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WHY DO UNIONIZED WORKERS HAVE MORE NONFATAL OCCUPATIONAL INJURIES?

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Most empirical studies have estimated a positive union-nonunion "injury gap," suggesting that unionized workers are ***more*** likely to have a nonfatal occupational injury than their nonunion counterparts. Using individual-level panel data for the first time, I study several explanations for this puzzling result. I find that controlling for time-invariant individual fixed effects already reduces the gap by around 40%. Some of the explanations that I study contribute in reducing this gap even further. I, however, do not find evidence of the gap becoming negative and the impact of unions on nonfatal injuries appears to be insignificant at best.

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"*There remain two puzzling results of the estimation of our model of coal mining injuries. The first of these is the fact that unionized mines have higher non-fatal accident rates than would be expected for non-union mines with the same characteristics.* [...]" (Boden 1977: 139)

"*The absence of any evidence of a significant union reduction of hazards runs counter to the conclusion one might draw on the basis of one's observation of actual union actions.*" (Viscusi 1979a: 231)

Most empirical studies suggest that unionized workers are *more* likely to have a nonfatal occupational injury than their nonunion counterparts. This result has puzzled researchers for more than three decades (as the quotes above illustrate[2]) since it clearly contradicts expectations based on anecdotal evidence and on unions' activities. This paper has two main goals: to provide new estimates of this impact using individual-level panel data for the first time, and to try to explain why unionized workers are more likely to have a nonfatal occupational injury.

On the first goal, my benchmark estimates using individual-level panel data suggest that union members are at least 34% more likely to have a nonfatal occupational injury than their nonunion counterparts. Moreover, for injuries with several days of incapacity, the injury gap between union and nonunion members seems to be considerably higher than 34%. I complement these results by presenting a summary of the empirical literature studying the impact of unions on occupational injuries. I find that unions are associated with more nonfatal occupational injuries in 27 of the 32 estimates that I consider in my summary. More surprisingly, of the five estimates that associate unions with less nonfatal occupational injuries, only one single estimate is statistically significant.

These empirical results are in stark contrast with the anecdotal evidence that attributes labor unions an influential role in improving occupational health and safety. Some authors have for example stressed the importance of unions in the development and passage of government legislation such as the Occupational Safety and Health Act in 1970 (Schurman et al. 1998: 134-6). Other prominent examples of unions' safety-enhancing activities include gaining recognition for occupational diseases caused by exposure to coal dust (Smith 1987), cotton dust (Botsch 1993), asbestos (Rosner and Markovitz 1991), radium (Clark 1997), and dibromochloropropane (Robinson 1991).

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[2] See also Chelius (1974: 727); Boden (1985: 500); Fishback (1986: 290); Fairris (1992: 205); Reardon (1996: 239); Smitha et al. (2001: 1007); and Robinson and Smallman (2006: 101).

In more general terms, labor unions are believed to influence occupational health and safety outcomes in several important ways. These include the provision of job hazard information, the protection of workers who refuse to accept hazardous assignments, and the assistance and representation of workers in accident compensation claims. Moreover, apart from influencing the regulatory process and its enforcement, unions bargain for the provision of protective equipment, for compensatory wages, and for the establishment of joint union-management health and safety committees.[3]

What could explain such a dramatic divergence between the anecdotal and the empirical evidence? Trying to provide an answer to this question is the second goal of this paper. I first explore the three explanations with the most consensus in the literature, which I label as "reporting", "selection", and "wages for safety." First, according to the **reporting** explanation, unions are believed to reduce the number of *actual* nonfatal injuries but also to increase the number of injuries that are *reported*. Since most data sources are not based on actual but on reported injuries, unions appear to be associated with more injuries in most of the cases. Second, proponents of the **selection** explanation argue that the positive association between unions and more nonfatal injuries is because unions are more likely to organize hazardous workplaces and not because unions are causing more injuries. Third, the **wages-for-safety** explanation suggests that unionized workers simply prefer higher wages than safer workplaces. Accordingly, unions campaign for higher wages but management reacts to this by reducing investment in occupational health and safety. As a result, unionized workers are paid higher wages at the expense of having more injuries.

Finally, I study two new explanations that have never been connected to labor unions before. The first one is called **moral hazard** and comes from the theoretical literature on occupational health and safety (Viscusi 1979b; Rea 1981; Carmichael 1986; and Lanoie 1991). The argument is that workers themselves might offset the benefits of a safer work environment by diminishing their own safety-enhancing efforts. Supported by the anecdotal evidence, I extend this explanation by arguing that it is labor unions that in many cases provide or bargain for the safer work environment. The increased safety and protection that unions provide enhance workers' feeling of safety, leading workers to adapt their behavior, for example, by working faster, becoming bolder, or by taking less safety precautions. This riskier behavior might then partially

[3]See Robinson (1991: 40); Beaumont (1983: 2); Viscusi (1979a: 230-1); Dorman (1996: 131-4); and Schurman et al. (1998).

offset the union safety efforts.

The second new explanation is called **distribution shifting**. In fact, according to my literature summary, most empirical studies associate unions with more nonfatal injuries but also with less fatalities. The explanation then is that the introduction of union-sponsored safety measures in a workplace might "convert" fatal injuries into nonfatal injuries. In the statistics, this will show up as an increase in nonfatal injuries but as a decrease in fatalities.

I explore all these five explanations mainly using panel data from the National Longitudinal Survey of Youth 1979 (NLSY79). Overall, I find little evidence for the wages-for-safety explanation, but it seems that each of the other explanations might explain part of the nonfatal injury gap. Moreover, my panel estimates show that simply controlling for time-invariant individual fixed effects already reduces the nonfatal injury gap by around 40%.

Evidence from the empirical literature

This section surveys the empirical literature investigating the impact of labor unions on occupational injuries. This literature usually estimates an equation of the form

$$INJURY = \beta\, UNION + \mathbf{X}'\boldsymbol{\gamma} + u, \tag{1}$$

where *INJURY* is some measure of the number or frequency of occupational injuries, *UNION* is a variable indicating union status, $\mathbf{X}$ is a vector of control variables, and u is the error term. The impact of unionism on occupational injuries is thus given by the estimate of β. Based on the anecdotal evidence and on the unions' activities briefly summarized in the introduction, one should expect unions to have a significant impact in reducing injuries, that is, the β coefficient is expected to be *negative* and significant.

Table 1 summarizes 25 studies estimating some variation of (1). As can be seen from the table, there is a remarkable heterogeneity between these studies, encompassing different countries, industries, years considered, data types, cross-sectional units, number of observations, and measures of the *UNION* and *INJURY* variables. The most important result for the purposes of this section is given in column 9. This column summarizes the type of *INJURY* variable used in each study and, in parenthesis, the sign and significance of the β coefficients, that is, the impact that the *UNION* variable had on the *INJURY* variable. Only the estimates that used a measure of fatal (FAT) or nonfatal injuries (NFI) for the *INJURY* variable were included in the table.[4] Note that some authors reported multiple estimates of β. This is typically done to experiment with different regression specifications, for sensitivity analysis, or when different dependent variables or data sets are employed. For each different *INJURY* variable, I chose the estimates that the author seemed to judge as the best, giving a total sample of 43 observations. Some key proportions of the final sample are summarized in the top panel of Table 2.

{{Place Table 1 about here}}

{{Place Table 2 about here}}

The bottom panel of Table 2 summarizes the number of estimates by type of injury. When considering all injury types, 5 estimates of β were negative and significant (at the 5% level), 22 were insignificant, and 16 were positive and significant. What can we conclude from this? Since, based on the anecdotal evidence and unions' activities, we were expecting β to be negative and significant, the results are clearly puzzling. Only in 5 of the 43 estimates, labor unions were significantly associated with fewer injuries.

A very interesting pattern, however, emerges if fatal and nonfatal injuries are considered separately. As the bottom panel of Table 2 (last two rows) shows, labor unions are in most cases associated with fewer fatalities but with more nonfatal injuries. In fact, labor unions are associated with more nonfatal injuries in 84% of the estimates and of these 60% are statistically significant. Even more surprising is the fact that the negative and significant association between unions and nonfatal injuries that we were expecting was found in only one single study! Moreover, as Table 1 indicates, the paradoxical positive association appears to be robust across countries, industries, years considered, data types, cross-sectional units, and measures of the union variable.

The most important conclusion that I draw from the existing empirical literature is, therefore, that the impact of unions on injuries appears to be different depending on the type of

[4] All studies containing only estimates using a different *INJURY* measure like the severity of the injuries, workers' compensation claims or benefits, or working conditions were not included in the table.

injury studied. While the association between unions and nonfatal injuries is in most cases positive, the association between unions and fatal injuries seems to be negative. My expectations regarding the impact of unions on injuries are only (partially) confirmed for fatal injuries. For nonfatal injuries, the empirical literature clearly contradicts most expectations based on anecdotal evidence.

New evidence from individual-level panel data

This section extends the empirical literature by providing estimates of the injury-union equation (1) using panel data at the individual level for the first time. The data come from the National Longitudinal Survey of Youth 1979 (NLSY79). This survey was administered for the first time in 1979, interviewing a sample of 12,686 American young men and women aged between 14 and 22 years. Until 1994, the cohort was interviewed every year. Since then, the survey has been conducted on a biennially basis. For this paper, the analysis has been restricted to the years for which information was available for all relevant *INJURY* and *UNION* variables. These years are 1988, 1989, 1990, 1992, 1993, 1994, 1996, 1998, and 2000, corresponding to the period in which the respondents were aged between 23 and 44 years.

The major advantage of this survey is that it provides detailed data on occupational injuries, on union status, and on an extensive set of questions on personal and job characteristics. The richness of the NLSY79 data makes possible to study the union impact on injuries at a depth that has not been possible before using other data sets. There are at least three reasons for this. First, as Table 1 shows, all previous estimations at the individual level were based on cross-sectional data. This type of data has several limitations. In particular, it only allows to make comparisons across individuals, and it is not possible to follow the same person over time. Second, in none of the data sets used before there was information on both *INJURY* and *UNION* variables. Researchers were obliged to match injury rates at the industry level from another data source to each individual for which they had information on their union status and other characteristics. The NLSY79, however, allows to calculate the probability of having an injury based on each individual's own experience and not on an average of the industry where they work. Third, the NLSY79 data set is the only one that has information on both union membership and on union coverage. This gives us two possibilities for measuring the *UNION* variable.

Definitions and sample means for the *INJURY*, *UNION*, and other variables used to estimate the injury-union equation (1) are reported in Table 3. The *INJURY* variable is called *NFI* and is based on the following question: “Since [date of last interview], have you had an incident at any job we previously discussed that resulted in an injury or illness to you?” Aside from the year 1991, this question was asked on every NLSY79 interview between 1988 and 2000. It is important to mention that the *INJURY* variable is defined for injuries of all severity degrees, that is, it comprises injuries that led to no time off as well as injuries that led to one or more days of incapacity. As Table 3, column 1 shows, in 6.2% of the cases, respondents reported having had a nonfatal work-related injury or illness between 1988 and 2000. The variables used to measure union status are union membership (*MEMBERSHIP*) and union coverage (*COVERAGE*). In general, not all workers covered by a union contract are members of a union. In fact, as Table 3, column 1 illustrates, in 18.7% of the cases, respondents reported being covered by a union contract while only in 14.2% of the cases, they reported being member of a labor union.

{{Place Table 3 about here}}

Table 3 also reports the sample means by dividing the sample into union and nonunion members (columns 2 and 3). Probably the most relevant comparison here is that, on average, union members are much more likely to report having had an injury than nonunion members (10.5% vs. 5.5%). As column 4 shows, this difference is significant at the 1% level. Obviously, this comparison is only suggestive, since it does not control for other potential differences between union and nonunion members. As columns 2 and 3 also reveal, there are indeed other important differences between the two groups, and almost all differences are statistically significant. For example, union members are on average less satisfied with their jobs, have a longer job tenure, work more hours per week, work in bigger firms, are more likely to be male, black, or Hispanic, and earn higher wages. Only in terms of health status (*HEALTH*), there appears to be no significant difference between union and nonunion members.

Table 4a reports the coefficient estimates from equation (1). As a benchmark against which to compare the fixed-effects estimates, columns 1 and 2 first report pooled OLS regressions. Columns 3 and 4 show the fixed effects estimates that exploit the panel nature of the data. Both models are estimated using the two different union status measures (*COVERAGE* and *MEM-*

BERSHIP) and always with nonfatal injuries (*NFI*) as the outcome variable. The estimated regressions include an extensive list of control variables containing measurements of the individuals' health, job satisfaction, tenure with employer and its square, firm size, hours per week worked, years of education, number of children, age, marital status, type of residence, and dummies for 8 years, 3 regions, 11 industries, and 11 occupations, for a total of 44 control variables. The OLS model also includes dummies for male, black and Hispanic[5] (see Tables 3 and 9 for complete definitions and summary statistics). Only the estimates based on linear probability models are reported in this paper. Other models, like the logit, yield very similar results.[6]

{{Place Table 4a about here}}

Table 4a gives a very clear picture of the impact of labor unions on nonfatal occupational injuries. Irrespective of the model or the *UNION* measure used, unions are clearly associated with *more* nonfatal injuries, after controlling for the extensive set of personal and job characteristics. The *UNION* coefficient is positive and highly significant in all estimations, confirming and reinforcing the pattern from the empirical literature summarized in the previous section. Also note that the estimates of the control variables are in accordance to expectations.

Turning to the interpretation of the *UNION* estimates, according to the OLS model, the probability of having an occupational injury is .0271 higher for covered workers and .0326 higher for union members. These values are not small. In fact, one way to put these values into perspective is by comparing them with the "nonunion baseline" values also reported at the bottom of Table 4a. The nonunion baseline is the average predicted injury probability of the nonunion workers. Adding the *UNION* estimates to the nonunion baseline values gives the average predicted injury probability of the union workers, which are .0563+.0271=8.34% for covered workers and .0567+.0326=8.93% for union members. The table also reports the "injury gap," which is the percentage increase in the injury probability for union compared to nonunion workers. The injury gap thus indicates that the probability of having an occupational injury increases by 48% for workers that change their union status from not covered to covered and by 57% if they change from nonmember to member.

[5]The gender and race dummies are not included in the fixed effects model as this model is not able to estimate the coefficient of time-invariant regressors.

[6]A Hausman test clearly rejects the random-effects model.

The fixed effects *UNION* estimates in columns 3 and 4 are much lower than the OLS estimates. The resulting injury gaps are reduced from 48% to 27% for covered workers and from 57% to 34% for union members. One important advantage of the fixed effects model is that it allows to control for the unobserved time-constant factors that affect *INJURY*. Even if the results from the two models are qualitatively similar, the OLS model clearly overestimates the *UNION* coefficient.

Table 4b reports estimates of the *UNION* coefficient similar to those of Table 4a but for the subsamples of males, females, blue collars and white collars[7]. The numbers in parenthesis and in square brackets are respectively the panel robust standard errors and the number of observations. All regressions include the full set of control variables. As in the previous table, the OLS model always overestimates the *UNION* coefficient, pointing to the importance of including individual fixed effects. Also, the *UNION* coefficient for the female subsample becomes insignificant at the conventional 5% level when moving from the OLS to the FE model. This is a result that I will explore more in depth in a section below. Moreover, the significance of the *UNION* coefficient for the blue-collars subsample is also reduced when using the *MEMBERSHIP* measure.[8]

{{Place Table 4b about here}}

Finally, note that the magnitude of my estimated union coefficients are difficult to compare to those from the literature. The reason is that previous studies have investigated the impact of unions on injuries at different aggregation levels (see Table 1, column 6), and only 5 studies have employed individual-level data. However, although in these 5 papers the union and the other regressors are measured at the individual level, the injury (dependent) variable is typically an injury rate, measured at the industry level. In other words, these 5 papers estimate the impact that a particular person is unionized on the *industry-level* injury rate. In contrast to this, the results that I report in this paper are based on each individual's own experience and not on an average of the industry where they work. For this reason, the NLSY79 allows me to directly compute the union-nonunion injury gap for the first time in the literature.

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[7]See Table 9 for the exact definition of "blue collar."

[8]In what follows, I will not report the estimates based on the *COVERAGE* variable. These estimates are qualitatively the same to those using the *MEMBERSHIP* variable but the resulting injury gap is always smaller.

Traditional explanations

The results from the previous two sections provide clear evidence of a positive association between labor unions and nonfatal injuries. In this section, we will try to understand why. As the last column in Table 1 shows, the literature has suggested several explanations for this paradoxical result. There are, in particular, three explanations that appear to be gaining some consensus among researchers. From the 25 studies summarized in Table 1, "reporting" was mentioned in 12 studies, "selection" in 6 studies, and "wages for safety" in 4 studies. This section will explore these three explanations in turn.

Reporting

The explanation most often mentioned in the literature is reporting. According to this explanation, unions are believed to reduce the number of *actual* injuries but also to increase the number of injuries that are *reported*. Since most data sources are not based on actual but on reported injuries, unions appear to be associated with more injuries in most of the cases.

There are at least two reasons why unions might increase the number of reported injuries. First, at the establishment level, unions might better monitor the reporting of injuries by employers. In fact, firms have an incentive to underreport injuries for different cost-saving reasons, for example, to reduce paperwork, to maintain lower insurance premia in the workers' compensation system, or to avoid triggering safety inspections from governmental authorities (Leigh et al. 2004: 11). Second, at the individual level, unionized workers might simply report more injuries because they might be less fearful of management retaliation. For instance, "[w]orkers who report health problems to supervisors may risk disciplinary action, denial of overtime or promotion opportunities, stigmatization, drug testing, harassment, or job loss" (Azaroff et al. 2002: 1422). Union members are often better protected against these types of retaliation.

Are the estimates based on the NLSY79 data set affected by less underreporting in unionized workplaces? I argue here that this does not seem to be the case. By construction, this data set is very different to all previous data sets that have been used in the literature to estimate the injury-union regression (1).[9] The NLSY79 data set is not based on information provided by firms, which have an incentive to underreport injuries, but by individuals during a private interview. Many different questions are asked to these individuals, which range from school attendance to family composition, and there is no apparent reason for them to give inaccurate information on potential occupational injuries. I use this different data construction as an argument against the "reporting" explanation and claim that the estimates in Table 4a do not appear to be affected by underreporting.

There is however one problem with this interpretation of the results. It is often the case that workers do not perceive some of the hazard risks in their workplace and better-informed unionized workers might be more likely to report an injury to the NLSY79 interviewers, simply because they are more aware of safety issues and not because they are having more injuries. In fact, some occupational injuries or illnesses take some time to manifest, and workers are not always sure if their workplace was at the origin of the injury or illness. One of the unions' safety activities is to provide workers with job hazard information (Donado and Wälde 2012). In that sense, if a unionized worker is more likely to report an occupational injury to the NLSY79 interviewers, the estimates of the injury gap in Table 4a would be biased upwards. Notice that this "information advantage" of union members is different to the "reporting" explanation from the literature. The literature uses reporting to explain that actual injuries, *of which workers and management are aware*, are not being reported because firms have cost-saving incentives to underreport them.

In any case, one possibility to assess if the results in Table 4a are biased upwards because of union workers' information advantage is to estimate the injury-union regression (1) for more severe injuries. In fact, it seems reasonable to assume that the unionized workers' knowledge advantage is lower, the more severe an injury is. More visible or severe injuries are more likely to be recognized by a worker that is not unionized. The information advantage bias should narrow, the more severe an injury is. Fortunately, the NLSY79 also asks respondents to indicate the number of work days missed due to the occupational injury. This variable, which I am calling *SEVERITY* (see Table 3 for definition and summary statistics), can be used to estimate the injury gap for different severity degrees.

Figure 1 plots the injury gaps that resulted from estimating regression (1) for increasing

[9]The only exception is one of the two estimates with nonfatal injuries as the outcome variable from Worral and Butler (1983). They use for this a measure of actual, not reported, injuries.

injury severity degrees. For at least zero days of incapacity, we obtain the injury gap of 34% that was already reported in column 4 of Table 4a. Moving to the right in the figure gives injury gaps corresponding to more and more severe injuries. For example, for at least 5 days of incapacity, the injury gap increases to 70%. For at least 60 days of incapacity, the injury gap is 47%. If the estimates were biased upwards due to an information advantage bias, we would expect a graphic with a falling trend. The graphic however exhibits no discernible trend, and the estimates do not appear to be biased, at least for the range of severity considered.[10] Restricting the sample to males, females, blue collars, or white collars leads to the same conclusion.

{{Place Figure 1 about here}}

Some caution should however be exercised with this *SEVERITY* variable since the number of work days missed due to the injury might not provide a perfect measure of severity. If a union worker is able to take more time off than a nonunion worker for the same type of injury, then the injuries for union workers will appear to be more severe. Restricting the sample to more severe injuries will still overestimate the "true" injury gap.

In conclusion, although the results presented in this section do not appear to support the reporting explanation, it is however not possible to rule out that reporting bias might still be an issue in other data sets and might explain part of the injury gap found in previous studies.

Selection

As the literature summary in Table 1 shows, the second most important explanation after reporting (REP) is selection (SEL). The selection explanation can be given two interpretations. The first interpretation is that the *UNION* variable might also be capturing the impact of workplace risk, suggesting that the *UNION* estimates are positive because union workplaces are riskier and not because unions are causing more injuries. The second interpretation is that the causality of *UNION* and *INJURY* might run in both directions. Unions might cause more injuries, but more injuries (or more hazardous workplaces) might also cause workers to form or join unions. Failing

[10] The *SEVERITY* variable does include information on injuries resulting in more than 90 days of incapacity. Since these types of injuries do not occur very often, the sample is very small and the estimates based on them are very imprecise.

to take into account this double causality might produce estimates that lead to the wrong conclusions. This section employs two different strategies to test each of these interpretations. The first strategy is to control for workplace risk. The second is to use instrumental variable methods to try to isolate the causal impact of unions on injuries.

Controlling for workplace risk

If the *UNION* variable is also capturing workplace risk, then the natural extension of the injury-union regression (1) is to include a new control variable that accounts for the average risk of the workplace where the worker is employed. In that way, the *UNION* coefficient can be "cleaned" from this influence.

Table 5 reports the estimates of the injury-union regression (1) that also control for workplace risk. In columns 2 and 3, the workplace risk variables are two questions from the NLSY79 that ask respondents to rate, on a scale of one to four, how dangerous (*DANGEROUS88*) and how unhealthy (*UNHEALTHY88*) their job were (see Table 3 for definitions and summary statistics). As shown by the sample means from Table 3 (columns 2 and 3), unionized workers rate on average their jobs as being more dangerous and unhealthier, pointing to the need for also including these variables in the regression. Unfortunately, these questions were only asked in 1988, and the estimates reported in columns 2 and 3 from Table 5 are OLS for this year only. Column 1 reports, as a benchmark, the same model as in columns 2 and 3 but without controlling for workplace risk.

{{Place Table 5 about here}}

Comparing the estimates from the benchmark model in column 1 with those of columns 2 and 3 shows that including the workplace risk variables clearly reduces the size of the *UNION* coefficient and it also reduces somewhat its significance. This suggests that the *UNION* variable might indeed be capturing some of the workplace risk. The two workplace risk variables are highly significant.

Now, in order to be able to exploit the panel nature of the NLSY79 data set by controlling for changes in workplace risk *over time*, I used a variable from a different data set that was available for all the years of the NLSY79 sample. The data for this new variable are based on the incidence rates from the Bureau of Labor Statistics (BLS) Survey of Occupational Injuries and Illnesses. The incidence rates are defined as the number of nonfatal occupational injury and illness cases per 100 full-time workers. These incidence rates are available for more than 200 industries for every year and represent a very good proxy of the average risk in each industry. These rates were transformed by multiplying them by 100 and by taking the log in order to obtain the final *INDUSTRYRISK* variable (see Table 3 for definition and summary statistics). Since the NLSY79 respondents also report the detailed industry where they work, it is possible to match the (transformed) BLS incidence rates with the NLSY79 respondents based on the industry codes provided in both data sets.[11]

The *UNION* fixed-effects estimates that also control for the *INDUSTRYRISK* variable are reported in Table 5, column 4. These estimates can be compared to those of Table 4a, column 4, showing that in this case the inclusion of the *INDUSTRYRISK* variable barely affects the *UNION* estimates. The *INDUSTRYRISK* variable turns out to be significant only at the 10% level.

One concern that might be raised regarding the results reported in column 4 is that the standard errors might be downward biased. The reason is that the *INDUSTRYRISK* variable is measured at the industry level, while all other variables are measured at the individual level. Assigning the same risk rate to workers within the same industry introduces correlation of the regression error terms for individuals in a given industry. Standard errors not corrected for this correlation might be underestimated. This problem is called the Moulton problem (Moulton 1986). In order to account for this, the model in column 5 clusters the standard errors not only at the individual but also at the industry level following the two-way clustering strategy proposed by Cameron et al. (2011). The results in column 5 show that clustering also at the industry level indeed increases somewhat the standard errors of the *UNION* and the *INDUSTRYRISK* variables. Even though the *UNION* variable remains highly significant, the *INDUSTRYRISK* variable is now

[11] The BLS data can be downloaded at ftp://ftp.bls.gov/pub/time.series/sh/ and at ftp://ftp.bls.gov/pub/time.series/hs/. Each NLSY79 respondent was matched to the (transformed) BLS incidence rates based on the respondents' reported industry code at the most precise level of industry breakdown that the two data sets allowed to. In many cases, this was at the three-digit level. Due to data limitations, however, it was not possible to assign every NLSY79 respondent to a particular industry-risk group. For example, the BLS survey does not provide incidence rates for the public administration sector. Despite these limitations, it was possible to construct more than 200 industry-risk groups for every year. The BLS data are based on the Standard Industrial Classification (SIC) System from 1972 and 1987, while the NLSY79 respondents are coded using the 1970 and the 1980 industry classification system of the Census of Population. The two data sets were merged using concordance tables that relate both classification systems to each other.

significant only at the 10.65% level. It is however interesting to note that excluding the industry dummies (see column 6) renders the *INDUSTRYRISK* variable highly significant. This suggests that the industry dummies were already capturing the over-time variation in workplace risk, without affecting the *UNION* coefficient by much.

To summarize this section, the results presented in Table 5 suggest that the *UNION* variable might indeed be capturing some of the workplace risk. The OLS estimates even indicate a reduction of around 45% in the injury gap. However, since including the workplace risk regressors still gives positive and significant *UNION* estimates, I conclude that there is more needed than this explanation alone to account for the full injury gap.

IV estimates

According to one of the definitions of endogeneity, the *UNION* variable is endogenous if it is correlated with the error term u in equation (1). This error term can be viewed as having two components, one time-variant ε_t and one time-invariant α, so that $u_t = \alpha + \varepsilon_t$, where t indexes time. The fixed-effects estimation approach that I used to estimate (1) already controls for union endogeneity if *UNION* is correlated only with the time-invariant component of the error.[12] In other words, if *UNION* is only correlated with α, the estimates presented in Table 4a are indeed giving the size of the *causal* union impact on injuries.

However, what if *UNION* is correlated with the time-variant component of the error? A stricter approach that controls for this type of union endogeneity is based on instrumental variable techniques. The challenge here is in finding an instrument for *UNION* that can also be used with the NLSY79 data set. One possibility is to use fringe benefits as Hildreth (2000), who instruments unionization with employer pension scheme provision. His rationale is that if unions are successful in obtaining fringe benefits for their members, then workers are more likely to join a union (see pp. 139-40). And indeed, there is some empirical evidence suggesting that unionized workers receive better fringe benefits than their nonunionized counterparts (Freeman and Medoff 1984, ch. 4). The problem with this instrument in my context is, however, that workplaces that can afford fringe benefits might also provide more safety. Moreover, it might also be argued that fringe benefits are

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[12]See Cameron and Trivedi (2005) for more details on this and on the estimation techniques used in this paper.

a consequence and not a cause of unionization.

Another instrument proposed in the literature is to use a lagged unionization variable as an instrument for current union status (see Chowdhury and Nickell 1985, Vella and Verbeek 1998, and Fernández-Val and Vella 2011). Estimating the impact of unions on wages (not on injuries), Vella and Verbeek (1998: 167) argue that lagged union status influences current status without having a direct impact on wages. Their argument is however less convincing if one considers the long-term impact of unions. For example, in my context of unions and safety, the instrument might be invalid if unions install durable safety equipment.

Despite the concerns with these instruments, in this section, I report the *UNION* coefficient estimates using fringe benefits and lagged *UNION* as instruments. Fortunately, the NLSY79 has detailed information on fringe benefits and it also allows to construct a lagged *UNION* variable. The fringe benefits that I use are dummies respectively equal to one if the employer made available a retirement plan (*RETIREMENT*), maternity/paternity leave (*MATERNITY*), or dental insurance (*DENTALINS*). Summary statistics of these variables are provided in Table 3.

Table 6 reports the estimates of the *UNION* coefficient using panel instrumental variables (IV) methods and adjusting for the full set of controls. Columns 1 to 3 show the fixed-effects IV estimates, each respectively using one of the instruments *RETIREMENT*, *MATERNITY*, or *DENTALINS*, while the estimates in column 4 use all these three instruments and are by fixed-effects two-stage least squares (2SLS). Column 5 reports the estimates by the so-called difference Generalized Method of Moments (GMM) using lagged levels of *UNION* as instruments. Finally, the estimates in column 6 are by the so-called system GMM and use lagged levels and lagged differences of *UNION* as instruments. The first-stage regressions for columns 1 to 4 are reported in the appendix (see Table 10).

{{Place Table 6 about here}}

In terms of the sign of the impact and its significance, Table 6 seems to give a very clear picture. Irrespective of the instrument or estimation technique used, all estimates are positive and significant. Moreover, the first-stage *F*-statistic clearly suggests that none of the instruments are weak, and the Hansen test for overidentified restrictions after 2SLS and GMM supports the validity of the instruments.

Now, even if Table 6 appears to give a very clear picture, there are two problems regarding these results that should be mentioned:

First, even if the Hansen test suggests that they are exogenous, the instruments that I am using might indeed be correlated with the error term in equation (1) and it might be difficult to justify their inclusion as explanatory variables in a structural model determining *UNION*. In fact, it might be much more realistic to assume that fringe benefits are a consequence of being unionized and not the opposite.

Second, as Table 6 shows, instrument selection has an important effect on the union coefficient values. The union coefficients range from .0214 to .2602. Although these results are not entirely satisfactory, they are not unusual in the empirical literature studying unions' effects on different outcome variables (such as wages, job quit intentions, and job satisfaction). In fact, several authors have documented before that estimates of unions' effects that account for union endogeneity fluctuate enormously and are in many cases very different to those that do not account for union endogeneity (Borjas 1979 (table 3); Freeman and Medoff 1982 (pp. 35-7); Lewis 1986; and Robinson 1989).

In conclusion, my estimates provide suggestive, but not definitive, evidence that unions are causing more nonfatal injuries. A definitive test of this explanation will require better instruments than those available in the NLSY79 data set.

Wages for safety

Wages for safety (WFS) is the third most important explanation in Table 1. Unfortunately, the literature has not been very specific about the theoretical model underlying this explanation. The most likely interpretation is that, given the same firm's production possibility frontier between wages and safety, unionized workers would choose higher wages in exchange for less safety. Lower safety levels might then lead to an increase in the number of injuries.

In addition to Duncan and Stafford (1980), several authors have suggested that unions have indeed put too much emphasis on wages at the expense of better safety measures. Bacow (1980: 101), for example, affirms that "[h]ealth and safety issues do not command a high position on union bargaining agendas because there is little political return on cleaning up the workplace; changes are often not recognized for years and the individuals most likely to benefit tend to be underrepresented." Nelking and Brown (1984: 117) affirm that "[w]orkers are often frustrated by the limited union influence over hazardous conditions. Preoccupied with bread and butter issues, some local officers regard health hazards as secondary." Moreover, Fishback (1986: 290) argues that "the [United Mine Workers of America] may have devoted more of their efforts to improving wages and organizing nonunion districts than to improving safety."

If unions have indeed put more emphasis on wages at the expense of safety, then one way to test this explanation empirically is by adding a wage variable to regression (1). If the inclusion of the wage variable reduces the union coefficient, then this would constitute support for the wages-for-safety explanation.

Table 7 reports the fixed effects estimates of (1) after adjusting for the full set of control variables. For convenience, column (1) simply replicates the estimates from Table 4a, column (4), that do not include the *WAGE* regressor. Column (2) reports the same estimates but restricting the sample to the observations for which the *WAGE* variable is available (but still without including the *WAGE* variable). The *UNION* estimates of the restricted and unrestricted samples in columns (1) and (2) are almost identical. Finally, column (3) shows the results if the *WAGE* variable is included. The results in columns (2) and (3) are comparable since they include the same observations. The definition and summary statistics of the *WAGE* variable are given in Table 3.

{{Place Table 7 about here}}

Comparing the estimates in columns (2) and (3) from Table 7 shows that the inclusion of the *WAGE* variable has slightly reduced the union coefficient. However, the *UNION* estimates in columns (2) and (3) are not statistically different from one another. Indeed, the *UNION* estimates in column (3) are within the 95% percent confidence interval of the estimates in column (2). In conclusion, the results in Table 7 do not provide support for the wages-for-safety explanation.[13]

New explanations

The results from the previous section suggest that none of the traditional explanations is

[13]One problem with these estimates is that the *WAGE* variable might be endogenous. It is however difficult to find a convincing instrument that is correlated with wages but uncorrelated with injuries.

enough to explain the positive injury gap between unionized and nonunionized workers. This section introduces two new explanations to the literature on unions and occupational injuries. The new explanations are respectively called “moral hazard” and “distribution shifting.”

Moral hazard and related explanations

There is one body of literature, not connected to labor unions, that argues that moral hazard from the workers’ side might mitigate the impact of better safety measures in reducing the injury probability. The argument is that workers themselves might (partially) offset the benefits of a safer work environment by diminishing their own safety-enhancing efforts (Viscusi 1979b; Rea 1981; Carmichael 1986; and Lanoie 1991). These authors however ignore the role of labor unions and suggest that it is the firms or the government that provide the safer work environment. Supported by the anecdotal evidence briefly summarized in the introduction, I extend this argument by stressing that it is labor unions that are at the origin of many occupational health and safety measures (Donado and Wälde 2012). However, the increased safety and protection that unions provide might enhance workers’ feeling of safety, leading workers to adapt their behavior, for example, by working faster, becoming bolder, or by taking less safety precautions. This riskier behavior might (partially) offset unions’ safety efforts.

This explanation is similar to Peltzman’s (1975) argument on why the introduction of auto safety measures (like seat belts or dual braking system) did not reduce highway death rates as intended. His explanation is that safety measures make drivers feel safer, and drivers adapt their behavior by driving faster or more carelessly than they would do without the safety measures. This change in behavior diminishes and maybe even offsets any positive effects of regulation. Several studies have found support for Peltzman’s explanation (see OECD 1990).

In order to make the argument clearer in the context of occupational health and safety, consider the following simple model. Suppose that p, the worker’s injury probability, depends on s, the safety measures provided by the firm, and on e, the worker’s own precautionary efforts. In the theoretical literature, s is always set by the firm or regulated by the government. However, since unions are often at the origin of many safety measures, we can instead let s be the outcome of a bargained agreement between the firm and the labor union or be a safety standard imposed by regulation due to unions’ influence (see Donado and Wälde 2012). Also suppose that e depends on

s since it is usually firms that first choose the level of safety measures and then workers that react by choosing how much precautionary effort to provide. The worker's injury probability is thus given by $p = p(s, e(s))$ and an increase in the firm's safety measures has the following impact on this injury probability

$$\frac{dp}{ds} = \frac{\partial p}{\partial s} + \frac{\partial p}{\partial e}\frac{\partial e}{\partial s}. \tag{2}$$

It is usually assumed that $\partial p / \partial s$ and $\partial p / \partial e$ are negative (that is, more firm's safety or more worker's effort reduce the injury probability), but the impact of more safety on workers' effort, $\partial e / \partial s,$ can be positive, negative, or equal to zero. In general, it can be shown that the sign of $\partial e / \partial s$ depends, at least in part, on whether e and s are substitutes, complements, or independent (e.g. Rea 1981, pp. 83-4). If $\partial e / \partial s$ is nonnegative (e and s are complements or independent), then an improvement in safety measures clearly reduces the injury probability in (2), that is, $dp / ds < 0.$ However, if $\partial e / \partial s$ is negative (e and s are substitutes), we speak of "moral hazard" and the sign of dp / ds depends on which of the two terms on the right-hand side of (2) dominates. It is difficult to think of a model specification in which the moral hazard effect is so strong that the second term dominates, leading to an overall increase in the injury probability. We might therefore expect that moral hazard might only be able to mitigate but not to offset (or more than offset) the impact of firm's safety measures on the workers injury probability.

However, if the moral hazard explanation is complemented with other arguments, then it might be possible to obtain an increase of the injury probability after an improvement in s. For example, some authors have shown theoretically that moral hazard can indeed lead to a higher injury probability in the presence of imperfect information concerning occupational risks (Rea 1981), or if the expected penalty for firms for noncompliance with governmental safety standards is extremely high (Viscusi 1979b: 121), or if the safety standards only provide incentives to change s while leaving the incentives to change e unaffected (Lanoie 1991: 94).

My estimates so far already provide some support for the moral hazard explanation. The reason is that I estimated most of my regressions by fixed effects. The fixed-effects estimator only relies on the so-called within variation, that is, the variation over time of a given individual (see fn.

11). My estimates are consistent with moral hazard since they imply that the injury probability of *the same* worker increases when the worker changes status from nonunion to union

In fact, when only cross-sectional data is available, it is only possible to estimate the union impact on injuries by comparing the group of unionized with the group of nonunionized workers in one single period of time. However, in order to find evidence of moral hazard, we still need to establish if *the same* worker is having more injuries after joining a union. The question is if there is an increase in the injury probability of a worker that in period one was not unionized and in period two joins a union. Has joining a union made any difference for this worker in terms of injury probability? This type of analysis can only be performed with panel data at the individual level, like the NLSY79, since only this type of data has information on the same person for two or more periods. None of the previous studies from the literature was able to perform such an analysis because of data limitations.

Now, one concern about my panel-data *UNION* estimates is that they might be contaminated by unobserved job characteristics that are correlated with union status, in particular, if these characteristics change when the worker changes his union status. One way to account for this possibility is to estimate the *UNION* coefficient after dropping all observations if a worker changes employer. The logic of this approach is that changes in the unobserved job characteristics should be smaller for job changers that stay with the same employer than for job changers that also change their employer.

The NLSY79 collects information on how many years a person has been working for the same employer and the point in time in which the person switches to a new employer. By restricting the sample, the *UNION* coefficient is identified using variation from union status changes for workers that remain with *the same* employer.

Table 8 shows the *UNION* estimates for two different ways of dropping the observations. In the first row, the regressions only use observations if the previous employer is the same as the current employer and drops all other observations.[14] For the estimates in the second row, the observations were dropped differently. The estimates exclude all observations if the respondent

[14] Restricting the sample to workers that do not change employer yields 54,259 observations, of which 9,749 are union members. The number of workers that change union status without changing employer is 2,225, which consists of 980 union joiners and 1,245 union leavers. Also note, as argued by Freeman (1984), that measurement error (misclassification of workers that join or leave a union) might bias panel estimates of union effects. In the context of this paper, measurement error in the changes of individuals joining or leaving a union might play a more important role for the subsample of workers that do not change their employer.

has been working for the same employer for less than 100 weeks (which roughly corresponds to two years). In other words, the estimates are for respondents that have been working for *the same* employer for two years or more. All estimates reported in the table are for the whole sample and the male, female, blue collar, and white collar subsamples. The numbers in parenthesis and in square brackets are respectively the panel robust standard errors and the number of observations.

{{Place Table 8 about here}}

As the table shows, the *UNION* coefficients for the whole sample are positive but significant only at the 10% level. However, while the coefficients are insignificant for females, they are still positive at the conventional 5% level for males. The female results can be compared to those of Table 4b, column 4, in which the significance of the female coefficients was already not very high. This suggests that unionization does not make much of a difference in terms of nonfatal injuries for females. Contrary to this, the results for the males subsample appear to indicate that, even when they stay with the same employer, male workers are more likely to have a nonfatal injury when they become unionized.

Now, it might also be possible that, *within the same employer*, some workers are moving from riskier production jobs to less risky management jobs when changing their status from union to nonunion. To explore this possibility, columns 4 and 5, re-estimate the model only for blue collars or white collars, respectively. The coefficients are now insignificant, maybe in part due to a reduction in the precision of the estimates because of the smaller sample size. They do however suggest that movement out of production jobs is a potential explanation for the injury gap. Also note that restricting the sample to male blue collars (results not reported) also gives insignificant coefficients.

To conclude this section, the evidence that I present here is not definitive but is consistent with the moral hazard explanation. However, this evidence is also consistent with other potential explanations. For instance, it looks as if moving from riskier production jobs to less risky management jobs might also be a valid explanation for the gap.

Distribution shifting

The bottom panel of Table 2 suggests that unions reduce fatalities but increase the likelihood of nonfatal injuries. From all the explanations that I have studied so far, only the "reporting" explanation seems to be consistent with this pattern. In fact, since fatal injuries are more difficult to hide, underreporting of this type of injuries is not usually a major problem in nonunionized workplaces. This means that when a workplace becomes unionized and unions reduce the number of fatal injuries (as one would expect), then the reducing impact of unions can be clearly seen in the data. As mentioned before, this is not always the case for nonfatal injuries since unions are expected to reduce the number of actual injuries but also to increase the number of reported injuries.

Being consistent with the pattern from Table 2 is clearly an advantage of the "reporting" explanation. Since all previous data sets employed in the literature are based on *reported* injuries, the reporting explanation might partially account for this pattern when employing those data sets. The only problem, however, is that the results that I presented in the "reporting" section do not seem to support this explanation since they suggest that unions are associated with more *actual* nonfatal injuries. It seems that there is more needed than the reporting explanation to be able to account for the pattern from Table 2.

Another explanation that I call "distribution shifting" looks at the impact that a reduction in fatalities might have on nonfatal injuries. Could it, for example, be possible that unions increase the likelihood of nonfatal injuries by reducing the likelihood of fatalities? In other words, might it be possible that better union-sponsored safety measures turn workplace accidents that would otherwise result in a fatality into "only" a severe nonfatal injury? Are unions converting fatalities into nonfatal injuries?

Although the NLSY79 data set is not appropriate for testing this explanation, two pieces of evidence can however be helpful in assessing its validity:

First, an estimation of the union impact on total injuries (fatal + nonfatal) should result in an insignificant *UNION* coefficient. This is only true if unions are effective in turning fatalities into nonfatal injuries by leaving the number of "no injuries" unaffected. The only two studies from the literature that have estimated the impact of unions on total injuries (and only for the coal mining industry) seem to be Boden (1977) and Appleton and Baker (1984). In both studies, the estimated union coefficient is positive and highly significant. As such, these results do not rule out this explanation. Instead, they suggest that this explanation alone is not able to account for the pattern in Table 2.

Second, a back-of-the-envelope calculation suggests that this explanation does not seem to fit the official data. In fact, based on published figures from the Bureau of Labor Statistics, there has been an average of around one workplace fatality for every 850 nonfatal injuries in the USA for the period 1994 to 2010.[15] If unions were able to reduce fatal injuries by Boal's (2009) estimate of 40%,[16] that is, if unions were able to turn 40% of the fatalities into nonfatal injuries, the percentage increase in nonfatal injuries would be less than 0.07%. My own estimates, however, point to a much higher union increase of 34% in the nonfatal injury probability. In other words, even if Boal's 40% estimate was very conservative, the number of fatalities to nonfatal injuries is simply too low for a reduction in fatalities to have any drastic impact on nonfatal injuries.

In conclusion, it seems that "distribution shifting" can only explain a very small fraction of the injury gap.

Conclusion

This paper begins by presenting a literature summary based on 25 empirical studies investigating the impact of labor unions on occupational injuries. The summary shows that most studies associate unions with more nonfatal injuries but with fewer fatalities. In particular, that unions are associated with more nonfatal injuries is puzzling since this result clearly contradicts expectations based on anecdotal evidence and on unions' safety-enhancing activities.

Using individual-level panel data for the first time, I re-estimate the impact of unions on nonfatal injuries and investigate if each of 5 potential explanations can account for this puzzling result. The explanations that I study comprise the three traditional explanations from the literature (reporting, selection, and wages for safety) plus two new explanations that I label "moral hazard" and "distribution shifting." My panel results show that controlling for time-invariant individual

[15]In 2001, for example, there were an estimated of 5,915 occupational fatalities and 5,215,600 nonfatal injuries in the USA, implying a nonfatal-to-fatal ratio of 882 for this year (see http://www.bls.gov/iif/).

[16]This is the estimated impact of unions on fatalities from Boal (2009), although for the coal mining industry and for the period 1897-1929.

characteristics reduces the nonfatal "injury gap" between union and nonunion workers by around 40%. Moreover, it seems that, except for "wages for safety," all other 4 explanations might each of them account for a fraction of the remaining injury gap. Overall, it appears that a combination of two or more explanations might even reduce the gap to zero, but it does not seem that the gap could ever become negative. I, therefore, conclude that the impact of unions in reducing nonfatal injuries might be at best insignificant.

Appendix

Definitions and summary statistics of the dummy variables

{{Place Table 9 about here}}

First-stage regressions

{{Place Table 10 about here}}

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Table 1. Studies investigating the impact of labor unions on occupational injuries

Study	**Country**	**Industry**	**Years**	**Data**	**Cross-sect. unit**	**Obs.**	**Union variable**	**Injury variable (impact)**	**Explanation given if impact positive and/or insignificant**
Chelius (1974)	USA	MA	67	CS	Establishments	2627	COV	NFI (ps)	
Boden (1977)	USA	CM	73-75	PA	Coal mines	6468(?)	MEM	NFI (pi, ps) FAT (ni)	NFI: 1) REP, 2) Labor-management strife, 3) WFS
Viscusi (1979a), app. F.2	USA	SE	69-70	CS	Blue collars	496	MEM	NFI (ps)	SEL
Leigh (1982)*	USA	SE	77	CS	Blue collars	369	MEM	NFI (ps)	
Worrall and Butler (1983)*	USA	SE	78	CS	Blue collars	2428-2608	MEM	NFI (pi, ps)	REP
Appleton and Baker (1984, 1985)*	USA	CM	79	CS	Coal mines	213	MEM	NFI (ps, ps, ps, pi)	1) Union’s job bidding system, 2) Low productivity, 3) Labor characteristics, 4) Other institutional factors
Boden (1985)	USA	CM	73-75	PA	Coal mines	5776	MEM	NFI (ps) FAT (ni)	NFI: REP
Fishback (1986)	USA	CM	12-23	PA	US states	198	MEM	FAT (ni, ni, pi)	1) WFS, 2) Public good aspects of safety may not have been important enough
Fishback (1987)	USA	CM	09-23	PA	US states	264	MEM	FAT (ni)	
Wallace (1987)	USA	CM	30-82	TS	CM industry	53	MEM	NFI (ns) FAT (ns)	
Garen (1988)	USA	SE	81-82	CS	Blue collars	2863	MEM	NFI (ni) FAT (ns)	
Fairris (1992)*	USA	PNS	69-70	CS	Blue collars	381	COV	NFI (pi)	1) SEL, 2) Union’s job bidding system, 3) WFS
Lanoie (1992)	Canada	SE	82-87	PA	Industries	140	MEM	NFI (ps)	REP
Nichols et al. (1995)	UK	MA	90	CS	Establishments	494	COV	NFI (ps)	SEL

(*continued on next page*)

Table 1. (*continued*)

Study	**Country**	**Industry**	**Years**	**Data**	**Cross-sect. unit**	**Obs.**	**Union variable**	**Injury variable (impact)**	**Explanation given if impact positive and/or insignificant**
Reilly et al. (1995)*	UK	MA	90	CS	Establishments	432	MEM	NFI (pi)	Variable used captures inadequately impact of unions on occupational health and safety
Reardon (1996)*	USA	CM	86-88	PA	Coal mines	10808	MEM	NFI+FAT (ni)	1) Union reduces probability of severe NFI by increasing reporting of less severe NFI, 2) Nonunion firms, in an attempt to remain nonunion, improve health and safety, 3) Union's safety committees and inspectors are not efficacious, 4) SEL
Hillage et al. (2000), app. C	UK	SE	98	CS	Establishments	1982	MEM	NFI (ps)	
Eaton and Nocerino (2000)	USA	PS	88-89	CS, PA	Workplaces	213	UR	NFI (pi, ni)	REP
Litwin (2000)*	UK	SE	98	CS	Workplaces	1640	MEM	NFI (ps)	1) WFS, 2) REP, 3) SEL
Thomason and Pozzebon (2002)	Canada	SE	95	CS	Firms	424	MEM	NFI (ps)	REP (?): Union workers file more compensation claims
Fenn and Ashby (2004)*	UK	SE	98	CS	Establishments	1636-1749	MEM	NFI (ps, pi, pi, ni)	1) REP, 2) More generous sick pay arrangements in union workplaces
Gray and Mendeloff (2005)	USA	MA	92-98	PA	Establishments	50276	MEM	NFI (ni)	
Robinson and Smallman (2006)	UK	MA, SV	98	CS	Establishments	1585-1597	MEM	NFI (ps, ps)	REP
Nichols et al. (2007)*	UK	MA	90	CS	Establishments	426	MEM	NFI (pi)	1) REP, 2) SEL
Boal (2008, 2009)*	USA	CM	02-29	PA	US states	210	MEM	FAT (ns)	REP
	USA	CM	1897-1928	PA	Coal mines	5779-7486	COV	NFI (pi) FAT (ns)	NFI: REP

Notes: * denotes that study focuses primarily on impact of unions on injuries. SHORTCUTS: **Industry**: coal mining (CM), manufacturing (MA), private nonagricultural sector (PNS), public sector (PS), several (SE), service (SV). **Data**: cross-sectional (CS), panel (PA), times series (TS). **Union and injury variables**: coverage (COV), membership (MEM), union resources (UR), nonfatal injury (NFI), fatal injury (FAT). **Impact of unions on injuries**: positive (p), negative (n), significant at the 5% level (s), and insignificant (i). **Explanation impact**: reporting (REP), selection (SEL), wages for safety (WFS).

Table 2. Key proportions and number of estimates by injury type of the 43 β estimates

Key proportions

Country		*Industry*		*Data*	
USA	70%	Coal mining	44%	Cross-sectional	58%
UK	26%	Manufacturing	12%	Panel	37%
Canada	5%	Other	44%	Time series	5%
Aggregation		*Union variable*		*Injury variable*	
Individual	16%	Membership	84%	Nonfatal injuries	74%
Establishment	65%	Coverage	12%	Fatalities	26 %
Industry	7%	Other	4%		
US states	12%				

Number of estimates

Injury type	*Negative significant*	*Negative insignificant*	*Positive insignificant*	*Positive significant*
All injuries	5	10	12	16
Fatal	4	6	1	0
Nonfatal	1	4	11	16

Notes: “Individual” includes blue collars, household heads, and workers. “Establishment” also includes coal mines, workplaces, and firms. In this table, “significant” means significant at the conventional 5% level.

Table 3. Definitions and sample means

Variable	Definition	Sample means			Difference nonmembers / members
		All	Nonunion member	Union member	
		(1)	(2)	(3)	(4)
NFI	1 if any work-related injury or illness	0.062	0.055	0.105	-0.050***
COVERAGE	1 if covered by union contract	0.187	0.051	1	-0.949***
MEMBERSHIP	1 if in union or employee association	0.142	0	1	--
HEALTH	1 if health limits kind of work	0.040	0.040	0.037	0.003
SATISF	Global job satisfaction on a scale of 1 to 4 (highest)	3.311	3.315	3.294	0.021**
TENURE	Total tenure in weeks with employer	212.8	196.8	308.9	-112.1***
TENURESQ	Square of tenure	95011	84408	158711	-74303***
FIRMSIZE	Log of number of employees at location of respondent's job	4.069	3.904	5.061	-1.156***
HOURSWEEK	Hours per week worked	40.32	40.26	40.72	-0.461***
EDUCATION	Highest grade completed	13.07	13.06	13.14	-0.079***
CHILDREN	Number of biological, adopted, or step-children in household	1.356	1.343	1.438	-0.096***
AGE	Age in years	31.98	31.87	32.65	-0.781***
MARRIED	1 if married	0.541	0.538	0.563	-0.026***
URBAN	1 if residence located in urban area	0.783	0.776	0.828	-0.052***
MALE	1 if male	0.516	0.504	0.590	-0.086***
BLACK	1 if black	0.280	0.270	0.340	-0.069***
HISPANIC	1 if Hispanic	0.180	0.178	0.192	-0.014***
SEVERITY	Number of work days missed due to *NFI*	23.81	21.44	31.23	-9.795***
DANGEROUS88	Job is dangerous on a scale of 1 to 4 (worst)	1.943	1.886	2.357	-0.471***
UNHEALTHY88	Unhealthy working conditions on a scale of 1 to 4 (worst)	1.793	1.734	2.224	-0.490***
INDUSTRYRISK	Log of injury and illness cases per 10000 full-time workers by industry	6.544	6.518	6.712	-0.194***
RETIREMENT	1 if employer made available retirement plan other than social security	0.612	0.566	0.871	-0.305***
MATERNITY	1 if employer made available materni-ty/paternity leave	0.642	0.614	0.799	-0.186***
DENTALINS	1 if employer made available dental insurance	0.591	0.549	0.829	-0.280***
WAGE	Log of hourly rate of pay	6.861	6.816	7.125	-0.309***

Notes: The statistics are for the years 88, 89, 90, 92, 93, 94, 96, 98, and 2000, except for *DANGEROUS88* and *UNHEALTHY88* that are only for the year 88. The complete definitions of the union status variables are: *COVERAGE*: "1 if wages set by collective bargaining, or if covered by union or employee contract, or if *MEMBERSHIP*=1". *MEMBERSHIP*: "1 if in union or employee association, 0 otherwise. Before 1994 also =0 if *COVERAGE*=0". In order to attenuate problems with measurement errors, I set *SEVERITY* as missing if *NFI* was missing or if *NFI*=0. Only variables for what the NLSY79 calls the "CPS job" or "job # 1" were used. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 4a. Pooled OLS and fixed-effects estimates of the injury-union regression

	Pooled OLS		**Fixed Effects**	
	(1)	(2)	(3)	(4)
COVERAGE	.0271		.0160	
	(.0037)***		(.0043)***	
MEMBERSHIP		.0326		.0201
		(.0044)***		(.0054)***
HEALTH	.1221	.1214	.0999	.0992
	(.0087)***	(.0086)***	(.0104)***	(.0103)***
SATISF	-.0105	-.0103	-.0098	-.0097
	(.0017)***	(.0017)***	(.0020)***	(.0020)***
TENURE	.0001	.0001	.0002	.0002
	(.0000)***	(.0000)***	(.0000)***	(.0000)***
TENURESQ	-10.4e-08	-10.2e-08	-13.2e-08	-13.2e-08
	(17.4e-09)***	(17.4e-09)***	(19.1e-09)***	(19.1e-09)***
FIRMSIZE	.0021	.0021	.0020	.0021
	(.0005)***	(.0005)***	(.0007)***	(.0007)***
HOURSWEEK	.0010	.0010	.0007	.0007
	(.0001)***	(.0001)***	(.0001)***	(.0001)***
EDUCATION	-.0029	-.0029	.0004	.0004
	(.0006)***	(.0006)***	(.0027)	(.0027)
CHILDREN	.0028	.0029	.0038	.0039
	(.0011)**	(.0011)***	(.0025)	(.0025)
AGE	.0002	.0002	.0032	.0035
	(.0005)	(.0005)	(.0041)	(.0040)
MARRIED	-.0041	-.0041	.0012	.0013
	(.0025)*	(.0025)*	(.0035)	(.0035)
URBAN	-.0024	-.0026	-.0062	-.0063
	(.0030)	(.0030)	(.0044)	(.0044)
MALE	.0049	.0050	--	--
	(.0028)*	(.0028)*		
BLACK	-.0212	-.0205	--	--
	(.0031)***	(.0031)***		
HISPANIC	-.0109	-.0106	--	--
	(.0035)***	(.0035)***		
Nonunion baseline	.0563	.0567	.0584	.0585
Injury gap	48%	57%	27%	34%
Observations	56855	56851	56855	56851

Notes: The table reports estimates of equation (1). The outcome variable is always *NFI* (see Table 3 for definition). All estimates are based on linear probability models and also include dummies for 8 years, 3 regions, 11 industries, and 11 occupations. The estimates in columns 1 and 2 are OLS and in columns 3 and 4 are fixed effects. All standard errors in parenthesis are panel robust (clustered at the individual level). The "nonunion baseline" is computed as the average predicted probability of the outcome variable using the estimated coefficients on the control variables. The "injury gap" is the percentage increase in the injury probability of nonunion member to union member. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 4b. Estimates of the injury-union regression for different subsamples

	Males		**Females**		**Blue collars**		**White collars**	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
COVERAGE	.0306	.0237	.0198	.0062	.0284	.0249	.0225	.0108
	(.0053)***	(.0061)***	(.0049)***	(.0060)	(.0068)***	(.0089)***	(.0040)***	(.0048)**
	[29707]	[29707]	[27148]	[27148]	[17840]	[17840]	[39015]	[39015]
MEMBERSHIP	.0340	.0252	.0275	.0135	.0298	.0179	.0299	.0205
	(.0062)***	(.0074)***	(.0061)***	(.0078)*	(.0077)***	(.0104)*	(.0050)***	(.0062)***
	[29702]	[29702]	[27149]	[27149]	[17830]	[17830]	[39021]	[39021]

Notes: The table reports estimates of the *UNION* coefficient in equation (1) for the subsamples "males", "females", "blue collars", and "white collars." The outcome variable is always *NFI* (see Table 3 for definition). All estimates are based on linear probability models and include the full set of control variables. The estimates in columns 1, 3, 5, and 7 are by OLS and in columns 2, 4, 6, and 8 are by fixed effects. All standard errors in parenthesis are panel robust (clustered at the individual level). The number of observations is reported in square brackets. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 5. Estimates of the injury-union regression controlling for workplace risk

	Benchmark88 (1)	*DANGEROUS88* (2)	*UNHEALTHY88* (3)	*INDUSTRYRISK_1* (4)	*INDUSTRYRISK_2* (5)	*INDUSTRYRISK_3* (6)
UNION	.0405	.0280	.0287	.0199	.0199	.0210
	(.0126)***	(.0124)**	(.0125)**	(.0062)***	(.0066)***	(.0066)***
Workplace risk		.0411	.0334	.0051	.0051	.0079
		(.0040)***	(.0041)***	(.0031)*	(.0032)	(.0026)***
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects?	No	No	No	Yes	Yes	Yes
Industry dummies?	Yes	Yes	Yes	Yes	Yes	No
Observations	7251	7209	7207	49392	48397	48397

Notes: The table reports estimates of the *UNION* coefficient in equation (1) only for the union status measure *MEMBERSHIP*. The outcome variable is always *NFI*. All estimates are based on linear probability models. The estimates in columns 1 to 3 are by OLS and are only for the year 1988. All estimates include the full set of control variables and one of the working condition variables (*DANGEROUS88*, *UNHEALTHY88* or *INDUSTRYRISK*). The estimates in columns 1 to 3 also include *MALE*, *BLACK* and *HISPANIC* (see Table 3 for definitions). The estimates in column 4 to 6 are by fixed effects and also include 8 year dummies. The estimates in column 6 do not include industry dummies. Standard errors in parenthesis in columns 1 to 3 are robust, in column 4 are panel robust (clustered at the individual level), and in columns 5 and 6 are panel robust (clustered at the individual and at the industry level). *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 6. Estimates of the injury-union regression controlling for union endogeneity

	IV1	**IV2**	**IV3**	**2SLS**	**diff GMM**	**sys GMM**
	(1)	(2)	(3)	(4)	(5)	(6)
UNION	.2094	.2602	.1549	.1882	.0289	.0214
	(.0524)***	(.0868)***	(.0572)***	(.0438)***	(.0159)*	(.0091)**
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	52081	49238	52850	48332	27065	42908
Instrument(s)	*RETIREMENT*	*MATERNITY*	*DENTALINS*	*RETIREMENT* *MATERNITY* *DENTALINS*	Lagged levels *UNION*	Lagged levels and differences *UNION*
F-statistic	235.96	128.47	198.80	105.97		
p-value	0.000	0.000	0.000	0.000		
Hansen				3.098	14.42	16.94
p-value				0.212	0.809	0.911

Notes: The table reports estimates of the *UNION* coefficient in equation (1) controlling for union endogeneity and only for the union status measure *MEMBERSHIP*. The outcome variable is always *NFI*. All estimates are based on linear probability models and include the full set of control variables. The estimates in columns 1 to 4 also include 8 year dummies. The GMM estimates in columns 5 and 6 exclude observations for uneven years. For the first differences, on which GMM estimates are based, I assumed that even years were consecutive, and only 6 year dummies were included as additional controls. Standard errors in parenthesis are panel robust (clustered at the individual level). The table also reports the first-stage *F*-statistic for weak instruments and the Hansen's instrument validity test. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 7. Wages-for-safety estimates

	Unrestricted sample	**Restricted sample**	**Restricted sample with wages**
	(1)	(2)	(3)
UNION	.0201	.0202	.0194
	(.0054)***	(.0055)***	(.0055)***
WAGE			.0066
			(.0025)***
Controls?	Yes	Yes	Yes
Person fixed effects?	Yes	Yes	Yes
Observations	56851	55472	55472

Notes: The table reports the coefficient estimates of equation (1). Column (1) replicates the results from Table 4a, column (4). Column (2) reports similar estimates that restrict the sample to the observations for which the *WAGE* variable is available. Finally, column (3) reports the estimates that include the *WAGE* variable. In all three columns, the union variable is *MEMBERSHIP* and the outcome variable is *NFI*. The estimates are based on a fixed-effects linear probability model that includes the full set of control variables plus 8 year dummies. Standard errors in parenthesis are panel robust (clustered at the individual level). *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 8. Estimates for different subsamples

	All	**Males**	**Females**	**Blue collars**	**White collars**
	(1)	(2)	(3)	(4)	(5)
Excluding obs. if employer changes	.0131 (.0079)* [36699]	.0245 (.0115)** [19179]	-.0023 (.0105) [17520]	.0125 (.0174) [10966]	.0125 (.0085) [25733]
Excluding obs. if tenure < 100 weeks	.0135 (.0080)* [33774]	.0242 (.0118)** [17813]	.0003 (.0103) [15961]	.0244 (.0180) [10099]	.0082 (.0084) [23675]

Notes: The table reports estimates of the *UNION* coefficient in equation (1) for different subsamples. All estimates are by fixed effects. The union variable is always *MEMBERSHIP* and the outcome variable is always *NFI* (see Table 3 for definitions). The first row of estimates only uses observations if the previous employer is the same as the current employer and drops all other observations. In the second row, the estimates exclude observations if the respondent has been working for the same employer for less than 100 weeks. All estimates are based on linear probability models and include the full set of control variables. All standard errors in parenthesis are panel robust (clustered at the individual level). The number of observations is reported in square brackets. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 9. Definitions and summary statistics of the region, industry, and occupation dummies

Variable	Definition	Mean	Std. Dev.
SOUTH	1 if region of residence South	0.395	0.489
NORTHEAST	1 if region of residence Northeast	0.171	0.376
NORTHCENT	1 if region of residence North Central	0.234	0.423
WEST	1 if region of residence West	0.200	0.400
AGRICU	Agriculture, forestry, and fisheries	0.025	0.158
MINING	Mining	0.006	0.076
CONSTRUC	Construction	0.075	0.263
MANUF	Manufacturing	0.181	0.385
TRANSP	Transportation, communications, and other public utilities	0.068	0.252
TRADE	Wholesale and retail trade	0.176	0.380
FINANCE	Finance, insurance, and real estate	0.060	0.238
BUSINESS	Business and repair services	0.080	0.271
PERSONAL	Personal services	0.045	0.208
ENTERTAIN	Entertainment and recreation services	0.014	0.118
PROFSERV	Professional and related services	0.206	0.405
PUBLIC	Public administration	0.060	0.238
PROFTECH	Professional, technical and kindred workers	0.171	0.376
MANAGER	Managers and administrators, except farm	0.124	0.330
SALES	Sales workers	0.044	0.204
CLERICAL	Clerical and unskilled workers	0.180	0.384
CRAFT	Craftsmen and kindred workers	0.115	0.319
OPERAT	Operatives, except transport	0.093	0.291
TROPERAT	Transport equipment operatives	0.041	0.198
LABORERS	Laborers, except farm	0.063	0.243
FARMER	Farmers and farm managers	0.003	0.057
FARMLAB	Farm laborers and foreman	0.007	0.085
SERVICE	Service workers, except private household	0.148	0.355
PRIVATE	Private household workers	0.009	0.094
BLUECOLLAR	1 if occupation *CRAFT*, *OPERAT*, *TROPERAT*, or *LABORERS*	0.313	0.464

Notes: Statistics are for the years 88, 89, 90, 92, 93, 94, 96, 98, and 2000. The industry and occupation dummies are based on the classification system of the 1970 Census of Population.

Table 10. First-stage regressions for Table 6. Dependent variable: *MEMBERSHIP*

RETIREMENT	.0630			.0458
	(.0041)***			(.0044)***
MATERNITY		.0381		.0141
		(.0034)***		(.0035)***
DENTALINS			.0568	.0394
			(.0040)***	(.0045)***
Controls?	Yes	Yes	Yes	Yes
Person fixed effects?	Yes	Yes	Yes	Yes
Observations	52081	49238	52850	48332
F-statistic	235.96	128.47	198.80	105.97
p-value	0.000	0.000	0.000	0.000

Notes: The table reports the instruments' coefficients of the first-stage regressions for Table 6. The outcome variable is always *MEMBERSHIP*. All estimates include the full set of control variables plus 8 year dummies. Standard errors in parenthesis are panel robust (clustered at the individual level). The table also reports the first-stage *F*-statistic for weak instruments. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Figure 1. Injury gaps for different injury severity degrees