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The impact of occupational safety and health regulation, 1973–1983

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Using a sample of manufacturing industries from 1973 to 1983, this article reexamines OSHA's impact on workplace safety. Evidence supporting OSHA's effectiveness is stronger than that presented in most previous studies but remains quite mixed. Only for the incidence of lost workday injuries and illnesses is there evidence of a statistically significant OSHA impact for an equation that is stable over the 1973–1983 period. The magnitude of the effect is modest, and the effect is not robust with respect to different risk variables. For the three risk variables examined, there is no evidence of endogeneity of the contemporaneous OSHA enforcement variable.

1. Introduction

■ The Occupational Safety and Health Administration (OSHA) began operations in 1971 as the most extensive effort to influence workplace conditions. This regulatory program also assumed broader significance as OSHA became a principal target of opponents of the new risk and environmental regulations. The overall regulatory strategy was to set standards for workplace health and safety, most of which were specification standards for workplace design (e.g., the width and spacing of handrails). The popular consensus that early OSHA operations epitomized ineffective governmental regulation was borne out in the academic studies as well (Oi, 1975; Mendeloff, 1979; Smith, 1976, 1979; Viscusi, 1979b, 1983; Zeckhauser and Nichols, 1979). Econometric studies found little or no impact of OSHA regulation on workplace safety, and this view continues to prevail in the literature. (See Viscusi (1986) for a review.)

No existing analysis, however, has considered the performance of OSHA after the mid-1970s. This failure to address the recent OSHA experience is of fundamental importance because there has been a major overhaul of OSHA's enforcement effort during the past two presidential administrations. In this article I shall update the analysis in Viscusi (1979b) to ascertain whether there has been a shift in OSHA's effectiveness or whether the additional information now available on risk levels and OSHA activity may now enable us to estimate reliably a small effect that OSHA may have had since its inception. Although the evidence is mixed, some of the results are more favorable to OSHA than those found in earlier studies.

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OSHA inspections may have a significant effect on workplace safety that occurs with a one-year lag. This effect is not, however, robust across risk measures or time periods of analysis, so that this study does not provide unqualified support of the agency's efficacy.

Section 2 provides a brief review of past studies of OSHA's efficacy, none of which has considered data more recent than those of 1978. In Section 3 I introduce the empirical framework to be used in my examination of a pooled sample of 20 two-digit manufacturing industries over an eleven-year period. The analysis focuses on a safety equation that relates the injury rate to a variety of variables, including current and lagged OSHA enforcement variables. The empirical results appear in Section 4, and Section 5 contains the conclusions.

2. Previous econometric studies of OSHA's effectiveness

■ OSHA's general approach to regulation has remained unchanged since its inception. OSHA sets standards for workplace design and workplace conditions (e.g., radiation exposure levels) that are rigid guidelines for industry behavior. To ascertain whether firms are in compliance, OSHA inspects firms and can issue penalties for existing violations and continued noncompliance. From an economic standpoint, firms will find compliance in their self-interest if the costs of compliance, which are often considerable, are below the expected costs of noncompliance.

In an earlier analysis in this Journal (Viscusi, 1979b), I assessed the impact of OSHA inspection and penalty variables on overall injury and illness rates and on enterprise investments in health and safety. For the 1972–1975 period analyzed, I was unable to find that OSHA had any significant effect on workplace safety. Similarly, the analysis of the 1974–1978 experience by Bartel and Thomas (1985) failed to yield any statistically significant effect of OSHA on the lost-workday accident rate. Because of the nature of the data on industry averages used in these studies, there may nevertheless be a small effect that could not be distinguished with available data.

An alternative to investigating data on industry averages is to use data on accident experiences at firms before and after an OSHA inspection. Examining firm-specific accident rates reduces the degree to which detailed information is lost in the aggregation process, but the focus on only inspected firms makes such analyses both more partial in scope and more susceptible to regression-to-the-mean effects if firms with temporarily high injury rates are the ones inspected.[1] Smith (1979) found that small firms inspected in 1973 exhibited a subsequent drop in the lost-workday rate, whereas firms inspected in 1974 did not. An update of this analysis by McCaffrey (1983) failed to yield any significant OSHA effects on manufacturing firms inspected in the 1976–1978 period. A possible explanation for this pattern is that the apparent 1973 effects of OSHA reflect either the overreporting of injuries in the initial years of OSHA or a remaining regression-to-the-mean effect that is more likely to be evident for smaller firms than for larger firms.

The strongest support of OSHA's effectiveness to date is Cooke and Gautschi's (1981) study of Maine manufacturing firms from 1970 to 1976. Their analysis of the relationship between the average days lost from injuries and OSHA citations produced evidence of a significant effect of OSHA on total lost workdays. There is the possibility, however, that the result is a spurious consequence of some regression-to-the-mean effect for firms that receive citations.

The size of the OSHA impacts in Cooke and Gautschi's analysis is consistent with the presence of such a bias. Their estimates imply that OSHA reduced lost workdays by 29 per 100 workers for firms with 200 or more employees and by 51 per 100 workers for firms with 300 or more workers. Since the average rate of total lost workdays in the manufacturing

[1] These studies attempted to control for such influences, and thus made the importance of this factor less than it might be in a less careful analysis.

industry was 79.5 per 100 workers in 1976, estimates of this magnitude imply that the rate of lost workdays would have been 50% greater in 1976 in the absence of OSHA enforcement. The 1976 rate was already above the rate of lost workdays in 1972, which was only 62.6 per 100 workers, so that for these estimates to be reasonable an explosion in the level of workplace hazards would have had to occur in the absence of OSHA. The implausibly high level of effects may stem from the biases involved in analyzing inspected firms only.

The final empirical approach is to analyze workers' compensation records. In Mendeloff's (1979) analysis of the California workers' compensation experience in 1947–1974, some injury categories exhibited increases after OSHA, and others declined.[2] The net effect is unclear. As in the other studies, there was no clearcut evidence in support of the agency's effectiveness.

3. Specification of the safety equation

■ The basic empirical framework used here follows that in Viscusi (1979b), and as a result will be sketched only briefly. The analysis here focuses on a sample of two-digit manufacturing industries from 1973 to 1983. The sample size is 220. Although OSHA began operations in 1971 and was fully operational in 1972, there was a change in the reporting system for injuries that led to a break in the risk series in 1971 and to a new injury definition beginning in 1972. Because of the inclusion of a lagged risk variable in the analysis, 1973 will be the starting year.[3] Focusing on two-digit manufacturing data avoids the problems of industry reclassification and missing data present in the three-digit series as well as the severe comparability problems with the OSHA statistics for the construction industry.[4]

The dependent variable for the subsequent analysis will be some measure of the risk level for industry i in year t, which I shall denote by $RISK_{it}$ for purposes of the general equation given below. Three different risk data series will be used. The first series, which is the only measure analyzed in Viscusi (1979b), is the most comprehensive as it pertains to the annual frequency of all injuries and illnesses recorded by the firm. As a rough rule, this series includes all accidents and work-related illnesses severe enough to restrict one's work or to require medical treatment (other than first aid). The comprehensiveness of this measure is consequently coupled with considerable ambiguity regarding the classification of minor accidents, so that there are substantial reporting difficulties. The overall injury and illness rate per 100 workers will be denoted by *IR*. The second measure is the annual frequency of cases that are severe enough to lead to at least one lost day of work or restricted workdays. This accident category is consequently more precisely defined than is *IR*. The incidence per 100 workers of cases involving lost-workday injuries or illnesses will be indicated by *LW*. The final risk measure is the total number of days of work lost per 100 workers due to injury and illness, *LWDAYS*. Unlike *LW*, which measures only the incidence of particular cases, this measure weights their severity by the amount of work the employee loses.

The bounded nature of the risk variables must be taken into account when they are used as dependent variables. In each case the value of the variable is nonnegative. Moreover, *IR* and *LW* represent the annual probabilities of occurrence of particular accident types, scaled up by a factor of 100. If we assume as a rough approximation that the chance of

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[2] In an update of his work to more recent years, Mendeloff has also found that some of the earlier effects were not robust (oral communication with the author).

[3] The 1971 risk figures using fragmentary, unpublished data were included in Viscusi (1979b) because the absence of a long time series for analyzing OSHA's impact made inclusion of even a highly imperfect set of data more attractive than it would be now, when additional data are available.

[4] In particular, OSHA's counting of construction inspections is different from that of manufacturing inspections. For a construction inspection where there are multiple contractors at a particular site, an inspection is counted for each of the contractors; data for these multiply counted inspections are not comparable to manufacturing inspection data.

multiple accidents for a particular worker is small, then we can treat *IR* and *LW* as being in the range [0, 100]. Following my approach in Viscusi (1979b), we can then use the logistic transformation of the variables as the dependent variable. The log-odds of the risk variables used will be ln $(IR/(100 - IR))$ and ln $(LW/(100 - LW))$. The effects of OSHA were similarly weak for other transformations, and for the sake of comparability with my earlier work I selected the log-odds measure. In the case of the rate of total lost workday *LWDAYS*, I shall use the natural logarithm of this variable to avoid the variable's otherwise constrained nature.

The mean risk levels for the sample of two-digit manufacturing industries analyzed appear in Table 1. From 1975 on, all three measures performed in similar fashion. The overall injury rate rose until 1977, was somewhat lower in 1978 and 1979, and dropped significantly in the 1980s. The changes for *LW* and *LWDAYS* are comparable, as they rose until 1979 and declined thereafter. The decline in the 1980s is due at least in part to the cyclical downturn over much of that period, which tends to lower the accident rate. The principal puzzle is why the overall injury and illness rate declined from 1972 to 1975, while the other two measures did not. Government officials generally believe that a principal contributor to this trend was overzealous reporting under the new injury recording system. The two lost-workday measures are less subject to errors in classification and consequently did not reflect this bias in the early reports.

In the empirical work below I use an injury rate equation of the general form

$$RISK_{it} = \beta_0 + \sum_{k=1}^{n} \beta_k x_{kit} + \mu_i + \lambda_t + e_{it}, \tag{1}$$

where x_{kit} values pertain to a series of variables dependent on the industry i and year t, μ_i reflects omitted effects pertaining to the industry, λ_t reflects omitted influences that vary across time but not across industries, and e_{it} is an independent and identically distributed random error term. If one estimates equation (1) by ordinary least squares by including only an intercept and the x_{kit} variables, then the estimates of β_k will be biased.

To account for the influence of the omitted industry-specific and time-specific influences, I shall adopt a fixed-effects model. Thus, the underlying assumption in formulating this model is that the constant term for the $RISK_{it}$ equation is given by $\beta_0 + \mu_i + \lambda_t$, where μ_i is a constant industry-specific effect and λ_t is the constant time-specific effect common to all industries. The μ_i's and λ_t's are each assumed to be parameters that vary across industry and across time, respectively, but do not vary randomly. If these assumptions hold, the fixed-effects model will yield consistent estimates of OSHA's effect on safety.

TABLE 1 **Means of Risk Levels and OSHA Policy Variables for the Two-Digit Manufacturing Industry Sample, 1973–1983**

Year	*IR*	*LW*	*LWDAYS*	*INSPECT*	*PENALTY*
1973	14.6	4.3	67.5	.0020	.19
1974	13.9	4.5	72.1	.0025	.20
1975	12.6	4.4	75.0	.0031	.35
1976	12.8	4.8	79.5	.0025	.41
1977	12.8	5.0	81.9	.0022	.50
1978	12.4	5.3	81.8	.0020	.65
1979	12.4	5.5	88.6	.0019	.64
1980	11.5	5.1	86.0	.0019	.54
1981	10.9	4.9	82.2	.0016	.22
1982	9.8	4.3	73.5	.0027	.25
1983	9.7	4.2	72.1	.0034	.35
1973–1983	12.1	4.8	78.2	.0023	.39

If, however, the μ_i and λ_t values are not fixed, but instead are random and correlated with the x_{kit} values, then the fixed-effects assumption underlying the model does not hold. Even in this situation, however, the fixed-effects estimator will yield best linear unbiased estimates conditional upon the μ_i and λ_t values for the sample.

The specific equation to be estimated is of the form

$$RISK_{it} = \beta_0 + \beta_1 OSHA_{it} + \beta_2 OSHA_{it-1} + \beta_3 RISK_{it-1} + \beta_4 PRODUCTION_{it} + \beta_5 FEMALE_{it} + \beta_6 \% CNGEMPLOYMENT_{it} + \beta_7 HOURS_{it} + \beta_8 OVERTIME_{it} + \beta_9 DUMMY\text{-}SIC_i + \beta_{10} DUMMY\text{-}YR_t + e_{it}, \quad (2)$$

where the equation includes a current and lagged OSHA enforcement variable ($OSHA_{it}$, $OSHA_{it-1}$), a lagged dependent variable, the fraction of production workers in the industry ($PRODUCTION_{it}$), the fraction of female workers in the industry ($FEMALE_{it}$), the percentage change in industry employment over the past year ($\% CNGEMPLOYMENT_{it}$), average weekly work hours ($HOURS_{it}$), average weekly overtime hours ($OVERTIME_{it}$), dummy variables for the industry ($DUMMY\text{-}SIC_i$) to capture the fixed industry effects μ_i, and dummy variables for each year ($DUMMY\text{-}YEAR_{it}$) to capture the fixed time-specific effects λ_t. In this fixed-effects model there are dummy variables for 19 of the 20 SIC codes and 10 of the 11 years to control for omitted industry-specific and year-specific effects. To prevent singularities one dummy variable has been omitted in each case.

The use of the fixed-effects model makes possible a fairly parsimonious equation in terms of the number of substantive variables included. Measures of most safety-related variables are available only on an industry-specific basis rather than on an annual one. Inclusion of industry characteristic variables, such as unionization or census-based measures of the worker mix, does not affect the magnitudes of the other coefficients estimated, since these variables are simply linear combinations of the industry group dummy variables. Thus, inclusion of the age and race mix variables, as in Viscusi (1979b), instead of some of the industry dummy variables would not alter the estimates of OSHA's effect on safety. Similarly, the year dummy variables are intended to capture any time-specific influences in changes in injury reporting practices, changes in the nature of OSHA standards, and other temporal influences. Aspects of the design and enforcement of OSHA regulations that are not captured in the OSHA variables used here, but which are time-dependent, will consequently be reflected in the year dummy variables. Each equation also includes a set of variables that is available by industry for each year.

The variables of primary interest are the regulatory enforcement variables, $OSHA_{it}$ and $OSHA_{it-1}$. Unfortunately, there is no ideal OSHA variable available that measures the expected costs of noncompliance. One can, however, construct proxies for the enforcement stringency. Table 1 presents the mean levels of these OSHA variables on an annual basis for the industries in this sample.

The first of these is $INSPECT_{it}$, which is the frequency of OSHA inspections per production worker in industry *i* and year *t*. This expected inspection rate variable is an employment-adjusted measure of the firm's anticipated likelihood of an OSHA inspection. The main cost to the firm is the inspection event itself, since severe penalties are seldom assessed and, if the firm makes the mandated changes, penalties may be waived altogether. The firm may, however, incur substantial compliance costs as a consequence of the inspection, and it may face the threat of considerable penalties if it does not make the required changes. The *INSPECT* trends in Table 1 reflect the changes in the OSHA enforcement effort. Inspections began to decline in 1977, as OSHA eliminated inspections for trivial violations and shifted the emphasis toward more thorough inspections targeted at serious hazards. The rise in inspection rates in 1982 and 1983 is a consequence of the introduction of "records-check inspections," which involve inspection at the worksite of accident reports the firm prepared for the U.S. Bureau of Labor Statistics (BLS). Record checks count as inspections even though the OSHA official does not determine whether any particular physical characteristics at the workplace meet OSHA standards. The addition of these inspections increased the absolute number of inspections undertaken.

The second OSHA policy measure is the assessed penalties per production worker, $PENALTY_{it}$. This variable accurately reflects the expected penalties incurred, but it does not reflect the ultimate financial impact an inspection might have, since a firm will face an ever-escalating scale of fines for continued noncompliance. OSHA penalties have never exceeded \$25 million and have dropped to \$6 million annually in recent years. The *PENALTY* values for the sample summarized in Table 1 reflect a similar change in policy emphasis. Penalties per worker rose to a peak of \$.65 in 1978 and had declined to \$.35 by 1983. Punitive penalties for noncomplying firms have been deemphasized; OSHA focuses its efforts on negotiating compliance schedules, which, if not met, will lead to more substantial penalties. The penalty threat for continued noncompliance and the cost of the mandated changes in workplace conditions are the principal costs of an OSHA inspection. The low level of penalties suggests that the threat of a random inspection imposes little immediate cost, but, if inspected and found out of compliance, a firm will face considerably greater financial incentives for compliance.

For each equation estimated, I included both the current and the lagged OSHA policy measures, $OSHA_{it}$ and $OSHA_{it-1}$, since firms may not respond immediately to the level of OSHA inspections in their industry—because of both the lag involved in making capital investment decisions and the nature of the compliance schedules negotiated by noncomplying firms. Longer lag structures were also explored, but no significant effects were found beyond a one-year lag.

If OSHA inspections are targeted strategically on a contemporaneous basis with the risk level, one might encounter some simultaneity between $OSHA_{it}$ and $RISK_{it}$. In practice, the risk level affects the inspection targeting with a lag. Overall, 86% of all OSHA inspections are general programmed inspections. To the extent that OSHA uses the industry injury rate to target the inspection, it does so with the lag of over one year involved in the generation of the pertinent BLS statistics. Even in the Reagan Administration's ambitious targeting effort based on firms' accident reports—the records-check inspections—a lag is involved. A firm subject to a records check in 1986 will be given a detailed inspection then if its average lost-workday rate for 1984–1985 (or 1983–1985 for small firms) exceeds the 1984 lost-workday rate for manufacturing. This procedure necessarily involves a lagged effect of past injury experiences on enforcement and leads to a recursive rather than a simultaneous formulation.

The second leading inspection category is inspections stemming from worker complaints, which comprise 9% of all inspections. These inspections are generally ineffective and lead to few violations, perhaps because workers use these inspections to express other work-related grievances (U.S. Department of Labor, 1982). Follow-up inspections of previously inspected workplaces are also not affected by $RISK_{it}$. The only inspection category jointly determined with the current risk level is investigations of fatalities, but these fatality risks are so small that they do not affect the published overall risk measures and comprise only a negligible part of the enforcement effort.[5] As a result, there is not likely to be any perceptible bias stemming from the influence of current risk levels on the enforcement strategy.

The potential presence of a simultaneity problem has been discussed by several authors, including myself, and, in one case—Bartel and Thomas (1985)—the authors explicitly assume a simultaneous relationship between OSHA enforcement and job risks. There has not,

[5] Overall, under 2% of all inspections involve investigations of fatalities.

however, been any formal test of whether there is, in fact, evidence of a significant simultaneous relationship between these variables. To resolve the simultaneity issue, I carried out a series of Hausman (1978) tests to see whether the $OSHA_{it}$ variable was endogenous. The results, which are detailed in the Appendix, indicate that in all cases one can reject the hypothesis that OSHA enforcement efforts and the industry risk levels are simultaneously determined. The empirical properties of the equation consequently accord with our expectations based on the nature of OSHA enforcement efforts.

The other variables included in equation (2) are of subsidiary interest. The lagged dependent variable $RISK_{it-1}$ is included as a proxy for the firm's previous level of health and safety capital. OSHA enforcement efforts influence additions to this stock in any year so that the combined influence of new safety-related investments and the previous safety stock will determine the risk level. On the basis of theoretical considerations, one would expect the previous stock of health and safety capital to be positively correlated with the present risk level, so that $RISK_{it-1}$ should have a positive sign.

The remaining variables are BLS measures of worker mix and cyclical factors. The fraction of production workers in the industry ($PRODUCTION_{it}$) is intended to capture changes in the mix of jobs in the industry, where production jobs tend to be more risky on average than white-collar positions. Similarly, jobs with a higher fraction of female workers ($FEMALE_{it}$) should involve less physical effort and pose lower risk. Finally, one would expect accidents to be procyclical, because of the increased intensity of operations (in terms of work pace, use of night shifts, etc.) and the introduction of more inexperienced workers into new work situations. The three variables capturing this influence were the percentage change in the industry's employment (*%CNGEMPLOYMENT*), average weekly work hours ($HOURS_{it}$), and average overtime hours ($OVERTIME_{it}$).

It is noteworthy that the mechanism by which OSHA enforcement influences safety—safety-related investments in the workplace—is not included in the equation. Such data are not particularly reliable because of the difficulty involved in identifying the safety-related component of capital investments and changes in the reporting of such data. Previous analysis in Viscusi (1979b) failed to identify a significant role for this variable. With this variable omitted, the OSHA coefficients will reflect the direct effect of enforcement on safety as well as the indirect effect through enterprise decisions. These semi-reduced-form estimates consequently will capture the full effect of OSHA enforcement, which is the principal concern in the analysis.

4. Empirical results

■ **Risk equations.** Table 2 summarizes the estimates of the various $RISK_{it}$ equations by using the $INSPECT_{it}$ variable as the measure of OSHA regulatory enforcement. The results for the non-OSHA coefficients are similar for equations using the other OSHA variables. The equations in Table 2 will consequently serve as the general reference point for discussion, and later we shall consider the role of the OSHA variables in more detail. In each case 220 observations were included in the regression.

I shall begin by focusing on the general character of the results for variables other than OSHA enforcement, as the OSHA variables' effects will be discussed subsequently with respect to a broader set of regression findings. Since the results parallel those in Viscusi (1979b), we shall discuss them only briefly. The most powerful variable overall is the lagged dependent variable, which has a consistent impact across all three equations. Roughly two-thirds of the risk level in any year will be transmitted to the subsequent year. Because of the capital-intensive nature of health and safety investments, firms that are risky are likely to remain so.

The cyclical variable that is consistently statistically significant is *%CNGEMPLOYMENT*. As expected, there is a positive relationship between increases in the employment

TABLE 2 **Job Risk Regression Results, 1973–1983**

Independent Variables*	Coefficients (Standard Errors)		
	$\ln\left[\frac{IR_{it}}{100 - IR_{it}}\right]$	$\ln\left[\frac{LW_{it}}{100 - LW_{it}}\right]$	$\ln(LWDAYS_{it})$
Intercept	−.404	−1.122	1.749
	(.542)	(.631)	(.897)
$INSPECT_{it}$	12.400	9.610	6.069
	(6.682)	(7.612)	(9.715)
$INSPECT_{it-1}$	−12.800	−16.637	−25.862
	(7.347)	(8.325)	(10.736)
Lagged Dependent Variable	.701	.618	.598
	(.050)	(.060)	(.068)
PRODUCTION	.0021	.0083	.0096
	(.0130)	(.0038)	(.0048)
FEMALE	−.0011	−.0060	−.0076
	(.0033)	(.0037)	(.0047)
%CNGEMPLOYMENT	.0071	.0071	.0046
	(.0012)	(.0013)	(.0017)
HOURS	−.0090	−.015	−.013
	(.0115)	(.013)	(.017)
OVERTIME	.013	.031	−.0037
	(.018)	(.020)	(.0262)
1974 d.v.	.0096	.077	.079
	(.0165)	(.019)	(.024)
1975 d.v.	−.0069	.106	.138
	(.0216)	(.025)	(.032)
1976 d.v.	.015	.129	.135
	(.024)	(.025)	(.035)
1977 d.v.	−.0012	.115	.123
	(.0201)	(.024)	(.033)
1978 d.v.	−.039	.144	.101
	(.019)	(.025)	(.035)
1979 d.v.	.0050	.181	.196
	(.0199)	(.027)	(.034)
1980 d.v.	−.042	.128	.143
	(.021)	(.031)	(.040)
1981 d.v.	−.059	.112	.117
	(.025)	(.030)	(.041)
1982 d.v.	−.113	.042	.036
	(.026)	(.029)	(.039)
1983 d.v.	−.074	.077	.100
	(.028)	(.027)	(.038)
$\bar{R}^2$	.99	.99	.98

* Each equation also includes a set of 19 industry dummy variables.

level in the industry and the observed risk level. This effect reflects the procyclical nature of job accidents.

The year dummy variables are of potential policy interest to the extent that they reflect omitted aspects of the OSHA enforcement effort. No consistent pattern is evident, however. The year dummy variables differ in sign depending on the risk variable, as they are usually negative for the overall injury and illness rate and positive otherwise. Much of this difference may be due to initial overreporting of total job injuries, which is more of a problem for the *IR* variable since it is more susceptible to classification problems.

The 1982 and 1983 dummy variables have particular policy significance since there is the possibility that some firms may have begun to misrepresent their lost-workday incidence statistics once records checks began to target inspections by using this risk measure in October 1981. No evidence of any such time-related downward shift in reported values of *LW* is observed, however. The coefficients of the time dummy variables for 1982 and 1983 are smaller than those for previous years, but not out of line with their trend since 1979.

□ **OSHA impacts.** The effect of the OSHA enforcement variables is summarized for different time periods of estimation in Tables 3(a)–3(c). Table 3(a) presents the results for the log-odds injury rate equations, Table 3(b) presents the findings for the log-odds of the incidence of lost-workday injuries and illnesses, and Table 3(c) presents the findings for the natural logarithm of the rate of total lost workdays.

In each case the results are presented for several time periods of particular policy interest. I shall consider each of these time periods in succession. The 1973–1975 estimation period is similar to that in Viscusi (1979b) and in other studies of the effectiveness of early OSHA operations.[6] For none of the specifications estimated in Tables 3(a)–3(c) is any significant OSHA effect on risk apparent during 1973–1975. This result is consistent with the literature.

The principal time period of interest for this study is 1973–1983. For the results pertaining to the overall injury and illness rate in Table 3(a), the *PENALTY* variables are never statistically significant at the 5% level. The only statistically significant coefficient with a negative sign is that of $INSPECT_{it-1}$. Because the contemporaneous $INSPECT_{it}$ variable has a positive sign, the combined influence of the *INSPECT* variables is less than that of the lagged value of $INSPECT_{it-1}$ alone, and the net effect is that the two influences nearly cancel out each other. Even when considered individually, the lagged effect of inspections is small in absolute terms; it reduces the rate of injuries and illnesses by an average of .3 per 100 workers. Because the level of the *IR* variable itself is not high, the percentage reduction in injuries generated by $INSPECT_{it-1}$ is somewhat greater—on the order of 2–3%. Owing to the mixed signs of the OSHA coefficients, however, any evidence of OSHA's effect on the total injury rate measure is, at best, questionable.

The results in Table 3(b) for the log-odds of the lost-workday rate equations show a significant effect for the 1973–1983 time period. The $INSPECT_{it-1}$ variable is the mechanism of influence. On a percentage basis, the overall effect of the two *INSPECT* variables combined is about 1.5% and for $INSPECT_{it-1}$ alone about 3.5%. Once again, the *PENALTY* variables are never statistically significant.

The 1973–1983 results for the rate of total lost workdays in Table 3(c) provide by far the strongest evidence of OSHA's effectiveness in promoting worker safety. The lagged $INSPECT_{it-1}$ effect in the 1973–1983 equation is significant, as before. It indicates an effect of 6% on injuries, or 5% after netting out the influence of $INSPECT_{it}$. In percentage terms this is a much larger effect than on the incidence of lost workdays. These results consequently suggest that OSHA's efforts have yielded the greatest dividends for the most severe classes of risk, injuries of long duration, a result consistent with the increased emphasis placed on serious violations in the enforcement effort.[7] In addition, although the size of the effects is almost an order of magnitude below that in Cooke and Gautschi (1981), the presence of a particularly large impact on the rate of total lost workdays is consistent with their finding that at the firm level OSHA has a major effect on severe risks.

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[6] As noted above, the year 1972 was not included because of problems with the 1971 BLS risk data, which are so unreliable that they have not been publicly released.

[7] More specifically, the proportion of OSHA violations that were designated as being "serious" by the OSHA inspector rose from .02 in FY1976 to .11 in FY1977, .25 in FY1978, .34 in FY1980, and .38 in FY1983 (on the basis of data generated by OSHA for this study). These data are consistent with a general reorientation of inspections toward more serious violations, but they do not provide conclusive evidence of such a linkage. For example, a deterioration in work safety could have led to more serious violations' being discovered.

TABLE 3 Summary of Results for Injury Rate, Lost-Workday Case Rate, and Rate of Lost-Workdays Equations

		(a) Injury Rate (*IR*) Equations						(b) Lost-Workday Case Rate (*LW*) Equations						(c) Rate of Lost Workdays (*LWDAYS*) Equations					
		Coefficients (Standard Errors)		Combined Mean Reduction in Risk Level		$OSHA_{it-1}$ Mean Reduction in Risk Level		Coefficients (Standard Errors)		Combined Mean Reduction in Risk Level		$OSHA_{it-1}$ Mean Reduction in Risk Level		Coefficients (Standard Errors)		Combined Mean Reduction in Risk Level		$OSHA_{it-1}$ Mean Reduction in Risk Level	
Estimation Time Period	OSHA Variable	$OSHA_{it}$	$OSHA_{it-1}$	Absolute	%age	Absolute	%age	$OSHA_{it}$	$OSHA_{it-1}$	Absolute	%age	Absolute	%age	$OSHA_{it}$	$OSHA_{it-1}$	Absolute	%age	Absolute	%age
1973–1975	*INSPECT*	14.281	−17.349	*	*	*	*	7.079	−7.265	*	*	*	*	18.659	−16.818	*	*	*	*
		(21.845)	(22.466)					(23.289)	(23.536)					(29.128)	(28.597)				
	PENALTY	−.019	−.128	*	*	*	*	−.050	−.056	*	*	*	*	−.153	−.132	*	*	*	*
		(.120)	(.165)					(.132)	(.180)					(.156)	(.213)				
1973–1983	*INSPECT*	12.400	−12.800	−.01	−.04	−.3	−2.6	9.610	−16.637	−.1	−1.5	−.2	−3.6	6.069	−25.862	−3.0	−4.7	−4.3	−6.1
		(6.682)	(7.347)					(7.612)	(8.325)					(9.715)	(10.736)				
	PENALTY	.024	−.018	*	*	*	*	.016	−.026	*	*	*	*	.015	−.025	*	*	*	*
		(.015)	(.015)					(.017)	(.017)					(.023)	(.022)				
1973–1979	*INSPECT*	−3.061	−6.104	*	*	*	*	−.678	−10.112	*	*	*	*	−8.353	−18.298	*	*	*	*
		(11.280)	(9.833)					(12.063)	(10.613)					(14.091)	(12.373)				
	PENALTY	.009	−.010	*	*	*	*	−.006	−.014	*	*	*	*	−.012	−.013	*	*	*	*
		(.018)	(.020)					(.019)	(.021)					(.023)	(.026)				
1980–1983	*INSPECT*	10.771	9.084	*	*	*	*	13.047	6.055	*	*	*	*	7.281	.498	*	*	*	*
		(9.818)	(12.633)					(12.444)	(15.980)					(16.241)	(21.244)				
	PENALTY	.066	−.008	*	*	*	*	.100	−.008	*	*	*	*	.092	.023	*	*	*	*
		(.034)	(.026)					(.041)	(.033)					(.054)	(.041)				

* Effects are not reported in cases where no OSHA coefficients are negative and statistically significant at the 5% level, one-tailed test.

The apparent evidence of a significant effect of the lagged OSHA inspection variable on both the lost-workday incidence rate and the rate of total lost workdays raises the broader issue of why Tables 3(b) and 3(c) imply relatively strong OSHA effects compared with most findings in the literature. One possibility is that OSHA may always have been effective, but only with more years of data is it possible to estimate the relationship precisely. Alternatively, there may have been a shift in OSHA's effectiveness over time so that more recent OSHA inspections have a stronger effect on safety. A third, and not unrelated, possibility is that there has been a shift in the underlying structure of the equations so that the pooled results are not valid.

To examine these possibilities, Tables 3(a)–3(c) report results for two different subperiods, 1973–1979 and 1980–1983. The 1980 break point in the risk data corresponds to a break point of 1979 in the lagged *OSHA* variable, which is the primary mechanism of influence. The year 1979 was selected as the policy shift point for OSHA because it represented the first full year of the recent change in OSHA's emphasis. In October 1978 Assistant Secretary of Labor Eula Bingham eliminated 928 "nitpicking" regulations to enable OSHA inspectors to focus on a more streamlined set of regulations. Explicit targeting of OSHA inspections by the OSHA policy office also began in the 1978–1980 period and has been furthered by the Reagan Administration's introduction of records-check inspections in October 1981. To isolate any specific effects of records-check inspections, I also estimated exploratory regressions that included an interaction of the OSHA variables with 1982–1983 dummy variables. No significant shift in OSHA's effectiveness was observed. Since the case-hours per inspection have dropped, the absence of a drop in the estimated impact of the OSHA variable is consistent with a possible increase in the efficacy per hour of inspection time, which one would expect if inspections are targeted more efficiently.

For the overall injury results in Table 3(a), one can reject the hypothesis that the regression coefficients are stable across the 1973–1979 and 1980–1983 subperiods.[8] Thus, the effect of the OSHA variables in the 1973–1983 equations, which on balance was roughly zero, may not be a reliable measure of the agency's impact. Examining the OSHA effects for the two subperiods does not, however, alter the spirit of these results. There are no significant OSHA coefficients in either of the two subperiods, so that the overall injury rate variable provides no strong evidence of OSHA's efficacy for any of the time periods considered. Since this risk measure is the one most susceptible to reporting problems, as noted earlier, these results do not, however, serve as a very reliable test of the agency's effectiveness.

In the case of the lost-workday incidence rate results reported in Table 3(b), one cannot reject the hypothesis that the coefficients are stable across the two subperiods.[9] Thus, because of its larger sample size, the 1973–1983 results, which yielded a statistically significant effect of the $INSPECT_{it-1}$ variable, are the most meaningful findings. Nevertheless, it is noteworthy that there is no apparent shift of the OSHA coefficients in the negative direction between 1973–1979 and 1980–1983, as one would expect if OSHA were becoming increasingly effective. Indeed, the coefficients for the inspection variables are both positive, but not statistically significant in 1980–1983. Thus, the evidence of OSHA's efficacy in the lost-workday incidence results appears to be the consequence of a larger sample rather than any evident improvement in OSHA's efficacy over time.

For the results pertaining to the rate of total lost workdays in Table 3(c), one can reject the hypothesis that the coefficients are stable across the two subperiods.[10] In this instance, as for Table 3(a), coefficients other than the OSHA variables were also shifting over time.

[8] The calculated F-statistic of 1.85 is above the critical F-values of approximately 1.54 (5% probability) and 1.83 (1% probability).

[9] The calculated F-statistic of 1.39 is well below the 5% F-value of approximately 1.54.

[10] The calculated F-statistic of 2.34 leads to rejection, at all reasonable significance levels, of the hypothesis that the coefficients are stable.

As a result, the strong OSHA effects found for 1973–1983 may be attributable, at least in part, to structural shifts other than those stemming from regulatory influences. Moreover, the temporal pattern of the coefficients is almost the opposite of what one would expect if OSHA inspections were becoming increasingly effective. The inspection variables' coefficients for 1980–1983 are positive and statistically insignificant at the usual levels, whereas the 1973–1979 coefficients are both negative and jointly significant at the 5% level, with the $INSPECT_{it-1}$ variable's being individually significant at the 10% level for a one-tailed test. The results for the rate of total lost workdays provide mixed overall support of OSHA's efficacy, but do not provide any support for the hypothesis that OSHA has been increasingly effective in recent years.

The following general patterns emerge. First, the evidence presented here regarding the agency's effectiveness is more favorable than the prevailing view in the literature, but it is still very mixed. Only the two lost-workday accident-risk measures yield any significant negative influences, and these effects are modest in terms of their size and are of questionable validity in the case of the rate of total lost workdays.

Second, when there is a significant effect, it is the *INSPECT* variable rather than *PENALTY* that is instrumental. The presence of OSHA and the threat of fines for continued noncompliance are more important than the modest financial incentives created by fines for violations discovered in a random inspection. The recent reduction in penalties for initial violations consequently may not have a major effect on safety, provided that there are major penalties for continued noncompliance.

5. Conclusion

■ Although OSHA's initial efforts are widely regarded as having been ineffective, the substantial reforms in OSHA's enforcement strategy during the past two presidential administrations raise the question of whether this conclusion remains valid. Furthermore, the additional data now available on OSHA's performance make it possible to get a broader perspective on the agency's impact than was afforded by the more limited studies of the 1970s. It appears that having a longer time period to assess the agency's impact is the more influential of these two considerations since there is no evident shift in the OSHA regulation coefficients in recent years.

Based on the results presented here, any conclusion regarding the efficacy of OSHA's regulatory policy must necessarily be guarded. It clearly is not correct to conclude that the agency has no effect on safety whatsoever. Although this result is not strong support for OSHA, the likelihood that the agency has a nonzero risk-reducing impact does represent an improvement over OSHA's traditional standing in the literature.

The significant influences of OSHA on safety that were observed are neither dramatic in terms of their magnitude nor robust across different measures of risk. The overall injury rate series provides no support of a positive net effect. The lost-workday incidence rate data suggest a significant effect for 1973–1983 for an equation that is stable over time. On the basis of these results, OSHA's effect appears to be in the range of 1.5–3.6% of the current lost-workday incidence rate. Viewed somewhat differently, OSHA prevents from 1–2 injuries involving at least one lost day of work per 1,000 workers annually. Analysis of subperiods of data for this risk measure provides no evidence of increased efficacy, however. Finally, the rate of total lost workdays measure provided the strongest support over the 1973–1983 period, but this equation shifted structurally over time. Further research is needed to ascertain whether the changes in the accident equation structure are due to OSHA or to other economic factors.

The existence of some beneficial effect on worker safety is expected on economic grounds. If the standards are related to worker safety, enforcement that increases firms' efforts to meet those standards will improve worker well-being. Although the actual penalties levied are inconsequential, the threat of additional penalties for continued noncompliance coupled with the role of workplace inspections has had a safety-enhancing effect. Firms' investments in health and safety should respond in this direction, although the magnitude of the impact has long been a matter of dispute.

Since the safety gains have fallen far short of the expectations at the time the agency was established, there is a need for greater realism when projecting the benefits of future regulations. For example, the usual assumption in regulatory analyses that regulations will completely eliminate particular risks is at sharp variance with reality.

Whether OSHA regulations are on balance beneficial is difficult to ascertain. The costs associated with compliance run in the billions, and there has never been a precise tally of the costs actually incurred, as opposed to the prospective costs calculated at the time of promulgation of the regulation. Most available cost studies fail to show examples where the benefits of OSHA standards exceed the costs, although the recent OSHA hazard communication standard is a prominent exception.

The existence of some significant beneficial effect should, however, suggest that the regulatory strategy is not so intrinsically flawed that OSHA can never play a constructive role. The remaining task for policy is to structure the regulatory approach and the enforcement strategy to ensure that the overall impact of the policies is in society's best interests.

Appendix

■ To test for the simultaneity of $OSHA_{it}$ and $RISK_{it}$ in equation (2) I use a Hausman (1978) specification test for simultaneity. The procedure involves developing an instrumental variables estimator for $OSHA_{it}$, which I shall denote by $OSHA^*_{it}$. Including this as an additional variable in equation (2) yields an associated coefficient β^*_1. If the coefficient of β^*_1 is statistically significant, then we cannot reject the hypothesis that the $OSHA_{it}$ variable is endogenous, and thus we must use an estimation procedure to address the simultaneity problem.

There is some flexibility in the choice of instruments that will be used to create $OSHA^*_{it}$. We must select which additional exogenous variables not in equation (2) we shall use as instruments and which particular exogenous variables from equation (2) we shall use as instruments. Since multicollinearity may be a problem, some or all of the exogenous variables may not be good instruments, as noted by Spencer and Berk (1981). The procedure we adopted was to use all other variables in equation (1) except for $OSHA_{it-1}$ and to augment this set with the fraction of firms in the industry that were found to be in compliance with OSHA regulations and the employee coverage per inspection (current and lagged one and two periods). The simultaneity tests were quite robust with respect to the choice of instruments.[11]

Table A1 reports the specification test results for the risk equations using the OSHA inspection variables (*INSPECT*) for the time periods 1973–1983, 1973–1979, and 1980–1983. Similar results were obtained with the *PENALTY* variable, but these findings are of subsidiary interest since none of the *PENALTY* variables is statistically significant in the equations estimated. The reported results are for equations using each of three risk measures. In each case we can reject the hypothesis of simultaneity at all reasonable significance levels.

TABLE A1

	Coefficients and Standard Errors for $OSHA^*_{it}$		
Dependent Variable	1973–1983	1973–1979	1980–1983
$\ln((IR_{it})/(100 - IR_{it}))$	−6.639	−38.884	−15.853
	(11.903)	(30.622)	(16.041)
$\ln((LW_{it})/(100 - LW_{it}))$	−5.401	−42.460	−14.674
	(13.458)	(32.619)	(20.566)
$\ln(LWDAYS_{it})$	−9.183	−48.916	−39.913
	(17.735)	(38.871)	(26.227)

[11] The exogenous variables not in the equation (the compliance rate and the three employee coverage per inspection variables) were also used separately as instruments, in conjunction with the SIC code dummy variables, and in conjunction with the SIC code and year dummy variables, and yielded similar results.

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