Cornell University
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Industrial & Labor Relations Review

Volume 58 | Number 4 Article 3

2005

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Abstract

This study examines the impact of OSHA inspections on injuries in manufacturing plants. The authors use the same model and some of the same plant-level data employed by several earlier studies that found large effects of OSHA inspections on injuries for 1979–85. These new estimates indicate that an OSHA inspection imposing a penalty reduced lost-workday injuries by about 19% in 1979–85, but that this effect fell to 11% in 1987–91, and to a statistically insignificant 1% in 1992–98. The authors cannot fully explain this overall decline, which they find for nearly all subgroups they examine—by inspection type, establishment size, and industry, for example. Among other findings are that, across the years studied, inspections with penalties were more effective than those without, and the effects on injury rates were greater in smaller plants and nonunion plants than in large plants and union plants.

Keywords

OSHA inspections, manufacturing injuries, work injuries, lost-workday injuries

Cover Page Footnote

The authors thank three agencies for financial help: the National Institute of Occupational Safety and Health (R01-OH03895-03) for supporting this analysis and the creation of the 1992–98 data set; the National Science Foundation (SES-8420920) for supporting creation of the 1979–85 data; and the Bureau of Labor Statistics (research contract J-9-J-5-0085) for supporting creation of the 1987–91 data. They also thank John Ruser, John Scholz, and David Weil for valuable comments on an earlier draft of this manuscript, Joseph DuBois for his help in working with the OSHA data, and Nichola Thomson for research assistance.

THE DECLINING EFFECTS OF OSHA INSPECTIONS ON MANUFACTURING INJURIES, 1979–1998

WAYNE B. GRAY and JOHN M. MENDELOFF*

This study examines the impact of OSHA inspections on injuries in manufacturing plants. The authors use the same model and some of the same plant-level data employed by several earlier studies that found large effects of OSHA inspections on injuries for 1979–85. These new estimates indicate that an OSHA inspection imposing a penalty reduced lost-workday injuries by about 19% in 1979–85, but that this effect fell to 11% in 1987–91, and to a statistically insignificant 1% in 1992–98. The authors cannot fully explain this overall decline, which they find for nearly all subgroups they examine—by inspection type, establishment size, and industry, for example. Among other findings are that, across the years studied, inspections with penalties were more effective than those without, and the effects on injury rates were greater in smaller plants and nonunion plants than in large plants and union plants.

Since Congress's establishment of the Occupational Safety and Health Administration (OSHA) in 1970 to prevent occupational injuries and illnesses, there has been considerable debate over the program's effectiveness. Each year, OSHA conducts tens of thousands of inspections and imposes millions of dollars in penalties, but most workplaces are only rarely visited, penalties are low relative to the cost of abating many workplace hazards, and many injuries are unrelated to OSHA standards.

Many empirical studies examining OSHA have been done, using both industry-level and plant-level data. Most of this research, including industry-level work by Viscusi (1979) and Bartel and Thomas (1985) and plant-level work by Smith (1979), McCaffrey (1983), and

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This paper uses confidential micro-data records of the Bureau of Labor Statistics (BLS), which can only be accessed at BLS Headquarters in Washington, D.C., and only by researchers who submit an acceptable research proposal to BLS. This paper's programs and datasets have been archived at BLS, and the authors would be happy to assist other researchers approved by BLS who wish to access them.

Industrial and Labor Relations Review, Vol. 58, No. 4 (July 2005).
0019-7939/00/5804 $01.00

Ruser and Smith (1991), has found little evidence of an impact on injuries; one exception is Viscusi's (1986) industry-level study, which found a statistically significant impact on injuries. In contrast to most of these prior findings, a series of studies by Scholz and Gray, using a large plant-level database for the 1979–85 period, found statistically significant effects. Depending on the analytical technique Scholz and Gray used, they found that an OSHA inspection that imposed a penalty was associated with a 15–22% decline in injuries over a three-year period (Scholz and Gray 1990; Gray and Scholz 1993).

The present study extends the Scholz-Gray data and analyses to more recent years. To the original 1979–85 dataset we add a 1987–91 dataset created earlier by Gray (1996), and a 1992–98 dataset created for this study. Although there are some differences in sample composition across the three data sets, we use the same variables and analyses for all three to make the results as comparable as possible. Notably, we limit the analysis to the 29 states in which the Federal OSHA administration operated the enforcement program, which were the only states with data available in the earlier periods. We conduct additional analyses for the 1992–98 period, for which we have data from all states and additional information such as union status not available for the earlier periods.

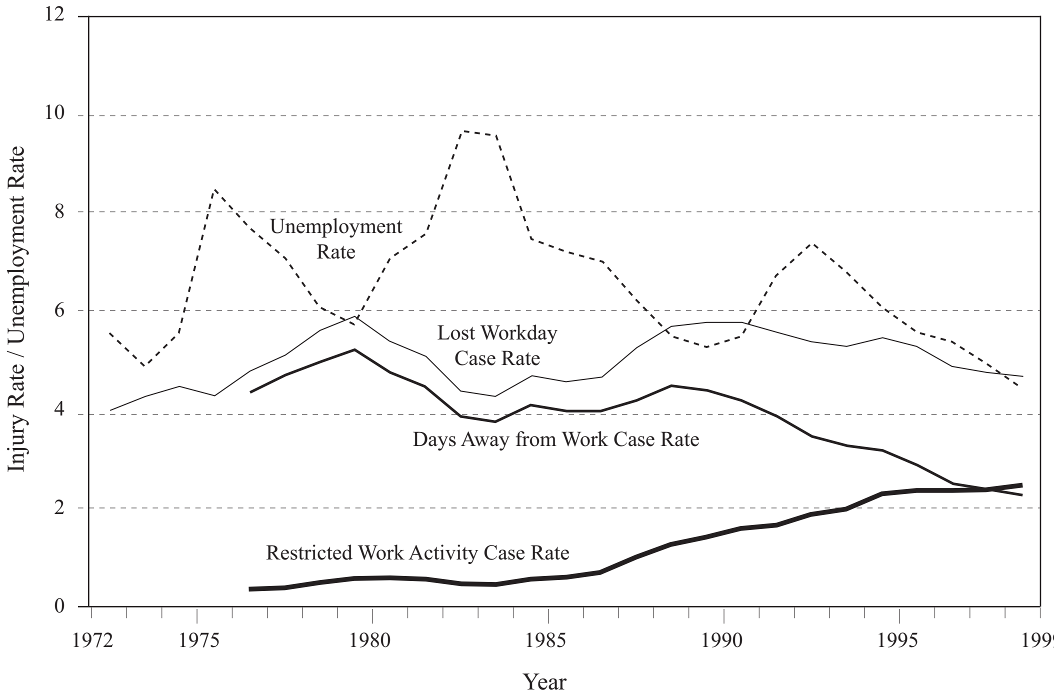
Background and Theory

Figure 1 shows the injury rate per 100 full-time manufacturing workers from 1972 to 1999. The numbers are based on reporting to the annual Survey of Occupational Injuries and Illnesses, conducted by the Bureau of Labor Statistics (BLS). The "lost workday case rate" is divided into two categories: cases with days away from work and cases with only restricted work activity. The rate for all lost-workday cases changed relatively little from 1972 until the early 1990s, except for the expected cyclical changes. Injury rates typically fall in recessions and increase in booms, primarily due to changes in the number of newly hired, inexperienced workers (Robinson 1988). However, in the 1990s the manufacturing injury rate dropped by about 25% despite continuous prosperity during those years. We also see in Figure 1 that the rate of injuries with restricted work activity rose substantially after the mid-1980s while the rate for cases with days away from work accounted for the decline in the 1990s (we return to this issue later).

OSHA may affect injuries through one or more of several mechanisms (Mendeloff 1979). The agency enforces a set of safety and health standards and may create new standards. It also provides information to employers and employees, both directly through consultations and training activities and indirectly through the provision of educational materials. Most of OSHA's resources are devoted to its enforcement program. Inspections, backed up by the threat of penalties for non-compliance, may push employers to comply with standards or even to improve their overall safety programs. The threat of inspection may also generate compliance actions in order to avoid expected penalties. Even though most workplaces are inspected infrequently, especially in industries with low injury rates, the ability of workers to request OSHA inspections enhances the inspections' potential deterrent effect.

Equation (1) summarizes a variety of factors that may influence the riskiness of working at plant i in year t ($\textsc{Risk}_{it}$). We begin with the inherent hazardousness of the plant, which may change over time ($\textsc{Hazard}_{it}$), the average experience or inexperience of the plant's work force ($\textsc{Exper}_{it}$), and the degree of worker fatigue ($\textsc{Fatigue}_{it}$). In addition, we have three factors affecting the attention paid by the plant to safety issues. The degree of general deterrence achieved by OSHA inspections at other plants in the same area and industry ($\textsc{GenDet}_{it}$) depends on both the expected probability of being inspected and the expected penalty for a violation (with penalty and probability getting equal weight if the firm is risk-neutral). There may be a separate impact of current or past inspections happening at this specific plant ($\textsc{Insp}_{it\text{-}s}$),

Figure 1. Changes in the Manufacturing Lost Workday Injury Rate per 100 Full-Time Workers and Its Components.



either because having an inspection leads the plant to revise its evaluation of the probability of future inspections or because OSHA follows up some inspections to ensure that hazards are corrected, with the possibility of much higher "failure to abate" penalties. Atten_{it} includes any other factors, such as plant unionization or workers' compensation costs, that could affect attention to safety.

$$\text{(1)} \quad \text{Risk}_{it} = f(\text{Hazard}_{it}, \text{Exper}_{it}, \text{Fatigue}_{it}, \text{GenDet}_{it}, \text{Insp}_{it\text{-}s}, \text{Atten}_{it}).$$

The actual number of injuries occurring in a workplace in a given year will depend strongly on the underlying riskiness, along with some random error term. These errors may be greater (in percentage terms) in smaller workplaces. To the extent that unusually high injuries at time t–1 lead to increased attention to safety issues at time t, we might expect some degree of negative autocorrelation in the unobserved random element of injuries.

One goal of this paper is to examine differences in the effects of inspections based on the characteristics of the establishment being inspected and of the inspection itself. The establishment characteristics we consider are the number of employees, whether the workers are represented by a union, and the establishment's industry. The inspection characteristics are whether a penalty was levied, the motivation for the inspection (programmed or complaint), the inspection type (safety or health), and whether the plant was located in a Federal OSHA or a State Plan state. We expect that the impact of an OSHA inspection will depend heavily on that establishment's intrinsic safety level, determined by the firm's demand and supply for safety. This affects how much "hazardousness" remains for OSHA to influence. In addition, plants could vary in their responsiveness to OSHA influence.

The firm's demand for safety will largely depend on the strength of the incentives provided by existing institutional arrangements. Smaller firms are partially insulated from the financial consequences of injuries by the limited extent of experience rating for them by workers' compensation insurers. Compared to larger firms, they may also be under less media scrutiny; large firms can incur large public relations costs if they are not perceived as good corporate citizens by their customers. The most trustworthy outcome data, which are for fatalities, do indicate that small establishments have fatality rates many times higher than those for large establishments in the same industry (Mendeloff and Kagey 1991).[1] Larger establishments also tend to have been in existence longer, so they will on average be more likely to have had prior OSHA inspections, probably diminishing the impact of the current inspection.

Larger firms and larger establishments are also more likely to have unionized work forces. Unions create a mechanism through which workers can bargain collectively over safety and health conditions. Unlike the market, which gives the greatest weight to the marginal worker, unions will tend to represent most fully those with the median preferences, who are likely to be older, more knowledgeable, and perhaps more risk-averse than the marginal worker. Viscusi found that wage premiums for risky jobs were considerably larger at unionized workplaces (1979b). Weil has shown that unionized workplaces are more likely than non-unionized ones to be inspected and that inspections there tend to be more intensive—taking more time and citing more violations (Weil 1991, 1996, 2001).[2] Unions also may make workers more knowledgeable about hazards and may increase their willingness to call for OSHA inspections in order to leverage their demands. In manufacturing in 1996–98, over 30% of complaint inspections were at unionized workplaces, compared to just 15% of programmed inspections.

On the supply side, larger firms and establishments are also more likely to employ on-site safety experts, whose presence increases awareness of government rules, reduces some of the marginal costs of meeting them, and also should foster the implementation of effective non-regulatory programs for injury prevention. These experts may also affect the demand side if they become in-house advocates for improved safety.

Industry factors may also play a role. If injuries are typically more costly in some industries than in others, then the incentive to prevent them will be greater there. Industry-specific technology will potentially affect not only the average cost of injuries, but also the average costs of prevention. For example, safety in outdoor environments (such as in logging) will be harder to maintain than in the more controllable environment inside a factory. Workers who are widely dispersed may be harder to monitor than those working in closer-knit units. It is also true that OSHA standards may be more relevant to the hazards in some industries than to those in others.

[1]For lost-workday injuries, the BLS Survey finds that there is an inverted "U" relationship between injury rates and establishment size, with the highest rate in the 50–99 category. The findings for fatalities suggest that the smallest establishments may under-report non-fatal injuries, although other interpretations are possible.

[2]In inspections in manufacturing from 1996 to 1998, we found that the number of serious violations cited in programmed inspections was larger for unionized workplaces than for nonunion workplaces for all size classes; the difference increases in larger size classes. For complaint inspections, the differences between union and nonunion workplaces were smaller, and the latter actually had a larger number of serious violations in establishments with fewer than 50 workers. However, it is possible that the higher number of violations cited at unionized workplaces reflects not a lower level of firm compliance, but either greater OSHA scrutiny or a lower threshold for an OSHA citation. Unions can and often do generate more pressure on employers to reduce workplace hazards.

The mechanisms used to generate OSHA inspections also interact with some of the characteristics discussed above. OSHA targeted its programmed inspections toward high-injury industries (based on state-industry injury rates), choosing inspection sites randomly within industry-state cells but excluding workplaces with fewer than 11 workers or those recently inspected (Siskind 1993). Complaint inspections were initiated by a written (formal) or oral (informal) notice from a worker or a union representative about an alleged violation or hazard at a workplace. Although large and small establishments had a roughly equal chance of receiving programmed inspections (at least for those with 11 or more employees), complaint inspections tended to be proportional to the number of workers at a workplace. As a result, the ratio of annual inspections to establishments in employment size classes ranged from 0.05 for establishments with fewer than 20 workers to 0.74 for establishments with more than 500 workers. Also, as noted earlier, inspections are more frequent at unionized than at non-unionized establishments. For these reasons, establishments that are large, unionized, or in high-injury-rate SICs are more likely to have had OSHA inspections. To the extent that OSHA inspections display declining marginal effectiveness, we might expect to find smaller effects there.[3]

Data

The basic data used to compare the impact of OSHA inspections over time come from three time periods: 1979–85, 1987–91, and 1992–98. These data remain essentially unchanged from those in the original Scholz-Gray analysis, pertaining to establishments that are in manufacturing industries and are located in the 29 Federal OSHA states where the primary enforcement responsibility is with OSHA (these states include about 60% of the national work force). Manufacturing workplaces have long been a focus of OSHA activity and are longer-lived and better-defined than workplaces in other sectors (such as construction). This is important, since we allow for the possibility that OSHA inspections affect injuries for a few years after the inspection. We combine establishment-level information on injuries and characteristics of OSHA inspections to create three comparable data sets. We also create a 50-state dataset for the 1992–98 period to test for differences in the impact of inspections based on establishment and inspection characteristics.

Our injury data come from the Bureau of Labor Statistics (BLS) Survey of Occupational Injuries and Illnesses, which gathers data for hundreds of thousands of establishments each year in a stratified sampling process that results in larger establishments being more likely to be in the sample. Since our model analyzes changes in an establishment's injuries over time, we require establishments to have BLS injury data for consecutive years. This necessarily results in large establishments being over-represented in our data sets, relative to all manufacturing establishments. We use the total number of lost-workday injuries during the year as our injury measure. Earlier work with the first two data sets also examined a measure of the seriousness of the injuries, the total number of days of work lost due to injuries at the plant; but because that information is not present after the BLS Survey was revised in 1992, we cannot use it here.

The BLS data are combined with information on OSHA inspections from OSHA's Integrated Management Information System (IMIS). One key determinant of inspection impact is whether a penalty was

[3]The long-term effects of prior inspections depend on whether the hazards that were cited and abated stay fixed. We suspect that when abatement involves changes in equipment, the correction is likely to last longer than when the hazard involves behavioral mistakes, but we would expect at least some degree of declining marginal effectiveness of inspections no matter what the category of hazard. Gray and Jones (1991) did report that, at least with respect to citing violations for overexposing workers to toxic substances, the first inspection has a bigger impact than subsequent ones.

imposed.[4] We also consider two types of inspections: programmed inspections, targeted by OSHA based on industry hazardousness, and complaint inspections, in which OSHA is responding to a written or oral worker complaint. These two types account for over 80% of all inspections during the time period studied.[5]

Following a technique developed by Fellegi and Sunter (1969) that calculates the probability of two records matching based on agreement or disagreement on their characteristics, we linked together the OSHA and BLS records, using name and address information to identify records that referred to the same establishment. The matching methodology is explained in more detail in Gray (1996).

Since our analysis focuses on injury changes, two consecutive years of BLS data are needed to generate one observation for analysis. Table 1 describes some features of the three data sets. The original Scholz-Gray data set was restricted to a balanced panel of establishments with BLS injury data available in all seven of the years from 1979 and 1985. Substantial cuts in the BLS Survey sample size later in the 1980s necessitated a broader sample in the later periods. The 1987–91 dataset includes all plants with at least two consecutive years of BLS Survey data; the 1992–98 dataset includes all plants with at least three consecutive years.

Econometric Issues

We use the following Scholz-Gray model as the basis of our analyses:

$$\text{(2)} \quad \mathrm{LWD}_{it} = a_t + b_0\textsc{Insp}_{it} + c_1\Delta\textsc{Emp}_{it} + c_2\Delta\textsc{Hour}_{it} + \mathrm{SIC2}_i + u_{it} + d_1 u_{it-1} + d_2 u_{it-2}.$$

The dependent variable (LWD) is the change in the log of the number of injuries, with b_0 showing the impact of OSHA inspections on the percentage change in injuries. Gray and Scholz (1993) performed extensive econometric tests of this specification using the 1979–85 dataset, finding strong evidence for the endogeneity of inspections when the dependent variable is not measured in "change" form: plants with more injuries get more inspections, yielding a (misleadingly) positive coefficient on Insp.[6] This endogeneity disappears when the change form is used. We follow the Scholz-Gray specification here to be consistent with that earlier work, and we apply the same model to all three time periods.

The focus of our model is on specific deterrence—reduction of injuries at the specific workplaces in which inspections occur. Having an inspection provides a "shock" that causes the plant to change its safety behavior, reducing workplace hazards and the expected number of injuries over time. We measure OSHA activity with a dummy variable, Insp, indicating that the plant had been inspected within the previous three years, so the change in injuries between 1983 and 1984, for example, depends on whether that plant had been inspected at any point between 1981 and 1984. In fact, our preferred inspection measure is PenInsp, which includes only inspections that imposed a penalty.[7] This

[4]As reported in Scholz and Gray (1990), having a serious violation cited during the inspection is essentially equivalent to having a penalty (95–99% overlap in our data).

[5]The remaining 20% are primarily accident and follow-up inspections, which we exclude from our analysis due to their limited focus. The 50-state data set for 1992-98 does include a small number of referral inspections (about 5% of the total), which usually represent referrals from a safety inspector to a health inspector or vice-versa; these are combined with the complaint inspections in the analysis.

[6]Using our data, we get a strong positive coefficient on Insp when using the level (rather than change) of injuries, even when lagged injuries are included as an explanatory variable.

[7]We tested alternative specifications of inspection effects: four "lagged inspection" dummies, total inspections in the last four years, total penalties imposed, number of violations found, and others (results available upon request). The PenInsp variable generally out-performed the other measures (consistent with a "shock" model of inspections or a diminishing marginal impact of repeated inspections). Using a single inspection dummy also makes it easier to compare effects across different inspection or plant characteristics.

follows Scholz and Gray (1990), who found that penalty inspections had a much greater impact on injuries than did non-penalty inspections.[8] In some models we allow for different effects at different-sized plants (PenInsp*Size dummies for 100–249, 250–499, and 500+ workers, with 1–99 workers as the base group); in others we allow for different inspection types: PrgInsp and CmplntInsp for all programmed and complaint inspections, or PrgInspP (PrgInspN) and CmplntInspP (CmplntInspN) for programmed and complaint inspections imposing (not imposing) penalties.

The other explanatory factors in equation (1) are changes in employment (ΔEmp_{it}) to measure changes in the experience of the work force and changes in hours (ΔHrs_{it}) to measure changes in worker fatigue. To the extent that innate hazardousness is fixed at a workplace, it is differenced out of the model by our use of injury changes. Trends in industry hazardousness or changes in general deterrence are measured by industry dummies (SIC2_i). Changes in OSHA policy or economy-wide trends in safety are absorbed into the year dummies, a_t. Finally, again following the Scholz-Gray model, we allow for the inclusion of second-order autoregressive errors in the model, expecting a surprisingly large number of injuries in one year to increase the plant's attention to safety, reducing injuries in the following year and generating negative autocorrelations in the errors (the d_1 and d_2 coefficients). There is also the possibility of heterogeneity in the errors, with smaller plants likely to see bigger percentage fluctuations over time; tests of a procedure allowing for robust standard errors yield statistical significance levels similar to those presented here.

One econometric method used in earlier analyses of the 1979–85 Scholz-Gray data was the Chamberlain[9] model, but unfortunately it was not practical to use that model with the 1992–98 data. The Chamberlain method requires a balanced panel. Given the smaller BLS injury data sample size in the 1990s, a balanced panel is a small and unrepresentative set of plants: only 11% of the establishments in our 1992–98 sample have BLS injury data for all 7 years, and these are almost all large plants, with only 7% having fewer than 250 employees. This would have precluded any analysis of OSHA's impact on smaller establishments, where OSHA does many of its inspections (and where our results show the largest inspection effects).

Results

Table 1 presents the means of the variables in the various data sets used in the analysis: data from Federal OSHA states for three time periods and an additional dataset from the 1990s that includes data from both Federal and State Plan states. Note that we observe declining injuries in each period, with relatively steep declines in days-away-from-work injuries offsetting increases in restricted work activity injuries. We see declines in employment and hours worked in each dataset, consistent with the steady decline in manufacturing employment in the economy. We also see declining OSHA inspection rates over the three periods, particularly for programmed inspections, while the median plant size increases from Medium in the first two periods to Big in the final period.

Table 2 shows the basic regressions of injury changes on inspections and inspections with penalty. The first column presents results from the original Scholz-Gray data set, covering 1979–85. The second and third columns present results from the 1987–91 and 1992–98 data sets. As expected, plants with growing employment

[8]Analyses including two OSHA measures—having had a penalty inspection (PenInsp) and having had only non-penalty inspections (NoPen)—always yielded statistically insignificant coefficients on NoPen (results available).

[9]This panel data model, developed by Chamberlain (1982, 1984), allows for more sophisticated specification testing and modeling of plant-specific effects (see Gray and Scholz 1993; Gray 1996).

Table 1. Database Description.

Independent Variable		*Federal OSHA Only* 1979–85	*Federal OSHA Only* 1987–91	*Federal OSHA Only* 1992–98	*Federal + State* 1992–98
Number of Observations		27,368	32,765	25,603	50,276
Number of Plants		6,842	14,386	8,161	16,036
Plants in BLS Dataset for All Years		6,842	3,118	860	1,575
Required Continuous Years of BLS Data		7 years	2 years	3 years	3 years
Variable Means by Period					
Continuous Variables					
LWD	log change in injuries	–0.046	–0.029	–0.043	–0.039
ΔEmp	log change in employment	–0.046	–0.029	–0.043	–0.008
ΔHour	log change in hours	–0.024	–0.032	–0.011	–0.007
DaysAw	log chg—days-away-from-work injuries	–0.075	–0.029	–0.082	–0.078
RstrctdWrk	log chg—restricted work injuries	0.036	0.059	0.040	0.043
OSHA Inspection Variables (dummy = 1 if any inspection of that type in years *t* through *t*–3)					
Insp	all inspections	0.625	0.383	0.258	0.329[a]
PenInsp	inspections with penalty	0.283	0.245	0.194	0.236[a]
PrgInsp	programmed inspections	0.522	0.228	0.112	0.164
CmplntInsp	complaint inspections	0.229	0.207	0.171	0.214[a]
PrgInspP	programmed insp. with penalty	0.221	0.141	0.093	0.127
CmplntInspP	complaint insp. with penalty	0.086	0.124	0.113	0.134[a]
Employment Size					
Small	employment <100	0.196	0.325	0.221	0.233
Medium	employment 100–249	0.332	0.300	0.222	0.230
Big	employment 250–499	0.239	0.181	0.240	0.240
Very Big	employment 500+	0.232	0.194	0.316	0.297
Establishment Characteristics (only for Federal and State Plan OSHA 1992–98 dataset)					
StPlan	establishment in State Plan state				0.491
Union	unionized estab. (based on inspection records)				0.208
MissInsp	not inspected 1990–98 (so no union information)				0.501

[a]The Federal and State Plan OSHA 1992–98 dataset includes referral inspections as well as programmed and complaint inspections. They are only a small part of the total, and are grouped with the complaint inspections.

and growing hours worked tended to have growing numbers of injuries, with the hours worked effect being smaller and declining somewhat over the three periods. The model estimates statistically significant autoregressive errors, with a 10% shock in injuries resulting in a 6–7% reduction in injuries over the following two years. Overall, the models explain one-quarter or more of the variance in injury changes across plants, with a slight decline in explanatory power over the three periods.

As found in the Scholz-Gray analysis, inspections with penalties had a larger impact than other inspections (comparing PenInsp with Insp). The main result in Table 2 is that both PenInsp and Insp coefficients became smaller in each succeeding period. Since a given inspection is included in Insp or PenInsp for four years in a row, the impact of a penalty inspection on injuries is four times these coefficients, declining from 19.2% to 11.6% and then to 1.2%; the last impact is statistically insignificant. Formal tests (available on request) confirm a statistically significant difference between the Period 3 results and the results for Period 1.

Table 2. Impact of OSHA Inspections on Injuries.
(Federal OSHA; three time periods)

	Period					
Indep. Var.	*1979–85*	*1987–91*	*1992–98*	*1979–85*	*1987–91*	*1992–98*
PenInsp	–0.048***	–0.029***	–0.003			
	(–8.20)	(–4.52)	(–0.38)			
Insp				–0.026***	–0.011**	–0.000
				(–4.65)	(–2.00)	(–0.02)
ΔEmp	0.492***	0.445***	0.496***	0.493***	0.445***	0.495***
	(18.14)	(21.24)	(22.34)	(18.20)	(21.24)	(22.34)
ΔHour	0.382***	0.282***	0.167***	0.381***	0.282***	0.167***
	(16.30)	(15.68)	(9.05)	(16.25)	(15.68)	(9.05)
d_1	–0.464***	–0.439***	–0.445***	–0.463***	–0.439***	–0.445***
	(–65.19)	(–53.84)	(–54.18)	(–64.94)	(–53.80)	(–54.18)
d_2	–0.232***	–0.180***	–0.186***	–0.231***	–0.180***	–0.186***
	(–29.28)	(–17.34)	(–18.75)	(–29.09)	(–17.37)	(–18.76)
R^2	0.284	0.258	0.240	0.283	0.258	0.240

Notes: t-statistics in parentheses; regressions include year and SIC2 dummies.
**Statistically significant at the .05 level; ***at the .01 level.

The initial regression results presented in Table 2 provide evidence of a substantial decline in the measured impact of OSHA inspections on injuries in manufacturing industries. We now consider different categories of inspections and establishments to see how widespread the decline was and to attempt to explain it.

Looking at inspection type, Scholz and Gray (1997) found that both complaint and programmed inspections affected injuries, but that complaint inspections were less dependent on penalties for their impact. Table 3 shows the impact of complaint and programmed inspections on injuries in each of the periods, confirming that programmed inspections were more dependent on penalties for their impact. In fact, in Periods 2 and 3, establishments that received a non-penalty programmed inspection experienced a statistically significant *increase* in their injuries after the inspection. The impact of complaint inspections with penalties declined more in the late 1980s, while the impact of programmed inspections with penalties declined more in the 1990s.

Next we allow the impact of OSHA inspections to differ depending on characteristics of the establishment. Table 4 examines the effects of inspections with penalties in four different establishment employment size categories: small, under 100; medium, 100–249; big, 250–499; and very big, over 500. We find weaker preventive effects of inspections in the very largest establishments than in smaller establishments (similar to results reported in Gray and Scholz 1991). More important, we observe declines in the estimated impact across the periods for all groups except the medium-sized establishments, and these declines are statistically significant.

We tested allowing for different OSHA effects across different industries, interacting PenInsp with two-digit SIC industry dummies separately for the three time periods, for a total of 57 coefficients. A few of these industry-period coefficients are individually significant (results available), but the coefficients for particular industries are not significantly correlated across the different periods. We examined whether these industry-period coefficients are correlated with industry characteristics in that period (industry investment rate, injury rate, and

Table 3. Impact of OSHA Inspections by Inspection Type.
(Federal OSHA; three time periods)

	Period		
Indep. Var.	*1979–85*	*1987–91*	*1992–98*
PrgInspP	–0.048***	–0.033***	–0.004
	(–6.93)	(–4.18)	(–0.44)
PrgInspN	–0.000	0.025**	0.046**
	(–0.03)	(2.55)	(2.14)
CmplntInspP	–0.030***	–0.014*	–0.004
	(–3.10)	(–1.78)	(–0.41)
CmplntInspN	–0.023***	–0.002	–0.006
	(–3.03)	(–0.18)	(–0.48)
ΔEmp	0.490***	0.446***	0.495***
	(18.10)	(21.26)	(22.33)
ΔHour	0.382***	0.282***	0.167***
	(16.33)	(15.67)	(9.06)
d_1	–0.464***	–0.440***	–0.445***
	(–65.23)	(–59.89)	(–54.21)
d_2	–0.232***	–0.180***	–0.186***
	(–29.34)	(–17.36)	(–18.77)
R^2	0.285	0.258	0.240

Notes: t-statistics in parentheses; regressions include year and SIC2 dummies.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

inspection rate), but found only weak correlations. As expected, higher investment rates in an industry and period were tied to smaller inspection effects (correlation of 0.24 with a p value of 0.08), but higher inspection rates were (surprisingly) tied to greater effects of inspections (correlation of –0.22 with a p value of 0.10), while higher injury rates had no effect (correlation of –.09 with a p value of 0.48).

Table 5 looks separately at the effects of inspections on different injury types, comparing the effect on days away from work (DaysAw) with the effect on days of restricted work activity (RstrctdWrk). One clear finding in Table 5 is that all of the impact of inspections with penalties was on DaysAw injuries—even in Period 1 there was no impact on RstrctdWrk injuries. There is even some indication that inspections were linked to increases in RstrctdWrk injuries in the late 1980s, but this apparent association disappears in the 1990s. The decline in the impact of OSHA inspections is seen in the DaysAw equations, declining from 20.8% to 11.6% to 5.2% across the three periods—though this impact remains statistically significant in Period 3, unlike the impact on all lost-workday injuries.

Federal and State OSHA Sample, 1992–98

We now turn to a more detailed examination of the most recent dataset, for which we have data from State Plan states as well as Federal OSHA ones. Because of the substantial differences in OSHA effects across injury types, and because of the growing importance of restricted work activity injuries in the 1990s, we present these results for each of the three injury types: the overall lost-workday injuries (LWD) and its two components, injuries with days away from work (DaysAw) and injuries with restricted work activity but no days away from work (RstrctdWrk). We begin with models of the effect of inspections with penalties

Table 4. Impact of OSHA Inspections by Employment Size.
(Federal OSHA; three time periods)

Indep. Var.	*Small (<100 Employees)*	*Medium (100–249 Employees)*	*Big (250–499 Employees)*	*Very Big (500+ Employees)*
PenInsp 1979–85	−0.077***	−0.035***	−0.054***	−0.033***
	(−5.16)	(−3.34)	(−4.56)	(−2.90)
PenInsp 1987–91	−0.033**	−0.023**	−0.054***	−0.017
	(−2.46)	(−2.03)	(−4.02)	(−1.51)
PenInsp 1992–98	0.008	−0.031*	−0.000	0.011
	(0.43)	(−1.80)	(−0.02)	(1.01)
ΔEmp	0.391***	0.517***	0.507***	0.476***
	(15.93)	(19.37)	(16.62)	(18.93)
ΔHour	0.179***	0.322***	0.319***	0.346***
	(9.05)	(14.04)	(12.06)	(15.32)
d_1	−0.502***	−0.472***	−0.413***	−0.379***
	(−52.53)	(−56.20)	(−44.31)	(−44.27)
d_2	−0.222***	−0.223***	−0.210***	−0.153***
	(−19.39)	(−22.63)	(−19.52)	(−15.29)
R^2	.267	.283	.256	.261
N	21,706	24,593	18,613	20,824

Notes: t-statistics in parentheses; regressions include year and SIC2 dummies. Regressions are for all 3 periods, separately by employment size category.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

and then introduce interactions with characteristics of the workplace and additional characteristics of the inspection.

The first three columns of Table 6 show the impact of penalty inspections (PenInsp) during this period. The estimated impact of a penalty inspection on overall lost-workday injuries (LWD) is similar to that found in the earlier analyses for Federal-only states, and statistically insignificant. However, we find statistically significant effects for the different injury types, with a negative effect on days-away-from-work injuries (DaysAw) being mostly offset by a positive effect on RstrctdWrk injuries.

In the right panel of Table 6 we test for a variety of inspection and establishment characteristics. The model includes both dummy variables and interaction terms for the size of the establishment, whether the workers were in a union (Union and Union* PenInsp), whether the plant was located in a "State Plan" state (StPlan and StPlan* PenInsp), and the year of the inspection (Year dummies and Year (trend) * PenInsp). Unionized workplaces were 9.7% of Small establishments; 18.4% of Medium; 24.0% of Big; and 32.3% of Very Big (21.8% unionized workplaces overall). Because we can identify union status only at plants that were inspected at some point in our inspection data (from 1990 to 1998), we also include a dummy variable (MissInsp) to control for uninspected establishments. With both MissInsp and PenInsp in the regression, their coefficients are measured with respect to workplaces that were inspected but had no penalty assessed.

In this expanded model, we see larger and more statistically significant preventive effects of penalty inspections, though here the PenInsp coefficient refers to a nonunionized workplace with fewer than 100 workers in a Federal OSHA state. The positive union interaction terms (Union* PenInsp) indicate a smaller impact of OSHA inspections in unionized workplaces, although this effect is statistically significant

Table 5. Impact of OSHA Inspections on Injury Types:
Days-Away-from-Work Injuries (DaysAw) vs. Restricted Work Activity Injuries (RstrctdWrk).
(Federal OSHA; three time periods)

	DaysAw			*RstrctdWrk*		
Indep. Var.	*1979–85*	*1987–91*	*1992–98*	*1979–85*	*1987–91*	*1992–98*
PenInsp	−0.052***	−0.029***	−0.013*	0.001	0.019***	0.000
	(−8.69)	(−4.52)	(−1.73)	(0.24)	(2.61)	(0.03)
ΔEmp	0.471***	0.430***	0.424***	0.061***	0.127***	0.299***
	(17.06)	(19.90)	(18.44)	(3.27)	(5.47)	(10.80)
ΔHour	0.364***	0.253***	0.136***	0.070***	0.114***	0.114***
	(15.22)	(13.68)	(7.13)	(4.46)	(5.71)	(4.94)
d_1	−0.462***	−0.437***	−0.423***	−0.158***	−0.341***	−0.368***
	(−64.67)	(−53.94)	(−51.83)	(−21.92)	(−44.80)	(−46.86)
d_2	−0.232***	−0.176***	−0.176***	−0.081***	−0.160***	−0.154***
	(−29.20)	(−17.05)	(−17.55)	(−9.39)	(−16.51)	(−16.62)
R^2	0.274	0.244	0.208	0.035	0.128	0.150

Notes: t-statistics in parentheses; regressions include year and SIC2 dummies.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

only for RstrctdWrk injuries. The point estimates show smaller inspection effects for plants in State Plan states, but not statistically significant differences.[10]

The expanded model also tests the importance of the year in which the inspection occurred. We saw earlier that the average effects of inspections had declined steadily from period to period. Based on this long-run trend, we expected to find the impact of inspections declining *within* the last period as well. Instead, interacting a time trend with PenInsp generates a negative and marginally significant coefficient for LWD injuries. Interacting specific year dummies with PenInsp (not shown), we find a decrease in impact between 1993 (the base year) and 1994, followed by steadily increasing effects through 1998, though the individual year effects are not statistically significant.

The control dummy variables measure differences in the injury trends across the different groups of establishments. The employment size dummy variables show that the LWD and DaysAw rates at smaller establishments were increasing, and their RstrctdWrk rates decreasing, relative to larger establishments. StPlan shows no difference in injury trends between Federal and State Plan states. Finally, uninspected plants tended to have declining numbers of LWD and DaysAw injuries, relative to inspected plants where no penalty was assessed.

In Table 7 we test for differences in impact based on inspection characteristics. We look at whether the inspection was programmed or due to a worker complaint. We estimate these effects separately for penalty and non-penalty inspections, and look at differences in effects between union and nonunion plants.

Comparing the programmed and complaint inspections, we see that 10 of the 12 comparisons show larger (more negative) effects from the complaint inspections. However, for only one complaint category for LWD injuries is the effect statistically significantly different from zero: penalty

[10]We also estimated sets of interactions between 2-digit SIC industry dummies and PenInsp separately for Federal and State Plan states. We found almost no agreement: only 8 of the 19 industry interaction coefficients had the same sign, and the correlation between the two sets of coefficients was only +0.07.

Table 6. Impact by Plant Characteristics.
(Federal and State OSHA, 1992–98)

Variable	*LWD*	*DaysAw*	*RstrctdWrk*	*LWD*	*DaysAw*	*RstrctdWrk*
PenInsp	−0.002	−0.014***	0.013**	−0.036**	−0.044***	−0.009
	(−0.43)	(−2.63)	(1.97)	(−2.32)	(−2.77)	(−0.44)
PenInsp*Medium				0.021	0.018	0.009
				(1.30)	(1.1)	(0.45)
PenInsp*Big				0.020	0.031*	−0.004
				(1.30)	(1.96)	(−0.17)
PenInsp*Very Big				0.044***	0.040***	0.014
				(3.02)	(2.60)	(0.74)
PenInsp*StPlan				0.008	0.010	0.019
				(0.87)	(0.95)	(1.45)
PenInsp*Union				0.013	0.003	0.028*
				(1.07)	(0.24)	(1.70)
PenInsp*Year				−0.006*	−0.004	−0.009**
				(−1.86)	(−1.13)	(−2.03)
Control Variables						
StPlan				−0.002	−0.004	−0.004
				(−0.36)	(−0.81)	(−0.63)
Union				−0.008	0.001	−0.017
				(−0.96)	(0.11)	(−1.53)
MissInsp				−0.022***	−0.016***	−0.022***
				(−3.85)	(−2.66)	(−2.81)
Medium				−0.021***	−0.038***	0.019**
				(−3.03)	(−5.25)	(2.14)
Big				−0.019***	−0.051***	0.043***
				(−2.83)	(−7.16)	(4.78)
Very Big				−0.038***	−0.072***	0.022**
				(−5.64)	(−10.34)	(2.47)
ΔEmp	0.467***	0.416***	0.271***	0.467***	0.418***	0.269***
	(27.51)	(23.62)	(12.71)	(27.50)	(23.74)	(12.59)
ΔHrs	0.221***	0.165***	0.157***	0.221***	0.165***	0.157***
	(15.37)	(11.05)	(8.74)	(15.38)	(11.11)	(8.70)
d_1	−0.454	−0.441	−0.374	−0.455	−0.444	−0.183
	(−75.35)	(−74.00)	(−65.26)	(−75.52)	(−74.48)	(−65.34)
d_2	−0.182	−0.172	−0.142	−0.183	−0.174	−0.142
	(−26.38)	(−25.15)	(−21.59)	(−26.52)	(−25.51)	(−21.61)
R^2	0.247	0.218	0.152	0.248	0.221	0.153

Regressions also include year dummies.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

inspections at nonunion workplaces. In union workplaces, imposing a penalty had more of an impact for programmed inspections, while for non-union inspections the pattern is less clear.

Comparing the coefficients based on unionization, we find that 11 of 12 show a bigger preventive effect at nonunion than at union establishments. Several of the differences in the effects of inspections at union and nonunion establishments are statistically significant. However, only the effect of complaint penalty inspections is significantly negative. Programmed non-penalty inspections at unionized workplaces are associated with a greater number of

Table 7. Interactions between Union and Inspection Characteristics. (Federal and State OSHA, 1992–98)

Union Status	*LWD*	*DaysAw*	*RstrctdWrk*
Programmed			
Penalty			
Union	0.008	0.000	0.025*
	(0.79)	(0.01)	(1.74)
Non-Union	–0.015	–0.006	–0.022*
	(–1.59)	(–0.59)	(–1.80)
Non-Penalty			
Union	0.042**	0.025	0.046*
	(2.15)	(1.22)	(1.76)
Non-Union	–0.003	–0.012	0.012
	(–0.20)	(–0.77)	(0.64)
Complaint			
Penalty			
Union	–0.006	–0.023**	0.013
	(–0.58)	(–2.11)	(0.97)
Non-Union	–0.023**	–0.036***	0.005
	(–2.28)	(–3.48)	(0.39)
Non-Penalty			
Union	–0.000	–0.032**	0.019
	(–0.04)	(–2.42)	(1.15)
Non-Union	–0.011	–0.009	–0.015
	(–0.95)	(–0.76)	(–1.00)
R^2	0.248	0.221	0.153

These regressions include second-order autoregressive lags, year dummies, and all the control variables from Table 6.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

LWD injuries. These results support the hypothesis that inspections are likely to have a greater impact on preventing injuries at nonunion establishments than at union establishments.

We performed additional analyses to test a variety of inspection characteristics. One notable result (available from the authors) compares the impact of safety inspections with that of health inspections. We find somewhat greater effects for health inspections than for safety inspections; though counter-intuitive, a similar result was reported in Gray and Scholz (1991) using the Period 1 data. Health inspections involve more inspector time than safety inspections (Siskind 2002), which may contribute to their greater impact.

Discussion and Conclusions

We have found a substantial decline between 1979 and 1998 in the measured impact of OSHA inspections: from a statistically significant 19% reduction in injuries over the three years after an inspection with penalty in Period 1, to a statistically insignificant 1% reduction in Period 3 (Table 2). This declining impact was shown to be fairly consistent across different types of inspections (Table 3) and different employment sizes (Table 4). We also found differences in inspection effects based on plant and inspection characteristics, with generally larger effects for inspections with penalties (Tables 2 and 3), programmed inspections (Table 3), smaller plants (Table 4), days-away-from-work injuries (Table 5), and nonunion plants (Table 6). In the earlier periods, programmed inspections with penalties had the greatest impact, but this disappeared in Period 3. Table 1 documents substantial changes in OSHA inspection activity. The last period had more inspections of the very largest plants, but this was more than offset by shifts in other size categories.[11]

One important aspect of inspections was a "records-check" policy during the early 1980s: programmed inspections began with a check of the plant's injury rate records and stopped there if the plant's injury rate was below the average rate for manufacturing (Siskind 1993). This policy was phased out in the later periods, seen in Table 1 by the large increase in the fraction of inspections with penalties (PenInsp/Insp): less than one-half in the first period and rising to over two-thirds in the last period. Ruser and Smith (1988, 1991) found evidence

[11]This calculation is based on multiplying the Period 1 coefficients for each size group in Table 4 by that group's share of plants in each of the periods from Table 1.

that the records-check procedure caused some under-reporting of injuries, but only at uninspected establishments. This would lead our results to understate (rather than overstate) the impact of inspections on injuries in the earlier period. If plants with penalty inspections have high pre-inspection injury rates (since they got a full inspection), we might expect declining injuries in subsequent years, but this should be captured by the autoregressive errors in our model. In any event, explanations tied to this policy could only explain a decline in impact between Period 1 and Period 2, not the continuing (and larger) decline in Period 3.

Some of the decline in inspection impact can be explained by the steady growth after the mid-1980s in "restricted work activity" (RstrctdWrk) injuries, while "days-away-from-work" (DaysAw) injuries were declining. As Figure 1 shows, RstrctdWrk injuries grew very slowly until the mid-1980s, then almost doubled in the two years from 1986 to 1988 and continued growing at over 10% per year until 1995. Thus by 1996 about half of all lost-workday cases in manufacturing did not involve days away from work. We saw in Table 5 that in every period, even the first, there was no evidence that OSHA inspections with penalties led to reductions in RstrctdWrk injuries. Based on an assumption of zero impact on RstrctdWrk cases, over a third of the drop in OSHA's impact could be accounted for by the growth of RstrctdWrk cases.

Unfortunately, this is an explanation in only a mechanical sense. It provides no insight into why the preventive effectiveness of an inspection should be related to how the injuries are treated after they occur. The concurrent decline in DaysAw and increase in RstrctdWrk strongly suggest a re-labeling of existing injuries rather than a shift to different injury types or severities.

Several other factors influencing inspection impact are discussed in Gray and Mendeloff (2002). Factors other than OSHA might have made uninspected workplaces more attentive to safety issues, so that an inspection would have less incremental impact: possibilities include increased attention to workers' compensation costs and greater assistance by insurers in reducing workplace hazards. During the 1990s OSHA did fewer but more intensive inspections, taking more hours of inspector time and emphasizing a problem-solving approach. The net impact of these additional factors is not easily quantified, and we are left without a comprehensive explanation for the decline.

What implications do our results have for understanding optimal OSHA policy? The substantial decline in the estimated impact of OSHA inspections on workplace injuries raises serious questions about the future role of inspections in making workplaces safer. Perhaps of more immediate relevance, the observed differences in the impact of inspections under different circumstances may make it possible to target regulatory resources more precisely toward those workplaces where they can be helpful. For example, OSHA inspections seem to have had substantial effects on injuries in smaller workplaces, even in the 1990s, whereas there is little evidence that inspections reduced injuries at the largest workplaces in recent years.

OSHA tends to inspect union plants more frequently than nonunion ones, whether driven by a greater frequency of complaint inspections or by a desire for political support from union leaders. However, OSHA inspections seem to have a larger impact on injuries at nonunion workplaces, consistent with union workers having more information about hazards and a greater ability to exercise "voice" effectively. Studies of unions have shown that unionized workplaces are more likely than nonunion workplaces to be in compliance with standards (Weil 1996), again consistent with union workers having greater ability to insist on hazard reductions, even without the pressure of an OSHA inspection. These results suggest that shifting some inspection resources toward smaller and nonunion plants may be worthwhile.

Our analysis does not find any difference between Federal and State Plan states in the average effectiveness of penalty inspections. The percentage of inspections that cited penalties was lower in State Plan states than in Federal states, but State Plan states carried out many more inspections. On net, State Plan states conducted more penalty inspections per 1,000 establishments, so the aggregate impact of specific deterrence might be larger in those states.

However, a complete evaluation of different inspection strategies cannot focus only on the deterrent effect of inspections that impose penalties. It is important to remember that not all inspections impose penalties, and we find that non-penalty inspections were in some cases followed by *increases* in injuries. A possible explanation is that a clean bill of health from inspectors tends to reduce managerial attention to safety. If the finding of increased injuries following some non-penalty inspections reflects a causal relationship, any estimates of the overall impact of all OSHA inspections should take into account both reductions and increases in injuries.

The fact that preventive effects now appear to be limited to the smallest workplaces is troubling in this regard, because those preventive effects will benefit relatively few workers, and might be swamped by any contrary effects at larger workplaces. This type of "composition effect" also complicates the relative assessment of the programs in State Plan states. Those states conduct more penalty inspections than Federal OSHA Plan states, but even more non-penalty inspections. If more workplaces are getting a clean bill of health in State Plan than in Federal OSHA Plan states, the resulting increase in employer complacency might be offsetting the preventive effects of the additional penalty inspections being conducted.

Finally, our research has discovered puzzles that merit further attention: OSHA inspections do not seem to affect restricted work activity injuries, which represent a large and rising share of injuries, and we have not identified a satisfying explanation for the substantial decline in the impact of penalty inspections on injuries.

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