WAGE COMPENSATION FOR DANGEROUS WORK REVISITED

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Using data from the 1982 Panel Study of Income Dynamics (PSID), the authors investigate the relationship between wages and the risk of work-related death or nonfatal injury. Including industry-level variables and using alternative risk measures dramatically affects measured wage compensation. The results cast doubt on the existence of compensating differentials for risk. Indeed, the strongest finding is the likely presence of negative compensation—relatively high risk and low wages—for nonunion workers. The role of rent-sharing or other forms of strategic bargaining behavior (captured by value-added per worker and other industry variables) and the gender distribution of both risk and wages demonstrate that noncompetitive elements in U.S. labor markets are sufficiently strong to overcome the competitive tendency toward equalizing differentials.

This paper addresses two issues crucial to the interpretation of studies attempting to estimate wage compensation for dangerous work: the effect of noncompetitive aspects of labor markets, and potential error in the methods used to attribute risk to individual workers. We explore these issues using individual-level worker data from the 1982 wave of the Panel Study of Income Dynamics (PSID), data that enable us to replicate earlier findings and demonstrate the effect of changes in specification or choice of risk variables. Our findings are important apart from their significance for earlier work, because they shed additional light on the role that occupational risk plays in the wage structure.

The theoretical case for wage compensation for risk is plausible but hardly certain. If workers have utility functions in which the expected likelihood and cost of occupational hazards enter as arguments, if they are fully informed of risks, if firms possess sufficient information on worker expectations and preferences (directly or through revealed preferences), if safety is costly to provide and not a public good, and if risk is fully transacted in anonymous, perfectly competitive labor markets, then workers will receive wage premia that exactly offset the disutility of assuming greater risk of injury or death. Of course, none of these assumptions applies in full, and if one or more of them is sufficiently at variance with the real world, actual compensation may be less than utility-offsetting, nonexistent, or even negative—a combination of low pay and poor working conditions.[1] Therefore,

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[1]For an extended discussion of these assumptions and the theoretical grounds for doubting their applicability, see Dorman (1996).

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the empirical researcher should not assume equalizing differences, but should attempt to identify the relationship between risk and wages in models employing the most defensible specifications and reliable data, whatever they may prove to be. That is, studies succeed if their results are reliable and robust, not necessarily if they generate large, statistically significant coefficients on risk. It is from this perspective that we turn to a consideration of previous work.

Noncompetitive Aspects of Wage Determination

The mechanism of compensating wage differentials for hazardous work operates, if at all, *ceteris paribus.* Since there is a raw correlation in U.S. data between greater risk and lower wages (Robinson 1991), potential evidence of wage compensation is dependent on econometric specification. Of course, in specifying wage equations, additional explanatory variables cannot be introduced solely for the purpose of generating the desired results; they must be justified on the basis of economic theory and prior empirical work. By the same token, potential explanatory variables cannot be *excluded* without similar justification.

Table 1 summarizes the independent variables employed in eight prominent risk-wage studies.[2] Broadly speaking, one can divide potential control variables into three groups, as follows. (1) *Standard demographic and human capital variables.* These are the "core" observations on worker characteristics, such as race, sex, age, region, work experience (general), blue-collar status, marital status, and education. Nearly all studies employ them. (2) *Nonstandard demographic variables.* These include job tenure (employer-specific), urban location, coverage by a collective bargaining agreement, and occupation. The more sporadic use of this second category may be due more to problems of data availability than to theoretical motivation. (3) *Employer and industry variables, such as firm size, union density, four-firm concentration ratio, industry growth rates, and industry dummies.* These are seldom used, although industry-level data (and dummies) are readily available to all researchers.

With the exception of Duncan and Holmlund (1983), all of the wage-risk studies under consideration assigned average occupational risk measures to individual workers. The risk measures are available by occupation level or industry level at varying degrees of aggregation. The first of these was chosen in the seminal study by Thaler and Rosen (1976) and also appeared in Arnould and Nichols (1983). These studies used a measure of excess fatality by occupation reported by the Society of Actuaries. A long-standing criticism of these data, however, is that they fail to distinguish between true occupational risk and the selection bias of occupational sorting (Lipsey 1976). As a result, most researchers have chosen to merge industry-average risk measures, whether generated by BLS (at two- or three-digit levels) or the NIOSH's National Traumatic Occupational Fatality project, which reports fatalities by major industry (1-digit SIC) and state. Seven of the eight studies summarized in Table 1 derived their risk measures in this fashion.

It is important to consider the implications of using the average level of risk in a worker's industry to proxy his or her risk at the individual level. A statistically significant positive risk coefficient, should it emerge, represents a wage premium captured by the worker for working in a dangerous industry; it would be shared by all workers in this industry whatever their (unobservable) level of individual risk. It is, in other words, an *industry* wage premium. It is not unreasonable to suppose that such a premium might exist, but it is equally plausible that industry premia exist for other reasons. To incorporate one such premium but exclude the others would bias individual-level results.

Indeed, past work in hedonic wage analysis suggests that the incorporation of workers' industry affiliation may alter coeffi-

[2]Table 1 does not record the use of transformations, such as interacted or squared variables.

Table 1. Selected Control Variables in Eight Wage-Risk Studies.

Variable	*Arnould & Nichols 1985*	*Dillingham 1985*	*Dorsey & Walzer 1983*	*Duncan & Holmlund 1983*	*Herzog & Schlottmann 1990*	*Martinello & Meng 1992*	*Moore & Viscusi 1988*	*Moore & Viscusi 1990*
Sex	NA	X	X	NA	NA	X	X	X
Race	X	X	X	NA	NA	X		X
Age	X	X	X		X	X	X	
Marital Status	X	X	X	X	X	X		
Health				X	X		X	X
Experience		X		X			X	X
Tenure		X				X		X
Education	X	X	X	X	X	X	X	X
Workers Compensation	X		X				X	X
Working Conditions				X				X
Overtime								X
Establishment Size						X		X
Urban	X							X
Region	X		X			X	X	X
Capital/Labor Ratio								
% Female								
Blue-Collar	X	X					X	X
Occupational Dummies		X		X	X	X		X
Union Coverage		X				X	X	X
Union Density	X				X			
Concentration Ratio						X		
Layoff Rate								X
Industry Unemployment								
Full Time	X							
Industry Growth Rate					X			
Industry Dummies		X				X		

cients on risk. Two of the eight studies summarized in Table 1 employed industry dummies, although one, Martinello and Meng (1992), used only three. The other, Dillingham (1985), found that the use of a full set of major industry and occupational dummies (six and five respectively) reduced both the size and significance of the risk coefficients. In his dummy-less models, Dillingham obtained positive coefficients on risk significant at the 5% level (two-tailed) in four of his five specifications. When dummies were inserted, however, only one of the coefficients remained significant at 5%, one was significant at 10%, and the others were statistically insignificant. Moreover, in each instance, adding a set of dummies reduced the size of the risk coefficient. Occupational dummies cut it in half, industry dummies (in the detailed industry models) by more than 75%. In broad terms, this is what would be expected in light of the literature on inter-industry wage differentials.[3]

Virtually the same results emerged from a recent study by Leigh (1995a). Leigh first estimated a conventional wage/risk equation using the 1982 PSID and both the BLS and NIOSH measures of fatal risk. Coefficients on these risk variables were positive and statistically significant, replicating conventional results. He then added a set of dummy variables representing workers' major industry affiliation, with the explicit aim of incorporating inter-industry wage differentials. Without dummies, both risk measures yielded positive, statistically significant coefficients; after their inclusion neither was statistically significant.[4]

Of course, industry premia may be identified not only through SIC affiliation, but also through industry-level characteristics, such as firm size and average capital intensity. One of the purposes of this paper is to test for noncompetitive aspects of the wage-risk relationship by incorporating a range of industry characteristics. Here we must be careful not to select variables arbitrarily or, worse, on the basis of their explanatory power for this particular sample. Without proceeding from first principles, a convenient set of criteria for model design can be derived from practice in adjacent literatures. For our purposes, the most appropriate examples can be found in the literatures that emerged (or, more accurately, were resurrected) during the 1980s focusing on inter-industry wage differentials, the firm size–wage effect, and other issues concerning the industrial wage structure. Collectively, they have generated substantial evidence for the presence of noncompetitive forces operating within contemporary labor markets. In this section we will briefly review these literatures, with the purpose of identifying the variables that have been used to account for noncompetitive effects.

The modern point of departure for the study of inter-industry wage differentials is a pair of papers, Dickens and Katz (1987) and Krueger and Summers (1987). Each contains an extensive review of the earlier literature, establishing that these differentials are robust with respect to data and models, and that their structure is remarkably constant across time. In addition, Dickens and Katz conducted their own study of U.S. data, finding that as much as a quarter of individual wage variation can be explained by industry-level premia, and they cast doubt on the ability of unmeasured variation in worker characteristics to ac-

[3]It is interesting to note that economists citing the Dillingham study have recorded only the results of his regressions *not* employing industry or occupational dummies, presumably on the grounds that vanishing coefficients on risk represent "unsuccessful" specifications. See Fisher et al. (1989), Miller (1990), and Viscusi (1992, 1993).

[4]Leigh also found comparable results in regressions using other samples. In part, our paper can be considered an extension of Leigh's work, distinguishing between the wage/risk relationships for union and nonunion workers, incorporating industry-level characteristics, and examining the resulting effects across alternative risk measures. The use of industry characteristics (gender and union density, capital-labor ratios, establishment size, and so on) enables us to explore the mechanisms at work beyond Leigh's general reference to "industry effects."

count for the industry wage differentials. Variables employed by Dickens and Katz include the percentage of an industry's work force that is female, percentage black, percentage production workers, industry average layoff rate, injury rate, unemployment rate, union density, average firm and establishment size, the concentration and capital-labor ratios, the ratio of research and development expenditures to sales, and average industry profitability. Subsequent research has largely supported the view that inter-industry differentials demonstrate noncompetitive aspects of wage determination, although there is debate over the extent to which unobserved individual heterogeneity might be responsible for measured industry effects as well (Allen 1995; Blackburn and Neumark 1992; Blackburn 1995; Fields and Wolff 1995; Gibbons and Katz 1992; Grey 1993; Keane 1993).

A second approach to the study of imperfectly competitive labor markets centers on the intriguing relationship between employer (or establishment) size and wages. A series of studies, most prominently Brown and Medoff (1989), has demonstrated that larger employers pay more *ceteris paribus*, and the main task is to explain why this should be. (Brown and Medoff 1990; Groshen 1991; Kruse 1992; Dunne and Schmitz 1995; Reilly 1995; Green et al. 1996.) As in the inter-industry differentials literature, individual-level factors—as picked up, for example, in fixed effects models—can explain relatively little of the size-wage effect. While some employer- and industry-level factors—especially the use of computers and other advanced equipment—appear to account for a portion of this effect, measures of hazardous working conditions once again do not.[5]

Finally, a number of studies have examined firm- and industry-level aspects of wage determination in other contexts. Among the objectives have been the disentangling of union membership premia from union density effects, the effect of industry structure on wage differences between men and women, and the role for differences in unemployment rates suggested by efficiency wage theory; variables of interest have included union density, industry unemployment, value added, the capital-labor ratio, firm liquidity, and the risk of plant closing. (Ashley and Jones 1996; Dunne and Roberts 1990; Green and Weisskopf 1990; Hodson and England 1986; Kim 1995; Currie and McConnell 1992; Curme and MacPherson 1991; Heywood 1989.)

In light of the important role played by industry- and employer-level characteristics in the above-listed studies of wage determination, it is clearly not sufficient for wage-risk analysis to assume, through its regression models, that competitive forces alone determine wages. The first major task of this paper is to incorporate widely accepted noncompetitive factors in individual wage regressions to test the robustness of risk coefficients. Only in this way can we determine the extent to which labor markets actually exhibit the competitive outcome of compensating wage differentials for risk. We will do this in two ways, by incorporating (1) detailed industry dummies and (2) selected industry-level variables.

Potential Measurement Error in Risk Variables

As we have already seen, concern over the possibility of measurement error is as old as the empirical literature on wage compensation. This is because, with few exceptions, wage-risk studies have taken the path of attributing to individual workers the average measured risk of their industrial or occupational category. These attributions may be inexact because categorical risk is itself mismeasured, because it is imperfectly

[5]Estimates of the portion of the size-wage effect attributable to unobserved worker heterogeneity range from 5% to 45% in Brown and Medoff (1989). Dangerous working conditions did not have explanatory power in that study or in Kruse (1992). The most promising employer characteristic that may account for the size effect is the use of advanced equipment; see Dunne and Schmitz (1995) and Reilly (1995).

correlated with individual risk, or, most likely, both.[6]

A prominent exception to the strategy of categorical risk imputation is Duncan and Holmlund (1983). This study went to great lengths to reduce measurement error, matching working conditions derived from the *Dictionary of Occupational Titles* with individually reported occupational hazards. In addition, it used a longitudinal sample of Swedish workers to construct a fixed effects model of logged changes in wages and working conditions. Thirteen potentially hazardous conditions were identified; of these, only four yielded positive and statistically significant wage (change) coefficients.

In analyses of individual data from the United States it became common to use BLS fatal and nonfatal injury data by industry, the former at a one- or two-digit level and the latter at a three-digit level. In an influential article, Moore and Viscusi (1988) demonstrated that replacing the BLS measure of fatalities with a (then) new series constructed by NIOSH, the National Traumatic Occupational Fatality (NTOF) database, attributed to individuals by state and major industry, doubled the estimate of the implicit value of life, due both to a greater coefficient on risk and to higher levels of measured risk.[7] The greater magnitude and statistical significance of measured compensation resulting from the use of the NTOF variable was seen as evidence that it provided a better approximation of individual-level risk.[8]

From an *a priori* standpoint, however, it is not clear which risk variable is preferable. On the one hand, the NTOF data are derived from a direct count of identified occupational fatalities drawn from death records, unlike the survey-derived BLS series, and this procedure, at least in principle, is not biased against reporting the deaths of independent contractors and employees of small firms, as is the BLS, which excludes both categories.[9] On the other hand, the two-digit matching of BLS data is probably more accurate than the one-digit by 50-state matching of NTOF. This is because a worker's state of residence has little direct significance for safety; it is best viewed as a weak proxy for the mix of detailed industries in the worker's locality. Moreover, death certificates have been shown to understate the incidence of occupational fatalities by a substantial amount, and this undercount is not neutral with respect to demographic, occupational, and industrial distribution (Leigh et al. 1997). Whether the advantages of NTOF outweigh the disadvantages cannot be known apart from empirical analysis.

Leigh (1995b) published a measure of risk of fatality by three-digit SIC, based on special studies conducted by BLS using 1980s data. This is a more accurate measure of risk by industry affiliation, and we

[6]Unless corrected, they will also lead to biased estimates of statistical significance. Dickens and Ross (1984) demonstrated that the standard OLS significance estimator is upwardly biased in regression models with merged individual and group-average data. The intuition behind this result is that the attribution of averaged data to individuals reduces the number of truly independent observations. Dickens and Ross offered a revised estimator, but we have not employed it in this study.

[7]The value of life interpretation of these results depends, of course, on the belief that the assumptions underlying the equalizing differences model are largely fulfilled.

[8]"The performance of the NTOF death-risk variable is consistently superior to that of the BLS risk measure. Although the BLS fatality variable coefficient is always positive, the largest t-ratio observed is 1.625, so that this measure at best has coefficients just shy of the level needed to achieve statistical significance at the usual 5% level (1.645). In contrast, the coefficients based on the NTOF variable are always positive, are several times larger than the comparable BLS coefficients, and never have t-ratios smaller than 3.35, thus passing even the most demanding tests for statistical significance. These results provide strong evidence of an errors-in-variables problem in the BLS data" (p. 485).

[9]NTOF makes no attempt to count fatal occupational illnesses, whereas the BLS annual survey does. Given the very small percentage of such illnesses actually registered by BLS, this may constitute a defect from the perspective of relative risk attribution.

use it in this study. More recently, BLS has introduced its Census of Fatal Occupational Injuries (CFOI). Because it is based on the most complete census of risk of fatality undertaken by a federal agency, it supersedes previous efforts. Unfortunately, it records the incidence of occupational fatalities only during the 1990s, and its use in conjunction with a sample from 1981 would therefore be questionable.

Finally, mention should be made of the BLS series on lost workday injuries and illnesses, since we use this as a basis for assessing risk of fatality variables. During the first half of the 1980s, OSHA's policy was to focus its inspections on establishments with above-average injury and illness experience. This created an incentive for firms to under-report the true incidence, and it is possible that the data we use suffer from this problem (Smith 1992). In addition, the structure of workers' compensation created an incentive for self-insured and experience-rated firms to under-report injuries. The greatest problem of under-reporting, however, shows up in the number of workdays missed due to injury, not in the number of lost workday injuries (Oleinick et al. 1995). Since this latter measure is the one we employ in this paper, we do not believe that our injury variable is seriously impaired.[10]

In light of the advantages and disadvantages of alternative measures of occupational risk, there is a useful purpose to be served by testing for the robustness of wage-risk relationships across different risk variables. In addition, the pattern of results arising from the use of these variables can shed light on their relative merits.

Data and Methods

To answer the questions posed above, we employed individual-, occupation-, and industry-level data and replicated our analyses across a set of four alternative measures of risk of fatality and one measure of risk of nonfatal injury. From the 1982 PSID we sampled both male and male and female heads of households who worked in manufacturing, mining, or construction for more than 20 hours per week during the 1981 calendar year. We examined an all-male sample beside a combined sample for consistency with previous work as well as to minimize the potential for unmeasured differences in risk exposure—although it is important to bear in mind that the risk variables employed in this and previous studies include female as well as male outcomes.[11]

We eliminate workers in transportation, because the industry-level variables we wish to employ are not available for that industry. Fortuitously, excluding transportation is justified on theoretical grounds as well, since this industry, although it exhibits above-average risk, is weighted toward types of risk, like motor vehicle accidents, that workers are likely to regard as the result of their own behavior and not as imposed by the employer. The literature on risk perception broadly supports the view that individuals respond differently to risks depending on whether they are seen as violations of personal autonomy, and in this respect transportation accidents are likely to differ from most other risks faced in mining, manufacturing, and construction in the responses they evoke.[12]

[10]The BLS annual survey of nonfatal injuries samples firms with fewer than 11 employees and extrapolates to provide its detailed industry estimates, unlike the annual survey of fatalities, which simply excludes these firms.

[11]We are indebted to an anonymous reviewer for this point.

[12]In general terms, people appear to attach little significance to risks that they believe, rightly or wrongly, to be under their control; they have a heightened aversion to risks that they regard as being imposed on them by others. The original argument concerning autonomy and risk was advanced by Starr (1969). For an overview of this issue and its relevance to occupational risk in transportation and agriculture, see Dorman (1996). One practical consequence of this difference in risk perception is that drivers' safety courses do not appear to promote reduced accidents (due to offsetting increases in risky behavior), whereas safety training in industry has a long record of effectiveness. (Potvin et al. 1988; Robertson

Table 2. Definitions of Industry-Level Variables.

Variable Name	*Definition*
NTOF×NONUN	Frequency of fatalities per 100,000 Workers (NTOF) by state and one-digit SIC × dummy for nonunion coverage
NTOF×UNION	Frequency of fatalities (NTOF) × dummy for union coverage
FATALBYIND×NONUN	Frequency of fatalities per 100,000 workers (BLS) by three-digit SIC × dummy for nonunion coverage
FATALBYIND×UNION	Frequency of fatalities (BLS) by industry × dummy for union coverage
FATALBYOCC×NONUN	Frequency of fatalities per 100,000 workers (BLS) by three-digit occupation × dummy for nonunion coverage
FATALBYOCC×UNION	Frequency of fatalities (BLS) by occupation × dummy for union coverage
FATALBYIND&OCC×NONUN	Frequency of fatalities (BLS) by industry-occupation interaction × dummy for nonunion coverage
FATALBYIND&OCC×UNION	Frequency of fatalities (BLS) by industry-occupation interaction × dummy for union coverage
INJDAYS×NONUN	Lost work day cases due to occupational injuries in 1981 per 100 workers × dummy for nonunion coverage
INJDAYS×UNION	Lost work day cases × dummy for union coverage
INJDAYS	Lost work days due to occupational injuries in 1981 per 100 workers
DISABPAY/AVGWAGE× INJDAYS	State weekly wage replacement maximum for temporary total disability / state average weekly wage for production and nonsupervisory workers × INJDAYS
CONSTRUC	Dummy for construction industry
DURMFTG	Dummy for durable manufacturing
NONDURMFTG	Dummy for nondurable manufacturing
ASSETS/WRKR	Gross depreciable assets per worker in 1982
%FEMALE	Percent female employees in 1983
UNIONDENS	Percent unionized in 1980
ESTABSIZE	Average number of employees per establishment
VALADDED	Value added per employee in 1982
UNEMPRATE	Industry unemployment rate in December 1981

Finally, depending on the regression specification employed, we eliminated other workers for whom variables are missing; we also eliminated workers in sales and professional occupations, again in an attempt to minimize unmeasured differences in risk exposure.[13] Sample sizes vary depending on the choice of risk measure and on whether women are included in the estimated model. Using the NTOF, BLS industry, BLS occupation, and combined BLS industry-occupation risk of fatality measures and BLS risk of nonfatal injury resulted in samples of 846, 820, 797, 773, and 846 (for men) and 1,013, 984, 952, 925, and 1,013 (for men and women), respectively.

Our choice of industry-level variables was based on their explanatory power in previous studies and their appropriateness to the sample we analyze. Thus, concentration ratios, imports, and exports were rejected, since national industry concentration is not meaningful in decentralized industries such as construction, and our

1980; Robertson 1984.) Of course, motor vehicle accidents figure prominently in the fatality rates of all industries; this factor alone should reduce our expectation of finding substantial wage compensation.

[13]Leigh (1995a) concluded that the BLS two- and three-digit industry fatality numbers "cannot be reasonably applied to white-collar workers."

Table 3. Prior Research Employing Industry-Level Variables.

Variable	*Previous Studies*
Capital-Labor Ratio	Allen (1995)*
	Currie and McConnell (1992)*
	Dickens and Katz (1987)*
	Dunne and Roberts (1990)*
	Green and Weisskopf (1990)[a]
	Hodson and England (1986)*[b]
	Reilly (1995)*
Female Density	Dickens and Katz (1987)*
	Green and Weisskopf (1990)[a]
	Hodson and England (1986)[b]
Union Density	Allen (1995)*
	Ashley and Jones (1996)*
	Dickens and Katz (1987)*
	Green and Weisskopf (1990)[a]
	Kim (1995)*
Establishment Size	Allen (1995)
	Ashley and Jones (1996)
	Dickens and Katz (1987)
	Dunne and Roberts (1990)*
	Green and Weisskopf (1990)[a]
	Hodson and England (1986)[b]
	Reilly (1995)*
Value Added per Worker	Alleen (1995)*
	Dunne and Roberts (1990)*
	Hodson and England (1986)[b]
Industry Unemploym. Rate	Ashley and Jones (1996)*
	Currie and McConnell (1992)*
	Dickens and Katz (1987)
	Green and Weisskopf (1990)[a]

*Denotes statistical significance in original study.

[a]The study employs factor analysis rather than regression.

[b]Statistical significance pertains to the male-only portion of the sample.

sample includes both tradable and nontradable sectors.[14] Industry-level data, except for union density, were derived from employment surveys and industrial censuses and were merged with data on individuals.[15] These variables are listed and defined in Table 2.

As mentioned above, there are several literatures in empirical economics that have investigated the role of employer- and industry-level characteristics in wage determination. Taken together, they are far too extensive to describe and summarize; here we will restrict ourselves to a few representative examples in order to explain our own selection of variables. All the cited studies investigate aspects of wage determination for U.S. workers during the past three decades. The inter-industry wage differential literature is represented by Dickens and Katz (1987) and Allen (1995); union wage premium research by Kim (1995) and Ashley and Jones (1996); bargaining and threat theory by Green and Weisskopf (1990) and Currie and McConnell (1992); the analysis of discrimination by Hodson and England (1986); the role of establishment size by Reilly (1995); and compensating differentials theory (for non-safety factors) by Dunne and Roberts (1990). Their use of the employer-level variables we have incorporated in this study is summarized in Table 3. Those instances in which the coefficients on these variables were statistically significant in at least one specification (by the original authors' criteria) are denoted with an asterisk. As can be seen, all of these variables have a record of explanatory power.[16]

Finally, to examine how robust the compensating differentials are when different risk variables are chosen, we used four measures of risk of fatality and one of risk of nonfatal injury. These were drawn from the NTOF series described above, aggregated over the years 1980–88, and the BLS Supplementary Data System, which records occupational fatalities by two- and three-digit industry and occupation for the years 1979–81, 1983, 1985, and 1986, as reported by Leigh (1995b), as well as BLS data for lost workday cases for 1981. Note that the BLS industry data for risk of fatality are more detailed than the one- and two-digit

[14]An additional comment should be made about our use of unemployment data. In keeping with the theme of this paper, we employed the industry, rather than regional, unemployment rate. It is possible, however, that a geographically based measure would be superior. (Blanchflower and Oswald 1994.)

[15]The source for our density variable is Kokkelenberg and Sockell (1985).

[16]Readers interested in theoretical arguments for these phenomena may wish to consult Weitzman (1989) and Montgomery (1991), which employ friction and search explanations, respectively.

fatality data used in past studies. In addition, for each worker we constructed a composite industry-occupation variable by taking the geometric mean of the two BLS fatality measures. For risk of nonfatal injury, used in Tables 7a–b, we employed BLS data on the incidence of lost workday cases by three-digit industry for 1981. In addition, we controlled for the effects of state-level differences in workers' compensation by including a variable that interacts the number of lost workdays for each worker's two-digit industry with the maximum weekly benefit for temporary total disability allotted by the worker's state, divided by the state's mean income for production and nonsupervisory workers. Since this approach differs from the approach adopted in other studies, a word of explanation is called for.

Previous studies, such as those reported by Moore and Viscusi (1988), have used the individual's expected Workers' Compensation (WC) replacement rate times a measure of the worker's expected risk. The first factor is estimated by calculating the degree of replacement applicable to the worker's income, given the relevant state formula. The purpose is to provide an estimate of the WC payment each worker can expect to receive in light of his or her income, level of risk, and state replacement formula. This approach generally yields a large, statistically significant, and negative coefficient on the WC variable; indeed, Moore and Viscusi found that, taken as a whole, WC fully pays for itself to employers in the form of wage reductions that exceed aggregate payroll premia. This is a remarkable result.

Unfortunately, the relationship between wages and WC replacement estimated in these studies is spurious, since it arises directly from the way in which the replacement rate is measured. Since most states replace two-thirds of income, state variation derives almost entirely from differences in the maximum weekly benefit. As a worker's income exceeds 150% of this amount, his or her effective replacement rate falls. Hence the strong relationship between wage and WC replacement rates—an artifact of measurement, not an indication of compensating wage differentials.

There is no fully satisfactory solution to this problem, given existing data. Our approach is predicated on the belief that the state cap, denominated by average state wages, is a reasonable proxy for a variety of unmeasured state differences in WC generosity. Following Moore and Viscusi, we continue the practice of interacting this variable with a measure representing the likelihood that a worker will actually receive these benefits. We have no *a priori* expectations regarding the sign of this composite variable (DisabPay/AvgWage×InjDays): it may be negative in accordance with conventional equalizing differences theory, or it may be positive if a state's WC generosity is correlated with other institutional factors favorable to workers.

The descriptive statistics for the largest analysis samples are presented in Table 4. The bulk of this population (77/81% for the male/combined samples) works in manufacturing, with the remainder divided between construction (19/16%) and mining (4%). 44/38% of the samples are in higher-skilled, craft occupations, and 46/45% were covered by a union contract in 1981. Among the more interesting aspects of the industry-level data are the high unemployment rate at the end of 1981 (12.7/12.5%) and the substantial cross-industry variance in such factors as capital-labor ratios, establishment size, percentage female, and value-added per worker.

Finally, Table 5 shows the mean and standard deviations for wages and the four risk variables by industry, union status, and education. Average wages range from \$8.65/hour (\$8.72 for men) in the construction industries to \$10.41/hour in mining. Mining is clearly the industry with the greatest risk of fatality; construction poses the greatest risk of any injury. We also observe in Table 5 that wages are higher among those covered by a union contract than among those who are not, whereas nonunionized workers face greater risk of fatality. The education comparisons in Table 5 reveal wages rising and risk falling with education, except for the NTOF mea-

Table 4. Descriptive Statistics for Analysis Samples.

Variable	Men Mean	Men Std. Dev.	Men and Women Mean	Men and Women Std. Dev.
Wage	9.01	3.91	8.51	3.93
LogWage	2.09	0.48	2.03	0.51
NonWhite	0.39		0.42	
SMSA	0.60		0.63	
AgeHead	35.75	11.81	35.88	11.84
Married	0.82		0.76	
UnionCov	0.46		0.45	
Experience	12.43	10.66	12.10	10.54
Tenure	97.65	99.29	94.65	96.62
WksVac	2.07	2.70	2.07	2.65
LowEduc	0.37		0.38	
HighEduc	0.12		0.12	
Craft	0.44		0.38	
Mining	0.04		0.04	
Construc	0.19		0.16	
DurMftg	0.49		0.50	
NondurMftg	0.28		0.31	
Assets/Wrkr	48.52	67.00	46.11	63.94
EstabSize	89.48	115.50	86.77	121.75
%Female	22.51	14.16	25.88	17.24

Variable	Men Mean	Men Std. Dev.	Men and Women Mean	Men and Women Std. Dev.
UnionDens	36.87	16.01	36.05	15.87
UnempRate	12.67	5.38	12.48	5.19
ValAdded	50.90	39.29	49.15	36.83
WC	72.25	46.32	68.18	45.16
NTOF	12.30	12.72	11.50	12.12
NTOF×Nonun	14.26	14.36	13.10	13.67
NTOF×Union	10.05	10.07	9.55	9.57
FatalByInd	16.39	27.33	14.59	25.52
FatalByInd ×Nonun	20.76	33.81	17.95	31.35
FatalByInd ×Union	11.34	15.57	10.47	14.67
FatalByOcc	13.80	14.92	12.35	14.17
FatalByOcc ×Nonun	15.30	15.89	13.40	14.98
FatalByOcc ×Union	12.15	13.59	11.11	13.06
Ind/OccFatal	13.23	15.79	11.77	14.87
FatalByInd& Occ×Nonun	16.00	18.89	13.80	17.65
FatalByInd& Occ×Union	10.16	10.61	9.36	10.16
Inj	5.50	2.34	5.19	2.41
InjDays×Nonun	5.53	2.36	5.14	5.53
InjDays×Union	5.47	2.33	5.25	5.47

sure—a point we will return to shortly. In general, the partitioning of the labor force by education generates much larger wage differentials than does partitioning by industry or union status and supports the impression that, in the absence of more extensive controls, greater risk is associated with lower wages. Finally, note that the inclusion of women in the sample always lowers both group-average wages and measures of occupational risk.

In order to test simultaneously for the roles of risk measurement and model specification, we adopted the following procedure. For each sample and for each of the five risk variables, we regressed the log of wages on risk of fatality, interacted with union status, and three sets of controls: a "conventional" set of human capital and other demographic variables, a second set incorporating the first plus dummies for two-digit industry, and a third incorporating the industry-level characteristics identified above. The last two models represent alternative methods of testing for noncompetitive effects: simply controlling for industry affiliation, as in Dillingham (1985) and Leigh (1995a), or ascribing industry-level variables (in addition to risk) to individuals as outlined by Table 3. Thus a total of 30 regressions were performed, across which we had 40 opportunities to observe the effects of specification change on risk coefficients—two risk variables, each subjected to two specification changes, across sets of regressions employing five different measures of risk, estimated for two samples.

Results

Table 6 reports the coefficients and t-statistics generated by the standard and two

Table 5. Wages and Risk by Industry, Union Status, and Education.

	Men		*Men and Women*			*Men*		*Men and Women*	
Variable	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*		*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*
Mining (N = 38,38)					Nonunionized (N = 458,657)				
Wage	10.41	3.58	10.41	3.58		7.58	3.56	6.80	3.45
NTOF	32.90	8.88	32.90	8.88		14.21	14.31	11.85	13.03
IndFatal	57.39	4.66	57.39	4.66		20.73	30.70	16.25	29.44
OccFatal	29.75	15.44	29.75	15.44		15.29	15.86	11.94	14.45
Ind/OccFatal	39.53	12.31	39.53	12.31		15.99	18.85	12.47	16.86
Inj	5.47	.93	5.47	.93		5.40	2.45	4.82	2.49
Construction (N = 160,162)					Less Than HS (N = 323,454)				
Wage	8.72	4.45	8.65	4.47		8.24	3.71	7.33	3.65
NTOF	30.56	12.67	30.38	12.69		11.20	11.50	9.73	10.66
IndFatal	20.40	7.00	20.37	6.97		18.06	33.81	14.56	29.37
OccFatal	17.32	12.48	17.29	12.40		15.15	17.01	11.98	15.37
Ind/OccFatal	17.63	7.86	17.61	7.81		14.73	20.07	11.78	17.80
Inj	5.96	.12	5.96	.12		5.63	2.54	5.11	2.63
Manufacturing (N = 666,957)					HS, No College (N = 431,578)				
Wage	9.03	3.88	7.96	3.89		9.31	4.06	8.42	4.09
NTOF	6.75	5.61	6.62	5.89		12.50	12.95	11.00	11.81
IndFatal	12.86	28.92	10.60	24.68		15.55	23.53	13.13	21.32
OccFatal	11.96	14.64	9.61	13.00		13.18	13.68	10.91	12.71
Ind/OccFatal	10.40	15.69	8.46	13.56		12.45	12.92	10.41	11.93
Inj	5.30	2.70	4.80	2.67		5.36	2.31	4.92	2.37
Unionized (N = 403,504)					College Degree (N = 110,133)				
Wage	10.72	3.77	9.90	3.96		10.22	4.06	9.55	4.13
NTOF	10.11	10.21	9.43	9.66		14.78	14.71	13.68	14.26
IndFatal	11.31	15.44	10.17	14.18		14.34	16.00	12.60	15.17
OccFatal	12.11	13.44	10.60	12.60		12.39	11.98	11.31	11.50
Ind/OccFatal	10.15	10.52	9.05	9.86		11.86	10.03	10.65	9.73
Inj	5.48	2.32	5.23	2.42		5.12	2.20	4.88	2.28

alternative specifications for all ten risk variables in both samples. To underline the role of model specification, changes in coefficients between the two specifications are noted if they exceed half the initial standard error.

Overall, the effects of specification change are striking: over half the risk coefficient pairs exceed this threshold, and the nonunion-NTOF interaction loses its statistical significance despite sub-threshold effects. All but two of the noted coefficient changes are negative. Taken together, these results provide strong support for the expectation that including industry-level effects will reduce measured wage compensation for risk. Except in the NTOF regressions, including the set of industry characteristics has a somewhat greater effect than including two-digit dummies; in the former model several nonunion-risk interactions approach or achieve statistical significance—note in particular the strongly negative coefficient on nonunion risk of nonfatal injury in both samples using the industry characteristics controls. This heightened effect can be interpreted as evidence of the value of using such characteristics as an alternative to simple industry controls in studies that try to incorporate inter-industry effects—and perhaps not only in wage-risk analysis but also in labor market analysis more generally.[17]

[17]In earlier versions of this study we examined specification changes in models incorporating risk of nonfatal injury and risk of fatality simultaneously; we have also performed this exercise on uninteracted

Table 6. Risk Coefficients in Standard, Industry Dummy, and Industry Variables Models. (t-Statistics in Parentheses)

	Men			*Men and Women*		
Variable	*Standard*	*Dummies*	*Variables*	*Standard*	*Dummies*	*Variables*
NTOF×UNION	0.0051	0.0057	0.0059	0.0056	0.0062	0.0063
	(2.63)	(2.32)	(2.36)	(2.92)	(2.61)	(2.67)
NTOF×NONUN	0.0034	0.0023	0.0028	0.0027	0.0017	0.0021
	(2.65)	(1.22)	(1.51)	(2.12)	(0.97)	(1.26)
FATALBYIND×UNION	0.0007	0.0006	0.0000**	0.0009	0.0008	0.0003**
	(0.58)	(0.44)	(0.04)	(0.69)	(0.54)	(0.19)
FATALBYIND×NONUN	0.0003	0.0001**	–0.0003**	–0.0002	–0.0004**	–0.0010**
	(0.48)	(0.12)	(–0.51)	(–0.27)	(–0.53)	(–1.49)
FATALBYOCC×UNION	0.0003	0.0004*	0.0003	0.0004	0.0006	0.0005
	(0.67)	(1.06)	(0.78)	(1.41)	(1.77)	(1.45)
FATALBYOCC×NONUN	–0.0004	–0.0003	–0.0007**	–0.0005	–0.0003	0.0007**
	(–0.88)	(–0.62)	(–1.34)	(–0.91)	(–0.56)	(–1.47)
FATALBYIND&OCC ×UNION	0.0001	–0.0004**	–0.0008**	0.0005	0.0000**	–0.0004**
	(0.06)	(–0.20)	(–0.48)	(0.31)	(0.03)	(–0.22)
FATALBYIND&OCC ×NONUN	–0.0002	–0.0011**	–0.0017**	–0.0007	–0.0015**	–0.0025**
	(–0.21)	(–0.97)	(–1.55)	(–0.78)	(–1.40)	(–2.36)
INJDAYS×UNION	0.0104	0.0218*	0.0110	0.0125	0.0172	0.0068**
	(1.01)	(1.56)	(1.05)	(1.30)	(1.31)	(.70)
INJDAYS×NONUN	–.0140	–.0132	–.0288**	–.0112	–.0154	–.0301**
	(–1.47)	(–0.97)	(–2.60)	(–1.24)	(–1.20)	(–2.93)

Dependent variable: ln wage.
*Increase in coefficient exceeds half the initial standard error; **decrease in coefficient exceeds half the initial standard error.

In order to explain the interactions underlying the regression data, we use the information provided by the correlation matrices in Tables 7a and 7b, as well as the detailed regression results for the industry characteristics model presented in Table 8.

As Table 8 demonstrates, for samples with men and women, two industry factors are statistically significant at the 5% level across all risk measures: value-added per worker and gender composition. The first suggests the appropriateness of a bilateral bargaining model in wage determination, in which the size of the pie to be divided plays a role alongside external market considerations, and the second reveals the sizable importance of controlling for the types of jobs traditionally dominated by women, even controlling for gender at the individual level. In our combined sample, a 30% difference in an industry's female labor force share would be associated with an industry wage differential, in the opposite direction, of more than 10%—this in addition to any effects of discrimination women may experience as individuals. (Note that this effect is muted but does not disappear in the male-only sample.)

risk variables. The pattern of statistically significant negative effects on risk coefficients is virtually the same in these regression sets. In particular, when risk of fatality was not interacted with union status, the NTOF risk coefficient was positive and statistically significant for models without the detailed industry variables. Models using the remaining risk of fatality variables found negative coefficients that were not statistically significant. When the industry characteristics were added, the NTOF coefficient remained positive while the other risk of fatality coefficients became more negative and became statistically different from zero. These results are available from the authors.

Table 7a. Correlation Matrix for Wage, Risk, and Industry-Level Variables (Men).

	LogWage	NTOF	IndFatal	OccFatal	Inj	ValAdded	EstabSize	%Female	UnionDens	Unemp	Assets/Wrkr
LogWage	1.000										
NTOF	–.001	1.000									
IndFatal	–.137	.188	1.000								
OccFatal	–.133	.172	.470	1.000							
Inj	–.117	.048	.470	.296	1.000						
ValAdded	.117	.176	.193	.144	–.142	1.000					
EstabSize	.123	–.242	–.230	–.144	–.191	.028	1.000				
%Female	–.068	–.362	–.295	–.190	–.471	.051	.061	1.000			
UnionDens	.225	–.178	–.257	–.176	.070	–.190	.449	–.270	1.000		
Unemp	–.115	.220	.261	.162	.317	–.355	–.164	–.427	.114	1.000	
Assets/Wrkr	.073	–.147	.081	.017	–.012	.125	–.071	.098	.095	–.259	1.000

Table 7b. Correlation Matrix for Wage, Risk, and Industry-Level Variables (Men and Women).

	LogWage	NTOF	IndFatal	OccFatal	Inj	ValAdded	EstabSize	%Female	UnionDens	Unemp	Assets/Wrkr
LogWage	1.000										
NTOF	.107	1.000									
IndFatal	–.033	.214	1.000								
OccFatal	.026	.222	.487	1.000							
Inj	.080	.125	.462	.346	1.000						
ValAdded	.197	.216	.210	.187	–.036	1.000					
EstabSize	.124	–.196	–.225	–.127	–.163	.052	1.000				
%Female	–.362	–.377	–.283	–.313	–.563	–.151	–.013	1.000			
UnionDens	.262	–.094	–.187	–.093	.189	–.113	.414	–.318	1.000		
Unemp	–.069	.194	.262	.161	.267	–.335	–.160	–.203	.158	1.000	
Assets/Wrkr	.129	–.092	.098	.058	.050	.166	–.073	–.089	.108	–.260	1.000

From Tables 7a–b we can explain the likely effects of these two variables on measured wage compensation for risk. %FEM possesses negative correlations with both LOGWAGE and all risk variables. Incorporating %FEM will therefore tend to reduce the predicted wage in those industries with lower risk levels. VALADDED, particularly for the combined sample, is associated with high wages and higher risk of fatality, replicating the effect of %FEM. More dangerous jobs in these samples (which exclude supervisory and professional workers) thus tend both to be more male-dominated and to have greater value added per worker—relevant aspects of wage determination in a less-than-fully-competitive world. Incorporating these effects reduces measured wage compensation for risk. To test for the role of VALADDED and %FEM, we performed a parallel set of regressions with all industry characteristics except these two; only five pairs of risk coefficients now exhibited significant change, the number of positive statistically significant coefficients didn't fall, and none of the risk coefficients became negative and statistically significant. (These results are available from the authors.)

As for the role of risk measurement itself, we note that even with the effect of moving to less competitive models, our results confirm Moore and Viscusi's claim that NTOF data yield higher and more statistically significant measures of wage compensation. As we have seen above, however, this should not be interpreted as evidence for the greater accuracy of this variable. While we do not have an objective yardstick against which to measure the relative merits of the three imputations of risk of fatality, by assigning categorical risk to individuals in our sample we are able to construct correlations between otherwise inconsistent disaggregations (for example, by industry and occupation).

Tables 7a–b report these correlations as well as the risk variables' "raw" relationship to LOGWAGE. It is striking that INDFATAL, OCCFATAL, and INJ are strongly cross-correlated, with coefficients between roughly .3 and .5, whereas the relationship between NTOF and the other risk variables is much weaker. Moreover, of the three risk of fatality variables, only NTOF fails to display a statistically significantly negative raw correlation with wages in the male-only sample. (The raw correlations between risk and wages in the combined sample are confounded by offsetting effects on men and women. Due presumably to lower pay in low-skill office occupations, women in nonprofessional and nonsupervisory jobs are likely to make substantially more money in more hazardous jobs and industries, again as measured by raw correlation.)

Suspicion that the larger coefficients on NTOF may reflect measurement error is heightened by the descriptive data in Table 5. All the other risk variables depict occupational risk falling as education rises, whereas, in the NTOF results, greater education is associated with *greater* risk. This is implausible. Taken together, these patterns cast doubt on the reliability of the NTOF variable—at least in the form of state by major industry grid—relative to the two competing risk of fatality measures.

Our results provide little basis for choosing between industry and occupation breakdowns of BLS fatality data, or our constructed interaction. On *a priori* grounds, however, there may be a case for the superiority of IND/OCCFATAL, since, in principle, both industry and occupational affiliation ought to be germane to risk. If so, this would add credence to the statistically significantly negative coefficients on nonunion risk that appear in the IND/OCCFATAL regressions, mirroring a similar result in the injury regressions. This is an interesting and disturbing outcome, to which we will return in the final section.

In summary, while only the coefficients on risk generated by the NTOF measure are statistically significantly positive across specifications, and the union-occupational risk interaction is weakly positive in the combined sample for some models employing risk of fatality by occupation and nonfatal injury risk, the nonunion risk interactions are generally negative and occasionally statistically significantly so. As impor-

Table 8. Wage-Risk Regressions with Industry-Level Controls.
(Dependent Variable: ln Wage; t-Statistics in Parentheses)

	NTOF		*IndFatal*		*OccFatal*		*Ind/OccFatal*		*Inj*	
Variable	*Men*	*M & W*	*Men*	*M & W*	*Men*	*M & W*	*Men*	*M & W*	*Men*	*M & W*
NonWhite	–.1424	–.1425	–.1526	–.1531	–.1435	–.1440	–.1576	–.1561	–.1433	–.1400
	(–5.21)	(–5.67)	(–5.52)	(–6.03)	(–5.24)	(–5.71)	(–5.59)	(–6.00)	(–5.28)	(–5.60)
SMSA	.1297	.1158	.1257	.1101	.1291	.1149	.1244	.1100	.1310	.1157
	(4.64)	(4.41)	(4.42)	(4.13)	(4.60)	(4.35)	(4.25)	(4.00)	(4.71)	(4.42)
Age	.0092	.0005	.0093	.0011	.0098	.0007	.0114	.0017	.0099	.0009
	(2.07)	(.17)	(2.09)	(.38)	(2.19)	(.25)	(2.45)	(.58)	(2.25	(0.31)
Married	.1772	.0972	.1684	.0914	.1744	.0957	.1654	.0910	.1640	.0893
	(5.07)	(3.19)	(4.71)	(2.96)	(4.97)	(3.13)	(4.55)	(2.88)	(4.67)	(2.93)
Female		–.1723		–.1788		–.1700		–.1688		–.1718
		(–4.27)		(–4.35)		(–4.19)		(–3.95)		(–4.27)
UnionCov	.2402	.2167	.2769	.2531	.2627	.2458	.2835	.2538	.0530	.0680
	(5.98)	(6.01)	(7.82)	(8.02)	(8.40)	(8.73)	(7.08)	(7.15)	(0.75)	(1.13)
Exper	–.0019	.0129	–.0013	.0126	–.0034	.0118	–.0013	.0132	–.0014	.0129
	(–.27)	(2.32)	(–.18)	(2.23)	(–.47)	(2.10)	(–.17)	(2.23)	(–0.20)	(2.33)
Exper^2	–.0002	–.0003	–.0002	–.0003	–.0002	–.0003	–.0003	–.0004	–.0002	–.0003
	(–1.49)	(–2.74)	(–1.38)	(–2.54)	(–1.32)	(–2.53)	(–1.86)	(–2.79)	(–1.67)	(–2.82)
Tenure	.0024	.0028	.0023	.0027	.0024	.0028	.0022	.0026	.0024	.0027
	(5.86)	(7.08)	(5.47)	(6.70)	(5.83)	(7.07)	(5.05)	(6.31)	(5.82)	(7.00)
Tenure^2	–.0000	–.0000	–.0000	–.0000	–.0000	–.0000	–.0000	–.0000	.0000	–.0000
	(–3.89)	(–5.06)	(–3.71)	(–4.88)	(–3.92)	(–5.11)	(–3.07)	(–4.23)	(–3.96)	(–5.077)
WksVac	.0209	.0198	.0193	.0178	.0197	.0185	.0175	.0163	.0198	.0197
	(3.94)	(4.01)	(3.62)	(3.60)	(3.71)	(3.73)	(3.28)	(3.25)	(3.75)	(4.0)
LowEduc	–.1235	–.1303	–.1325	–.1385	–.1167	–.1237	–.1166	–.1273	–.1219	–.127
	(–4.23)	(–4.86)	(–4.48)	(–5.10)	(–3.96)	(–4.57)	(–3.83)	(–4.54)	(–4.20)	(–4.76)
HighEduc	.0996	.1025	.0978	.0990	.1035	.1065	.0972	.0978	.1005	.1007
	(2.44)	(2.67)	(2.36)	(2.54)	(2.52)	(2.74)	(2.30)	(2.45)	(2.47)	(2.63)

Continued

tant as this pattern is the finding of extreme sensitivity to choice of risk variable.[18]

Briefly reviewing Table 8, we find that most coefficients have plausible signs and magnitudes. Thus, being a racial minority, a woman, married, and an urban resident have their predicted effects. Human capital variables perform well, with women appearing to benefit more than men from both general and firm-specific experience. The positive and statistically significant coefficients on WksVac are further evidence of negative wage compensation and, implicitly, of noncompetitive labor market effects. It is interesting to note that the estimated return to education is slightly lower when industry-level variables are taken into account. The measured union wage premium is consistent with previous research, although it virtually disappears in several models employing risk of nonfatal injury interacted with union status. (In these cases the premium reappears in the form of *not* receiving negative compensation for risk.) Establishment size plays little role in wage determination in these results, a finding at variance with many of the studies cited earlier. This is probably due to our use of union-risk interactions and a union density variable, both of which are highly correlated with size. Finally, the workers' compensation variable is never statistically

[18]In this respect we extend the results obtained in Leigh (1991).

Table 8. Continued.

	NTOF		*IndFatal*		*OccFatal*		*Ind/OccFatal*		*Inj*	
Variable	*Men*	*M & W*	*Men*	*M & W*	*Men*	*M & W*	*Men*	*M & W*	*Men*	*M & W*
Craft	.1406	.1420	.1469	.1475	.1395	.1398	.1556	.1552	.1435	.1442
	(5.02)	(5.22)	(5.18)	(5.37)	(4.94)	(5.09)	(5.36)	(5.46)	(5.15)	(5.325)
Mining	−.0129	−.0340	.1069	.0768	.1175	.0832	.1287	.0874	.0565	.0339
	(−.11)	(−.29)	(.90)	(.67)	(1.00)	(.74)	(1.05)	(.74)	(0.50)	(.03)
Construc	−.0429	−.0612	.0515	−.0120	.0562	.0288	.0473	.0127	.0126	−.0101
	(−.67)	(−1.00)	(1.04)	(−.26)	(1.19)	(.63)	(.96)	(.27)	(0.25)	(−.21)
NondurMftg	−.0212	−.0003	−.0279	−.0123	−.0194	−.0016	−.0190	−.0121	−.0286	−.0074
	(−.52)	(−.01)	(−.66)	(−.33)	(−.46)	(−.04)	(−.43)	(−.31)	(−0.70)	(−.20)
Assets/Wrkr	.0003	.0003	.0003	.0003	.0002	.0003	.0002	.0002	.0003	.0003
	(1.28)	(1.29)	(1.25)	(1.35)	(1.13)	(1.21)	(.97)	(1.07)	(1.22)	(1.25)
EstabSize	.0001	.0001	.0001	.0002	.0001	.0002	.0001	.0002	.0013	.0001
	(.80)	(1.10)	(1.04)	(1.40)	(.85)	(1.25)	(.95)	(1.33)	(2.12)	(−0.62)
%Female	−.0024	−.0038	−.0015	−.0034	−.0023	−.0039	−.0021	−.0038	.0000	−.0047
	(−1.60)	(−3.09)	(−.92)	(−2.66)	(−1.53)	(−3.10)	(−1.26)	(−2.85)	(0.34)	(−3.60)
UnionDens	.0015	.0013	.0015	.0010	.0019	.0016	.0012	.0007	−.0034	.0015
	(1.41)	(1.36)	(1.29)	(.96)	(1.74)	(1.59)	(.96)	(.65)	(−2.09)	(1.55)
UnempRate	−.0012	.0014	−.0012	−.0007	−.0018	−.0020	−.0006	−.0003	.0018	−.0015
	(−.38)	(.48)	(−.36)	(−.23)	(−.57)	(−.68)	(−.18)	(−.10)	(1.63)	(1.55)
ValAdded	.0011	.0013	.0010	.0013	.0010	.0012	.0012	.0015	.0006	.0014
	(1.84)	(2.26)	(1.70)	(2.22)	(1.70)	(2.10)	(1.95)	(2.50)	(0.19)	(2.41)
UnionFatal	.0059	.0063	.0000	.0003	.0003	.0005	−.0008	−.0004	.0110	.0068
	(2.36)	(2.67)	(.04)	(.19)	(.78)	(1.45)	(−.48)	(−.22)	(1.05)	(.70)
NonunFatal	.0028	.0021	−.0003	−.0010	−.0007	−.0007	−.0017	−.0025	−.0288	−.0300
	(1.51)	(1.26)	(−.51)	(−1.49)	(−1.34)	(−1.47)	(−1.55)	(−2.36)	(−2.60)	(−2.93)
DisabPay/Avg	−.0004	−.0004	−.0002	−.0001	−.0003	−.0003	−.0001	−.0000	−.0000	−.0001
Wage×InjDays	(−1.33)	(−1.14)	(−.49)	(−.18)	(−1.02)	(−.90)	(−.35)	(−.03)	(−0.09)	(.31)
Adj R^2	.43	.47	.43	.47	.43	.46	.44	.48	.43	.47

different from zero.

Setting aside the issue of statistical significance, an increase of one standard deviation in measured WC generosity could account for a wage decrease of somewhat under 2%. From a policy perspective, however, it is important to bear in mind that most of the variation in this measure is generated by the risk interaction, not differences in the state wage replacement limits. In general, most demographic variables tend to be statistically significant, while the industry dummies and most industry level characteristics (including most risk variables) are not. This may reflect either the greater role of workers' personal characteristics in wage determination or simply the consequence of measuring one set of variables at the individual level and the other at the level of group averages. In this context it should be borne in mind that industry-level characteristics, as employed in this paper, are proxies for influences operating above the level of the individual. They would presumably take different values for workers in different occupations or firms in a fully specified model. We performed F-tests on the industry-level variables as a group; in all specifications we could reject the hypothesis that they are collectively statistically insignificant.

Discussion

Estimations of wage compensation for risk are highly sensitive to model specification and choice of risk variable; indeed, in only a few specifications does statistically significantly positive compensation appear at all. Moreover, incorporation of industry-level controls appropriate to a world of less-than-perfect competition results in the near disappearance of evidence for offsetting wage differentials for risk of fatal and nonfatal injury, and the sole risk of fatality measure that generates positive compensation estimates in this specification, NTOF, is the one that possesses the least plausibility. These results cast doubt on the very existence of compensating differentials for all workers, union and nonunion alike.

The different results for union and nonunion workers pose an additional question, however. The presence of estimated wage compensation for risk does not, as we have seen, imply that this compensation is fully offsetting in worker utility, and that the estimates can therefore be extrapolated to provide a value of human life or health. Additional evidence is required for this second step. Yet the large differences in the wage-risk relationship between union and nonunion workers suggest the opposite, that the interpretation of risk coefficients as market willingness-to-pay is unwarranted. In general, only among those workers most insulated from labor market competition are these coefficients ever statistically significant and positive, and it is reasonable to suppose that the increased tendency of these workers to display wage compensation has more to do with bargaining power than with systematically different utility schedules. This interpretation, in turn, implies that even those workers receiving compensation may receive less-than-equalizing premia.

If coefficients on risk are not interpreted as workers' revealed preference for safety, however, how should they be interpreted? One possibility is that they represent the degree to which worker preferences, whatever they may be, are given weight in market outcomes. On this view, for example, unionized workers might receive a measure of wage compensation for risk while the nonunionized do not, not because of a difference in utility maps, but because they have a greater opportunity to influence the provision of wages and working conditions under circumstances in which market-based options alone do not provide sufficient leverage. One might say that workers who belong to a union are rewarded by their employers "as if" they had higher values of life and health.

Alternatively, we can say that life appears to be of little value for disadvantaged workers not because they attach less value to life, or even because their desired tradeoff between income and safety is sensitive to low wages—our use of LOGWAGE controls for this effect—but because they face a restricted set of options in which their preferences for safety are not given much weight. In plain terms, nonunion workers in dangerous jobs are, in many cases, simply unlucky: they have found their way into situations of high risk and low pay and would presumably move to a better job it they could. If such workers are numerous enough, their lives will appear disposable, as indicated by negative coefficients on risk. This would suggest a meaning to the phrase "value of life" different from the one that characterizes most of the literature, but it is hardly devoid of significance. From the perspective of public policy, dropping the assumption that risk coefficients fully reflect workers' desired tradeoffs strengthens the case for regulatory policies to promote safe working conditions, but differences in wage compensation across the work force provide a basis for assigning a higher priority to policies that target the conditions of the less-compensated.

In summary, the evidence adduced in this paper supports the view that one or more of the assumptions underlying the conventional theoretical model of equalizing differences is strongly inapplicable. In particular, the role of rent-sharing or other forms of strategic bargaining behavior (captured by value-added per worker and other industry variables) and the gender distribution of both risk and wages demonstrate that noncompetitive elements in modern U.S. labor markets are sufficiently strong to overcome the competitive tendency toward equalizing differentials. Unionized workers may or may not receive hazard pay, but nonunionized workers in dangerous jobs are likely to be paid less than their counterparts in less dangerous jobs—a result far more consistent with limited mobility or segmented labor markets than with the frictionless competitive model that is typically the basis for deducing compensating wage differentials.

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