Inside the Black Box: How do OSHA Inspections Lead to Reductions in Workplace Injuries?*

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We examine different models of employers' responses to OSHA inspections. The "detection/correction" model assumes that responses are limited to correcting the violations that inspectors cite. The "behavioral shock" model assumes that firms respond by paying more attention to safety issues, even those unrelated to OSHA standards. We test whether some injury types are more affected by inspections than others, or by citations of particular OSHA standards. We conclude that, although citing particular standards can reduce injury types specifically related to those hazards, inspections also affect a wider range of injuries, suggesting a broader impact on managerial attention to safety.

I. INTRODUCTION

By identifying the types of injuries that are prevented by inspections by the Occupational Safety and Health Administration (OSHA), this paper attempts to clarify the causal factors underlying the preventive actions that firms undertake. The literature on firms' responses to regulation includes several major debates. The traditional economic analysis assumes profit-maximizing firms whose decisions about safety and compliance are guided by calculations about the expected marginal costs and benefits to the firm. In contrast, writers in the socio-legal tradition, arguing that there appears to be far more compliance than could be explained by an economic calculus, have emphasized the role of norms of law-abidingness and of ethical behavior.

Another challenge to the profit-maximizing assumption comes from the behavioral theory of the firm (Cyert & March 1963), which rejects profit-maximization in favor of firms having bounded rationality and paying

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sequential attention to problems. In contrast to the traditional model, which assumes that the firm is operating at the optimum and that the disturbance caused by an OSHA inspection can be modeled by examining the costs and benefits of compliance, in the behavioral model inspections reduce injuries by spurring firms to pay more attention to safety.

The work presented here builds on our earlier work on the effects of OSHA inspections. In Gray and Mendeloff (2002), we examined the effects of inspections with penalties in manufacturing in 1992–98. Although we found no preventive effects of these inspections at establishments with more than 250 workers, there were effects at smaller workplaces, especially non-union workplaces with fewer than a hundred workers. In this paper, we test different models that have been proposed as explanations for why inspections that cite penalties reduce injuries.

We identify three different models. First, some analyses of OSHA have assumed that the injuries prevented by inspections are limited to those that are cited by compliance officers. Presumably, the mechanism here is that inspectors identify and cite violations of standards; employers correct the violations in order to avoid potentially large penalties for "failure to abate;" and the abating of the violation reduces the number of injuries caused by that particular type of hazard. The inspection reduces injuries primarily by making the cost of non-compliance with a potentially injury-causing hazard exceed the benefit of non-compliance. If this model is true, then we should expect that the injuries prevented by inspection would be limited to those caused by the hazards that were cited. For example, citation of a violation of machine-guarding standards should be associated with a reduction in injuries due to getting caught in machines. The response in this model is obviously consistent with a profit-maximizing calculus; however, it could also be consistent with a norm of law-abidingness: the firm had slipped from its policy of compliance and is anxious to return to that status.

Second, a somewhat broader model is that inspections, especially those that levy penalties, induce a greater *overall* compliance effort by the establishment. In this case, we might expect a reduction in injuries that are caused by any type of violation of OSHA standards, not just the type or types cited during the inspection. In the profit-maximizing model, the inspection may increase the firm's estimate of the probability of a future inspection; whether it increases its expectation about future penalties probably depends on the penalties it receives relative to its expectation. But, again, this second model is also consistent with a norms-based model. The inspection reminds firms of their legal obligations and moral obligations to comply (Thornton, Gunningham & Kagan 2005).

Third, the behavioral model of the firm assumes that managers cannot optimize with respect to all aspects of their operations and tend to focus their attention on what appears to be most important at the time. As applied to OSHA enforcement by Scholz and Gray (1990), an inspection that finds serious problems at a workplace may surprise management and

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lead them to pay more attention to safety issues. Being forced to pay a penalty for the violation may attract further attention. It is further presumed that when managers pay more attention to safety concerns this is done in a general way, and could include hazards that are outside the scope of those addressed by existing OSHA standards.

In this paper, we attempt to contribute toward testing these models by examining whether the injury reductions that we observe after inspections are limited to the types of injuries covered by the particular OSHA standards being cited (Model 1), to all injuries covered by OSHA standards but not to uncovered injuries (Model 2), or to a still broader set of injuries including those unrelated to OSHA standards (Model 3). One analysis examines the impact of inspections with penalties on different injury categories, some of which are much less closely related to OSHA standards than others. A second analysis looks at inspections that cited particular violations of standards to see whether the types of injuries reduced in those inspections correspond to the types of violations cited.

Our major finding is that the injuries prevented by penalty inspections at manufacturing establishments with fewer than 250 workers from 1992–98 were not limited to those related to OSHA standards. In particular, the largest effects were on "exertion" injuries, a category which OSHA standards do not address. Thus it does appear that OSHA penalty inspections often induce managers to pay more attention to safety issues in a manner that is not limited to compliance with OSHA standards. However, we also find evidence that citations of one of the OSHA standards we examined, the general requirements for personal protective equipment (PPE), did prevent the particular types of hazards addressed by compliance with this standard.

In addition to the theoretical issues raised by this paper, a better understanding of the mechanisms underlying inspection impacts on workplace injuries has practical implications as well. These include implications for the design of both inspection targeting plans and program evaluations, which we examine in the last part of the paper.

II. DATA AND METHODS

A. BLS INJURY DATA

The injury information in all of our datasets came from confidential plant-level injury data from the Bureau of Labor Statistics (BLS) Survey of Occupational Injuries and Illnesses. BLS collects data from 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 we looked at changes in an establishment's injuries over time, we focused on those establishments that have BLS injury data for consecutive years. This necessarily resulted in large establishments being over-represented in our datasets,

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relative to all manufacturing establishments. We used the number of injuries with days away from work (DAW) during the year as our injury measure. In addition to injury data, the BLS survey includes name and address information about the establishment.

We included in our sample only establishments in manufacturing industries. The restriction to manufacturing was chosen because the BLS sample was larger there than in other sectors and participation in the survey was more likely to be continuous. Information on injuries at the establishments was merged with characteristics of OSHA inspections at those establishments to create the analysis dataset.

Since our analysis focused on injury changes, two consecutive years of BLS injury data were needed to generate one observation for analysis. For the period from 1992 to 1998, we included all establishments with three or more consecutive years of BLS Survey data. The resulting dataset has 50,276 observations from 16,036 establishments.

B. OSHA INSPECTION DATA

The information on inspections comes from OSHA's Integrated Management Information System (IMIS), which contains data on all federal OSHA inspections since the early 1970s. In addition, since 1990, it also includes inspection data from all of the states where the inspection program is conducted by the states under section 18(b) of the Occupational Safety and Health Act 1970. The data include the opening date of the inspection and the name and address of the establishment being inspected, along with the inspection characteristics and results. Our key measure of inspection results was whether or not a penalty was imposed. This measure was very highly correlated with whether a serious violation was cited; use of the latter measure generated very similar results.

In our dataset we include three types of inspections: programmed inspections, targeted by OSHA based on industry hazardousness; complaint inspections, where OSHA is responding to a written worker complaint; and referral inspections, usually cases where a health inspector makes a referral to a safety inspector or vice versa. These three types include the great majority of all inspections: among the 218,800 inspections conducted by OSHA programs from 1990 to 1998 in manufacturing establishments, 43.4 percent were programmed, 32.3 percent were complaint, 7.1 percent were referrals, and 17.2 percent were other (mainly follow-ups and accident investigations). Follow-up inspections were excluded from our analysis because they represent the second stage of a single intervention at the establishment and are not intended to reexamine the entire workplace. Accident investigations were excluded because they inevitably involve reverse causation, with injuries causing these inspections rather than the other way around. In our sample there were many more complaint inspections than programmed inspections, reflecting the disproportional percentage of large workplaces in our sample.

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For almost all of the period examined in this paper, OSHA targeted its programmed inspections by first identifying all establishments that were in four-digit SICs with rates above a state's manufacturing average. Then, it chose inspection sites *randomly* within this set, excluding only those workplaces with fewer than eleven workers or those where OSHA had conducted a comprehensive inspection within the previous two years. Thus there is no reason to think that programmed inspections are triggered by higher injury rates at the inspected plant. 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. There is some evidence that complaint inspections do occur at establishments with relatively high injury rates within their industry; however, it is not clear whether this fact represents a temporary fluctuation (which could give rise to "regression to the mean" bias) or a longer-term pattern of poor performance.

The OSHA and BLS records were linked together, using various establishment characteristics to identify records that referred to the same establishment. These characteristics included name, address, zip code, city, state, employment size, and industry. The matching algorithms were based on a technique developed by Fellagi and Sunter (1969), which calculates the probabilities of two records matching, based on agreement or disagreement on all the characteristics. The matching methodology is explained in more detail in Gray (1996).

C. INJURY TYPE DATA

Beginning in 1992, BLS began to collect additional information on the subset of lost workday cases that involved days away from work (DAW). (The other category of "lost workday case" included those in which the worker experienced "restricted work activity" but did not lose time from the job.) The information on DAW cases includes descriptions of the "nature of injury or illness," "part of body affected," the "source of injury or illness," and "event or agency" (as well as information on the characteristics of the injured worker). All coding of the injury descriptions was carried out by BLS. In this paper, we use only the "event or agency codes."

Employers participating in the survey were told to include information on all of the "days away from work" cases. The survey form for 1998 injuries included a message to employers that "We have designed this survey to ensure that you don't have to report more than approximately 30 cases. If you find that you have significantly more, please go to ***If you Need Help*** . . . at the back of this package and call the phone number listed for your State for assistance." In fact, however, neither in 1998 nor in any other year do we find any sizable number of cases with exactly thirty or around thirty cases, indicating that any truncation of the sample that occurred as a result of this guideline was minor. Some establishments reported many hundreds of DAW cases. Therefore, we have used the entire sample of reported DAW cases.

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Table 1 shows the categories of event types used here and the distribution of injuries among them. The categories we include have approximately 95 percent of all of the DAW cases reported to BLS and a similar proportion in our sample. The main event categories omitted dealt with "transportation incidents" and "non-classifiable events." We screened the individual "days away from work" records to identify which BLS-assigned "event" type had been assigned to them. If the "event" type fell in the categories described in Table 1, the case was assigned an "ETYPE." Then all of the events were added up into plant-year-ETYPE cells, which gave the number of injuries of that type occurring at that plant in that year.

We chose these categories with an eye to distinguishing injury types that were more or less closely related to standards. Some categories appeared to be relatively easy to classify. In general, OSHA has no formal standards dealing with "bodily reaction and lifting." Of the 192,000 injuries in this category in 1998 in manufacturing, 58,000 were related to "overexertion in lifting," 32,000 were due to "repetitive motion," 21,000 to "bending, climbing, crawling, reaching, and twisting," and 21,000 to "overexertion in pulling or pushing objects." In contrast, OSHA has a large set of frequently cited standards dealing with the guarding of machines, conveyers, and other equipment. "Caught-in running equipment or machinery" injuries constituted over half of those in the broader category of "caught-in or compressed by equipment or objects."

Other categories that we believed would be relatively preventable by compliance with standards were "rubbed or abraded by foreign matter in eye," "exposure to harmful substances or environment," "falls to lower level," and "struck by" injuries. The first two injury types could be addressed by standards requiring personal protective equipment. Many of the third type could be addressed by standards about railings, stairs, ladders, and scaffolds. The majority of the injuries in the fourth category were caused by "flying objects" or by a "slipping handheld object."

The other two categories we judged less preventable by compliance with standards were "struck against object" and "falls on the same level." Most of the first category involved running into a "stationary object." The second category here is more questionable. There are standards requiring that floors be kept clean and dry and that passages be unobstructed; it is unknown how many falls are due to such hazards.

D. MODEL

The basic model estimated here takes the following form:

$$dDAW_t = a_t + b_1 IPENX_t + c_1 dEMP_{it} + c_2 dHRS_{it} + SIC2_i + YEAR_t + u_{it} \tag{1}$$

The dependent variable is the change in the log of the number of injuries, which allows us to interpret coefficients in terms of their impact on the

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Table 1. Distribution of Days Away from Work (DAW) Injuries Among Event Codes, Manufacturing

ETYPE	**BLS Event Codes**	**Percentage of DAW Cases In ETYPEs in BLS Survey, 1992-98**	**Percentage of Cases in Our Sample (n = 338,211)**
A1	020-029 (Struck by objects and equipment)	14.0	11.8
A2	030-039 (Caught in or compressed by objects or equipment)	8.1	8.5
A2a	031 (Caught in or compressed by running machinery)	4.7	4.2
A3	110-129 (Falls to lower level)	3.2	2.4
A4	053 (Rubbed or abraded by foreign matter in eye)	2.8	2.3
A5	310-339 (Exposure to harmful substances or environments)	5.4	3.6
B1	210-239 (Bodily reaction and exertion)	45.8	49.6
B2	130-139 (Falls on same level)	7.5	8.1
B3	010-019 (Struck against object or equipment)	7.8	8.9
A	ETYPES A1, A2, A3, A4, and A5	33.5	28.6
B	ETYPES B1, B2, and B3	61.1	66.6
A+B		94.6	95.2

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percentage change in injuries.[1] Gray and Scholz (1993) perform extensive econometric tests of this specification, 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 *IPENX*.

The central explanatory variable, *IPENX*, is related to whether an establishment had any OSHA inspections with penalties, recalling as noted above that we included only programmed, complaint, and referral inspections in our data set. Based on previous work (Scholz & Gray 1990), we know that the impact of OSHA inspections is concentrated in those inspections which impose penalties, and that those inspection effects may show up several years after the inspection. Therefore, we construct *IPENX* as a dummy variable, which takes on the value of 1 if the establishment had a penalty inspection within the prior four years.[2] For example, the change in the number of injuries at an establishment from 1995 to 1996 would have *IPENX* equal to 1 if the establishment had an inspection with penalty in 1996, 1995, 1994, or 1993. *IPENX* equals 1 in 22 percent of our observations.

Control variables include the annual log change in the number of employees (*dEMP*) and the annual log change in the number of hours worked (*dHRS*). The coefficients on both of these variables are expected to be positive, with *dEMP* reflecting the role of inexperience for newly hired workers and *dHRS* reflecting the hazards associated with working longer hours. Other control variables include dummy variables for year and for the two-digit manufacturing Standard Industrial Classification (*SIC*2, with *SIC*20 as the excluded category). We also examined four-digit SIC controls and state dummy variables in preliminary runs, but they had little effect on the coefficients of the policy variables.

III. RESULTS

Table 2 includes about half of the injury cases in Table 1, because it is limited to establishments with less than 250 workers. Table 3 includes about one-quarter of the cases in Table 1, because it further restricts the sample to establishments in federal OSHA states (federal OSHA operates the enforcement program directly in 29 states; in the others, the program is state-run but supervised by federal OSHA. We limited our standard-specific analyses to federal OSHA states because several large State Plan states use their own distinct systems for coding standards and others add their own codes to the federal codes. It would have taken a substantial effort to "cross-walk" the different state systems to identify the parallel standards). Table 4 includes about half of the cases in Table 1 because it limits the analysis to federal OSHA states, but includes establishments of all sizes.

Table 2 examines the effect of an inspection with a penalty (*IPENX*) on the change in the number of injuries in different event-type categories. Here

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Table 2. The Effect of Penalty Inspections (IPENX) on Different Injury Event Types, Days Away from Work Injuries, Manufacturing Plants < 250, 1992–98

	Injury Event Type									
	Related to OSHA Standards						Unrelated to OSHA Standards			
	A1 Struck-by	**A2 Caught-in**	**A3 High Fall**	**A4 Eye Abrasion**	**A5 Toxic Exposure**	**All A A1-A5**	**B1 Exertion**	**B2 Struck Against**	**B3 Fall Same Level**	**All B B1-B3**
Intercept	−0.018	−0.023*	−0.010	0.011	−0.004	−0.023	−0.045**	−0.045	−0.009	−0.048**
	(−1.35)	(−1.89)	(−1.45)	(−1.54)	(−0.49)	(−1.39)	(−2.68)	(−1.68)	(−0.77)	(−2.76)
IPENX	−0.006	−0.007	0.0001	−0.009*	−0.010*	−0.018*	−0.026**	−0.016**	−0.003	−0.028**
	(−0.68)	(−0.94)	(0.01)	(−1.92)	(−1.93)	(−1.79)	(−2.51)	(−2.12)	(−0.49)	(−2.58)
DLEMP	0.103***	0.058***	0.019**	0.043***	0.025**	0.185***	0.246***	0.082***	0.080***	0.292***
	(5.62)	(3.58)	(2.16)	(4.26)	(2.15)	(8.31)	(10.81)	(5.09)	(5.24)	(12.35)
DLHRS	0.046***	0.052***	0.007	0.003	0.020**	0.080***	0.081***	0.031**	0.026**	0.100***
	(3.04)	(3.79)	(0.88)	(0.42)	(2.09)	(4.31)	(4.29)	(2.28)	(2.00)	(5.07)
R^2	0.012	0.009	0.002	0.005	0.004	0.025	0.034	0.009	0.009	0.045

Notes: For example, the number of eye abrasions is predicted to declined by an average of 0.9 percent in the four years following the inspection, for a cumulative decline of 3.6 percent.
Each regression also includes control variables for the two-digit SIC code and the year.
* denotes significance at the 0.10 level; ** at the 0.05 level; *** at the 0.01 level (two-tailed test).
23,395 Observations; t-statistics in parentheses)

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Table 3. The Effect of Citing Particular Violations on Different Injury Event Types, Manufacturing Establishments < 250, in Federal OSHA States, 1992–98

	Injury Event Type									
	Related to OSHA Standards						Unrelated to OSHA Standards			
Violation Type	A1 Struck-by	A2 Caught-in	A3 High Fall	A4 Eye Abrasion	A5 Toxic Exposure	All A A1-A5	B1 Exertion	B2 Struck Against	B3 Fall Same Level	All B B1-B3
Machine Guards	0.014	0.010	−0.025**	−0.005	0.006	−0.005	−0.028	−0.011	−0.017	−0.023
	(0.63)	(0.52)	(−2.36)	(−0.45)	(−1.06)	(−0.20)	(−1.03)	(−0.56)	(−0.89)	(−0.80)
PPE	−0.014	−0.164**	0.027	−0.061***	−0.007	−0.070*	−0.073*	0.002	−0.019	−0.067
	(−0.42)	(−2.06)	(1.63)	(−3.47)	(−0.31)	(−1.67)	(−1.71)	(0.07)	(−0.65)	(−1.49)
Electrical	−0.014	0.018	0.008	0.004	−0.003	0.008	0.015	−0.014	0.002	−0.007
Wiring	(−0.55)	(0.78)	(0.60)	(0.03)	(−0.02)	(0.27)	(0.46)	(−0.61)	(0.07)	(−0.22)
Forklift	0.029	−0.024	0.003	−0.005	−0.018	−0.019	−0.013	−0.026	0.032	−0.017
Trucks	(0.77)	(−0.68)	(0.17)	(−0.25)	(−0.75)	(−0.40)	(−0.28)	(−0.76)	(0.96)	(−0.34)
Fire	−0.023	−0.002	0.014	−0.006	0.024	−0.001	0.031	0.046	0.009	0.050
Extinguishers	(−0.69)	(−0.08)	(0.87)	(−0.36)	(1.15)	(−0.27)	(0.75)	(1.55)	(0.33)	(1.15)
R^2	0.016	0.012	0.004	0.007	0.005	0.028	0.036	0.009	0.010	0.045

Notes: For example, an inspection citing a violation of the personal protective equipment (PPE) standard leads to, on average, a reduction of 6.1 percent in eye abrasion injuries in each of the four years following the inspection, for a cumulative reduction of 24.4 percent.
Each regression also includes DLEMP, DLHRS, and control variables for the two-digit SIC code and the year.
* denotes significance at the 0.10 level; ** at the 0.05 level; *** at the 0.01 level (two-tailed test).
11,446 Observations; t-statistics in parentheses.

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Table 4. The Effects of Citing Particular Violations on Different Injury Event Types, All Establishments, Manufacturing, Federal OSHA States, 1992–98

	Injury Event Type									
	Related to OSHA Standards						Unrelated to OSHA Standards			
Violation Type	A1 Struck-by	A2 Caught-in	A3 High Fall	A4 Eye Abrasion	A5 Toxic Exposure	All A A1-A5	B1 Exertion	B2 Struck Against	B3 Fall Same Level	All B B1-B3
Machine	0.013	0.007	−0.021	−0.003	−0.007	0.003	−0.018	−0.001	0.007	−0.007
Guards	(0.86)	(0.50)	(−0.23)	(−0.36)	(−0.41)	(0.17)	(−0.99)	(−0.10)	(0.50)	(−0.41)
PPE	−0.032	−0.056***	0.031**	−0.022*	−0.030**	−0.054**	−0.048*	0.009	−0.024	−0.043*
	(−1.51)	(−2.82)	(2.46)	(−1.91)	(−2.05)	(−2.20)	(−1.94)	(0.49)	(−1.19)	(−1.70)
Electrical	−0.010	0.004	−0.003	−0.009	0.010	0.002	0.009	−0.015	−0.004	−0.008
Wiring	(−0.55)	(0.22)	(−0.30)	(−0.86)	(0.81)	(0.09)	(0.41)	(−0.91)	(−0.22)	(−0.38)
Forklift	0.010	−0.016	−0.019	0.001	0.022	−0.038	−0.002	0.000	0.007	−0.007
Trucks	(0.41)	(−1.31)	(−0.23)	(0.07)	(0.41)	(−0.13)	(−0.07)	(0.01)	(0.50)	(−0.41)
Fire	0.003	0.003	0.016	0.013	0.018	0.011	0.018	0.011	0.007	0.026
Extinguishers	(0.12)	(0.12)	(1.12)	(0.99)	(1.11)	(0.39)	(0.99)	(0.10)	(0.30)	(0.94)
R^2	0.011	0.009	0.003	0.003	0.004	0.022	0.028	0.006	0.01	0.037

Notes: Each regression also includes DLEMP, DLHRS, and control variables for the two-digit SIC code and the year.
* denotes significance at the 0.10 level; ** denotes at the 0.05 level; *** at the 0.01 level (two-tailed test).
25,603 Observations; t-statistics in parentheses.

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we focused on establishments with fewer than 250 workers, where the largest effects were found in earlier work (Gray & Mendeloff 2002). Table 2 displays the coefficients for the five injury types that were believed to be more related to standards (Category *A*) and the three injury types that were believed to be less related to standards (Category *B*). Of the eight specific injury types, the *IPENX* coefficient is significant and negative for four of them: *A*4 (eye abrasions), *A*5 (harmful exposures), *B*1 (exertion), and *B*2 (struck against). The first two were significant at the 0.10 levels, two-tailed test; the latter two at the 0.05 level. Since *IPENX* remains "turned on" for four years, its effect is cumulative: an *IPENX* coefficient of −0.010 means that the number of injuries at the establishment will decline by a total of 4 percent over the four years following the inspection.

Probably the most notable finding in Table 2 is that penalty inspections did not appear to reduce injuries in the category with the clearest relation to OSHA standards (*A*2 caught in or compressed by objects or equipment), while they did appear to decrease injuries in the category most clearly unrelated to OSHA standards (*B*1, exertion and bodily movement). As a further test, we ran another regression focused only on injuries where the worker was "caught or compressed by running machinery." This is a subcategory of the *A*2 category with roughly half the number of injuries. The result (not shown) was that the *IPENX* coefficient was positive and not significant.

The *IPENX* coefficients for the aggregate categories reflect the subcategory results. The *B* group is dominated by the *B*1 category and also shows over a 10 percent decrease over four years. For the *A* category, the *IPENX* coefficient is smaller, but still significant at the 0.10 level. Despite the difference in size, we cannot reject the hypothesis that the coefficients for the two categories are the same.

We also ran these regressions (not shown) for establishments with more than 249 employees. None of the *IPENX* coefficients came close to statistical significance. We expected this result because the *IPENX* variable had shown no preventive impact at larger establishments in our earlier work. We also ran regressions for small (i.e., under 250) establishments separately for the states where federal OSHA directly operates the enforcement programs and for the states where states operate them under OSHA supervision (State Plan states). In both regressions (not shown), the *IPENX* coefficients for the injury event types that had been significant in Table 2 did not change very much, except for the aggregated *A* category, which sank well below statistical significance.

Lastly, we consider the effects of citing different types of violations on different types of injuries. The detection/correction model (Model 1) implies that the types of injuries prevented will be related to the types of violations that are cited. Citing a machine guarding violation and correcting that hazard could lead to the prevention of "caught-in or between" injuries, but not "falls from heights."[3]

We looked at commonly cited standards, in order to have enough observations to estimate the standard's impact (if any). We also chose standards

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that varied in their effect on injuries in general or on the particular event types we measure. We chose two standards because we thought they could have an appreciable effect on injuries: 1910.0212, the standard for general requirements for machine guarding; and 1910.0132, the general requirement for personal protective equipment (PPE). As a test of discriminant validity, we included three standards that we thought would not have an effect on the injury event types we were examining: 1910.0303, the standard for electrical wiring; 1910.0178, the standard for forklift trucks; and 1910.0157, the standard for portable fire extinguishers. Our event categories did not include injuries caused by fires, which, in any event, are not very numerous. Our event category of "exposures to harmful substances or environments" does include "contact with electric current," and its subcategory of "contact with wiring, transformers, or other electrical components", but this subcategory included only 192 injuries in manufacturing in 1998. Lastly, although we included OSHA standards for forklift trucks, we did not include injuries caused by moving vehicles in our event categories.

In addition to the standards identified above, we also looked at two broader groups of standards. In addition to 1910.0212, the general requirements for machine guarding, we created a category for violations of *any* of the machine guarding standards (0212-0219). And in addition to 1910.0132, the general requirements for PPE, we also created a category that included *all* of the standards for PPE (0132-0139), e.g., specific standards for hand protection, face protection, etc.).

The variables we use below are analogous to the *IPENX* variable described above; they represent dummy variables about whether any section of the particular standard was cited at an establishment during any of the four years leading up to our data on the change in the number of injuries. For the 25,603 federal state observations, violation figures are shown in Table 5.

We would expect the machine guarding violations to affect the event type "caught-in or between," especially the sub-category of "caught-in or between machinery or moving parts." We would expect the PPE violations

Table 5. Violation Figures for Federal State Observations

Variable	**Standard**	**Coverage**	**Number of violations**
*IPENXA*1	1910.0212	General machine guarding requirements	2,463
*IPENXA*2	1910.0132	General PPE requirements	941
*IPENXA*3	1910.0303	Electrical wiring	1,732
*IPENXA*4	1910.0178	Industrial trucks	724
*IPENXA*5	1910.0157	Fire extinguishers	770
*IPENXA*6		All machine guarding violations	3,156
*IPENXA*7		All PPE violations	1,973

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to prevent "eye abrasions," "exposures to harmful exposures or environments," and, perhaps, "caught-in or between injuries."

Because many different violation types are often cited in the same inspection, our analysis controls for all these violation types together, rather than looking at the effects of one violation type at a time. Table 3 provides this analysis for inspections at manufacturing establishments with fewer than 250 employees in federal states.

With one exception, the only statistically significant effects on injuries are found when the standard for general requirements for personal protective equipment is cited. The PPE coefficients for caught-in (*A*2) and eye abrasion (*A*4) injuries were significant and negative at the 0.05 level. For both, citations of the PPE standard led to reductions of about 25 percent in the number of injuries in those categories through three years following the year of the inspection. Citations of the PPE standard also had statistically significant effects (at the 0.10 level) on exertion injuries (*B*1) and on the aggregated category of *A* injuries. The substantive effect of these citations was close to 30 percent. The only other statistically significant effect we found was the unexpected effect of citing the general machine guarding standard on falls from heights (high falls, *A*3). We also looked at the effect (not shown) of citing the machine guarding standard specifically on injuries due to getting caught-in machinery, but found no sign of impact.

In regressions (not shown) with data only from larger establishments (more than 249 workers) in federal OSHA states, we found significant effects of some standards on specific injury types, unlike the results found in Table 2 for the overall effect of all penalty inspections. The PPE variable coefficients were negative and significant for both "caught-in injuries" (*A*2) and "harmful exposure" (*A*5) injuries. For two other categories—the impact of PPE citations on "high falls" (*A*3) and the effect of citing forklift truck violations on "harmful exposures"—the effect of citations were significant but positive, not negative.

Because we found effects of specific violation types for both smaller and larger establishments, we examine these effects for the combined sample in Table 4. Here, citations of the PPE standard are the only ones to prevent injuries. The effects are widespread. In the *A* categories, we find statistically significant reductions for caught-in injuries (*A*2), eye abrasions (*A*4), and exposures (*A*5), as well as for the aggregated *A* category. For injuries in the *B* categories, we find statistically significant prevention effects for exertion injuries (*B*1) and for the aggregated *B* category.

In summary, for one of the two standards where we believed we might find impacts, we did. We found no effects of citations for the other three standards. One difference between the findings for specific violations of the PPE standard and those for the *IPENX* variable is that with the former the coefficients are larger for the *A* categories, while with *IPENX* they are larger for the *B* categories. These differences, however, are not statistically significant.

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IV. DISCUSSION

Our findings from Table 2 indicate that the effects of penalty inspections are not limited to injury types that are closely related to OSHA standards. The best example of this is that the coefficient for "exertion" injuries was large, negative, and significant, while the coefficient for "caught-in" injuries was far from significant. Given that the "exertion" category has by far the largest number of injuries as well as the largest coefficient, it clearly contributes a large share of the injuries prevented by inspections. Although OSHA has sometimes used its "general duty" clause to cite employers for poor ergonomic practices, there is general agreement that this is a much weaker tool than a regular standard.[4] Although we did see inspection effects for some of the event types judged more preventable by compliance with OSHA standards, the overall impact was greater for the categories judged less preventable.

We interpret the results from Table 2 as providing support for the behavioral model of employer responses. Responses clearly were not restricted to measures that improved compliance with OSHA standards. One reviewer of this paper noted that employers might intend to focus their efforts on only those hazards covered by OSHA standards, but that their activities (e.g., training) could spill over onto other types of hazards, unrelated to OSHA standards. We view this interpretation as consistent with a behavioral model, at least in terms of observable outcomes and policy implications: the bluntness of employer tools and the uncertainty about their effects generate impacts of OSHA inspections on a much wider range of injuries than would be expected from a simple examination of OSHA standards.

Our regressions for particular standards in Tables 3 and 4 attempt to assess whether there are links between the particular standards cited and the types of injury events that decreased. Of the two standards we expected to have an impact, we found strong effects for one and almost no effects for the other. The standards that we expected not to have an impact, did not.

Our ability to test Model 2 is somewhat limited. The machine guarding standard was the one where the predicted effects were most closely linked to a specific injury type (caught-in or between injuries). If we had found an effect there, then Model 2 would predict that we would find effects on other more standards-related (category *A*) injuries, but not on unrelated injuries. However, because we did not find that the machine guarding standards even affected caught-in or between injuries, using them to test Model 2 does not seem appropriate.

Citations of the personal protective equipment (PPE) standard did have an effect on several injury categories. The problem here is that it is plausible that direct compliance could account for several of these effects. Protective eyewear can prevent eye abrasions. Protective gloves can prevent dermatitis and burns, which account for a majority of the injuries in the category of "harmful exposures." Gloves, shoes, and hard hats can prevent "struck-by"

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injuries and some "caught-in" injuries. Thus it is hard in this case to distinguish the predictions of Model 1 and Model 2.

It is possible, indeed likely, that all three models are operative to some extent. We are struck by the somewhat different pattern of effects when we look at the effect of specific standards in Tables 3 and 4 compared to the effect of a penalty inspection where we do not know what kinds of standards were cited in Table 2. In Table 2, the largest and most significant coefficients are clearly for injury types that are not related to standards. In Tables 3 and 4, the coefficients for injury categories that we identified as more standards-related are relatively larger and more significant. Thus, it appears that when the PPE standard (and perhaps others not examined in this study) is cited, that there is a relatively larger impact on standards-related injuries, through either Model 1 or Model 2.

The limitations of this study should be kept in mind. As we noted, we looked only at DAW injuries because they are the only ones for which event type data are collected. One problem with this restriction is that in our analysis of event types, we cannot tell whether injuries are actually being prevented or whether they are being transferred to restricted work activity (RWA) status. We should emphasize that our earlier work did show that penalty inspections reduced the overall category of lost workday injuries. So the question is not whether these inspections reduce injuries overall; but whether we can rule out the possibility that *for a given sub-category*, substitution is occurring instead of prevention. Also, the question is not whether employers have substituted RWA for DAW injuries. Many certainly have, but by itself that need not cause a bias in our results. A bias could be created if two conditions were met: (a) employers are more likely to substitute RWA for DAW injuries if they have inspections with penalties; and (b) the extent of this substitution is different for different types of injuries. For example, if a penalty inspection gets employers to switch Category *B* DAW injuries to RWA (while Category *A* injuries remain unchanged), we could mistakenly find significant negative coefficients on *IPENX* for Category *B* injuries and not for Category *A*.

Regarding the first condition, our earlier work (Gray & Mendeloff 2002) showed that the effects of penalty inspections on RWA injuries were usually positive, but they were not statistically significant except in the case of unionized workplaces, which comprised only about 25 percent of the sample.

The second condition could be met if some injury types are easier to treat as "restricted work activity" cases than others. The fact that the number of DAW "crushing" injuries went up by 3 percent from 1992 to 1998, while the number of "exertion" injuries went down by 34 percent suggests that many of the latter could have been converted into RWA injuries. The exact features that facilitate this switch are not all apparent, but it seems that injury severity could be a proxy, with less serious injuries being easier to switch to RWA. We do find a negative correlation (−0.304) between severity (median days lost in 1998) and DAW reductions (percentage change in number of

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DAW injuries between 1992 and 1998) across eight different injury types. We also find a negative correlation (−0.203) between severity and the estimated preventive effect of inspections for those injury types. The signs of these correlations are consistent with the second condition, although with n = 8 they are far from statistical significance.

Overall, we find weak support for the first condition and some support for the second. So it is possible that our data being limited to DAW injuries may bias our results, though these calculations do not prove that such a bias exists (OSHA inspections might really have a greater impact on less severe injuries, for reasons other than reclassifying injuries from DAW to RWA).

A limitation of the analysis of specific standards is that we have been able to examine only a handful of standards. Since multiple standards are often cited during an inspection, the pattern of intercorrelations with other standards not included might lead to different results if the analysis could fully account for them.

The practical importance of distinguishing between the three different models lies largely in the greater potential scope of impact for OSHA enforcement policy as we move from the first to the second to the third model. Most observers have judged that current OSHA standards apply to only a minority of all injuries (Mendeloff 1979). (The proposed ergonomics standard, killed by Congress in 2001, was, in part, an attempt to address that gap.) If this is true, even if inspections can spur compliance with all current OSHA standards (Model 2), not just those cited in an inspection (Model 1), the potential scope of injury prevention remains limited. Our findings here suggest that the impact of OSHA inspections extends to injuries that are unrelated to standards (Model 3) and could therefore be considerably larger than predicted by Model 1 or Model 2. In addition, evaluations of OSHA's impact which assume wrongly that only "standards-related" injuries can be prevented may miss effects outside that category.

The findings about the strong contribution from citing violations of the personal protective equipment standard suggests that OSHA should pay more attention to this issue in its inspections, and that it should consider giving more emphasis to this issue in its informational activities.

A final implication pertains to targeting inspections. Historically, OSHA's programmed inspections have been targeted largely on the basis of the injury rate of the industry. Establishments were chosen randomly from within high-rate industries. More recently, OSHA has used establishment-specific data, which allows it to target high-rate establishments, rather than only high-rate industries. However, both of these programs focused primarily, if not totally, on manufacturing. One rationale for that focus was that OSHA standards are more relevant to the hazards in manufacturing than to those in other industries, and thus that compliance officers are more likely to find violations in that sector than in others. With Model 1 or perhaps Model 2, the expected number of violations found seems like a reasonable basis for

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targeting inspections. Model 3 suggests a somewhat more complicated analysis. Although finding violations that justify levying a penalty does seem to be a prerequisite for an inspection's impact, the penalty itself appears to generate improvements that go beyond compliance with standards and that are not necessarily related to the number of violations detected. Therefore, OSHA might want to explore these targeting issues in more detail.

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NOTES

1. For all of the variables expressed in logs, we took the log of $(x + 1)$ rather than the log of x, in order to avoid taking the log of zero.
2. Alternative specifications were tested: four "lagged inspection" dummies, total inspections in last four years, total penalties imposed, numbers of violations found, and others. The *IPENX* 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 the impacts of citing different violations.
3. Federal OSHA standards for general industry have the following form: 1910.3452 (a)(1)(i). The first component of this expression—"1910"—refers to Part 1910 of the code of federal regulations. It is a suffix for all of OSHA's general industry standards. The next 4 digits refer to the topic of the particular standard. For example, "0212" is the general requirement for machine guarding. "0213" includes the requirements for woodworking machinery; "0214" addresses abrasive wheel grinders; "0216" and "0217" both concern mechanical power presses, and so on. Each one of these paragraphs typically includes dozens of sub-paragraphs, which describe OSHA's requirements for the particular equipment or operation.
4. Personal communication with an OSHA Regional Director.

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