policies to influence both access to benefits and the level of payments. A prominent policy component is the maximum weekly wage replacement cap, which provides an upper limit on an injured worker's monetary recovery. Other policy components have played a less prominent role in previous empirical assessments of workers' compensation but also may significantly affect a worker's ability to become, or remain, eligible for workers' compensation. Such policies—including medical examination requirements, time limits for benefits, coverage of attorneys' fees, and death benefits—may also influence firms' incentives for safety by limiting the number and duration of workers' compensation claims and, in turn, program costs. This article incorporates these nonmonetary measures of workers' compensation generosity by state in an empirical analysis to estimate their effect on workplace safety.

A principal concern in this article is the asymmetric effect on safety in response to workers' compensation reforms. The structure and economic impact of workers' compensation for firms' safety-related investments is similar to that of other safety policies (Viscusi, 1983). With downward irreversibility in firms' investments related to safety, there will be a greater response to increases in financial incentives for safety than comparable decreases. While increases in workers' compensation benefits might cause firms to invest in capital-intensive safety initiatives (e.g., buying newer equipment or investing in machinery or a protocol to ensure safe practices), their ability to relinquish this safety capital in response to decreases in such benefits is constrained, at least in the short run. Moreover, as workers grow accustomed to their current level of safety, the existence of an endowment effect in safety would make decreasing safety levels difficult for employers. There consequently should be an asymmetric effect of workers' compensation policy changes on fatality rates.

Using confidential, state-level microdata from the Census of Fatal Occupational Injuries (CFOI) that was accessed on-site at the U.S. Bureau of Labor Statistics (BLS), this article examines the effect of changes in states' workers' compensation policies on worker fatality rates. Section 2 discusses the relationship of our contribution to existing work regarding the effect of workers' compensation on safety, Section 3 outlines the methodology used in the article, and Section 4 discusses the empirical strategy. Section 5 presents the results, which document the asymmetric effect of changes in workers' compensation on fatality rates, consistent with economic models that posit downward irreversibility in safety-related investments. The conclusion in Section 6 also observes that components of workers' compensation laws other than the state maximum benefit cap have an important role in determining the resulting level of worker safety.

2. The Asymmetric Effect of Workers' Compensation Changes on Safety

This article focuses on the relationship between workers' compensation provisions and workplace safety. While workplace safety is directly regulated by a number of state and federal agencies, such as the Occupational Safety and Health Act or the Mine Safety and Health Administration (Morantz, 2009, 2011), programs such as workers' compensation[1] can indirectly provide incentives for firms to reduce accident risk in two ways: (1) any actual payout by firms for injury costs provides an incentive to decrease worker injuries, and (2) the role of experience rating linking firm safety to workers' compensation insurance premiums affects the level of disincentive.

While the first effect is relatively self-explanatory, the second is a bit more nuanced. Firms often purchase workers' compensation insurance for

1. Not all firms participate in workers' compensation programs. Morantz (2010) discusses the features of multi-state firms that opt out of the workers' compensation program in Texas. While there might be differences in reaction based on this lower level of participation in Texas, Butler (1996) found no significant difference in fatality rates in most industries between firms subscribing to the workers' compensation program and those that did not.

worker injury claims. In offering such insurance policies, insurance companies adjust premiums based not only on the firm's industry but on an individual firm accident history, a process called "experience rating." The degree to which the accident record affects experience rating varies based on the size of the firm. For small- and medium-sized firms, insurance companies find it difficult to perfectly experience rate due to the high variance in injury rates. Larger firms are more likely to be more accurately experience-rated, given that random fluctuations in safety rates will be small. Due to this closer connection between insurance premiums and accident records, large firms have more of an incentive to keep their injury rates low. Prior work has found evidence of this inverted relationship between size and sensitivity to policy changes: Ruser (1985, 1991) finds that while increases in workers' compensation benefits generally increase nonfatal injury rates, this relationship is smaller for larger firms, while Moore and Viscusi (1989) find that the deterrent effect of workers' compensation for worker fatalities is greater for large firms. Accordingly, our analysis controls for establishment size.

A large body of work, well summarized in Butler et al. (2013), has addressed the role of workers' compensation in incentivizing safety, largely with respect to nonfatal injuries (Chelius, 1982; Butler and Worrall, 1983; Butler and Worrall, 1985; Ruser, 1985; Chelius and Smith, 1987; Krueger, 1988). As Ruser and Butler (2010) suggest, the theoretical effect of increases in workers' compensation benefits is ambiguous: while workers' compensation provides incentives for companies to enhance safety, it also decreases incentives for workers to take care. Additionally, a more generous workers' compensation program might also provide incentives for workers to falsely report workplace injuries or claim recreational injuries as workplace injuries. Prior studies have examined benefit elasticities—the increase in claim frequency in response to an increase in expected benefits—to understand whose incentives are dominant, the firm's or the workers'. The resulting evidence has been mixed: some studies find significantly positive frequency elasticities (Butler and Worrall, 1983), and others find elasticities closer to 0 (Guo and Burton, 2010; Bronchetti and McInerney, 2012).

Using nonfatal injuries as measures of safety, however, exacerbates some of the theoretical ambiguities noted above: (1) workers may make false claims or claim compensation for nonwork injuries; (2) workers may exaggerate claims; and (3) workers may take less care in anticipation of more generous benefits. Evidence of these effects has been mixed and often hard to disentangle. Butler and Worrall (1991) address this issue by separately estimating "claims reporting" moral hazard (workers making false claims) from "risk-bearing" moral hazard (workers taking less care); they find that the effect of claims reporting moral hazard more than offsets the risk-bearing moral hazard. Smith (1990) suggests that the existence of a "Monday effect" would provide evidence of false claims. Reasoning that some nonwork injuries could be more easily passed off as work injuries than others (i.e., strains vs. cuts), Smith hypothesizes, and finds evidence, that those easy-to-conceal injuries are more likely to be reported on Mondays or days following long weekends. Much work has attempted to revisit and understand this Monday effect. By exploiting variation in worker access to insurance, Card and McCall (1996) challenge this claim, finding that workers with low insurance coverage were no more likely to report injuries on Monday than workers with a higher level of coverage. Ruser (1998) finds that while an increase in benefits increases the likelihood of reporting any injury on a Monday, it does not increase the likelihood that a worker reports a back sprain, relative to reporting a fracture or cut. Butler et al. (2014) test multiple theories to explain the Monday effect: they do not find evidence that the higher reporting of injuries on Monday varies based on the availability of health insurance (i.e., "economic" factors). They similarly find no evidence supporting the notion that the additional injuries result from coming in to work "cold" on a Monday (i.e., "ergonomic" factors). Instead, they attribute the effect to "work aversion," the differential sensitivity to injuries occurring because a worker does not want to come back to work. Other research has concentrated on the decision to file a claim (Biddle and Roberts, 2003; Lakdawalla et al., 2007) and the effect of benefit increases on claim duration (Neuhauser and Raphael, 2004; Ruser et al., 2004).

Analyzing the effect of workers' compensation on fatality rates alleviates concerns regarding false claims, exaggerated claims, and moral hazard.[2]

2. Fatality rates, however, do not capture all safety-related effects of workers' compensation. For example, since fatalities tend to arise from the most severe injuries, we may see the effect of a workers' compensation reform on only the most severe cases.

Since fatalities are well-documented events, there is low chance for false reporting; similarly, workers are unlikely to exaggerate a fatality. Moreover, the injuries that have a potential to lead to fatality are likely to involve higher health cost stakes than other types of injuries. This potential severity makes it likely that workers will be less sensitive to the generosity of workers' compensation benefits when choosing their level of safety-related care. Moore and Viscusi's (1989) study on the effect of workers' compensation on fatality rates found that absent workers' compensation, fatality rates would have increased by over 20%. Similarly, Ruser (1993) compares the effect of workers' compensation on fatality rates to its effect on the frequencies of injuries requiring nonlost-workdays, restricted workdays, and days away from work. Ruser finds that workers' compensation benefits increased the frequencies of nonfatal injuries but reduced the frequencies of fatalities.

In recent years, states have largely standardized certain measures of workers' compensation: most states pay a maximum weekly rate of two-thirds of wage rate, subject to some cap. The maximum weekly compensation rate, however, is only one aspect of workers' compensation generosity. Other aspects involve the legal standards and medical regulations governing the ability to receive benefits. Guo and Burton (2010), in studying the periods of 1975–1989 and 1990–1999, incorporate some measure of nonmonetary workers' compensation policy by examining the effect of changing compensability rules on injury rates. Additionally, they calculate a "benefit allowance stringency" rate—which measures how many injured workers do not qualify for workers' compensation benefits—and permanent partial disability benefit share. This article addresses such considerations by looking at other measures of nonmonetary changes to workers' compensation regimes.

Such analysis is particularly relevant for studying the current landscape of workers' compensation. The Department of Labor (2016) has suggested that some states may have continued to engage in a "race to the bottom," competing to cut workers' compensation costs to attract businesses to the

---

If a workers' compensation reform results in fewer fatalities but more nonfatal injuries (of various severities), focusing on fatalities alone would mask the nonfatal injury effects. More generally, patterns of fatal and nonfatal risks not only may differ across states but may be negatively correlated (Mendeloff and Burns, 2013). Mendeloff and Burns conclude that fatality rates are a better measure of the effects of policies on the level of workplace safety.

state. Grabell and Berkes (2015) similarly discuss the prevalence of barriers restricting access to benefits, attributing them in part to the 2001 and 2007–2009 recessions. Both sources suggest a host of new techniques used to reduce worker access to benefits: increasing burdens of proof for injury, instituting caps on medical payments, limiting duration of benefits, restricting medical treatment options, and allowing care to be denied by doctors who have not seen the patient in person. These measures would not be captured in the weekly maximum benefits cap but nonetheless affect how easily workers can receive compensation for their injuries and, in turn, how costly workplace accidents are for a company.

While previous work on claim frequency has found a relatively symmetric response to benefit increases or decreases (Butler et al., 1997), we examine the symmetry of effects on fatality risks. In response to greater workers' compensation benefits, firms may invest in safer workplaces by purchasing newer equipment with better safeguards. Firms may also establish, or expand, departments dedicated to guaranteeing compliance with safety protocol.[3] A decrease in such benefits, however, might not cause a firm to divest itself of these safety measures immediately. First, insofar as the new equipment is capital, it might be impossible or costly to get rid of in the short run. Moreover, as technology advances, there might be less ability to switch back to less safe equipment. Second, there might be an endowment effect in safety (Kahneman et al., 1991); once given safety-enhancing materials, workers might be reticent to allow changes that make workplaces more dangerous. Depending on how active the union is, or how competitive the industry, this preference might be important to firms. Even in nonunionized contexts, the endowment effect aspect has general cost ramifications in terms of the compensating differentials that firms must pay workers to incur risks. Accordingly, we expect firms' workplace safety to increase with greater workers' compensation protections but not necessarily decrease with less protection.

---

3. Firms can take many other actions to create a culture of safety. Such measures include encouraging workers to report near accidents (Atkins Energy America), creating safety teams consisting of both management and employees (ACCO Brands), and conducting regular safety audits, both internal and external. https://www.ehstoday.com/americas-safest-companies-awards/2017-america-s-safest-companies.

Given these considerations, this article seeks to measure the sensitivity of workplace safety to traditional workers' compensation measures such as weekly wage replacement maximums as well as other legal changes that affect generosity of the state provisions. It uses these measures to determine the current relationship between fatalities and workers' compensation requirements, as in Moore and Viscusi (1989).

3. Empirical Strategy

This article uses difference-in-differences estimation to determine the effect of changes in workers' compensation policy. Our main specification is a log rate model using periods corresponding to policy years 2003 and 2006. As noted below, we use log(rate + 1) as our dependent variable to accommodate fatality rates that have the value of zero. While this is our main specification, we implement several different empirical strategies throughout the article, including count data models, to check the assumptions underlying this analysis.

The variables of interest are *Increase* and *Decrease*, which indicate whether workers' compensation generosity of a given state increased or decreased relative to its level in the previous period. *Increase* takes on a value of 1 if states increased the generosity of workers' compensation and a value of 0 otherwise. *Decrease* is defined analogously for decreases in workers' compensation. Section 4.2.2 provides a detailed description of the procedure used for assessing increases and decreases.

Maximum Benefits is the maximum weekly workers' compensation cap in a state each period. The main specification examines the effect of a general increase or decrease in workers' compensation generosity:

$$\begin{aligned}\mathrm{Ln}(FatalityRate_{sjtz} + 1) = Size^{'}\alpha + \beta_1 Increase_{zt}\\ + \beta_2 Decrease_{zt} + \beta_3 MaximumBenefits_{zt}\\ + \delta_t + \gamma_j + \theta_z + \varepsilon \qquad (1)\end{aligned}$$

for s enterprise size, t periods, j industry, and z state (with δ, γ, and θ as fixed effects).

To control for time-invariant unobservables by state, we include state fixed effects; however, including state fixed effects is a demanding restriction. This is particularly true for *Maximum Benefits* since, as noted above, this variable varies little across time, with very few changes over the sample period.

Any increase in legally mandated workers' compensation coverage should cause a firm to invest more heavily in workplace safety, as injuries become marginally more expensive. As noted above, however, this effect may not be symmetric since investments in safety are not necessarily easily reversible. This suggests that

$$\beta_1 < 0 \quad \text{and} \quad \beta_2 \geq 0. \tag{2}$$

Moreover, higher weekly compensation caps would increase firms' potential monetary obligation if a worker got hurt, incentivizing firms to invest in more safety. Accordingly, we should find that:

$$\beta_3 < 0. \tag{3}$$

Similarly, based on the link between imperfect experience rating and establishment size, we expect larger firms to be more responsive to experience rating. This should result in relatively lower fatality rates for large firms. Since we omit the intermediate establishment group with 20–99 employees as our comparison group, we expect that

$$\alpha_{0-19} > 0, \text{ and } \alpha_{100+} < 0. \tag{4}$$

Given that our variables of interest are policy-based, there are several concerns we must address. A potential concern is that of reverse causality, such that low fatality rates cause more generous workers' compensation regimes. In order to account for this potential influence, we focus on policy changes from the year 2003 to 2006, allowing for comparison of fatality rates from 2003–2005 and 2006–2008. This focus on 3- and 2-year averages of fatality rates following distinct policy years helps to disentangle temporally this effect. In addition to ruling out reverse causality, inclusion of multi-year averages allows us to account for lagged influence of a policy change.[4]

4. Moreover, for some specifications, fatality rates can have very thin cells such that taking averages of fatality rates for given industry-state-size combinations provides a more stable comparison.

As a robustness check, however, we also include a year-by-year analysis. This year-by-year analysis allows us to run a falsification test, using the lagged fatality rate as the dependent variable. If lower fatality rates cause more generous benefits, *Increase* should be significantly negative in these specifications.

Alternatively, there may be concerns that our hypothesized negative relationship between fatality rates and worker's compensation benefits could be a function of a large scale workplace accident event that increases the fatality rate and prompts workers' compensation reforms, after which the fatality rate returns to its long-term average. Such an event might involve multiple deaths/injuries or produce considerable press coverage. For example, the BP *Deepwater Horizon* explosion occurred in April 2010, prompting seemingly endless press coverage. In addition to causing considerable environmental damage, the explosion on the drilling rig itself endangered numerous crew members. Eleven workers were never found and presumed dead; seventeen sustained injuries (Pallardy, 2018). Similarly, in 2005, the BP chemical refinery explosion in Texas killed 15 workers and injured 180 (Reuters, 2011). If workers' compensation generosity in the affected states increased in response to such significant work accidents without any corresponding change in investment on companies' parts, the negative effect of *Increase* might then just be an artifact of regression to the mean.

We find this to be unlikely for the following reasons. First, given that the effect is identified by multiple states in each period, the likelihood that an idiosyncratic event drives the results is lower. Similarly, the lagged fatality rate robustness check provides a check on this relationship. If higher fatality rates caused more generous benefits, we would expect that the coefficient on *Increase* would be significantly positive in the lagged fatality rate regressions. Finally, and most importantly, workers' compensation laws do not seem to adapt rapidly to such focal events. Even the *Deepwater Horizon* explosion, one of the most prominent employment disasters, did not result in increases in workers' compensation generosity in the affected states after 2010, according to Table 1.

Finally, there may be concerns that the presence of an omitted variable that influences both state policy and worker safety might cause a negative effect between *Increase* and *Fatality Rate*. A plausible variable of this type might be demographic characteristics of a state, such as political affiliation, which might lead to both unionization (a proxy for worker safety) and regulatory reform. However, the inclusion of state fixed effects should control for the effect of such time-invariant effects on fatality rates. As a robustness

Table 1. States Whose Workers' Compensation Protections Increased or Decreased*

	Increase	Decrease
2004	New Jersey	California
	Rhode Island	Georgia
		Iowa
		Tennessee
		Vermont
2005	Illinois	Alaska
		Idaho
		Missouri
		North Dakota
		Oklahoma
		Texas
2006	Connecticut	Indiana
	Georgia	Ohio
	Wisconsin	South Carolina
2007	Arizona	Arkansas
	Georgia	Delaware
	Rhode Island	Montana
		Nebraska
		New York
		South Carolina
2008	Minnesota	North Dakota
	Wisconsin	
2009	Nevada	
	New Hampshire	
	North Dakota	
	Oregon	
	South Dakota	
	Utah	
	Wyoming	
2010	Alaska	New York
	Colorado	Oklahoma
	South Dakota	Rhode Island
2011		Illinois
		Kansas
		Michigan
		Montana
		North Carolina
		North Dakota

Table 1. (Continued)

	Increase	Decrease
		Oklahoma
		Washington
2012	Wisconsin	California
		Louisiana
		Mississippi
2013	Minnesota	Georgia
	Missouri	Indiana
	Rhode Island	Kansas
		North Carolina
		North Dakota
		Oklahoma
		South Dakota
		Tennessee
2014	Alabama	Alaska
	Colorado	Connecticut
	Illinois	Delaware
		North Dakota

Notes: *The following states are not coded as changing generosity because the reforms only affected specific industries or subgroups and are accordingly not reflected in Table 1: Arizona (in 2013), California (in 2013), and Massachusetts (in 2014). These omissions do not affect our analysis, as they occur after our analysis time frame.

check, we also include state-specific time trends in our single-year regressions. This is a very demanding test of the data; however, the results are included for completeness.

Given the above methodology, the next two sections discuss how we construct the principal variables of interest: the fatality rates and the workers' compensation generosity change variables.

4. Data

4.1. Fatality Rate Construction

Our analysis of the effect of workers' compensation focuses on the fatality rate, which is the most common measure used in the literature. We also explore the robustness of our results using as the risk measure the count of the number of fatalities. Each of these variables utilizes state-level data from the CFOI. The BLS creates this census of occupational fatalities, verifying each fatality using multiple sources. For this reason, CFOI data are considered the gold standard for measuring occupational fatalities (Viscusi, 2013).

While some broad fatality rates are public, we used confidential microdata to construct establishment size-industry-state fatality rates with which to measure workplace safety. These data are difficult to obtain. A confidential data agreement between the BLS and the researcher's institution provides off-site access to most variables such as the deceased worker's characteristics, industry, and establishment size. However, some information, including the state in which the injury occurred, can only be accessed on-site at the BLS, for which additional permissions and reviews are required. Knowledge of the state in which the fatality occurred is essential to being able to assess the impact of state workers' compensation laws on safety. Using these on-site data, we construct fatality rates that vary by establishment size,[5] industry, and state of injury.

These fatality rates are constructed by dividing fatality numerators from the CFOI data by employment denominators from the Current Population Survey (CPS) Merged Outgoing Rotation Groups (MORG). Both the numerator and denominator only count fatalities and employment for nonmilitary workers between ages 16 and 64.

Our main specification uses data from the CFOI and MORG for years 2003–2008. We prefer this specification because the industry and occupation classification codes are stable during this period: for the CFOI data, the industry was classified according to 2002 North American Industry Classification System (NAICS) and occupation according to 2000 Standard Occupational Classification (SOC).[6] The MORG data during this period is classified according to 2000 Census codes in both industry and occupation. NAICS-Census and SOC-Census crosswalks are used to match the fatality and employment data.

As an extension, we incorporate data from 2009 to 2014. In order to make use of later years, we have to make further adjustments. For CFOI

5. We would ideally like to control for firm size but, as in the analysis by Ruser (1991), the CFOI data only have establishment size.

6. The BLS notes that CFOI data was classified in a drastically new way in 2003. The BLS considers this coding change as a break in the sample, making it ill-advised to include 2002 data.

data, years 2009–2013 are coded according to NAICS 2007, and year 2014 is coded according to NAICS 2012. According to the MORG labeling files,[7] years 2003–2014 have the same industry Census codes. First, we use crosswalks between NAICS 2002 to 2007 and NAICS 2002 to 2012 to categorize CFOI data into NAICS 2002 codes.[8] We then use our usual NAICS-Census codes crosswalks to categorize industries into 11 industry categories.

To construct the numerators of these fatality rates, the number of fatalities are summed by establishment size, industry, state, and period. Firms are separated into three establishment size categories: 0–19 employees, 20–99 employees, and 100 or more employees. The omitted category in our subsequent regressions is 20–99 employees. These categories were chosen based on existing BLS categories for establishment size. Similarly, fatalities are separated into 11 industries[9] and 50 states (excluding the District of Columbia). Rates are constructed for two periods: for years 2003–2005 and 2006–2008. Because 3-year rates may undermine sensitivity to policy changes within the range, we also report robustness checks using 2-year rate averages.

Denominators for employment are drawn from the CPS MORG data using the general procedure described in Gentry and Viscusi (2016). In particular, we construct hours-based denominators by multiplying the average number of hours worked annually in each industry-state-year category by

7. Available at http://www.nber.org/morg/sources/labels/labels14.do.

8. When one 2007 or 2012 NAICS code is attached to multiple 2002 NAICS codes, we probabilistically assign the code to one 2002 NAICS code. This generally does not matter because we consider industry categories at a more aggregated level.

9. The following industries are included: (1) mining; (2) construction; (3) manufacturing; (4) wholesale/retail trade; (5) transportation and warehousing and utilities; (6) information; (7) finance and insurance, real estate, rental and leasing; (8) professional, scientific, and technical services; management; and administrative, support, and waste management services; (9) educational services and health care and social assistance; (10) arts, entertainment, and recreation and accommodation and food services; and (11) other services (except public administration). We do not consider NAICS code 11 (agriculture, forestry, fishing and hunting) because states often exempt agricultural industries from workers' compensation regulations.

the number of employees in each category.[10] The BLS moved to such hours-based rates after 2007, reasoning that this measure better accounts for the time that a worker is exposed to occupational risk.[11]

Unlike the CFOI fatality data, the MORG data does not collect information on the establishment size where employees work. To delineate denominators by establishment size, data from the BLS[12] are used to calculate the proportion[13] of employment in a state, year, and industry for each establishment size. The proportion of workers in each state-industry-year category employed by various establishment sizes is estimated by calculating the first month employment levels for each establishment size as a proportion of total employment in a given state, year, and industry. These proportions are then multiplied by MORG data to get employment estimates for each establishment size within each state, industry, and year. These employment estimates, which vary by state, period, industry, and establishment size, are the denominators for the fatality rates.

Fatality counts are then divided by these denominators to produce state, industry, and establishment size-specific fatality rates. The 3-year (and 2-year) averages of fatality rates were created by dividing the sum of the 3 (2) yearly numerators by the sum of the 3 (2) yearly denominators. Finally, these rates are multiplied by 200,000,000 to standardize fatality rates to the equivalent of fatalities per 100,000 full-time workers with an average work year of 2,000 hours.

10. We calculate this by calculating the average weekly hours worked in each year-state-industry category. For categories with missing average weekly hours (around 0.1% of the sample), we impute the average weekly hours over all industries, states, and years. We multiply the average weekly hours by 50 for an average annual hour estimate.

11. This procedure is broadly outlined in Northwood (2010). As in Gentry and Viscusi (2016), we did not implement all of Northwood's restrictions; however, her outlined procedure is informative.

12. The data are from the Quarterly Census of Employment and Wages for years 2003–2014, https://www.bls.gov/cew/datatoc.htm.

13. For missing values of monthly employment, we impute the value by multiplying the number of establishments by an imputed measure of the number of employees. For most of the size classes, we impute the midpoint of the establishment size class. For the largest size class, 1,000 employees or more, we impute the number of employees as 2,000.

4.2. Measures of Workers' Compensation Generosity

4.2.1. *Maximum weekly compensation cap.* Two measures of workers' compensation packages serve to assess the effect of workers' compensation on fatality rates. First, the analysis includes the maximum weekly workers' compensation cap. These values are taken from the Social Security Administration website and is measured by state for years 2003 and 2006.[14] This measure reflects the maximum workers' compensation benefit a worker can receive by date of injury. The maximum benefits cap has largely stabilized within states; accordingly, the important variation occurs across states. Variation over time is minimal and mostly attributable to inflation and cost-of-living adjustments.

4.2.2. *Comprehensive workers' compensation measure.* The second measure of workers' compensation provisions involves compensation restrictions that might not be reflected by the maximum weekly compensation ceiling. ProPublica provides a measure of workers' compensation generosity over time.[15] ProPublica assigns an ordinal generosity level (ranging between 1 and 20) to each state in 2002 and then tracks changes in generosity over time.[16] Many states can be (and generally are) assigned the same level of generosity at any given time. These measures incorporate information from the National Council on Compensation Insurance Annual Statistical Bulletin, reports from the Workers Compensation Research Institute, and stakeholder interviews.

---

14. This information is taken from https://secure.ssa.gov/poms.nsf/lnx/0452150045. When multiple values are given based on date of injury, we choose the later date of injury, to reflect current costs of injury. When multiple changes are made over a year, the values are averaged. For the extended sample, we extract the maximum rate for 2003, 2006, 2009, and 2012.

15. This information is from Grabell and Berkes (2015) and Qiu and Grabell (2015).

16. To create the initial ranking, ProPublica used rankings of average statutory benefits from Actuarial & Technical Solutions of Bohemia, N.Y., and a measure of the reforms suggested by a 1972 presidential commission on workers' compensation that were adopted by each state. The changes in rank were calculated by examining state reforms in conjunction with research on the effect of such reforms on predicted benefit payments and the significance of the change given historical norms. Further explanation is available at http://projects.propublica.org/graphics/workers-comp-reform-by-state.

While the measures are based in part on confidential and often proprietary data, making it impossible to fully recreate, we submit it to a number of tests to ensure accuracy and unbiasedness. First, we verified the passage of a random sample of roughly half the reported reforms. We did this by searching for contemporaneous press announcements detailing the reform's passage or by directly examining the new or modified statutes promulgating the reforms. Moreover, for the reforms from 2004 to 2006—those underlying our main specification—we manually code each itemized reform description as intuitively either increasing or decreasing workers' compensation generosity. We then created an "average effect" of all reforms passed within a state-year combination and compared these effects to the changes inferred by ProPublica's actual ranking changes. We found that out of 20 reported state-year changes, 18 of our computed "average effects" were in the same direction as ProPublica's reported change.[17] That is, under an assumption that each itemized result produces equal effects on workers' compensation generosity,[18] our recategorization would result in the same treatment variables.

Given the lack of cardinal interpretation of the 1–20 ranking, we exercise caution in using this data to capture the magnitude of policy changes. We do not consider the actual level for each state at any given period. Instead, we measure the direction of changes in level for each state relative to the previous period. This approach captures the relevant direction of policy changes but does not assign cardinal significance to the initial ranking. We then aggregate this rank change into two binary variables indicating whether the state generosity increased or decreased relative to the previous period, leading to the variables *Increase* and *Decrease*, respectively. Given that for any period, most states do not change ranks, the omitted category consists of states whose coverage did not change in this period. Since we consider

17. The two that did not match were Alaska in 2005 and Illinois in 2005. We computed the same number of "positive" changes as negative changes in these years, resulting in an average effect of 0. ProPublica coded Alaska as decreasing in generosity in 2005 and Illinois as increasing, resulting in a symmetric discrepancy.

18. This is clearly not an accurate assumption; however, this assumption does dispel concerns about extreme mis-weighting on the part of ProPublica.

years 2003–2014, we assign a state's initial rank as their rank in 2003 and consider subsequent changes in workers' compensation generosity.[19]

The states that increase or decrease workers' compensation generosity from year to year are listed in Table 1. For example, in 2004, Georgia decreased its generosity relative to its original level. It remained at that level until 2006, when it increased. The state's generosity again increased in 2007 before decreasing in 2013.

For the period analysis, the variables *Increase* (*Decrease*) takes the value of one if the state level in a given period is greater (less) than the level in the prior period. In a four-period model, contrasting years 2003, 2006, 2009, and 2012, the variables *Increase* and *Decrease* have the value of zero in year 2003. To illustrate the nature of the coding, consider the state of Georgia. Georgia had a lower generosity level in 2006 than it did in 2003,[20] so *Increase* = 0 and *Decrease* = 1 in period 2. In 2009, Georgia was still higher level than it was in 2006 (given the increase in 2007), so *Increase* = 1 and *Decrease* = 0 in period 3. In 2012, Georgia had not changed from its position in 2009, so *Increase* = 0 and *Decrease* = 0 in period 4. As illustrated, while the period-by-period analysis introduces some measurement error in the independent variables *Increase* and *Decrease*, this error should be random and only bias our results downward. This snapshot-analysis also helps us distinguish lagged effects from new effects in a given year. We also display year-by-year effects for robustness.

The generosity measures provide other details about the regulatory change that are descriptively informative.[21] Workers' compensation laws can change on many dimensions. States can change legal standards governing allocation of workers' compensation benefits, including standards required to prove injury or limits on company liability. States also change

---

19. Consequently, *Increase* and *Decrease* are always zero in the initial period, 2003.

20. Table 1 lists Georgia as decreasing in 2004 and subsequently increasing in 2006. Net of these changes, Georgia is considered a decrease in this analysis because the resulting level in 2006 is still lower than it was in 2003.

21. These policies often involve some statutory revision. In determining the timing of enactment of the policies, we take the ProPublica timing as given. We find this a reasonable assumption for two reasons: First, ProPublica's variable indicates the state of a given state's workers' compensation regime at a given time; thus, the reforms are merely explanation for the change in state. Secondly, once a reform is passed, employers are on notice as to its prospective impact.

death benefits payable to workers' families, including provisions for funerals and other monetary transfers to survivors. States can also directly alter the compensation given to victims by changing maximum weekly payment caps, or the time horizon over which such benefits could be received. Similarly, states can increase the requirements to qualify as disabled or otherwise make it harder to begin receiving benefits. Even seemingly neutral changes can have disproportionate effects on a worker's ability to recover. States sometimes impose caps on attorneys' fees; however, these fee caps govern only fees paid by workers' compensation claimants, not companies (Department of Labor, 2016). States have also changed requirements for medical examinations that often relate to criteria for proving and treating injuries, as well as to restrictions on the type of physician who can perform such examinations. Finally, states may also restrict availability of special funds or subsidy programs.

While the detailed categorization of policy changes is fascinating, the detail is too fine to incorporate quantitatively into the analysis. Additionally, multiple types of changes often occur at the same time, making each type of effect difficult to disentangle.

However, given that generosity is measured on a 20-point scale, we construct a separate binary variable as a sensitivity analysis to incorporate some measure of the magnitude of the change. The indicator variable *Big* takes the value of one if a state increased or decreased its qualitative benefit generosity score by more than one level in a given period. We interact *Big* with *Increase* or *Decrease* to take into account the direction of these large changes. By considering these multiple ways of changing workers' compensation laws, this evaluation of a state's workers' compensation protection more holistically captures the many ways firms can avoid liability, and accordingly, the firm's varying incentives to provide a safer workplace.

5. Results

5.1. Descriptive Statistics

While Table 1 holistically summarizes the changes in state law from 2003 to 2008, it is useful to summarize the states that changed their laws in each direction from 2003 to 2006. The inclusion of an asterisk indicates whether a state's change is classified as *Big*. The following states increased workers' compensation generosity: Connecticut, Illinois*, New Jersey*, Rhode Island, and Wisconsin. In contrast, Alaska, California*, Georgia,[22] Idaho, Indiana, Iowa, Missouri*, North Dakota*, Ohio*, Oklahoma*, South Carolina, Tennessee*, Texas, and Vermont decreased workers' compensation coverage. Similarly, the following analysis incorporates as a control the weekly maximum benefit cap imposed by each state. The mean of this variable, reported in nominal dollars, is $666.76 (standard deviation $187.98).

The dependent variable in our main specification is the 3-year average of the fatality rate, which has a mean of 5.58 (standard deviation 28.19) and is evenly distributed across all three size classes. Table 2 lists the average fatality rates by industry type and establishment size. Column (1) reports fatalities for establishment sizes of 0–19, column (2) for size 20–99, and column (3) for size 100+. Notably, however, the potential difference in fatality rates by establishment size might be diluted by our reliance on the available BLS establishment sizes; defining a large establishment as one with 100 or more workers might dilute the effect of incentives faced only by extremely large firms. Even with this limitation, however, there are visible differences across the firm size categories.

The last row of Table 2 displays the total mean fatality rate across all industries: the smallest category of establishment size has the largest mean fatality rate—7.71. The medium category is significantly smaller, and the largest category is in the intermediate range. However, focusing on the total mean fatality rate masks considerable heterogeneity by industry. In *Mining* and in *Information*, large firms have higher fatality risks than even small firms. The pattern of higher risk levels at very small enterprises would be in line with the literature suggesting that larger firms have greater incentives to keep injury rates low, due to more accurate experience rating relative to the very small firms. The next section will indicate whether this pattern continues after controlling for other factors, such as state, that might influence the firm size effect.

22. As noted above, Table 1 lists Georgia as decreasing in 2004 and subsequently increasing in 2006. Net of these changes, Georgia is considered a decrease in this analysis because the resulting level in 2006 is still lower than it was in 2003.

Table 2. Average Fatality Rates per 100,000 Worker Equivalents by Industry and Size

Industry Category	Size 0–19	Size 20–99	Size 100+
Mining	27.13	15.79	33.36
	(39.98)	(25.80)	(152.98)
Construction	10.97	6.35	8.90
	(6.03)	(5.05)	(8.21)
Manufacturing	5.11	4.12	2.03
	(5.69)	(17.59)	(2.37)
Wholesale/Retail Trade	3.84	1.57	1.33
	(2.53)	(1.64)	(1.37)
Transportation and Warehousing; Utilities	21.46	6.74	6.44
	(17.01)	(6.15)	(7.93)
Information	2.09	1.07	2.62
	(4.90)	(3.04)	(4.93)
Finance and Insurance; Real Estate and Rental and Leasing	1.52	0.63	0.57
	(1.90)	(1.37)	(1.33)
Professional, Scientific, and Tech. Services; Mgmt.; Admin., Support, and Waste Mgmt.	5.21	1.74	2.95
	(3.80)	(1.90)	(12.87)
Educational Services; Health Care and Social Assistance	0.88	0.52	0.83
	(0.99)	(0.81)	(1.04)
Arts, Entertainment, and Recreation; Accommodation, and Food Services	4.11	0.71	1.84
	(3.35)	(0.93)	(4.70)
Other Services (excluding Public Administration)	2.70	1.31	1.06
	(1.72)	(4.82)	(2.60)
Total	7.71	3.64	5.37
	(15.80)	(10.68)	(44.97)

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file.

5.2. Regression Results

Turning from the purely descriptive statistics, this subsection begins by reporting the results of our preferred specification, a period-based analysis that allows fatality rates to vary with establishment size. The results demonstrate that an increase in workers' compensation generosity has a statistically significant negative effect on fatalities; however, there is no evidence of a countervailing risk effect from a decrease in such generosity. After reporting the results of our preferred specification, we run a number of robustness checks to further explore this effect. Subsequent analyses include examining heterogeneous treatment effects by size of the policy change or capital-intensiveness of industries affected. Moreover, to test the robustness of the results to different specifications, we present results of single-year regressions, regressions with fatality rates that do not vary by establishment size, and regressions using years 2003–2014. These robustness checks consistently confirm the predicted asymmetric effect of changes in workers' compensation generosity.

5.2.1. *Three-year and 2-year average regressions, with size controls.* Table 3 shows the results of equation (1). To control for unobserved characteristics of states enacting policy change, each column includes state fixed effects. Three-year averages are used in order to reduce the number of categories with zero fatalities or employment. However, this aggregation may also mask the true effects of the policy changes. For this reason, we run robustness checks using 2-year rate averages.[23] Columns (1)–(2) use 3-year fatality rate averages and columns (3)–(4) use 2-year fatality rate averages for all workers.

As noted above, *Increase* and *Decrease* are the treatment effects, taking the value of one only when a state increases or decreases its workers' compensation generosity package in 2006. We hypothesized that *Increase* would be significantly negative, as firms invest more in protections to avoid worker injury. In contrast, *Decrease* should be insignificant, as companies are unlikely to affirmatively reduce the workplace protections already in place. This hypothesis is largely borne out in the data. *Increase* is always negative and significant, and *Decrease* is insignificant.

The magnitude of *Increase* is both statistically and practically significant. The median decline in fatality rates for states that increase workers' compensation generosity amounts to a reduction of 0.44 fatalities out of

23. The 2-year rate averages use fatality rates from 2003–2004 and 2006–2007.

Table 3. OLS Regressions for 2003–2005 and 2006–2008

	ln(3-Year Avg. Rate+1)		ln(2-Year Avg. Rate+1)	
	(1)	(2)	(3)	(4)
Increase	−0.206***	−0.208***	−0.249***	−0.255***
	(0.076)	(0.071)	(0.068)	(0.065)
Decrease	−0.003	−0.005	−0.078	−0.083
	(0.075)	(0.071)	(0.086)	(0.080)
Maximum Benefits /1000	−0.062		−0.192	
	(0.561)		(0.537)	
Size 0–19	0.583***	0.583***	0.568***	0.568***
	(0.044)	(0.044)	(0.052)	(0.052)
100+	0.044	0.044	0.037	0.037
	(0.041)	(0.041)	(0.046)	(0.046)
N	3,285	3,285	3,277	3,277
R^2	0.369	0.368	0.321	0.320
F-Test(Increase = Decrease)	0.0027	0.0025	0.0206	0.0214
F-Test(Abs(Increase) = Abs(Decrease))	0.0027	0.0025	0.0206	0.0214

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. Variables included but not shown include an indicator variable for period 2006–2008, indicator variables for states, and indicator variables for 11 industry categories. Clustered standard errors are listed. Significance levels: * for $P < 0.10$, ** for $P < 0.05$, *** for $P < 0.01$.

100,000 full-time workers.[24] Given that the mean fatality rate is 5.58 fatalities out of 100,000 full-time workers, this effect is practically substantial and significant. The magnitude of the effect varies by establishment size and by industry, as indicated by Appendix Table A1. High risk industries, such as construction and transportation and warehousing/utilities, observe the largest impact—roughly 1/100,000 reduction in the fatality rate. Lower risk industries, such as educational services, finance and insurance, and professional services experience more moderate reductions, ranging from 0.22 to 0.39 per 100,000. The magnitudes of the impacts also vary by firm size and are broadly in line with the average fatality rate statistics reported in Table 2.

24. This measure is calculated using the coefficients in Table 3, column (1), and calculating the fatality rate reductions attributable to *Increase*, using the average fixed effect for states that increased and the average fixed effect of those which did not change for the control group. These effects are calculated for 11 industries and 3 establishment sizes, as reported in Appendix Table A1, and the reported measure is the median effect across industry and size.

The results on size confirm previous findings in the literature. Relative to firms with 20–99 employees, firms with 100 or more employees do not have significantly different fatality rates; however, firms with 0–19 employees have significantly higher fatality rates. This pattern is largely consistent with the hypothesis that smaller firms are less likely to be accurately experience-rated than larger firms, although the preassigned BLS size groupings might mask the level at which this effect becomes significant. Given that this effect is consistent, we will not display coefficients for size in the following tables.[25]

The relationship between fatality rates and *Maximum Benefits* is small and generally statistically insignificant. There are two potential reasons for the apparent lack of an effect. First, as previously mentioned, the weekly maximum thresholds have become more uniform over time, exhibiting little or no time variations within states other than for inflation adjustments. Moreover, as most variation in weekly maximum replacement caps occurs across states rather than within states over time, the inclusion of state fixed effects further reduces any impact of the *Maximum Benefits* variable. Second, variations in the other measures of workers' compensation generosity likely eclipse the effect of weekly maximum replacement rates. Given the insignificance of these results, the following tables will include but not display the coefficients on *Maximum Benefits*.

The last two rows of Table 3 provide F-tests for the two variables of interest. While a sufficient test of our hypothesis requires comparing states that increased (decreased) workers' compensation generosity to states that do not change, we include these F-tests to compare the coefficient of *Increase* to the coefficient of *Decrease*. The second to last line presents the results of an F-test for equality of *Increase* and *Decrease*. All specifications find that the coefficient for *Increase* is significantly different from the coefficient for *Decrease*. The last line reports the F-test corresponding to whether the absolute value of the coefficient for *Increase* and the coefficient for *Decrease* are significantly different. This would test whether the magnitude of the effect, not just direction, is different for *Increase* and *Decrease*. Since the

25. These results are available upon request.

nominal value for *Decrease* is negative, these values are the same as the preceding row. These results suggest that firms are sensitive to various policies governing the potential liability for worker injury.[26]

5.2.2. *Heterogeneous policy effects.* While Table 3 presents the results of our preferred specification, we allow for a number of extensions exploring the heterogeneous nature of these policy effects, displayed in Table 4. While we hesitate to assign cardinal significance to ProPublica's ordinal state ranks, preferring instead to focus on increases or decreases in generosity, columns (1) and (3) present an attempt to explore the impact of the magnitude of such changes. *Big* is an indicator variable, which takes the value of one when the rank of a state's generosity changes by more than one level. *Increase* × *Big* thus takes the value of one when a state increases by more than one level and *Decrease* × *Big* takes the value of one when a state decreases by more than one level. As seen in columns (1) and (3), the interaction terms are insignificant, suggesting that assigning cardinal significance to these rankings yields no evidence of systematic variations with the larger increases in the ordinal score.

Diving deeper into the mechanism of this asymmetry, we categorize some industries as more capital-intensive than others. As before, the absence of a significantly positive effect of a decrease in workers' compensation generosity can be explained by difficulty in relinquishing capital in the short run, the endowment effect in safety, or a ratcheting effect in technology. If irreversible capital investments alone drive this asymmetry, we should see an insignificant effect of *Decrease* for capital-intensive industries and a positive effect for noncapital-intensive industries. Conversely, since the hypothesized effect of an *Increase* involves a mix of new capital (such as equipment) and safety protocol, if the new equipment is the main mechanism by which workplaces become safer, we could see more capital-intensive

26. Another way to analyze the effect of workers' compensation generosity is not to use fatality rates but to use count data models; these models still use our period-based analysis. Accordingly, the dependent variable is the number of fatalities for a 3-year period, using employment for the same 3-year period as a control variable. Appendix Table A2 presents these results: *Increase* is negative and significant, while *Decrease* is positive and insignificant.

Table 4. Extensions with Establishment Size†

	3-Year Avg. Rate		2-Year Avg. Rate	
	(1)	(2)	(3)	(4)
Increase	−0.243***	−0.174**	−0.265***	−0.211***
	(0.064)	(0.079)	(0.072)	(0.070)
Decrease	0.039	−0.026	−0.020	−0.108
	(0.087)	(0.070)	(0.111)	(0.082)
Increase × Big	0.079		0.023	
	(0.103)		(0.085)	
Decrease × Big	−0.110		−0.150	
	(0.076)		(0.100)	
Increase × Capital		−0.175*		−0.209
		(0.103)		(0.136)
Decrease × Capital		0.122		0.161*
		(0.090)		(0.089)
N	3,285	3,285	3,277	3,277
R^2	0.447	0.447	0.401	0.402

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. Variables included but not shown include maximum weekly benefit caps, an indicator variable for period 2006–2008, indicator variables for states, indicator variables for size, and indicator variables for 11 industry categories. Clustered standard errors are listed. Significance levels: * for $P < 0.10$, ** for $P < 0.05$, *** for $P < 0.01$; †Columns (3) and (4) have a different number of observations than columns (1) and (2) because averaging over 2 years rather than three can result in more fatality rates with undefined denominators (i.e., more cells with zero employment).

industries with a stronger negative fatality effect. As a rough proxy for capital intensity, we designate the manufacturing and construction industries as more capital-intensive, and columns (2) and (4) include interaction terms for *Increase* and *Decrease* and these industries.

None of the *Capital* interaction terms are statistically significant at the usual levels. For the 3-year averages, there is no significant effect for *Decrease* × *Capital* in column (2) and a weakly positive effect for the 2-year averages in column (4). While the interaction term in column (4) is significant, the sum of the main effect and interaction effect is not significantly different from zero ($P = 0.690$).[27] For *Increase* × *Capital*, the results are similarly mixed. *Increase* × *Capital* is significantly negative in column (2) but not column (4); however, the entire *Increase* effect is significantly negative in both columns.

27. The comparable *P*-value in column (2) is 0.4380.

The effects of capital intensity are difficult to parse, likely due to the rough measure of capital intensity and the aforementioned relationship of the effect of *Decrease* and employer investment in safety.

5.2.3. *Alternative specifications: single-year rates regressions and count models, with size controls.* As noted above, separating the data into periods 2003–2005 and 2006–2008 has several advantages, including accounting for lagged effects of policy changes, reducing the likelihood of capturing reverse causality, and reducing noise. Use of the CFOI for other purposes, such as studies of the value of a statistical life, have tended to avoid use of single-year fatality rates because of the influence of random year-to-year fluctuations in fatalities on cells that do not have a large number of fatalities. To show the robustness of these results, however, in Table 5 we present the results of single-year fatality rates. In these runs, as before, *Increase* and *Decrease* are constructed relative to the state's level in the previous year and each fatality rate only consists of 1 year. This specification allows us to include year fixed effects, rather than a period effect. The results in column (1) are largely similar to our main specifications: *Increase* is significantly negative, though only at the 0.10 level, and *Decrease* is insignificant. The F-tests, however, indicate that we cannot reject the null hypothesis that the two coefficients of interest are the same. This is likely a function of the noise created by the single-year rates, which are more subject to random variations in fatalities, particularly for cells with a small number of worker deaths. Column (2) lists the results from including state-specific time trends, to control for time-variant unobservables within state. The results are robust to this additional control.

Finally, Table 5 column (3) uses lagged log rates as the dependent variable. As noted above, we do this in order to provide a test for alternative explanations, such as reverse causality or regression to the mean after a disaster. If *Increase* is positive and significant in this run, this provides evidence of regression to the mean, i.e., higher fatality rates lead to increases in workers' compensation generosity. If instead *Increase* is negative and significant in this run, this might provide evidence of reverse causality, i.e., lower fatality rates cause a more generous workers' compensation

Table 5. Alternative Specifications: Single-Year Rates

	Single-Year Rates		Lagged Single-Year Rates
	(1)	(2)	(3)
Increase	−0.123*	−0.135**	0.055
	(0.063)	(0.060)	(0.064)
Decrease	−0.056	−0.061	0.042
	(0.052)	(0.056)	(0.047)
Inclusion of State-Specific Time Trends	No	Yes	No
Observations	9,741	9,741	8,096
R^2	0.327	0.335	0.329
F-test(Increase = Decrease)	0.4028	0.3611	0.8747
F-Test(Abs(Increase) = Abs(Decrease))	0.4028	0.3611	0.8747

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. Variables included but not shown include maximum weekly benefit caps, indicator variable for years, indicator variables for states, indicator variables for establishment size, and indicator variables for 11 industry categories. Clustered standard errors are listed. Significance levels: * for $P < 0.10$, ** for $P < 0.05$, *** for $P < 0.01$.

scheme. Column (3) tests these predictions. *Increase* is insignificant, providing evidence that reverse causality and regression to the mean are less of a concern.

5.2.4. *Instrumental variables estimation.* As discussed above, the concerns about endogeneity are addressed in part based on the period-based analysis, as well as the falsification tests we implement above. To further address this issue, we instrument for the policy variables *Increase* and *Decrease* using policy variables from unrelated fields.

To instrument for the possibility that some variation in fatality rates over time drives policy changes, we use changes in state political composition as well as changes in election and environmental policy. *PercentDemocrat* is a continuous measure that measures the percent of state legislators that identify as Democrat in years 2003 and 2006.[28] We interact this measure with two policy variables: *Mail* indicates whether a state enacted a bill regarding mail voting, under the theory that it increases access to elections.[29]

28. These data are collected from http://www.ncsl.org/research/about-state-legislatures/partisan-composition.aspx.

29. These data are collected from the National Conference of State Legislatures, http://www.ncsl.org/research/elections-and-campaigns/2001-2010-database-of-election-reform-legislation.aspx.

Similarly, *Energy* is an indicator variable for states that create renewable fuel standard mandates.[30] As with *Increase* and *Decrease*, *Mail* and *Energy* have a 2003 baseline. Each variable takes a value of one in 2006 if any policy was undertaken in the relevant state in 2004, 2005, and 2006. This construction is compatible with the level-based analysis for *Increase* and *Decrease*.

The rationale behind the use of these variables is that changes in election and environmental policy should only be correlated with labor policy through the bundling of platforms by progressive and conservative parties. Thus, changes in election and environmental policy should only identify off of changes in political power of progressive and conservative movements that might lead to adoption of more generous workers' compensation policies. These variables are plausibly excludable in the injury equation; changes in electoral or environmental policy should not affect injury rates except through a progressive/conservative agenda that shifts workers' compensation.

The first stage summary statistics suggest that our choice of instruments is largely appropriate.[31] The F-tests (P-value) for the excluded variables from the first stage regressions are significant (9.87 ($P < 0.001$) for *Increase* and 9.07 ($P < 0.001$) for *Decrease*). Because the use of weak instruments might only exacerbate bias, Sanderson–Windmeijer F-statistics measure the strength of the identification for each of the endogenous regressors (10.63 for *Increase* and 6.06 for *Decrease*). The Kleibergen–Paap Wald statistic for the null hypothesis that the equation is weakly identified, suggested by Baum et al. (2007) in the presence of clustered errors, is 4.943.[32]

30. These data are collected from the Alternative Fuels Data Center.

31. We use xtivreg2 for this analysis (Schaffer, 2010).

32. The Cragg–Donald F statistic, applicable when observations are i.i.d., is 168.389, which would imply strong instruments in the absence of clustered errors. Critical values for the Kleibergen–Paap F statistic are not available; in the absence of these, Baum et al. (2007) suggest that researchers may use the Stock–Young critical values for Cragg–Donald F statistics, albeit with caution. Using this metric, an F statistic of 4.79 would correspond to 30% IV bias, relative to OLS. Accordingly, although this does not preclude some bias in the IV, one can reject the null that the maximal relative bias is greater than 30%.

Moreover, the null of the weak instrument-robust inference test is that the endogenous regressors are insignificant in the main equation and that the orthogonality conditions are valid. Two of the three statistics presented reject this null [Anderson–Rubin Wald test

Table 6. Instrumental Variable Results

	Ln(3-Year Avg. + 1)
Increase	−0.228*
	(0.129)
Decrease	−0.103
	(0.157)
N	3,219
Underidentification test: Kleibergen–Paap rk LM statistic (*P*-value)	7.977 (0.0924)
Overidentification test: Hansen J statistic (*P*-value)	2.980 (0.3948)
Weak Identification: Kleibergen–Paap rk F statistic	4.943
Weak Identification for Individual Regressors:	*Increase* (10.63)
Sanderson–Windmeijer F-statistics	*Decrease* (6.06)

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. Variables included but not shown include maximum weekly benefit caps, indicator variable for period, indicator variables for states, indicator variables for establishment size, and indicator variables for 11 industry categories. Clustered standard errors are listed. Significance levels: * for $P < 0.10$, ** for $P < 0.05$, *** for $P < 0.01$.

Moreover, when the number of instruments outnumber the endogenous regressors, an overidentification test indicates whether the instruments are valid, i.e., if the instruments are uncorrelated with the error. The Hansen J statistic in Table 6, 2.980, indicates that the null hypothesis that the instruments are valid is not rejected. Finally, to address the concern that the instrumented equation is underidentified, we include the Kleibergen—Paap rk LM statistic, 7.977. This statistic rejects at the 10% level the null hypothesis that the equation is underidentified, providing more confidence in our choice of instruments. With these tests in mind, we turn to the results.

The results of this instrumental variable approach are displayed in Table 6 and support the ordinary least squares (OLS) results. Notably, the number of observations is slightly smaller than the main regression, as the number of legislators were missing for Nebraska. The results lose some significance but are consistent with the OLS regressions. *Increase* is still negative, significant

F 10.08 (*P*-value <0.001), Anderson–Rubin Wald test χ^2 51.76 (*P*-value <0.001), and Stock–Wright LM S statistic $\chi^2$7.23 (*P*-value = 0.2038)] at the 10% level, and *Decrease* is insignificant. The magnitudes are largely similar to the OLS results.

The consistency of the IV approach and the OLS results, along with the evidence supporting the appropriateness of the chosen instruments, suggest that changes in worker's compensation generosity do drive the changes in fatality rates. In addition to the period-based analysis and the lagged rate falsification test, the IV analysis provides confidence in our results as being robust to time-variant unobservables.

5.2.5. *Additional extensions.* Given that our results are robust to various specifications, this section concentrates on the robustness of these results to the exclusion of size and to the expansion of the study period from 2003–2008 to 2003–2014. Table 7 presents the results of these robustness checks. Column (1) displays results for the log of 3-year average of fatality rates, and column (2) displays results for the log of 2-year averages.

Panels A and C present the results based on fatality rates that do not vary based on establishment size. Excluding size from our analysis has several benefits: given the concern about thin cells due to the relative infrequency of fatalities, eliminating size categories allows us to use more refined industry categories. In addition, we can include information from the public administration industry, which we had to previously exclude because we had no information on establishment size for the employment denominators. This leaves 20 industry categories.[33] Given this new dataset, we can check the robustness of our findings to the exclusion of size.

Moreover, in order to ensure that these results are not confined to this particular time period, we ran an expanded version of the model on data from

33. The following industries are included: (1) mining; (2) construction; (3) durable goods manufacturing; (4) nondurable goods manufacturing; (5) wholesale trade; (6) retail trade; (7) transportation and warehousing; (8) utilities; (9) information; (10) finance and insurance; (11) real estate, rental, and leasing; (12) professional and technical services; (13) management, administrative, and waste management services; (14) educational services; (15) health care and social assistance; 16) arts, entertainment, and recreation; (17) accommodation and food services; and (18) private households; (19) other services (except public administration), and (20) public administration. As before, we do not consider agriculture because states often exempt agricultural industries from workers compensation regulations.

Table 7. Alternative Specifications

	Ln(3-Year Avg. + 1)	Ln(2-Year Avg. + 1)
Panel A: With No Size, 20 industry categories, 2003–2008		
Increase	−0.095	−0.176***
	(0.061)	(0.054)
Decrease	0.064	0.012
	(0.058)	(0.074)
Observations	1,999	1,997
Panel B: With Size, 11 industry categories, 2003–2014		
Increase	−0.155***	−0.178***
	(0.050)	(0.066)
Decrease	0.021	0.002
	(0.035)	(0.048)
Observations	6,571	6,558
Panel C: With No Size, 20 industry categories, 2003–2014		
Increase	−0.110***	−0.109**
	(0.039)	(0.041)
Decrease	0.060*	0.054
	(0.032)	(0.042)
Observations	3,999	3,996

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. Variables included but not shown include maximum weekly benefit caps, an indicator variable for periods, indicator variables for states, indicator variables for size (where applicable), and indicator variables for industry categories. Clustered standard errors are listed. Significance levels: * for $P < 0.10$, ** for $P < 0.05$, *** for $P < 0.01$.

2003 to 2014. We do not use this as our main specification, as this period spans a number of coding changes in the CFOI data, described above. Panel B displays results from this extended period for the original specification, which allowed fatality rates to vary by establishment size. Panel C also displays results from the extended period but, as in Panel A, ignores establishment size and includes a more granular industry measure. As before, the results are consistent: *Increase* leads to a significant decline in the fatality rate, while *Decrease* has an insignificant effect. While *Increase* is not statistically significant in Panel A in which there are no controls for size categories, the additional observations included in the counterpart regressions in Panel C lead to *Increase* having a statistically significant negative effect as in the other regressions.

In sum, a variety of specifications confirm our initial conclusion: increases in workers' compensation generosity cause a reduction in fatality rates and counts of fatalities, while comparable decreases produce no discernible effect. This conclusion is robust not only to the consideration of count data models but to fatality rates that consist of single years (with and without the inclusion of state-specific time trends). Moreover, the results from instrumental variable estimation are similarly consistent, implying that the OLS results are not driven by time-variant unobservables. Finally, the results remain consistent for fatality rates that do not vary based on establishment size, and fatality rates that use data from an extended sample period. Given the robustness of the results to these variations, we can conclude that changes in workers' compensation generosity demonstrably affect employer safety incentives in a predictably asymmetric way.

6. Conclusion

Given the move towards uniform maximum weekly compensation rates, assessing whether workers' compensation programs foster workplace safety must be explored using more comprehensive measures of workers' compensation generosity. Using such a holistic measure, this article examines the effect of workers' compensation policy on workplace safety, capturing the myriad ways that changes in state law could either insulate firms from, or further expose firms to, liability from injury. By focusing on fatality rates as the job safety measure, many of the theoretical issues that confound the relationship between workers' compensation and the safety incentives for employers are mitigated.

The effect of increases in workers' compensation is illuminating in understanding the incentives workers' compensation providers to firms to provide safer workplaces. However, the effect of decreases in workers' compensation is also of critical importance: will states' potential "race to the bottom" in eroding workers' compensation benefits lead to less safe workplaces?

Our results present a consistent conclusion: employers are clearly sensitive to workers' compensation policy when it provides additional or more generous benefits. We find statistically significant decreases in fatality rates in states that increase workers' compensation policy generosity, suggesting that employers invest more heavily in safety. On the other hand, when these protections decline, we find no evidence of statistically significant increases in fatality rates. This pattern is consistent with the theory that employers will not easily reduce the existing workplace safety measures. The absence of a resurgence in fatalities after decreases in workers' compensation is persistent, regardless of what specification we examine, or what type of fatality rate we use.

The long run effects on safety may, however, be more pronounced if this asymmetry is driven by irreversible capital investment in the short run. If it takes time for companies to eliminate their capital investments, workers may eventually see a decline in workplace safety. Similarly, in the case of prospective safety investments, we might expect new firms or firms newly investing in safety protocols to invest in less safety equipment and protocols. Within our time period, however, we do not find clear evidence of net adverse effects on safety. If instead the asymmetry exists due to an endowment effect in safety, or industrial innovations created in response to the need for safer procedures, we would not expect worker safety to decline in the long run either.

This asymmetry in response is somewhat comforting for workers. While there may be many concerns with reductions in workers' compensation protections with respect to actual compensation for the injured worker, our results suggest that such changes might not be as problematic in terms of employer incentives for safety.

Appendix

Table A1. Effects of Increases in Workers' Compensation on Fatality Rates by Firm Size and Industry

	Size 0–19	Size 20–99	Size 100+
Mining	−1.48	−0.83	−0.86
Construction	−1.91	−1.07	−1.12
Manufacturing	−0.79	−0.44	−0.46
Wholesale/Retail Trade	−0.66	−0.37	−0.39
Transportation and Warehousing; Utilities	−1.94	−1.08	−1.13
Information	−0.45	−0.25	−0.26
Finance and Insurance; Real Estate and Rental and Leasing	−0.39	−0.22	−0.23
Professional, Scientific, and Tech. Services; Mgmt.; Admin., Support, and Waste Mgmt.	−0.70	−0.39	−0.41
Educational Services; Health Care and Social Assistance	−0.39	−0.22	−0.23
Arts, Entertainment, and Recreation; Accommodation, and Food Services	−0.57	−0.32	−0.33
Other Services (excluding Public Administration)	−0.48	−0.27	−0.28

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. These values are the fatality rate reductions implied by the difference-in-difference ln(rate + 1) estimates. These average effects were calculated with the coefficients in Table 3, column (1), using average fixed effects for states for which Increase = 1, relative to the average fixed effect for states which did not change workers' compensation generosity.

Table A2. Alternative Specifications: Negative Binomial Models

	(1)	(2)
Increase	−0.211**	−0.198*
	(0.105)	(0.102)
Decrease	0.032	0.046
	(0.099)	(0.097)
Max Benefits/1000	0.517	
	(0.924)	
Size 0–19	0.846***	0.847***
	(0.050)	(0.050)
100+	0.131*	0.131*
	(0.072)	(0.072)
Observations	3,285	3,285
F-Test(Increase = Decrease)	0.0016	0.0038
F-Test(Abs(Increase) = Abs(Decrease))	0.3459	0.4006

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. Variables included but not shown include employment, an indicator variable for period 2006–2008, indicator variables for states, and 11 indicator variables for industry categories. Clustered standard errors are listed. Significance levels: * for $P < 0.10$, ** for $P < 0.05$, *** for $P < 0.01$.

Table A3. Full Results for all Alternative Specifications

	3-Year Avg.	2-Year Avg.
Panel A: With No Size, 20 industry categories, 2003–2008		
Increase	−0.095	−0.176***
	(0.061)	(0.054)
Decrease	0.064	0.012
	(0.058)	(0.074)
Maximum Benefits/1000	−0.170	−0.024
	(0.492)	(0.475)
Constant	1.827***	1.712***
	(0.297)	(0.276)
Observations	1,999	1,997
Panel B: With Size, 11 industry categories, 2003–2014		
Increase	−0.155***	−0.178***
	(0.050)	(0.066)
Decrease	0.021	0.002
	(0.035)	(0.048)
Maximum Benefits/1000	0.397	0.549
	(0.336)	(0.469)
Size 0–19	0.581***	0.568***
	(0.031)	(0.034)
Size 100+	−0.012	−0.022
	(0.023)	(0.025)
Constant	0.300	0.243
	(0.201)	(0.276)
Observations	6,571	6,558
Panel C: With No Size, 20 industry categories, 2003–2014		
Increase	−0.110***	−0.109**
	(0.039)	(0.041)
Decrease	0.060*	0.054
	(0.032)	(0.042)
Maximum Benefits/1000	0.208	0.460
	(0.296)	(0.369)
Constant	1.565***	1.443***
	(0.183)	(0.228)
Observations	3,999	3,996

Notes: Source: U.S. Bureau of Labor Statistics and calculations by the authors. The totals and calculations of fatal injury data were generated by researchers at Vanderbilt University with restricted access to the Census of Fatal Occupational Injuries research file. Variables included but not shown include an indicator variable for periods, indicator variables for states, and indicator variables for industry categories. Clustered standard errors are listed. Significance levels: * for $P < 0.10$, ** for $P < 0.05$, *** for $P < 0.01$.

References

Baum, Christopher F., Mark E. Schaffer, and Steven Stillman. 2007. Enhanced routines for instrumental variables/GMM estimation and testing. Working paper, Boston College, Department of Economics (No. 667).

Biddle, Jeff A., and Karen Roberts. 2003. "Claiming Behavior in Workers' Compensation," 70 *Journal of Risk and Insurance* 759–80.

Bronchetti, Erin Todd, and Melissa McInerney. 2012. "Revisiting Incentive Effects in Workers' Compensation: Do Higher Benefits Really Induce More Claims," 65 *ILR Review: Journal of Work & Policy* 286–315.

Butler, Richard J. 1983. "Wage and Injury Rate Response to Shifting Levels of Workers' Compensation" in J. D. Worrall, ed., *Safety and the Work Force*. Ithaca, NY: Cornell University, ILR Press.

Butler, Richard J. 1996. "Lost Injury Days: Moral Hazard Differences between Tort and Workers' Compensation," 63 *The Journal of Risk and Insurance* 405–33.

Butler, Richard J., B. Delworth Gardner, and Harold H. Gardner. 1997. "Workers' Compensation Costs when Maximum Benefits Change," 15 *Journal of Risk and Uncertainty* 259–69.

Butler, Richard J., Harold H. Gardner, and Nathan L. Kleinman. 2013. "Workers' Compensation: Occupational Injury Insurance's Influence on the Workplace" in G. Dionne, ed., *Handbook of Insurance*. New York, NY: Springer (pp. 449–69).

Butler, Richard J., Nathan L. Kleinman, and Harold H. Gardner. 2014. "I Don't Like Mondays: Explaining Monday Work Injury Claims," 67 *ILR Review* 762–83.

Butler, Richard J., and John D. Worrall. 1983. "Workers' Compensation: Benefit and Injury Claims Rates in the Seventies," 65 *The Review of Economics and Statistics*, 580–9.

Butler, Richard J., and John D. Worrall. 1985. "Work Injury Compensation and the Duration of Nonwork Spells," 95 *The Economic Journal* 714–24.

Butler, Richard J., and John D. Worrall. 1991. "Claims Reporting and Risk Bearing Moral Hazard in Workers' Compensation," 58 *Journal of Risk and Insurance*, 191–204.

Card, D., and Brian P. McCall. 1996. "Is Workers' Compensation Covering Uninsured Medical Costs? Evidence from the 'Monday Effect'," 49 *ILR Review* 690–706.

Chelius, James R. 1982. "The Influence of Workers' Compensation on Safety Incentives," 35 *ILR Review* 235–42.

Chelius, James R., and Robert S. Smith 1987. "Firm Size and Regulatory Compliance Costs: The Case of Workers' Compensation Insurance," 6 *Journal of Policy Analysis and Management* 193–206.

Department of Labor. 2016. "Does the Workers' Compensation System Fulfill its Obligations to Injured Workers?" Available at: https://www.dol.gov/asp/Workers

CompensationSystem/WorkersCompensationSystemReport.pdf (accessed July 29, 2017).

Gentry, Elissa P., and W. Kip Viscusi. 2016. "The Fatality and Morbidity Components of the Value of Statistical Life," 46 *Journal of Health Economics* 90–9.

Grabell, M., and Howard Berkes. 2015, March 4. "The Demolition of Workers' Compensation," *ProPublica*. Available at: https://www.propublica.org/article/the-demolition-of-workers-compensation (accessed August 26, 2016).

Guo, Xuguang, and John F. Burton, Jr. 2010. "Workers' Compensation: Recent Developments in Moral Hazard and Benefit Payments," 63 *ILR Review* 340–55.

Kahneman, Daniel, Jack L. Knetsch, and Richard H. Thaler. 1991. "Anomalies: The Endowment Effect, Loss Aversion, and Status Quo Bias," 5 *Journal of Economic Perspectives* 193–206.

Krueger, A. B. 1988. *Moral Hazard in Workers' Compensation Insurance*. Industrial Relations Section, Princeton University.

Lakdawalla, Darius N., Robert T. Reville, and Seth A. Seabury. 2007. "How Does Health Insurance Affect Workers' Compensation Filing," 45 *Economic Inquiry* 286–303.

Mendeloff, John, and Rachel Burns. 2013. "States with Low Non-Fatal Injury Rates have High Fatality Rates and Vice-Versa," 56 *American Journal of Industrial Medicine* 509–19.

Moore, Michael J., and W. Kip Viscusi. 1989. "Promoting Safety Through Workers' Compensation: The Efficacy and Net Wage Costs of Injury Insurance," 20 *The RAND Journal of Economics*, 499–515.

Morantz, Alison D. 2009. "Has Devolution Injured American Workers? State and Federal Enforcement of Construction Safety," 25 *Journal of Law, Economics, and Organization* 183–210.

Morantz, Alison D. 2010. "Opting Out of Workers' Compensation in Texas: A Survey of Large, Multi-State Nonsubscribers," in Daniel P. Kessler, ed., *Regulation vs. Litigation: Perspectives from Economics and Law*. University of Chicago Press. Available at: http://www.nber.org/chapters/c11965.pdf.

Morantz, Alison D. 2011. "Does Unionization Strengthen Regulatory Enforcement-An Empirical Study of the Mine Safety and Health Administration," 14 *NYU Journal of Legislation & Public Policy* 697–727.

Neuhauser, Frank, and Steven Raphael. 2004. "The Effect of an Increase in Worker's Compensation Benefits on the Duration and Frequency of Benefit Receipt," 86 *The Review of Economics and Statistics* 288–302.

Northwood, Joyce. 2010. "Change to Hours-Based Fatality Rates in the Census of Fatal Occupational Injuries," U.S. Bureau of Labor Statistics. Available at: https://www.bls.gov/opub/mlr/cwc/change-to-hours-based-fatality-rates-in-the-census-of-fatal-occupational-injuries.pdf (accessed June 5, 2019).

Pallardy, Richard. "Deepwater Horizon Oil Spill of 2010." Available at: https://www.britannica.com/event/Deepwater-Horizon-oil-spill-of-2010 (accessed April 13, 2018).

Qiu, Yue, and Michael Grabell. 2015. "Workers' Compensation Reforms by State." ProPublica. Available at https://projects.propublica.org/graphics/workers-comp-reform-by-state (accessed August 26, 2016).

Reuters. "Problems at BP's U.S. Operations." Available at: https://www.reuters.com/article/bp-operations-idUSN0310147720110503 (accessed May 3, 2011).

Ruser, J. W. 1985. "Workers' Compensation Insurance, Experience-Rating, and Occupational Injuries," 16 *The RAND Journal of Economics* 487–503.

Ruser, J.W. 1991. "Workers' Compensation and Occupational Injuries and Illnesses," 9 *Journal of Labor Economics* 325–50.

Ruser, J. W. 1993. "Workers' Compensation and the Distribution of Occupational Injuries," 28 *Journal of Human Resources*, 593–617.

Ruser, J. W. 1998. "Does Workers' Compensation Encourage Hard to Diagnose Injuries?" 65 *Journal of Risk and Insurance*, 101–124.

Ruser, John W., and Richard Butler. 2010. "The Economics of Occupational Safety and Health," 5 *Foundations and Trends in Microeconomics* 301–354.

Ruser, John W., Michael R. Pergamit, and Parvati Krishnamurty. 2004. "Workers' Compensation Reforms and Benefit Claiming" in *3rd International Conference on Health Economics, Policy and Management, Athens, Greece.*

Schaffer, Mark E. 2010. "xtivreg2: Stata Module to Perform Extended IV/2SLS, GMM and AC/HAC, LIML and k-Class Regression for Panel Data Models." Available at: http://ideas.repec.org/c/boc/bocode/s456501.html (accessed May 13, 2019).

Smith, R. S. 1990. "Mostly on Monday: Is Workers' Compensation Covering Off-the-Job Injuries?" in Philip S. Borba and David Appel, eds., *Benefits, Costs, and Cycles in Workers' Compensation.* Dordrecht: Springer (pp. 115–28).

Viscusi, W. Kip. 1983. "Frameworks for Analyzing the Effects of Risk and Environmental Regulations on Productivity," 73 *American Economic Review* 793–801.

Viscusi, W. Kip. 2013. "Using Data from the Census of Fatal Occupational Injuries to Estimate the 'Value of a Statistical Life,'" *Monthly Labor Review* 1–17.