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Coal Mine Safety: Do Unions Make a Difference?

Alison D. Morantz[‡]

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Abstract

Although the United Mine Workers of America (UMWA) has always advocated strongly for miners' safety, prior empirical literature contains no evidence that unionization reduced mine injuries or fatalities during the 1970s and '80s. This study uses a more comprehensive dataset and updated methodology to examine the relationship between unionization and underground, bituminous coal mine safety from 1993 to 2008. I find that unionization predicts a substantial and significant decline in traumatic mining injuries and fatalities, the two measures that I argue are the least prone to reporting bias. These disparities are especially pronounced among larger mines. My best estimates imply that overall, unionization predicts about a 17-33% drop in traumatic injuries and about a 33-72% drop in fatalities. However, unionization is also associated with higher total and non-traumatic injuries, suggesting that injury reporting practices differ substantially between union and nonunion mines. Unionization's attenuating effect on the predicted frequency of traumatic injuries seems to have grown since the mid 1990s.

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1. **Introduction**

Empirical literature on the relationship between unionization and workplace safety presents a curious puzzle. On one hand, scholars have documented numerous ways in which unions help to promote safe work practices. For example, unions typically play a critical role in educating workers about on-the-job hazards; incentivizing workers to take greater care on the job; attracting more safety-conscious workers; inducing employers to mitigate known hazards; increasing regulatory scrutiny; and developing safety-related innovations. Yet most empirical studies of the relationship between unionization and important safety outcomes, such as injuries and fatalities, have failed to find any statistically significant evidence of a "union safety effect" (Morantz 2009).

Prior research on the coal mining industry typifies this perplexing pattern. Coal miners' unions, especially the dominant United Mine Workers of America (UMWA), have advocated vigorously for improved worker safety since their inception. When the UMWA adopted its first Constitution in 1890, for example, three of its "Eleven Points" called for improvements in the safety and health conditions of miners (Fox 1990:22-25). Organized labor was also instrumental in the passage of the Mining Safety and Health Act of 1969 (the "Coal Act"), the first statute to pave the way for comprehensive federal enforcement of occupational hazards at all surface and underground coal mines (Fox 1990:470-73). More recently, the UMWA played a particularly critical role in broadening the provisions of the Coal Act and encouraging the formation of state regulatory agencies (Fox 1990:462-470, 474, 504). By the 1980s, the UMWA's Health and Safety Department had developed an extensive tripartite structure including a Washington, D.C.-based international staff; regionally-based health and safety representatives tasked with liaising with Mining Safety and Health Administration (MSHA) District Offices; and mine-level health and safety committees that surveil day-to-day mine conditions. The myriad activities of mine-level health and safety committees include advocating on behalf of individual miners; conducting independent inspections; accompanying MSHA inspectors during official inspections; participating in pre- and post-inspection meetings; tracking MSHA appeals; providing training for miners; and, in extreme cases, shutting down hazardous sections of a mine, a power conferred by the UMWA's collective bargaining agreement with the Bituminous Coal Operator's Association (BCOA) (Weil 1987; Weil 1994). Nevertheless, most empirical studies focusing on the 1970s and '80s have reported, if anything, a counterintuitive *positive* relationship between a union’s presence at a mine and the frequency of reported injuries and accidents.

This paper re-examines the link between unionization and mining hazards using more recent data, a broader set of control variables, and updated statistical techniques. Highly granular MSHA data on injuries and mine characteristics, combined with confidential data obtained from the Department of Energy, enable me to examine whether discrete safety outcomes differ significantly between union and nonunion mines. Focusing on underground mines that extract bituminous coal, I find that unionization predicts large and robust declines in traumatic injuries and fatalities, the two safety outcomes in my study that I argue are the least prone to reporting bias. These effects – which are starkly at odds with previous literature – are especially pronounced among larger mines, and in the period since the early 1990s. The fact that unionization also predicts a significant *increase* in total and non-traumatic injuries – measures that are highly susceptible to reporting bias – lends credence to concerns that injury reporting differs significantly between union and nonunion settings.

The remainder of the paper is organized as follows. Section Two summarizes prior literature on the relationship between unions and mine safety. Section Three describes in detail the datasets upon which I rely. Section Four outlines my identification strategy and considers several potential sources of bias. Section Five presents my main empirical findings. Section Six further explores the likelihood of omitted variable bias, and offers several possible explanations for why the union safety effect might have intensified around the turn of the Twentieth Century. Section Seven concludes.

2 Literature Review

In the past few decades, scholars have examined the relationship between unions and workplace safety in a wide range of industries, such as the U.S. construction sector (Dedobbeleer et al. 1990), U.S. manufacturing (Fairris 1995), British manufacturing (Reilly et al. 1995, Nichols et al. 2007), forest product mills in British Columbia (Havlovic and McShane 1997), and the New Jersey public sector (Eaton and Nocerino 2000). Most such studies have failed to find a statistically significant negative relationship between unionism and the frequency of workplace accidents. Similarly, empirical scholarship examining aggregate cross-industry data from the U.S., Canada, and Great Britain has rarely reported any robust evidence of a union safety effect. (Morantz 2009).

Given its inherent hazardousness, the mining sector has attracted a disproportionate share of scholarly attention. Several recent historical studies suggest that unions exerted, if anything, a salutary effect on miners' safety during the early twentieth century (Fishback 1986; 1987:324; Boal 2009). However, empirical scholarship focusing on the decades since the passage of the Coal Act (1969) has very reached different conclusions. Boden (1977:116) and Connerton (1978), the first two empirical studies focusing on the latter Twentieth Century, examine data from 1973-75 and 1974-75, respectively. Although neither study focuses specifically on unionism, both include union status as a control variable and report that union mines experienced significantly more disabling injuries, *ceteris paribus*, than their nonunion counterparts. A landmark study on underground coal mine safety sponsored by the National Research Council (1982) also briefly addresses the relationship between unionism and mine safety. Examining data from 1978-80, the authors observe that the seemingly perverse positive relationship between union status and disabling injuries disappears when one focuses on the subset of injuries that are least susceptible to differences in reporting practices.[1] The authors also report that the lower fatality rate among union mines disappears when one accounts for mine size. On this basis, the NRC study suggests that there is probably no relationship at all between unionization and underground coal mine safety (NRC 1982:95-96).

Appleton and Baker (1984), the first study to focus specifically on the relationship between mine unionization and occupational safety, analyzes cross-sectional data from a single year (1978) culled from 213 mines in eastern Kentucky and western Virginia. Controlling for several mine-specific covariates, the authors report that both total injuries and relatively serious injuries are significantly higher at union mines. They hypothesize that the union job-bidding system, and/or union miners' postulated lower job motivation and productivity, could explain these results. Several later comments (Bennett and Passmore 1985; Weeks 1985) critique Appleton and Baker's conclusions by pointing out limitations in their data and methodology.

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[1] "Intermediate" injuries, adjudged by the study's authors to be the least prone to reporting bias, are defined so as to comprise "all fatal and permanent disability injuries as well as all injuries resulting from roof/side falls, machinery, haulage, or electrical/explosive accidents" (NRC 1982:82). The report states, "The rationale for defining [the intermediate injury rate] rested on the belief that reporting inconsistencies would occur most frequently for the degree 3-5 material handling and slipping/bumping injuries. Consequently, for consistency in reporting, [the intermediate injury rate] is felt to lie somewhere between the [fatality and permanent disability rate], where reporting differences are felt to be negligible, and the [disabling injury rate], where they might not be. We thus regard [the intermediate injury rate] as a compromise measure of safety that includes ample numbers of injuries for most statistical purposes and provides for reasonably good consistency between mines in the reporting of injuries" (NRC 1982:83-84).

In short, prior scholars have generally reported a *positive* relationship, if any at all, between union status and reported mining inuries during the modern era. There are, however, several compelling reasons to question the accuracy and contemporary relevance of these findings.

First, as Appleton and Baker (1984:140) point out, the accident reporting system in use before 1978 suffered from extremely poor reporting practices, and therefore underreporting of injuries by nonunion mines could have biased the results of Boden (1977) and Connerton (1978).

Second, most prior scholarship relies upon data that is geographically restricted, highly aggregated, time-invariant, or otherwise small in sample size. For instance, the 213 mines analyzed in Appleton and Baker (1984) comprised less than 10% of all coal mines that were active in 1978.

Third, all of the statistical analysis in prior studies relies on ordinary least squares regression modeling. Under standard assumptions, Poisson and negative binomial models are known to yield less biased estimates, and therefore have become the preferred approach for analysis of “count data” such as injuries and fatalities (Cameron and Trivedi 1998:1-3).

Finally, the labor strife that characterized most of the 1970s, which included periodic strikes and work stoppages, may have limited unions’ capacity to improve safety practices. Although Appleton and Baker limit their study of bituminous mining to what they characterize as a single “non-strike year” in the hopes of circumventing this problem, government statistics indicate that 414 bituminous coal mine strikes took place in 1978 and that the national labor-management climate remained highly adversarial (Staats 1981: 12-25; Darmstadter 1997: 27-31). Moreover, even if unions were relatively ineffectual during the 1970s, their impact may have changed in recent decades, as the UMWA become more familiar with MSHA’s regulatory procedures and expanded the scope of its internal health and safety programs (Weil 1994: 197).

In short, analysis of more recent data may not only bear more directly on unions’ contemporary relevance, but may also yield more credible estimates of unions’ true long-term effect. To my knowledge, no study has directly investigated the relationship between unionism and mine safety since 1980.[2]

The goal of the present article is to fill this gap in the literature by examining the 1993-

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[2] Reardon (1996) analyzes coal mining data from 1986-88, but he does not compare the probabilities of accidents occurring across union and non-union settings. Rather, he focuses on the probability that a *reported accident* has already resulted (or will likely result) in a fatality or permanently disabling injury.

2008 period with comprehensive, granular data and up-to-date econometric methods. I pose, in turn, a series of questions regarding the relationship between unionization and mine safety during this period. First, are there statistically significant disparities, *ceteris paribus*, between the rate of occupational injuries in union and nonunion coal mines? Second, do any such disparities persist if one focuses on measures of injury rates that are relatively impervious to reporting bias? Third, have such disparities remained constant, or have they fluctuated over time? Finally, given the inherent limitations of this observational study, what plausible inferences can be drawn regarding the true relationship between unionization and mine safety?

3 Data

The analysis presented in this paper relies primarily on MSHA's historical database from 1993-2008. This database includes quarterly data on the characteristics of each coal mine under MSHA's purview, and on each accident or injury that was reported to MSHA during this period. Although enormously detailed, the dataset has two important limitations. First and foremost, it contains little information on the union status of individual mines. Although MSHA originally collected data on unionization, the survey fell into disuse by the 1990s and historical records on union status were not preserved.[3] In 2007 MSHA conducted a one-time survey of mines in an effort to identify which were operating under union contracts, and in what year those mines became unionized. One can thus obtain a snapshot of the union status of U.S. mines in 2007, but it is impossible to determine from this source whether a particular mine was unionized in prior years (and, if so, for how long). Second, although the MSHA database contains comprehensive data on coal production and employment, it lacks information on each mine's geological characteristics (such as mean coal bed thickness), economic constraints (such as whether it is a subsidiary of a larger firm), and predominant technological approach (such as the relative prevalence of longwall, shortwall, continuous, and conventional mining techniques).

To remedy these shortcomings, I supplement the MSHA database with data from the Department of Energy's Energy Information Administration (EIA). The EIA database encompasses every mine in the U.S. that produces an appreciable amount of coal.[4] Most

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[3] Phone conversation with MSHA's George Fesak, Director of Program Evaluation and Information Resources, on 8/14/08.

[4] According to the EIA Coal Production and Preparation Report (Form EIA-7A), the EIA collects data on mines with operations that "produced and/or processed 10,000 or more short tons of coal and/or worked 5,000 hours or more

importantly for my purposes, the EIA database contains a "union ID" field indicating whether each mine was unionized in a given year and, if so, by which union.[5] The data also contain detailed information on the geological and economic characteristics of each mine, including the number of coal beds, the thickness of each coal bed, the value of captive and open production, productive capacity, recoverable reserves, and (for underground mines) the share of production attributable to conventional, continuous, longwall, shortwall, and other mining methods.[6] Merging the MSHA and EIA datasets allows me to assemble a detailed picture of safety-related outcomes at each union and nonunion coal mine in the country between 1993 and 2008. (Precise definitions of the variables included in this final dataset are presented, along with their respective sources, in the Appendix.)

I restrict the sample in several ways to ensure that the attributes of the union and non-union mines being compared are as similar as possible.[7] First, like most previous scholars, I confine my analysis to underground coal mines. (Surface coal mines, which have very different risk profiles and production characteristics, are also much less likely to be unionized.[8]) Second, I restrict the sample to bituminous coal mines, since none of the anthracite and lignite coal mines in the dataset operated under union contracts. Third, I drop any mine-quarters in which a mine reported zero coal production and/or zero hours worked.[9] (While injuries do occasionally occur

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during the reporting year." Of the MSHA mines that meet our criteria for inclusion, 28 (1.1%) do not appear in the EIA data. As a result, I am forced to exclude these from my analysis.

[5] The EIA considers this data unreliable prior to 1993. Phone Conversation with Vlad Dorjets, Lead Economist at EIA, on 2/25/2010. Since the EIA's union data are reported annually, whereas injury data are reported quarterly, I make the simplifying assumption that the union status recorded for a particular year applies to all four quarters of that year. In 1.5% of cases, the EIA data and MSHA's 2007 survey data contained conflicting information on union status. In these cases I rely on the EIA data, since it is based on written, mine-level surveys.

[6] Since some of these variables are considered trade secrets by the mines that provide them, I obtained these data on a confidential basis. EIA staff indicated that two of these variables, recoverable reserves and captive production, are unreliable for observations before 1998 (E-mail correspondence with William Watson, EIA, 12/7/2010), so I exclude them from the main analysis to preserve the chronological range of my sample. Results for the post-1998 period, in which I include both of these variables, are presented on the Companion Website, www.stanford.edu/group/coal_mining_safety. My findings are consistent with those presented in Section Five.

[7] As a robustness check, I refine the sample further using propensity score matching. The purpose of this procedure, as described by Ho et al. (2007), is to balance the distributions of the covariates across the "treatment" and "control" groups. The "balanced" sample consists of 1,087 mines for which the estimated likelihoods (or propensity scores) of unionization are similarly distributed across the union and nonunion subsamples. Results for this sample are available on the Companion Website; they generally echo the findings presented in Section Five, albeit often at a lower level of statistical significance.

[8] Results for the entire sample of coal mines (both underground and surface) are presented on the Companion Website.

[9] Out of 41,428 initial mine-quarters, 4,182 (10.1%) have either zero coal production or zero hours worked. An additional 1,242 mine-quarters (3.0%) are dropped either because they are missing mean coal bed thickness,

when a mine is not producing coal, the circumstances and triggering causes of such accidents are likely to differ from those that occur during active production periods.)

Once these restrictions are imposed, the final sample contains 2,414 mines, each of which was active, on average, for 15 of the 64 quarters under observation.[10] Figure 1 shows the geographical distribution of the mines in the sample. While the mines are spread across 17 states, 93% are located in the coal mining regions of Kentucky, Pennsylvania, West Virginia, and Virginia. Figure 2 displays the percentage of active mines that were unionized in each quarter. In keeping with the general trend for most U.S. industries, the unionization rate declined steadily, from 21.3% in 1993 to 9.6% in 2008.

Each injury report submitted to MSHA contains information on the nature and source of the injury, the body part(s) affected, the activity the employee was engaged in at the time of the incident, and the severity of the injury (which ranges from “first aid” to “fatality”). Using these fields, I tabulate four different injury counts: fatal injuries (“fatalities”), “traumatic” injuries[11], “non-traumatic” injuries[12], and total injuries. For each tabulation, I include only injuries that occured in the underground subunit of a mine.[13] Table 2 presents injury counts (and percentages) for both union and non-nion mines. Although fatalities uniformly comprise a very small fraction (0.3-0.5%) of total accidents, the relative share of non-traumatic injuries in total injuries is

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underground production, or mining type information, or because they report production despite being coded as abandoned.

[10] The underground coal mining industry exhibits high rates of entry and exit due to periodic fluctuations in demand and the costs of production. For example, out of the 864 mines that were active in the first quarter of 1993, fewer than 22% were still active in the first quarter 2000 and only 7.3% remained active in the final quarter of 2008. Similarly, out of the 477 mines that were active in the final quarter of 2008, only 36% had been active in the first quarter of 2000, and only 13% had been active in the first quarter of 1993.

[11] Because a “traumatic” injury, by definition, is caused by a discrete accident that a miner sustains during working hours, its work-relatedness is rarely in dispute as long as the miner’s account of the incident is deemed credible. In contrast, the diagnosis of non-traumatic injuries, such as cumulative or repetitive-motion injuries, often relies primarily on the patient’s self-report of subjective symptoms. Because the existence – let alone the work-relatedness – of some of these injuries may be difficult to verify using “evidence-based medicine,” the frequency with which such claims are filed and approved can vary widely across employers. The category of “traumatic” injuries, intended to encompass the subset of injuries that are the least prone to underreporting, was defined in consultation with Professor Mark Cullen, M.D., the Chief of Stanford University’s Division of General Internal Medicine. According to Dr. Cullen, the critical determining factor in determining whether or not an injury is reported is not the triggering *cause* of the injury, but rather the characteristics of the injury itself. More specifically, injuries of at least moderate severity, whose effects are readily visible, that are “traumatic” (rather than cumulative)[11] in ature are generally the least prone to reporting bias. The following injuries were deemed to meet these criteria: amputations; enucleations; fractures; chips; cuts and lacerations; punctures; burns/scalds; and chemical, electrical, and laser burns. So defined, “traumatic” injuries account for 36.5% of the injuries reported during the period of observation.

[12] All injuries that are not classified as “traumatic” injuries are classified as “non-traumatic” injuries.

[13] As a robustness check, I also estimate models in which *all* injuries occurring at underground mines – including those that occur above ground – are included in the injury counts. These results, presented on the Companion Website, do not materially change my findings.

markedly higher at union mines than at non-union mines (70.7% versus 59.6%).

Figure 3 provides a preliminary comparison of recent trends across union and non-union mines by plotting, respectively, the total frequencies of total and traumatic injuries (per 2,000 hours worked) from 1993 to 2008. Two general patterns are apparent. First, regardless of union status, the frequency of traumatic injuries has remained relatively constant over time, whereas the frequency of total injuries has declined steadily since the early 1990s. Secondly, although the direction and magnitude of the union-nonunion disparity fluctuated by year and injury type in the early 1990s, by the end of the decade, union mines were usually reporting lower injury rates than non-union mines regardless of the metric examined.

4 Methodology

To explore the relationship between union status and safety outcomes, I estimate negative binomial regression models in which the dependent variables are, respectively, total injuries, non-traumatic injuries, traumatic injuries, and fatalities.[14] The total number of hours worked is used as an exposure term, and standard errors are clustered on mine. In addition to a dummy variable indicating the presence of a union, I include several other covariates (listed in the Appendix) that, based on prior literature and/or conversations with industry stakeholders, were deemed likely to affect mine safety. Specifically, I control for the age of each mine; the size of its workforce; the state in which it is located; its productivity; the percentage of coal extracted using each of five techniques (shortwall, longwall, conventional, continuous, and other); whether it is a subsidiary of a larger firm; which of its respective areas ("subunits") are actively producing or processing coal; the log of the total workforce employed by its "controller" (i.e., owner); the total number of coal beds that it contains; and the mean thickness of its coal beds. Table 1 presents descriptive statistics for each of these covariates.

Several prior studies by Weil (1987:181-84; 1991:23; 1992:124-25) suggest that unions' effects on workplace safety may vary by employer size. For example, unions at large and small facilities may differ in their capacity to exercise their "walkaround" rights during MSHA inspections; to form powerful health and safety committees; to independently conduct inspections; and to enforce open-door policies among safety and health personnel. To explore whether unions' impact varies by mine size, I fit several models including interaction terms

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[14] Tests of overdispersion consistently indicate that a negative binomial model is preferable to a Poisson model.

between union status and mine size quartiles.

For total, traumatic, and non-traumatic injuries, I use the most granular time period available, the "mine-quarter," as the unit of analysis. However, because fatalities are such rare events, using quarterly data is problematic when modeling fatality counts. There is often too little variation across observations to obtain valid estimates. Therefore, I use the "mine-year" as the unit of analysis in all fatality regressions.

By including a broader set of covariates than has been used in previous studies, I attempt to minimize omitted variable bias. Nevertheless, there are several potentially confounding characteristics of union and nonunion miners—such as disparities in miners' demographics and remuneration levels —for which I cannot control. These limitations, including their implications for the interpretation of my findings, are discussed in Section Six.

Other types of unobservable, mine-level heterogeneity could also bias my analysis. For example, unusually hazardous geological conditions may affect a mine's injury rate as well as the likelihood that its employees will vote for unionization. In theory, a promising way to control for unobservable heterogeneity across mines is to use (mine-level) fixed effects to explore whether a given mine's safety record changes in predictable ways when it ceases (or begins) operating under a union contract. In practice, however, estimating fixed-effects models in this context creates more identification problems than it solves. First, only a few underground coal mines (6.1%) changed union status during the period examined. Second, these mines seem to be highly unrepresentative of the population as a whole.[15] Any identification strategy predicated upon this idiosyncratic subgroup would likely yield biased estimates of unionization's true effect. In short, despite its intuitive appeal, a fixed-effects modeling approach appears ill-suited to the peculiarities of the mining industry during this period.[16]

Importantly, most of the statistical biases discussed in prior literature will tend, if anything, to attenuate unionization's measured effect. For example, virtually all scholars who

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[15] Industry stakeholders recounted that, in recent decades, mines that underwent changes in union status typically did so in the wake of adverse economic shocks, such as sudden changes in the regulatory environment. The data seem to bear out this claim. At least 59% of coal mines that became unionized, and 84% of mines that de-unionized, during the sample period experienced major disruptions (such as dramatic declines or total shutdowns of operations) during the same quarter in which the transition took place. Such operational discontinuities are likely to have exerted an independent effect on safety practices, making it difficult to empirically isolate the effect of (de-)unionization. Moreover, the unusually precarious environment in which unions were forced to operate before or after these transitions may have constrained their capacity to influence mine safety practices.

[16] Notwithstanding these significant methodological concerns, for the benefit of the interested reader, the Companion Website presents results from several mine- and controller-level fixed-effects models.

consider the possibility of selection bias have argued, on both theoretical and empirical grounds, that inherently hazardous mines are *more* likely to unionize (Brown 1995; Leigh 1982; Worrall and Butler 1983; Hirsch and Berger 1984; Hills 1985; Robinson 1988b; Robinson 1991). If this is correct, then because I cannot control for each mine's inherent perilousness, any estimates of unions' beneficial impact will be biased downward.[17]

Another type of bias that has received much attention in the literature, often referred to as "reporting bias," stems from the fact that injury reporting practices may differ across union and nonunion environments. For example, non-union miners may fail to report legitimate injuries due to a fear of reprisal from their employers. On the other hand, some unions may encourage, or at least facilitate, the reporting of fraudulent or exaggerated claims (Hirsch et al. 1997; Morse et al. 2003). Even in the absence of outright employer intimidation or employee fraud, institutional norms may differ regarding what "counts" as a compensable occupational injury. For example, Azaroff et al. (2002) suggest that an array of subtle attitudinal barriers that impede the detection and reporting of injuries are less pronounced in unionized workplaces, especially for injuries that are relatively minor and/or hard to diagnose. In apparent support of this hypothesis, Hirsch et al. (1997) and Morse et al. (2003) find that even among workers that self-report similar rates of occupational injuries, union workers are more likely to receive workers' compensation benefits. Here again, reporting bias will tend to diminish the measured impact of unionization.

Fortunately, my data enable me to explore the magnitude of reporting bias indirectly. I examine four different injury categories that differ in their relative susceptibility to this bias: non-traumatic injuries, total injuries, traumatic injuries, and fatalities. As illustrated in Figure 4, non-traumatic injuries are hypothesized to be the most prone to reporting bias because they (by definition) include cumulative injuries whose work-relatedness can be difficult to confirm. At the opposite end of the continuum are workplace fatalities, which are virtually impossible to hide from authorities and regulators. The remaining two measures – total and traumatic injuries – are expected to fall in between these two extremes. Total injuries are less prone to reporting bias than non-traumatic injuries, since they include fatalities and severe traumatic injuries as well as

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[17] One might imagine, alternatively, a form of adverse selection in which the *most* dangerous mines are the *least* likely to unionize. For example, if the most dangerous mines are the least profitable, and therefore the most likely to shut down in adverse economic conditions, workers may vote against unions for fear that any increase in marginal (or fixed) costs would trigger a mine shutdown. Alternatively, mine operators that invest the least in workplace safety may invest the most in (or become especially skilled at) defeating union certification elections. Although this form of adverse selection seems possible – especially in monopsonistic or oligopsonistic labor markets – I am unaware of any prior literature that confirms its existence.

minor cuts and scrapes. Traumatic injuries are hypothesized to be even less susceptible to reporting bias than total injuries, since they exclude cumulative injuries.

If there is significant reporting bias across union and nonunion mines, the union safety effect (if any) should appear strongest in the fatality rate models; somewhat weaker in the traumatic injury rate models; weaker still in the total injury rate models; and weakest of all in the non-traumatic injury rate models. In other words, I hypothesize that union status will predict more and more injuries as the focus of inquiry shifts from fatalities, to traumatic injuries, to total injuries, to non-traumatic injuries.

Although the paper summarizes my main findings, space constraints prevent me from reproducing detailed results from each and every model specification and robustness check that was performed. For the benefit of the interested reader, an ancillary website ("Companion Website") located at www.stanford.edu/group/coal_mining_safety presents a number of additional model specifications and robustness checks.

5 Results

Tables 3-7 present the study's main findings for the four different outcome measures described earlier: non-traumatic injuries, total injuries, traumatic injuries, and fatalities. To probe changes over time, each model is estimated separately on the entire sample (1993-2008) and on data from three discrete time periods (1993-1997, 1998-2002, and 2003-2008). For ease of interpretation, I transform each coefficient into an incident rate ratio (IRR), whereby a coefficient of 1 indicates no change at all in predicted injuries; coefficients between 0 and 1 represent a predicted fall in injuries (e.g. a coefficient of 0.97 represents a 3% decline); and coefficients greater than one represent predicted increases (e.g. a coefficient of 1.03 represents a 3% rise).

The baseline model results presented in Table 3, which capture the average or "net" effect of unionization across all mines, display a striking pattern. On one hand, unionization predicts a very sizable, robust, and statistically significant *increase* in non-traumatic injuries. (Across all time periods, the IRR point estimates imply an increase of at least 25%.) The results for total injuries are similar but more muted: although unionization predicts a significant and sizable (more than 25%) increase in total injuries in the mid 1990s, the disparity loses significance in later years, diminishing and even reversing direction after the turn of the century.

The traumatic injury results, on the other hand, present a very different picture; unionization now forecasts a sizable, increasing, and highly significant *decline* in traumatic injuries, with the IRR point estimates predicting a drop of more than 10% across all time periods. In the fatality rate models, unionization predicts an even larger drop in fatal injuries. For all years, the point estimate predicts a statistically significant decline of about 57% (with a 95% confidence interval of 33-72%). Interestingly, if one confines scrutiny to the traumatic injury models, the union safety effect appears to have intensified somewhat since the early 1990s.

In short, the baseline model results are broadly consistent with both of the hypotheses originally posed. First and foremost, unionization predicts a large and statistically significant decline in those mine accidents that are least vulnerable to reporting bias. Secondly, the dramatic extent to which unions' measured impact varies by injury type suggests that there are indeed significant discrepancies in reporting practices across union and non-union mines.[18] Unions' measured effect on traumatic injuries appears to have intensified around the turn of the millenium.

Tables 4-7 probe the extent to which the trends observed differ by mine size. Viewed in light of prior literature, the results are somewhat counter-intuitive. Most scholarship suggests that larger firms – regardless of union status – have the strongest intrinsic incentives to invest in workplace safety (Weil 1987:124-28, Genn 1993:220-230, Fenn and Veljanovski 1988:1065; Reilly et al. 1995:280; Ruser 1985:485; Frick and Walters 1998:368). Therefore, one might expect unions' impact on workplace safety to be the greatest in smaller mines. Yet Tables 6 and 7 reveal the opposite trend: unionization's depressive effect on traumatic and fatal injuries is the greatest and most robust among larger mines. Similarly in Tables 4 and 5, the rise in non-traumatic and total injuries associated with unionization is typically (although not universally) more pronounced in the two upper size quartiles. What explains this seemingly counter-intuitive finding? Perhaps unions are better equipped to influence workplace safety and injury reporting policies in mines that exceed a certain size threshold. For example, it may be difficult for unions in small mines (defined here as those with fewer than 28 employees) to establish active health and safety committees, to routinely conduct independent inspections, or to consistently accompany MSHA inspectors on their tours.

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[18] The fact that as noted in Table 2, traumatic injuries comprise a much smaller percentage of total injuries in union mines (29.3%) than in non-union mines (40.4%) might also be construed as circumstantial evidence of reporting bias.

Although not the focus of this study, the other covariates included as right-hand-side variables reveal several interesting patterns. Table 8 displays the full regression coefficients for all of the baseline models. Although many of the estimated effects mirror those of prior studies, some either conflict with previous estimates or illuminate relationships that prior scholarship has not fully explored. The Companion Website discusses these and other ancillary findings.[19]

6 Interpretation

Taken at face value, my results are broadly consistent with the hypotheses that unionization improved "real" mine safety levels (as reflected in traumatic and fatal injury rates) around the turn of the twenty-first century; that reporting bias confounds empirical identification of the union safety effect, especially when the outcome measures examined include minor and non-traumatic injuries; and that the union safety effect has become more pronounced since the early 1990s.

Yet several important questions remain. First, what is the likelihood that omitted variable bias has confounded my identification strategy?

One potentially consequential mine-level characteristic that I cannot observe is the age distribution of the workforce. Although some epidemiological literature on the frequency of accidents by age group suggests that younger and less experienced miners sustain more injuries on the job (e.g. Laflamme and Blank 1996), the scholarship is not unanimous on this point. (See, for example, Souza 2009). Based on a careful review of existing literature, Salminen (2004) reports a bifurcated pattern, in which young workers are more susceptible to non-fatal injuries and older workers are more prone to occupational fatalities. If the distribution of age or experience differs substantially across union and nonunion mines—and if such differentials independently affect miners' likelihood of sustaining traumatic or fatal injuries – this could bias my results. Unfortunately, demographic variables are unavailable at the mine level, making it

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[19] The robustness checks described on the website include the following: using propensity score matching to define a subset of homogenous mines, and replicating the same models on this subset; fitting several mine-level fixed-effects models on data obtained from those few mines that switched from union to nonunion status, or vice versa, during the sample period; including controller dummies as an independent variables; expanding the sample to include surface mines; fitting models in which three alternative subsets of injuries (intermediate injuries, fatalities excluding major disasters, and fatalities only from explosions and collapses) are the dependent variables; and including as independent variables two data fields that the EIA has only reliably collected since 1997 (recoverable reserves and percent captive production) on data collected since 1997.

difficult to verify the existence, let alone to estimate the magnitude, of such biases.[20] The only source that facilitates any age comparisons is the Current Population Survey (CPS), which includes questions regarding age, occupation, and union membership. Although the small sample size allows for only rough comparisons, the data do confirm that, at least since 1980, unionized miners have been significantly older than their nonunionized counterparts.[21]

Even so, this discrepancy in age seems unlikely to explain much of the estimated union safety effect, for two reasons. First, although the union-nonunion gap in the frequency of traumatic injuries widened markedly during the 1990s, the gap in the proportions of "young" miners (under age 30) at union and nonunion mines changed little during this period. Secondly, the union-nonunion differential in the proportion of miners that are over 50 years old widened considerably during the 1990s. If Salminen (2004) is correct that the likelihood of sustaining a fatal injury increases with age, one would expect fatality rates to have risen disproportionately in union mines, biasing my results downward. Yet if anything, unions' salutary effect on mining fatalities slightly intensified during this period.

Several stakeholders suggested that unionized miners are also somewhat more experienced than their nonunionized counterparts, and that total compensation (including fringe benefits) is higher at union mines, although both of these disparities have diminished in recent decades. Unfortunately, there are no data available with which to test the validity of these claims.[22]

In short, I cannot rule out the possibility that omitted variable bias – such as differentials

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[20] The decennial survey administered by the U.S. Census Bureau – even the "long" form administered to 5% of the population for the Public-Use Microdata Samples (PUMS) – contains no information on union membership. The U.S. Census Bureau's Longitudinal Employer-Household Dynamics Program (LEHD) does contain mine-level demographic data. However, the LEHD dataset excludes Kentucky and Pennsylvania, which contain nearly half (47%) of all underground, bituminous mines in the U.S., and data for West Virginia and Virginia – which contain an additional 46% of mines in our sample – are available only for 1997 onwards. At the present time, the LEHD dataset only includes data through 2004, although the Census plans to augment the LEHD with data through 2008 by the end of 2011. Additionally, since the Census Bureau and MSHA use different employer identifiers, merging these two datasets would pose significant challenges. (Interview with Angela Andrus, Census Research Data Center, February 9, 2011; Interview with Emily Isenberg at the LEHD Program, U.S. Census.)

[21] In 1990, for example, the typical (median) unionized miner was in his early 40s; by 2000, he was in his late 40s. In contrast, the median nonunion miner was about 40 in 1990 and about 45 in 2000. I use CPS Outgoing Rotation Group (ORG) survey data to derive these statistics, restricting the CPS data to observations within the Coal Mining Industry, in the labor force and not self-employed.

[22] The CPS does not ask any questions regarding the prevalence or magnitude of "fringe" benefits such as pensions or life insurance. Questions regarding job tenure are collected every other year as part of the January supplement, which typically includes about fifteen respondents from the mining industry, of whom only a handful belong to a union. Due to these extremely small sample sizes, one cannot draw any meaningful inferences regarding whether (and to what extent) the average tenure of union and nonunion miners has differed in recent years.

in age, experience, or total remuneration between union and nonunion mines – have partly confounded my analysis. Nevertheless, the scant information available on disparities in miner demographics do not correlate particularly well with the trends I observe in the data.

If my findings do in fact reflect genuine disparities in workplace safety, this raises a second important question: why do my estimates differ so sharply from prior literature? There are two possibilities.

First, it could be that a union safety effect has always existed, but has simply eluded detection due to data constraints and the methodological limitations of prior work. Although I cannot replicate my analysis on data from prior to 1993 (since the data no longer exist), when I analyze my own data using a methodology similar to that of Appleton and Baker (1984), the results are substantively very similar to those reported above.[23] Although far from conclusive, this replication exercise suggests that the union safety effect may indeed be a relatively recent phenomenon.

If so, then a final puzzle demands careful scrutiny: why didn't these same disparities emerge in the 1970s? Several possibilities merit investigation.

First, fluctuations over time in the stringency of MSHA's regulatory scrutiny may affect union and nonunion mines differently. For example, Weil (1987), examining data from the early 1980s, finds that union mines were subject to more stringent regulatory scrutiny.[24] If MSHA inspects union mines more intensively than nonunion mines, and if this differential has widened over time, this could help explain the observed trends.

Secondly, unions may have shifted their institutional priorities near the turn of the century, consciously choosing to forfeit potential wage increases in exchange for enhanced levels of workplace safety. CPS data do show some convergence in median (real) wages of union and nonunion miners since the early 2000s. However, there are several reasons to doubt that the UMWA's leadership has deliberately pursued such a strategy.[25]

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[23] See the Companion Website for a detailed description of my attempt to replicate Appleton and Baker's methodology using the more recent dataset.

[24] Weil (1987) finds that union mines are more likely to designate employee representatives; receive more frequent MSHA inspections of longer average duration; are granted shorter periods in which to abate violations; are granted fewer abatement extensions; receive more citations per inspection; pay higher penalties per violation; and are less successful in reducing penalty amounts through MSHA's internal administrative appeals process than non-union mines (pp. 120-185).

[25] First, according to the UMWA leadership, the disparity in benefits between union and nonunion miners has progressively widened even as the gap in hourly wages has narrowed. Therefore, they claimed, the true overall disparity in union/nonunion compensation has changed little in recent years. To the best of my knowledge, this

Finally and more subtly, it may have taken time for the UMWA's leadership to train a cadre of union members capable of effectively exercising their statutory and contractual rights. In the words of one union official, "It can take a generation to institutionalize a robust safety culture and build a corps of experienced miners who can train the newcomers."[26] The labor strife that characterized much of the 1970s (and to a lesser extent the 1980s) likely impeded unions' capacity to enact meaningful changes. Weil (1994:199-200) has identified the election of Rich Trumka in 1982 to the presidency of the UMWA as a critical turning point, after which the union prioritized and funded the training of health and safety committee members. By the late 1980s and early 1990s, under the leadership of Joseph Main, the UMWA's Department of Health and Safety took more systematic measures to train its rank and file, such as the institution of local union training programs.[27] In short, changes in the leadership and institutional focus of the UMWA during the 1970s and '80s, designed to increase the union's long-term impact on mine safety, may not have come to fruition until the 1990s.

7 Conclusion

Although the United Mine Workers of America has always been a vigorous advocate for miners' safety, prior empirical literature has failed to detect any evidence of a union safety effect on injury or fatality rates. If anything, prior scholarship has reported a puzzling negative relationship between unionization and mine safety during the 1970s, the decade immediately following the Coal Act's passage. This study uses more comprehensive data and updated statistical methods to re-examine the relationship between unionization and mine safety. I focus on the 1993-2008 period, for which reliable mine-level information on union status is available, and use a variety of techniques to mitigate potential sources of bias.

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assertion cannot be tested with available data. (Telephone conferences with Brian Sanson, May 21, 2010; and Phil Smith, May 28, 2010.) Second, the UMWA's leadership explained that young miners that began entering the workforce in large numbers in the first decade of the 21st century are much less likely to have family members who are miners, or to have grown up in "mining towns" where explosions and collapses are part of the collective memory. As a result, they show comparatively little interest in safety issues. As one official put it, "it has become very difficult to organize on safety issues." (Telephone conference with Phil Smith, May 28, 2010.) Finally, CPS data show no significant convergence in *mean* real wages of union and nonunion miners. The recent convergence in *median* wages could be driven, therefore, by a growing similarity in the respective proportions of inexperienced miners on the payroll, rather than a more general congruity in pay scales. The extreme paucity of miners surveyed for the CPS sample makes it difficult to conclusively resolve the issue.

[26] Telephone interview with Phil Smith, UMWA, May 28, 2010.

[27] Weil (1987:200); Telephone interview with Michael Buckner, UMWA's Director of Research from 1981-2005, on March 3, 2011.

I find that unionization predicts a sizable and robust decline in both traumatic injuries and fatalities, the two safety outcomes that I argue are least prone to reporting bias. I construe these results as evidence for a “real” union safety effect in U.S. underground coal mining. At the same time, I find that unionization predicts higher total and non-traumatic injuries, lending credence to claims that injury reporting practices differ significantly between union and nonunion mines.

Interestingly, my analysis also suggests that the union safety effect on traumatic injuries has intensified in recent years. I propose several possible explanations for this trend, including changes in MSHA’s regulatory scrutiny, the increasing sophistication and professionalization of UMWA safety programs, an overall improvement in labor relations since the 1970s, and the growing competitive pressures faced by union leaders. Exploring the plausibility of such explanations—along with the precise mechanisms whereby unions affect safety outcomes—would be a promising avenue for future inquiry.

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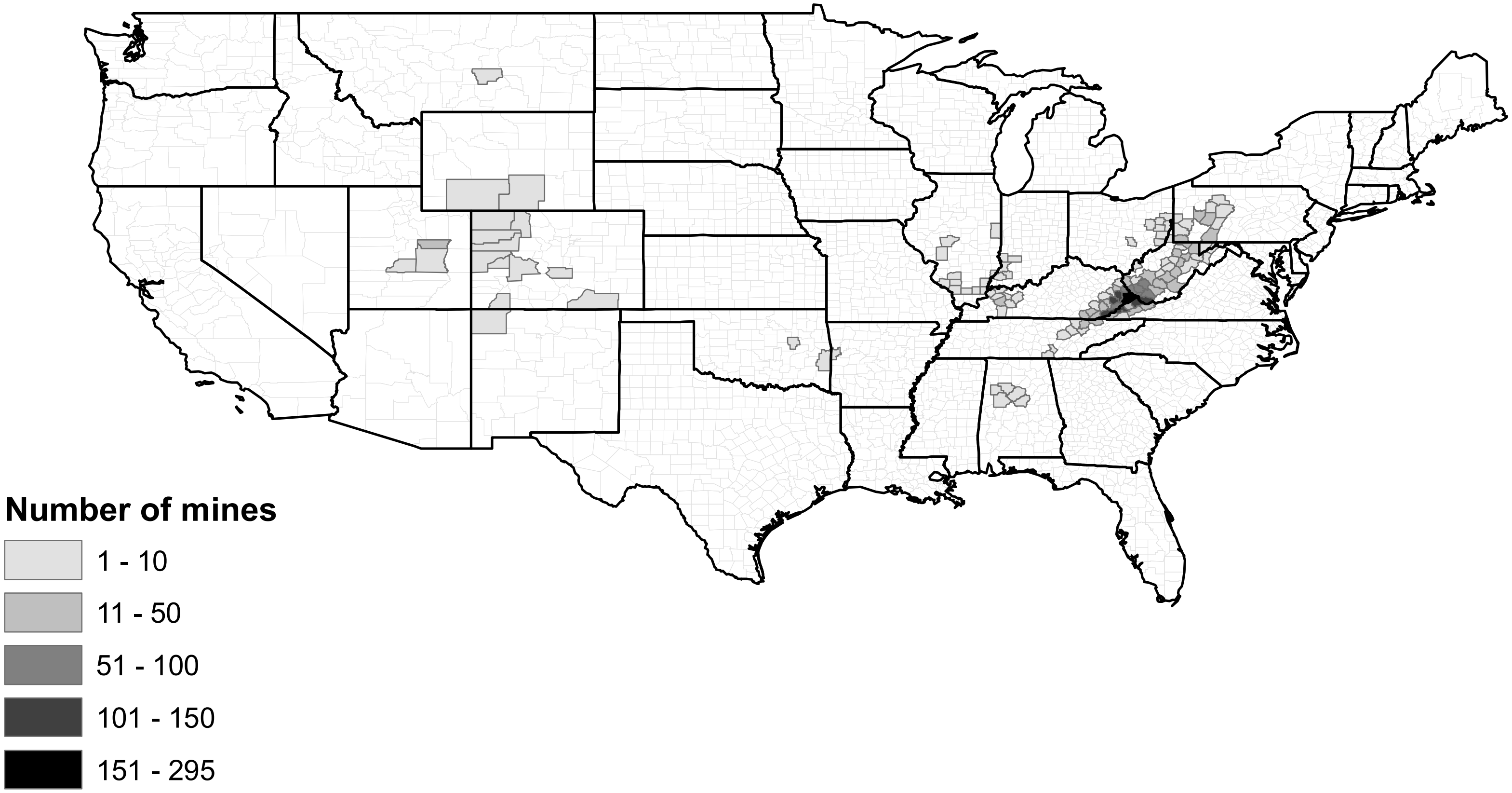
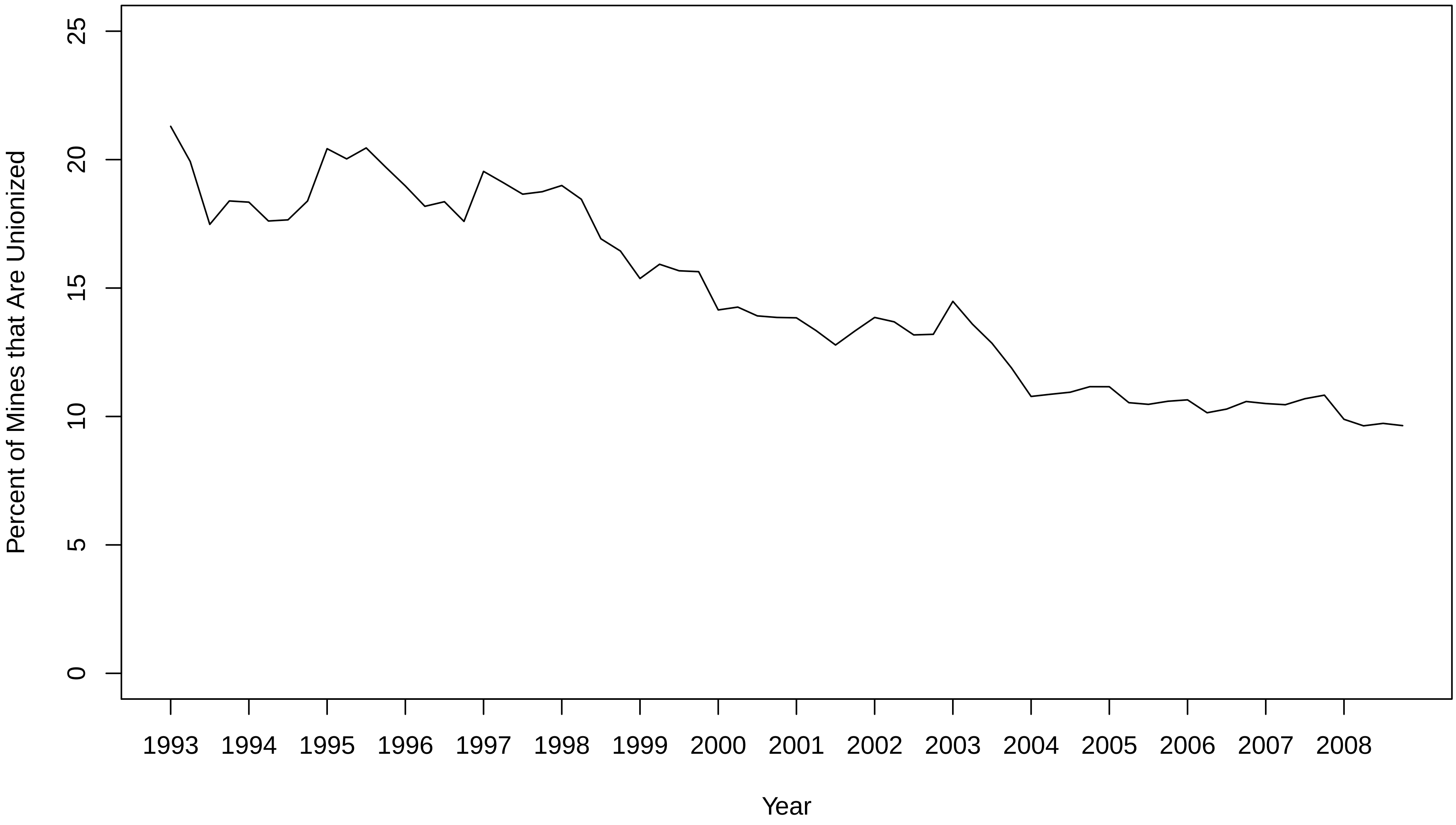
Figure 1. Underground Bituminous Coal Mines by County



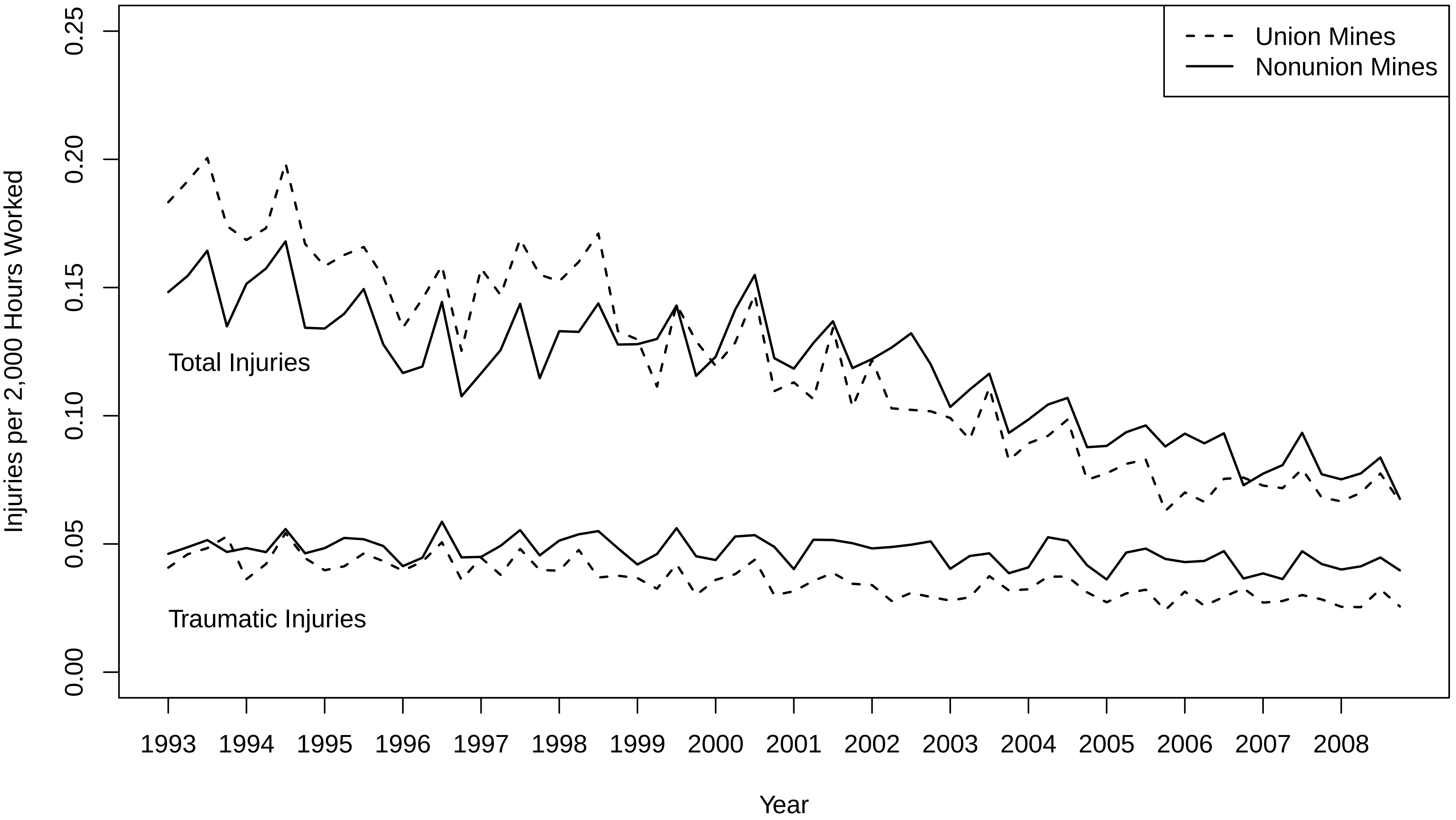
County information was provided by MSHA. The county-level mine counts incorporate all 2,414 underground bituminous coal mines that were active for at least one quarter between 1993 and 2008. Note that, due to high rates of entry and exit in the industry, no more than half of the sample was active in any given quarter.

Figure 2. Union Penetration



The figure above shows, for each quarter in the sample, the percentage of active mines that are listed as unionized in the EIA dataset.

Figure 3. Rates of Total and Traumatic Injuries



I calculate hourly injury rates by dividing the number of injuries of each type that occur across all union or nonunion mines in a given quarter by the total number of hours worked in the underground subunits of those mines during that quarter. I then scale these quantities by 2,000 to generate rates of injuries per 2,000 hours worked.

Figure 4. Susceptibility of Injury Types to Reporting Bias

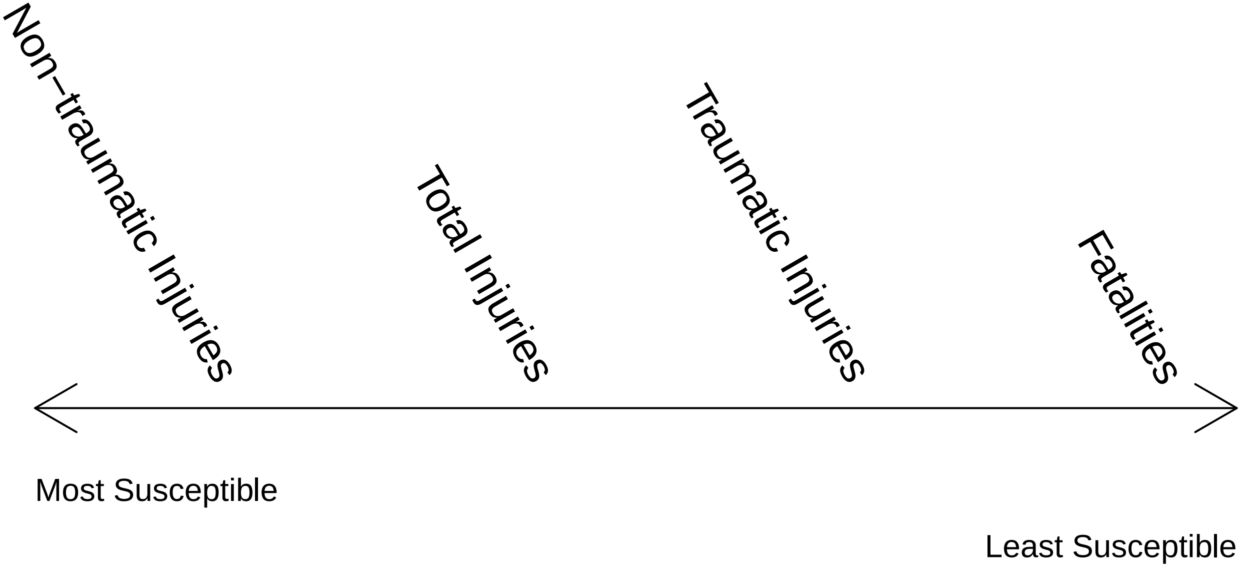


Table 1. Descriptive Statistics

Variable	Union Mines	Nonunion Mines	Variable	Union Mines	Nonunion Mines
Injury Rates (underground injuries per 2000 hrs)			**Geological variables, continued**		
Non-traumatic injuries	0.089	0.069	Mean coal bed thickness (yards)	1.724	1.322
Total injuries	0.126	0.115		(0.75)	(0.62)
Traumatic injuries	0.037	0.047			
Fatalities	0.0003	0.0006	**Detailed operational characteristics**		
			Controller employees[a]	6495.242	2380.413
Basic operational characteristics				(8268.61)	(4585.466)
Mine age (years)	33.143	18.205	Subsidiary indicator	32.5%	18.8%
	(18.76)	(15.03)			
Productivity[b]	8.514	7.962	Subunits contained[c]		
	(4.75)	(5.24)	surface	84.0%	81.5%
Employees	167.868	49.419	mill or prep plant	26.0%	3.7%
	(173.83)	(69.10)			
			Type of mine[d]		
1-14 employees	2051	22899	Conventional	0.080	0.177
	(37.2%)	(75.1%)		(0.27)	(0.38)
15-27 employees	877	4543	Continuous	0.663)	.782
	(15.9%)	(14.9%)		(0.43)	(0.41)
28-57 employees	821	1860	Longwall	0.253	0.033
	(14.9%)	(6.1%)		(0.38)	(0.16)
58+ employees	1764	1189	Shortwall	0.002	0.001
	(32.0%)	(3.9%)		(0.04)	(0.01)
Geological variables			Other	0.002	0.010
Coal beds	1.021	1.016		(0.04)	(0.09)
	(0.14)	(0.13)			
			Total sample size, in mine-qtrs	5513	30491

Notes:

This table reports aggregate injury rates, the mean and standard deviation of all continuous variables, and percentage breakdowns for categorical variables. The unit of observation is the mine-quarter.

[a] The natural log of this variable is used in all applicable regressions, but the mean and standard deviation of the unlogged variable are presented here.

[d] Productivity is measured in thousands of tons of coal per man-year.

[c] The variables are dummy variables indicating whether the given mine contains a subunit of the given type. The percentages presented above correspond to the proportion of the sample for which the variable equals 1. Because many mines have multiple subunits or no subunits, the percentages do not sum to 100.

[d] The variables are expressed as percentages that sum to 100% for each mine. For instance, a mine may be 70% conventional, 25% continuous, and 5% other. The vast majority of mines are either 100% conventional or 100% continuous.

Table 2. Injury Type Breakdown

Injury Type	All Mines: Frequency	All Mines: % of Total	Union Mines: Frequency	Union Mines: % of Total	Nonunion Mines: Frequency	Nonunion Mines: % of Total
Non-traumatic[a]	46064	63.8%	19657	70.7%	26407	59.6%
Total	72153	100.0%	27822	100.0%	44332	100.0%
Traumatic[b]	26089	36.2%	8169	29.3%	18025	40.4%
Fatality	302	0.4%	72	0.3%	230	0.5%

Notes:

This table reports the frequency of each injury type, as well as the share of total injuries that each category represents. Note that these categories are not mutually exclusive.

[a] The non-traumatic injury category is comprised of all injuries not classified as traumatic (see below). Note that the non-traumatic and traumatic injury counts sum to the total injury count.

[b] The traumatic injury category is comprised of the following: amputations, enucleations, fractures, chips, cuts and lacerations, punctures, burns and scalds, crushing, foreign bodies in eyes, dislocations, electric shocks, and chemical, electrical and laser burns. See footnote 11 for more details on this injury category.

Table 3. Effect of Union Status on Injury Frequency: Baseline Models

Injury Type	Coefficient	All Years	1993-1997	1998-2002	2003-2008
Non-traumatic	Union	1.332***	1.459***	1.269***	1.259***
		(0.07)	(0.08)	(0.11)	(0.10)
		[1.20, 1.47]	[1.31, 1.63]	[1.08, 1.50]	[1.07, 1.48]
Total	Union	1.105**	1.258***	1.071	0.954
		(0.05)	(0.06)	(0.08)	(0.07)
		[1.01, 1.20]	[1.14, 1.39]	[0.93, 1.23]	[0.83, 1.09]
Traumatic	Union	0.745***	0.878*	0.710***	0.641***
		(0.04)	(0.06)	(0.05)	(0.05)
		[0.67, 0.83]	[0.77, 1.00]	[0.62, 0.81]	[0.54, 0.76]
Observations		36004	14421	10537	11046
# of Union Mines		352	284	160	81
Total # of Mines		2414	1580	1075	924

Injury Type	Coefficient	All Years	1993-1997	1998-2002	2003-2008
Fatality	Union	0.434***	0.380***	0.316***	0.416**
		(0.10)	(0.12)	(0.12)	(0.18)
		[0.28, 0.67]	[0.20, 0.71]	[0.15, 0.68]	[0.18, 0.98]
Observations		10557	4322	3106	3129
# of Union Mines		352	284	160	81
Total # of Mines		2414	1580	1075	924

Significance levels: *** 1% ** 5% * 10%
IRR Estimates: The table reports IRR (incidence rate ratio) coefficients on the union in negative binomial regressions on various injuries types. Hours worked is used as the exposure term. Standard errors are shown in parentheses and are clustered at the mine level. 95% confidence intervals are shown in brackets.
Sample: The sample is restricted to underground, bituminous coal mines from the MSHA and EIA datasets that were active from 1993-2008. 5,424 mine-quarter observations with no production, production after abandonment, or other data problems were excluded. See page x for more details about the composition of the sample.
Dependent Variables: *Non-traumatic injuries* is a tally of all underground injuries that were not classified as "traumatic" by my definition. *Total injuries* is a tally of all underground injuries at the mine. *Traumatic injuries* is a tally of all underground, traumatic injuries at each mine. The traumatic injury category is comprised of the following: amputations, enucleations, fractures, chips, cuts and lacerations, punctures, burns and scalds, crushing, foreign bodies in eyes, dislocations, electric shocks, and chemical, electrical and laser burns. See footnote 11 for more details on this injury category. *Fatalities* is a tally of all underground fatalities at each mine.
Control Variables: All regressions include controls for basic mine attributes (state, mine age, mine size groups, quarter dummies), operational characteristics (productivity, mine size, mining type, number of controller employees, subsidiary status, and subunit indicators), and geological characteristics (number of coal beds, mean coal bed thickness). Year dummies are substituted for the fatality regressions.
Unit of Observation: The unit of observation is the mine-quarter for the non-traumatic, total, and traumatic injuries regressions. The unit of observation is the mine-year for fatality regressions.

Table 4. Effect of Union Status on Injury Rates: Non-traumatic Injuries

Model	Coefficient	All Years	1993-1997	1998-2002	2003-2008
Baseline Model	Union	1.332***	1.459***	1.269***	1.259***
		(0.07)	(0.08)	(0.11)	(0.10)
		[1.20, 1.47]	[1.31, 1.63]	[1.08, 1.50]	[1.07, 1.48]
Discrete Interaction	Union X Size Group 1	1.119	1.247	1.021	0.886
		(0.16)	(0.22)	(0.23)	(0.34)
	Union X Size Group 2	1.253**	1.326***	1.205	1.256
		(0.12)	(0.13)	(0.21)	(0.24)
	Union X Size Group 3	1.409***	1.231**	1.646***	1.713***
		(0.14)	(0.11)	(0.27)	(0.24)
	Union X Size Group 4	1.330***	1.631***	1.163	1.167*
		(0.09)	(0.12)	(0.11)	(0.10)
Observations		36004	14421	10537	11046
# of Union Mines		352	284	160	81
Total # of Mines		2414	1580	1075	924

Significance levels: *** 1% ** 5% * 10%
IRR Estimates: The table reports IRR (incidence rate ratio) coefficients on the union variables and union-size interaction variables in negative binomial regressions on non-traumatic injuries. Hours worked is used as the exposure term. Standard errors are shown in parentheses and are clustered at the mine level. 95% confidence intervals are shown in brackets for the baseline regressions.
Sample: The sample is restricted to underground, bituminous coal mines from the MSHA and EIA datasets that were active from 1993-2008. 5,424 mine-quarter observations with no production, production after abandonment, or other data problems were excluded. See page 7 for more details about the composition of the sample.
Dependent Variable: The dependent variable is a tally of all underground injuries that were not classified as "traumatic" by my definition. See footnote 11 for more details on this injury category.
Union Coefficient and Size Controls: In the *baseline model*, the union coefficient is a simple indicator variable, and size dummies for each group are included (the dummy for size group 1 is dropped). In the *model with size interaction terms*, the union indicator is supplanted by four indicator variables interacting union with each of the four mine size groups, while the original size dummies are still included. Refer to Table 1 for more information about the mine size groups.
Control Variables: All regressions include controls for basic mine attributes (state, mine age, quarter dummies), operational characteristics (productivity, mine size, mining type, number of controller employees, subsidiary status, and subunit indicators), and geological characteristics (number of coal beds, mean coal bed thickness).
Unit of Observation: The unit of observation is the mine-quarter.

Table 5. Effect of Union Status on Injury Frequency: Total Injuries

Model	Coefficient	All Years	1993-1997	1998-2002	2003-2008
Baseline Model	Union	1.105**	1.258***	1.071	0.954
		(0.05)	(0.06)	(0.08)	(0.07)
		[1.01, 1.20]	[1.14, 1.39]	[0.93, 1.23]	[0.83, 1.09]
Model with Size interaction terms	Union X Size Group 1	1.206	1.337*	1.114	1.039
		(0.14)	(0.20)	(0.23)	(0.33)
	Union X Size Group 2	1.146	1.217**	1.101	1.119
		(0.10)	(0.11)	(0.15)	(0.22)
	Union X Size Group 3	1.203**	1.133	1.389**	1.264**
		(0.10)	(0.09)	(0.19)	(0.15)
	Union X Size Group 4	1.062	1.318***	0.963	0.890
		(0.06)	(0.09)	(0.08)	(0.07)
Observations		36004	14421	10537	11046
# of Union Mines		352	284	160	81
Total # of Mines		2414	1580	1075	924

Significance levels: *** 1% ** 5% * 10%

IRR Estimates: The table reports IRR (incidence rate ratio) coefficients on the union variables and union-size interaction variables in negative binomial regressions on total injuries. Hours worked is used as the exposure term. Standard errors are shown in parentheses and are clustered at the mine level. 95% confidence intervals are shown in brackets for the baseline regressions.

Sample: The sample is restricted to underground, bituminous coal mines from the MSHA and EIA datasets that were active from 1993-2008. 5,424 mine-quarter observations with no production, production after abandonment, or other data problems were excluded. See page 7 for more details about the composition of the sample.

Dependent Variable: The dependent variable is a tally of all reported injuries that occurred underground at each mine. Because many underground mines have non-underground subunits, this will not necessarily be equal to the total number of injuries occurring at the mine.

Union Coefficient and Size Controls: In the *baseline model*, the union coefficient is a simple indicator variable, and size dummies for each group are included (the dummy for size group 1 is dropped). In the *model with size interaction terms*, the union indicator is supplanted by four indicator variables interacting union with each of the four mine size groups, while the original size dummies are still included. Refer to Table 1 for more information about the mine size groups.

Control Variables: All regressions include controls for basic mine attributes (state, mine age, quarter dummies), operational characteristics (productivity, mine size, mining type, number of controller employees, subsidiary status, and subunit indicators), and geological characteristics (number of coal beds, mean coal bed thickness).

Unit of Observation: The unit of observation is the mine-quarter.

Table 6. Effect of Union Status on Injury Frequency: Traumatic injuries

Model	Coefficient	All Years	1993-1997	1998-2002	2003-2008
Baseline Model	Union	0.745***	0.878*	0.710***	0.641***
		(0.04)	(0.06)	(0.05)	(0.05)
		[0.67, 0.83]	[0.77, 1.00]	[0.62, 0.81]	[0.54, 0.76]
Model with Size interaction terms	Union X Size Group 1	1.414**	1.605**	1.339	1.312
		(0.22)	(0.33)	(0.39)	(0.62)
	Union X Size Group 2	0.897	0.969	0.874	0.841
		(0.09)	(0.13)	(0.14)	(0.32)
	Union X Size Group 3	0.843**	0.948	0.877	0.678***
		(0.06)	(0.09)	(0.10)	(0.10)
	Union X Size Group 4	0.711***	0.835**	0.665***	0.632***
		(0.04)	(0.07)	(0.05)	(0.06)
Observations		36004	14421	10537	11046
# of Union Mines		352	284	160	81
Total # of Mines		2414	1580	1075	924

Significance levels: *** 1% ** 5% * 10%
IRR Estimates: The table reports IRR (incidence rate ratio) coefficients on the union variables and union-size interaction variables in negative binomial regressions on traumatic injuries. Hours worked is used as the exposure term. Standard errors are shown in parentheses and are clustered at the mine level. 95% confidence intervals are shown in brackets for the baseline regressions.
Sample: The sample is restricted to underground, bituminous coal mines from the MSHA and EIA datasets that were active from 1993-2008. 5,424 mine-quarter observations with no production, production after abandonment, or other data problems were excluded. See page 7 for more details about the composition of the sample.
Dependent Variable: The dependent variable is a tally of all underground, traumatic injuries at each mine. The traumatic injury category is comprised of the following: amputations, enucleations, fractures, chips, cuts and lacerations, punctures, burns and scalds, crushing, foreign bodies in eyes, dislocations, electric shocks, and chemical, electrical and laser burns. See footnote 11 for more details on this injury category.
Union Coefficient and Size Controls: In the *baseline model*, the union coefficient is a simple indicator variable, and size dummies for each group are included (the dummy for size group 1 is dropped). In the *model with size interaction terms*, the union indicator is supplanted by four indicator variables interacting union with each of the four mine size groups, while the original size dummies are still included. Refer to Table 1 for more information about the mine size groups.
Control Variables: All regressions include controls for basic mine attributes (state, mine age, quarter dummies), operational characteristics (productivity, mine size, mining type, number of controller employees, subsidiary status, and subunit indicators), and geological characteristics (number of coal beds, mean coal bed thickness).
Unit of Observation: The unit of observation is the mine-quarter.

Table 7. Effect of Union Status on Injury Frequency: Fatalities

Model	Coefficient	All Years	1993-1997	1998-2002	2003-2008
Baseline Model	Union	0.434***	0.380***	0.316***	0.416**
		(0.10)	(0.12)	(0.12)	(0.18)
		[0.28, 0.67]	[0.20, 0.71]	[0.15, 0.68]	[0.18, 0.98]
Model with Size interaction terms	Union X Size Group 1[a]	1.389	2.514		
		(1.51)	(2.96)		
	Union X Size Group 2[a]				
	Union X Size Group 3[a]	0.525		0.781	1.561
		(0.24)		(0.63)	(0.91)
	Union X Size Group 4	0.422***	0.434**	0.268***	0.262***
		(0.11)	(0.16)	(0.11)	(0.12)
Observations		10557	4322	3106	3129
# of Union Mines		352	284	160	81
Total # of Mines		2414	1580	1075	924

Significance levels: *** 1% ** 5% * 10%
IRR Estimates: The table reports IRR (incidence rate ratio) coefficients on the union variables and union-size interaction variables in negative binomial regressions on fatalities. Hours worked is used as the exposure term. Standard errors are shown in parentheses and are clustered at the mine level. 95% confidence intervals are shown in brackets for the baseline regressions.
Sample: The sample is restricted to underground, bituminous coal mines from the MSHA and EIA datasets that were active from 1993-2008. 809 mine-year observations with no production, production after abandonment, or other data problems were excluded. See page 7 for more details about the composition of the sample.
Dependent Variable: The dependent variable is a tally of all fatalities that occurred underground at each mine.
Union Coefficient and Size Controls: In the *baseline model*, the union coefficient is a simple indicator variable, and size dummies for each group are included (the dummy for size group 1 is dropped. In the *model with size interaction terms*, the union indicator is supplanted by four indicator variables interacting union with each of the four mine size groups, while the original size dummies are still included. Refer to Table 1 for more information about the mine size groups.
Control Variables: All regressions include controls for basic mine attributes (state, mine age, year dummies), operational characteristics (productivity, mine size, continuous and conventional mining type, number of controller employees, subsidiary status, and subunit indicators), and geological characteristics (number of coal beds, mean coal bed thickness).
Unit of Observation: The unit of observation is the mine-year.

[a] No fatalities occurred at any union mines in at any time in size group 2, after 1997 in size group 1, or before 1998 in size group 3. Therefore, these regressions do not have interpretable results.

Table 8: Effect of Union Status on Injury Frequency: Baseline Models, Full Covariate Results

	Non-trm	Total	Traumatic	Fatalities
Union indicator	1.332***	1.105**	0.745***	0.434***
	(0.07)	(0.05)	(0.04)	(0.10)
Mine age (years)	1.002	1.002*	1.001	1.008
	(0.00)	(0.00)	(0.00)	(0.02)
Productivity (000s of tons/man-year)	0.993*	0.998	1.002	0.958**
	(0.00)	(0.00)	(0.00)	(0.02)
Size Group 2 Indicator	1.081	1.086*	1.129**	0.809
	(0.05)	(0.05)	(0.06)	(0.33)
Size Group 3 Indicator	1.141**	1.165***	1.267***	1.102
	(0.06)	(0.06)	(0.07)	(0.43)
Size Group 4 Indicator	1.183***	1.241***	1.418***	1.115
	(0.07)	(0.07)	(0.09)	(0.44)
# of Coal Beds	1.036	1.005	0.966	0.680
	(0.09)	(0.08)	(0.08)	(0.33)
Mean Bed Thickness (yards)	1.014	0.999	0.973	1.046
	(0.03)	(0.02)	(0.03)	(0.13)
Subsidiary indicator	0.846***	0.894***	0.966	1.012
	(0.04)	(0.03)	(0.03)	(0.20)
Controller Employees	0.947***	0.976**	1.016	0.939
	(0.01)	(0.01)	(0.01)	(0.06)
Continuous mining proportion [a]	1.512**	1.481**	1.428**	1.941**
	(0.30)	(0.25)	(0.24)	(0.65)
Conventional mining proportion [a]	1.404*	1.366*	1.273	2.601**
	(0.28)	(0.23)	(0.22)	(1.09)
Longwall mining proportion [a]	1.026	0.984	0.974	--[a]
	(0.23)	(0.19)	(0.19)	
Shortwall mining proportion [a]	1.154	1.125	1.142	--[a]
	(0.32)	(0.25)	(0.26)	
Surface subunit indicator [b]	1.338***	1.270***	1.163***	1.405
	(0.05)	(0.04)	(0.05)	(0.39)
Mill or prep plant indicator	0.914	0.918	0.952	1.380
	(0.06)	(0.06)	(0.07)	(0.36)
Quarter/year fixed effects	Y	Y	Y	Y
State fixed effects [c]	Y	Y	Y	Y
Observations	36004	36004	36004	10557
# of Union mines	352	352	352	352
# of Total Mines	2414	2414	2414	2414

Significance levels: *** 1% ** 5% * 10%

This table reports the full regression output for each of my baseline models, using the full sample from 1993 to 2008. The unit of observation is the mine-quarter, except for the fatalities regression, which is at the mine-year level.

[a] These variables are expressed as fractions that sum to 1 for each mine (see Appendix for details). The vast majority of mines are either wholly conventional or wholly continuous. The non-traumatic-injury, total-injury, and traumatic-injury models all include indicators for “continuous,” “conventional,” “shortwall,” and “longwall,” mining types. The regressions on fatalities only include indicators for “continuous” and “conventional” mining as the regressions failed to converge when the other mining-type indicators were included.

[b] Many underground mines contain surface subunits where some production takes place.

[c] An expanded version of this table in which state fixed effects are reported is available on the Companion Website.

Appendix. Variable Dictionary

Variable Name	Variable Definition	Source
Dependent variables		
Non-traumatic injuries[a]	Total number of injuries not classified as traumatic	MSHA[b]
Total injuries[a]	Total number of injuries and fatalities reported	MSHA
Traumatic injuries[a]	A subset of injuries that are least prone to reporting bias	MSHA
Fatalities[a]	Total number of fatalities reported	MSHA
Basic mine attributes		
State dummies	1 if mine is located in a given state, 0 otherwise	MSHA
Mine age	Age of mine in years (top censored at 1950)	MSHA
Union indicator	1 if mine is unionized, 0 otherwise	EIA[c]
Quarter/year indicators	1 if observation is for a given year or quarter, 0 otherwise	MSHA
Basic operational characteristics		
Productivity	Thousands of tons of coal produced per man-year	MSHA
Mine size	1 if workforce falls in given size range, 0 otherwise Size ranges include 1-14 employees, 15-27 employees, 28-57 employees, and 58+ employees	MSHA
Detailed operational characteristics		
Ln (controller employees)	Natural log of size of workforce across all mines run by a given controller (owner)	MSHA
Subsidiary indicator	1 if mine is a subsidiary of a larger firm, 0 otherwise	EIA
Subunit indicator	1 if mine contains a given subunit, 0 otherwise Subunit types include *surface* and *mill or prep plant*.	MSHA
Mining type	Proportion of underground operation that is of a given Type, expressed as fraction between 0 and 1. Types include *conventional*, *continuous*, *longwall*, *shortwall*, and *other*.	EIA
Geological characteristics		
Number of coal beds	Number of coal beds at the mine site.	EIA
Mean coal bed thickness	The mean thickness of all coal beds at the mine, in feet	EIA

Notes:

[a] See page 8 for more precise definitions of these injury categories.

[b] Mine Safety and Health Administration.

[c] Energy Information Administration. These data were obtained on a confidential basis, as some of the operational and geological characteristics listed above are considered trade secrets.