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A BEHAVIORAL APPROACH TO COMPLIANCE: OSHA ENFORCEMENT'S IMPACT ON WORKPLACE ACCIDENTS

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ABSTRACT

This study tests for effects of OSHA enforcement, using data on injuries and OSHA inspections for 6,842 manufacturing plants between 1979 and 1985. We use measures of general deterrence (expected inspections at plants like this one) and specific deterrence (actual inspections at this plant). Both measures of deterrence are found to affect accidents, with a 10% increase in inspections with penalties predicted to reduce accidents by 2%. The existence of specific deterrence effects, the importance of lagged effects, the asymmetrical effects of probability and amount of penalty on accidents, and the tendency of injury rates to self-correct over a few years support a behavioral model of the firm's response to enforcement rather than the traditional 'expected penalty' model of deterrence theory.

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The field of regulatory analysis has virtually ignored enforcement policy, despite the critical link enforcement provides between regulatory rules and economic outcomes. Enforcement provides incentives for regulated entities to comply with regulatory rules. Appropriately designed enforcement policy may also provide some correctives for the inefficiencies inherent in rule-based regulatory systems (Scholz, 1984a). Existing models of enforcement have been limited to a simple deterrence model which we believe provides an insufficient basis for understanding the response of firms to regulatory enforcement.

Recently, the study of regulatory enforcement has been aided considerably by the availability of detailed enforcement information from the Occupational Safety and Health Administration (OSHA). Unfortunately, the lack of panel data on how individual firms respond over time to enforcement actions has been a major impediment to this research. Theoretical models have been limited to relatively simple notions of deterrence. Empirical estimations have been limited to tests based on cross-sectional firm-level data (Smith, 1979; McCaffrey, 1983) or annual data aggregated to national (Viscusi, 1979), industrial categorization (Viscusi, 1986a; Bartel and Thomas, 1985) or injury category (Mendeloff, 1979). These studies have found mixed results in terms of overall effectiveness of OSHA inspections, and each suffers from different threats to the validity of their empirical findings (see Viscusi, 1986b, for a review.)

This article reports our efforts to develop an alternative model of firm responses to regulatory enforcement and to test the model on a dataset that incorporates enforcement actions and accident experiences of individual firms over time. The model we use is based on the assumptions of the behavioral theory of the firm (Cyert and March, 1963) about the limited ability of firms to evaluate and respond to enforcement threats. The model is tested on a unique database that combines the accident experience of 6,842 manufacturing plants between 1979 and 1985 with the OSHA enforcement records for those firms.

I. THE MODEL: ACCIDENTS, SAFETY EXPENDITURES, AND ENFORCEMENT

The number of accidents at a plant depends on a variety of factors. These include the technology in use at the plant (manufacturers of lumber and wood products averaged 11.1 lost workday incidents per 100 workers in 1979, compared with 5.6 for manufacturing as a whole), and the size and quality of the plant's workforce (more workers, less experienced workers, and more tired workers are associated with more accidents). Other factors include the expenditures made by the firm to increase safety, both through physical capital (safer equipment) and human capital (more safety training for workers). Finally (as their name suggests) accidents have a substantial random component, which cannot be eliminated completely by anything the firm does.

The classical model allows the optimal level of safety expenditures by the firm to depend on the costs of the expenditure relative to the costs of the accidents being prevented. Paralleling the theory of deterrence developed in criminology (Becker, 1968), OSHA inspections provide a further incentive for safety expenditures: avoiding the penalties for being found in violation of OSHA regulations. Both the occurrence of accidents and OSHA penalties are random variables, so the firm (assumed to be risk-neutral) will consider the expected values of both variables when deciding how much to spend on safety.

More formally, let the expected number of accidents, A, be a function of the firm's technology (T), workforce (W) and safety expenditures (S):

(1) $E(A) = a(T,W,S)$, with $a'<0$ and $a''>0$,

where a' and a'' refer to the derivatives of a with respect to S. Let the expected penalties, P, be a function of OSHA's enforcement policy (X), as well as the firm-related variables:

(2) $E(P) = p(X,T,W,S)$, with $-1<p'<0$ and $p''>0$

(as with a' and a'', p' and p'' focus on the effects of safety expenditures). If the average cost of each accident to the firm is C (including both direct costs, such as damage to machinery and compensation to workers, and indirect costs, such as higher wages for hazardous employment), the firm will choose its safety expenditures to minimize its accident- and safety-related costs:

(3) $\min_S \; (C*a(T,W,S) + p(X,T,W,S) + S)$.

Examining the first order condition for a minimum (that this is a minimum, not a maximum, follows from the assumptions about the derivatives of a and p), the optimal safety expenditure, S*, will satisfy:

(4) $a'(S^*) = -(1+p')/C$,

leading to

(5) $dS^*/dC > 0$ and $dS^*/d|p'| > 0$

so that increases in either the expected costs of an accident or OSHA penalties will lead to greater expenditures on safety. In effect, the presence of OSHA penalties reduces the cost to the firm of safety expenditures: p' of every dollar spent on safety is paid for by reducing expected OSHA penalties. This assumes that safety spending reduces both accidents and OSHA penalties.[1] The assumed risk-neutrality of the firm implies that only the expected value of OSHA penalties matters, so that a high probability of getting a small penalty would be equivalent to a low probability of getting a large penalty.

The behavioral theory of the firm (Cyert and March, 1963) provides an alternative framework for modelling firm decisions. The theory includes four major concepts: quasi-resolution of conflict (addressing multiple goals sequentially rather than simultaneously); uncertainty avoidance (short-run reaction to feedback rather than long-run planning); problemistic search (solving particular problems, rather than general optimization); and organizational learning (adaptation of goals and attention rules as the environment changes). These concepts are based on observations of business decisionmaking processes, in particular observing that firm's behavior deviates systematically from optimal performance (which would simultaneously maximize expected profit over all possible behaviors) because of limitations on the firm's decisionmaking ability.

Research in behavioral decision theory has found consistent patterns in which decisionmaking behavior by individuals deviates from optimality as defined by expected value theory, particularly where probabilistic relationships and expected values are involved (see Schoemaker, 1982). We suspect that firm behavior exhibits similar patterns, as suggested by the behavioral theory of attention allocation in which attention is focused in each period on that area where the firm's performance fell furthest below expectations. This sort of firm behavior, called 'putting out fires', was examined in more mathematical detail by Radner (1975). He showed that this is an effective strategy for 'survival' of the firm (if there are any effective strategies), and that it tends to keep the firm's performance in the different areas 'close together'.

Our model of the decision process affecting accidents leads us to four hypotheses implicit in the behavioral model which are not implied by the general optimization model. First, an unexpected increase in accidents will cause managers to pay more attention to safety. This should lead to a reduction in accidents in later years until the firm's attention turns back to other areas. Similarly, a lower rate of accidents than usual should lead to less attention and the possibility of rising accidents in later years. Since managerial attention and safety expenditures are not measured directly in the data, we should find that the unexplained changes in the number of accidents over time are negatively autocorrelated.

It is less clear in the maximizing model presented earlier that S^* should depend on unexpected changes in past accident rates. What matters for S^* is not the number of accidents, A, but the effectiveness of safety expenditures in reducing expected accidents (a'), which ought to be more related to the technology of the firm than to transitory shifts in A. Shifts in S^* due, for example, to shifts in technology would be relatively permanent, and would lead to positive, not negative, autocorrelation in the unexplained changes in accidents. If safety expenditures take the form of changes in the firm's capital stock, we would also expect to see positive autocorrelation, as one year's increase in S^* reduces A for many years to come.

Second, it could take several years to observe the full effect on accidents of changes in OSHA enforcement. This is due to the time needed for organizational learning: the firm's decision processes are only modified slowly, as the firm learns of the changes in its operating environment. The more peripheral the information to primary organizational processes, the longer the lag between environmental changes and responses by the firm. An increase in enforcement is expected to raise safety expenditures and reduce accidents as in the classical model, but with some time delay dependent on the capacity of the monitoring system and the firm's ability to act on available information. The simple classical model presented above does not allow explicitly for such delays, although it might exhibit some delayed effects if safety expenditures were embodied in the firm's capital stock, reducing accidents for several years.

After a major increase in enforcement a variety of patterns of lagged effects on accidents could occur, as illustrated in Figure 1. If firms respond with changes in operations and administration that quickly reduce exposure to risks, but decay in effectiveness as attention turns elsewhere, Panel A might result. Capital-intensive expenditures to reduce risk take more time to plan and implement, but once in place provide a more permanent reduction in accidents, as in Panel B. In some cases, both kinds of effects could combine, leading to C.

Third, an inspection of a given firm could have a large effect on that firm's expenditures on safety. If the firm had been focusing on concerns other than safety, behavioral theory suggests that the inspection might lead the firm to focus on safety issues (similarly to the effect of a sudden increase in accidents, mentioned above). The deterrence analyses developed in the study of criminology and deviance behavior have long distinguished between general deterrence-- the effect of an act of legal punishment on the subsequent behavior of the general populace-- and specific deterrence--the effect of the punishment on the subsequent behavior of the individual being punished. These analyses suggest that the specific deterrence effect implicit in the behavioral model could be considerably greater than the classical effect associated with changes in the level of general enforcement.

Classical theory would suggest that there should be no specific deterrence effect (other than the direct abatement of cited violations) unless the firm concluded from the inspection that it had been wrong in its estimate of receiving a penalty.[2] Firms are assumed to hold rational expectations, in that they expect to receive penalties at the rate actually experienced, on average, by similar firms. An alternative basis for firms' expectations could be a Bayesian learning model, with firms updating their prior expectations about enforcement based on their own experience. Such a model would yield an effect like specific deterrence, with recently-inspected firms paying more attention to safety as they revise upward their expectations of enforcement. In terms of the simple model, S would then be a function of past penalties (specific enforcement) received by the firm, in addition to the expected penalties (general enforcement) faced by the firm.

Fourth, marginal changes in the probability of a penalty and the average penalty amount may have quite different general deterrence effects on accident rates, depending on which of the two is most salient to the firm's monitoring of the external environment. It may be that firms have good information on the number of similar firms that are penalized, but not on the amount of penalties (or conversely may pay more attention to a few extremely large penalties). This issue is important in determining the optimal policy mix of wide-ranging coverage and intensive inspections. In our model, general deterrence is represented independently by the probability and the average amount of penalties.

The classical deterrence model treats the probability of being inspected and the penalty level as perfect substitutes for risk-neutral firms, as noted earlier. If firms are risk-averse, they will tend to react more to the penalty level than to the probability of being penalized. If firms react more to the probability than to the average penalty, risk-loving behavior (or some other deviation from the simple classical model) is indicated.

In the empirical work, we measure two separate effects of OSHA enforcement on safety. The first is the specific deterrence effect (associated with the behavioral theory), in which an inspection of a particular plant will have an effect on safety in that plant. The second is the general deterrence effect (associated with the classical theory), in which an increase in the (perceived) expected penalty of non-compliance will have an effect on safety in all plants. This expected penalty is further divided into the probability of being penalized and the average value of the penalty, corresponding to the separate probability and amount used to calculate expected value. These alternative enforcement measures allow us to compare the relative impact of each kind of enforcement activity on safety.

II. DATA DESCRIPTION

The dataset assembled for this project combines information over time on both accident rates and OSHA enforcement, data which was not available at the plant level for previous studies. A dataset produced by the Bureau of Labor Statistics (BLS) that contained plant-level accident records from 1979-1985 was merged with the Occupational Safety and Health Administration's Management Information System (MIS) file containing enforcement actions for all plants during the same period. The BLS file matched records from the BLS Annual Survey for all plants with data for each year from 1979 to 1985, based on a common identification number available in the annual files (Ruser and Smith, 1988). All plants in this file that were located in the twenty-eight states with federal OSHA enforcement covered by OSHA's MIS were then matched with the

OSHA enforcement file.

Since no common identification number was available in both OSHA and BLS datasets, we employed a sophisticated record-matching program based on the technique of Fellegi and Sunter (1969), as described in Gray (1987). Both datasets contain various characteristics of the plant, including: firm name, address, zip code, city, state, employment, and industry. These characteristics were used to match plants in one dataset to plants in the other, based on the probability of agreement on particular variables.[3] To protect the confidentiality of firms in the BLS Annual Survey, all merging operations were done at BLS and no firm identifiers were provided on the final matched data tape. Unfortunately, this precluded us from adding further firm-specific variables potentially relevant to accident behavior.

The final dataset consists of 6,842 plants with data from 1979 through 1985. For each year, we know employment and hours worked, as well as the number of lost workday injuries, and the total number of lost workdays. Each OSHA inspection of the plant during the 1979-85 period is recorded, including information about the kind of inspection and the citations and penalties assessed as a result of the inspection. The final plant-specific identifier is an industry code, which is limited to the two-digit SIC level to avoid breaching confidentiality restrictions.

The plants in the dataset are not perfectly representative of the manufacturing sector, as can be seen by the comparisons in Table 1. The BLS surveys are based on stratified random samples that over-sample large plants, so plants that were included in seven consecutive surveys are considerably larger than the typical manufacturing plant. They averaged 523 workers in 1979, compared with 87 workers for all manufacturing plants subject to OSHA enforcement.[4] The average lost workday incidence rate in 1979 of 6.97 is above the 5.9 rate for the entire manufacturing sector.

The plants in the sample were also relatively heavily inspected by OSHA. About 26% of them were inspected in a given year, compared with 2.3% in 1980 for all plants under OSHA's jurisdiction. Furthermore, plants in the sample represented almost 20% of the employees covered by OSHA inspections in 1979, and account for an even greater percentage of accidents in the manufacturing sector. The sample accounted for about 4% of OSHA inspections. In short, the sample represents a considerable if not necessarily representative proportion of OSHA's total jurisdiction in manufacturing. We cannot be certain that our estimates of OSHA impacts are generalizable to all manufacturing firms, but we believe that this sample may be better than a representative sample for studying the nature of the impacts of enforcement on accident rates.

III. ESTIMATION PROCEDURE

A.General Deterrence

The estimation procedure for our model requires two stages. The first equation estimates predicted probability and amount of penalty for each firm in each year. These general deterrence measures are then incorporated in the second stage estimation of the determinants of firm accident rates. To test the effect of general deterrence on accident rates, we need to measure the expected penalty faced by a non-complying plant. Most previous studies have used levels of enforcement activities, generally aggregated to the industry level, as a proxy for expected penalties. We make use of our data on actual penalties imposed in plants in our sample to obtain plant-specific predictions of expected probabilities and amounts of penalties. We use a number of variables, including both the traditional enforcement measures aggregated to the two-digit industry level and plant-specific accident characteristics, to calculate the two components of predicted expected penalties for the plant: the predicted probability of an inspection with penalty in a given year (PPROB) and the predicted penalty amount if an inspection with penalty occurred (PAMT).

All variables used in the study are listed in Table 2, and the results for the equations predicting PPROB and PAMT are given in Table 3. Since the dependent variable in the PPROB equation is either zero or one, a probit estimation procedure was used. The expected penalty received, conditional on having had some penalty imposed, was estimated using ordinary least squares on the subsample of 4,735 plant-years in which an inspection with penalty took place. Alternative estimation methods, including a linear probability model for PPROB and a tobit model (attempting to capture both the penalty amount and the probability of being penalized) were also tested, but did not affect the results materially.

In both equations, the level of OSHA's enforcement activities in the firm's two-digit SIC category were represented by the annual number of inspections with penalties divided by the number of firms in that industry (INDPROB), and by the average penalty per inspection with penalty (INDAMT). These measures represent the 'enforcement budget' devoted by OSHA to firms in a given SIC category, which might be observed during the year through communications centering around trade associations.

Firm-specific measures included two measures of size and two related to accidents. Size is important for OSHA targeting decisions for increasing the number of exposed workers whose working conditions are inspected; accidents are important because high accident rates indicate risky environments in which OSHA enforcement might be most effective in reducing accidents. Federal OSHA's records-check policy, for example, attempts to focus inspections on high accident firms by selecting primarily four-digit SIC industries with injury rates above the national average, and by terminating inspections if the plant's records indicate that its injury rate lies below the manufacturing average. We include one measure of accidents averaged over the two prior years (AVE2NUM), and one measure of changes in accidents between the second and first year prior to the current year ($PCHNUM_{t-1}$). Size measures include total hours of work in the year (LOGHOURS) and average employment during the year (LOGEMPS), both used in log form to minimize excessive influence of the largest firms on the estimating procedure. Year dummies were added to reflect annual variations in overall OSHA enforcement activities.

All primary variables except changes in accidents were significant determinants of the probability that a firm would be penalized in a given year, while only the number of penalties per firm was significant in predicting the amount of penalty. The prior level of accidents was significantly related to the probability of an inspection, but the change in accidents was not. Predicted enforcement variables (PPROB and PAMT) were generated from these equations, and used to measure the expected enforcement faced by the plant in the second stage of the estimation.[5]

B. Determinants of Accidents

The second stage of the estimation examines the determinants of a plant's accidents. Two different measures of accidents are used: the number of lost workday injuries that occurred in the year and the total number of lost workdays in the year. This allows us to check the independent variables for differences between their effect on the frequency (PCHNUM) and the seriousness of accidents (PCHDAYS). Since PCHDAYS is heavily influenced by a few long-term injuries (like back injuries) and those factors that keep some individuals out longer than others, it is more difficult to predict than PCHNUM. Of course, there may be measurement problems involved in either dependent variable that would limit our ability to explain it.

For each version of the dependent variable we use the percentage change in accidents rather than the level of accidents, for a number of reasons. First, the change form incorporates the bounded rationality notion of feedback, where the firm would pay more attention to an increase in the number of accidents than to a high number of accidents. Using changes rather than levels helps to reduce the problem of unmeasured variables (such as the inherent safety of a plant's production process) that change only slowly. The change form also minimizes the possibility of simultaneity bias caused by OSHA's policy of targeting inspections on firms with high accident rates, a policy confirmed by our initial exploratory analyses.[6] Simultaneity biases due to the impact of compliance levels (or accidents in our case) on enforcement have been a major problem in deterrence research (Nagin, 1978); both the autoregressive errors and the change form of our estimation are designed to minimize this bias. Our analysis of expected penalties shows that inspections tend to be targeted more toward firms with a high level of accidents than toward firms with a growing number of accidents. The percentage change in accidents is used, rather than the change in the number of accidents, to keep the very large plants in the sample (which have large numbers of accidents because of their large workforces) from dominating the results.

The use of the change form for the dependent variables also has implications for the appropriate ways to measure the independent variables. The general deterrence model suggests that changes in the expected penalty for non-compliance will lead to changes in the optimal compliance level for the firm, and nence lead to changes in the accident rate (so the general enforcement variables enter the equation in change form). However, the behavioral model suggests that an inspection with penalty focuses the firm's attention on improving safety at that plant, so that the specific enforcement variable would enter directly in zero-one rather than in change form.

The other explanatory variables include employment and hours, as well as industry and year. Changes in employment or hours worked will change the opportunities for accidents to happen, and hence change accident rates. In addition, increasing employment will generally involve hiring new employees, and increasing hours per worker may involve increasing worker fatigue, both of which will increase accidents. This indicates using changes in employment and hours rather than levels; here percentage changes are used to avoid giving very different values to firms of different sizes. If controlling for the industry of the plant only explains differences in the inherent hazardousness of the technology used in the plant, then the industry controls will not be significant. However, different industries could be facing different rates of progress in developing safer equipment, which could lead to differences in the change in accidents across industries. Year dummies are also included, to control for any macroeconomic changes that affect safety across all industries.

The final form of the equation to be estimated is given below. The residual part of changes in accidents is allowed to depend on past residuals in an autoregressive framework, to test the first hypothesis. The effects of general as well as specific enforcement variables on accidents are estimated over a number of years to test the second and third hypotheses, and the effect of the probability and amount components of expected penalties are estimated separately to test the fourth hypothesis.

$$PCHY_t = b_0 + \sum_{i=0}^{2} b_{1i}*DPPROB_{t-i} + \sum_{i=0}^{2} b_{2i}*DPAMT_{t-i} + \sum_{i=0}^{3} b_{3i}*IPEN_{t-i}$$

$$+ b_4*PCHHOURS_t + b_5*PCHEMPS_t + \sum_{i=83}^{85} b_{6i}*YEAR_i + \sum_{i=21}^{39} b_{7i}*IND_i + v_t,$$

with $v_t = e_t + a_1*v_{t-1} + a_2*v_{t-2} + a_3*v_{t-3}$.

The general deterrence variables, DPPROB and DPAMT, are the change in expected probability of penalty and the change in expected penalty amount, respectively. IPEN is a zero-one dummy indicating whether an inspection with penalty occurred at that plant in that year, PCHHOURS and PCHEMP measure the percentage changes in hours worked and number of employees, and YEAR and IND are series of dummy variables.

Here e(t) is assumed to be an independent, normally distributed series with a mean of 0. The coefficients a_1, a_2, and a_3 represent the impact on current accidents of unexplained changes in accident rates from one, two, and three years ago, respectively. The behavioral theory implies that these coefficients should be significant and negative. Furthermore, we expect their absolute values to sum to one or less, in order for the model to be stable. By explicitly modeling this self-correction process, we minimize the regression to the mean problem previously recognized in deterrence research. This problem would arise in our context if many enforcement actions (inspections with penalties) tended to happen during a period of high accident rates, after which both accident rates and inspections fell. The drop in accident rates back to its long-run value might be attributed mistakenly to the effectiveness of the inspections in the previous period. By modeling the process driving regression to the mean, we reduce the potential bias in estimates of enforcement effects.

Since our data series for each firm is limited to seven years, we have limited our estimation of lagged effects to three years of previous data for the autoregressive process as well as for the enforcement variables.[7] Given the controversies about alternative techniques for estimating finite lag structures (see Judge et. al., 1980) and the lack of a single, clearly defined lag structure in our model, we have allowed each of the lagged variables to enter the equation directly. This would generally be expected to lead to multicollinearity and lack of precision on individual lag coefficients, but this appears not to be a problem, since most estimates were significant and were robust to alternative specifications of the equation not reported in this paper.

IV. DISCUSSION OF RESULTS

Table 4 reports the basic estimations of enforcement effects on lost workday incidents and lost work days. As expected, the independent variables explain less of the variance in lost workdays (6%) than of the variance in lost workday incidents (12%). Both equations support the same conclusions about the importance of the behavioral theory of the firm and about the impact of enforcement actions on accidents, although the estimated effects are slightly different in the two equations.

A. Hypothesis 1: Self-correcting Mechanisms

One of the most striking and robust findings in this and all other estimations we ran is the strong tendency of 'surprises' in the accident rate to be compensated for and to return to zero within three years. The autoregressive coefficients a_1, a_2, and a_3 are consistently negative and highly significant. The estimated impact on accidents decreases for more distant shocks, consistent with the assumption that recent shocks are the most important in driving firm behavior. In the estimation based on PCHNUM, any change in a given year is compensated by a 49% change in the opposite direction in the following year, a 32% change in the second year, and a 13% change in the third year. The effect of the residual (unexplained) variation is almost fully (94%) compensated for over the next three years. This process alone explains more variance than the other variables combined, raising the explained variance from 12% for all independent variables to 29% for the autoregressive estimates for the dependent variable of incidents, and from 6% to 27% for lost workdays.

It should be noted that this autoregressive process is not explained by a stochastic process in which an unusually high number of accidents in one year is followed by a regression to the mean in the next year, since such a process would not produce multi-year correlations among residuals. The observed self-correcting process supports a behavioral theory in which a year with an unexpectedly high number of accidents is counteracted over time by the firm's efforts to improve safety.

The fact that almost 100% of the initial shock is compensated for is difficult to explain in terms of classical theory, which would predict some permanent shift in the level of accidents when those (unobserved) factors change which affect the optimal level of safety expenditures. If those unmeasured factors changed slowly over time, we would observe positive coefficients for a_1, a_2 and a_3, as the changes brought about in any period would continue to have effects in the same direction in following periods. The strong negative coefficients indicate that self-correction rather than lagged adjustments to optimal levels is more important in accounting for changes in accident rates.

B. Hypothesis 2: Lagged Effects of General Deterrence

The results in Table 4 demonstrate that both the expected probability and the amount of penalties exert an overall permanent negative effect on accident rates. The total effect of a unit increase in DPPROB, obtained by adding the coefficients for the current and lagged values, equals -.75 for the number of incidents and -.21 for lost workdays, with the comparable figures for DPAMT being -1.57 and -.87 respectively. The significant positive coefficient in both equations for the current value of DPPROB is troublesome, but may be due to the inclusion of so many general deterrence terms (three each of DPPROB and DPAMT).[8] The insignificance of the second lag of DPPROB and the first lag of DPAMT in the lost workday equation is less troublesome, given the greater difficulty in predicting lost workdays. On the whole, these estimates support the hypothesis that firms respond to increases in expected penalties by reducing hazards in the workplace.

The length of time required for changes in enforcement to produce their full effect is longer than had been found previously. While other enforcement studies have found one year lags between enforcement and effect (Viscusi, 1986a and Scholz, 1987), our estimates suggest that the effect continues beyond the first period as well, and that studies using only a single period may not capture the full effect of enforcement changes. This length of delay is consistent with the assumption that implementation of changes to affect accidents takes considerable time. The fact that the coefficients do not become positive in longer lags indicates that the initial effect does not decay over time, so the effects appear to be permanent (most consistent with Panel B in Figure 1).

Of course, lagged effects distributed over a number of time periods are not necessarily evidence of suboptimality: some economic models interpret distributed lags as representing optimal behavior within the constraints of fixed investments and contractual obligations (see Gujarati, 1988). Distributed lags could also be specified more formally in terms of a Bayesian learning process in which the lagged effects of changes in the objective environment are due to the firm's utilization of a 'moving average' estimation of its objective environment in safety decisions. Our attempts to force the lagged coefficients to follow some smooth decay pattern over time (consistent with gradual learning) were rejected by the data in favor of the separate (and fluctuating) coefficients reported here.

C. Hypothesis 3: Specific Deterrence

The results in Table 4 confirm the behavioral hypothesis that the surprise involved in the actual imposition of a penalty has an effect on behavior over and above the general deterrence effect of expected probabilities and amounts of penalties. In both equations, the primary effect occurs in the first and second year after an inspection, confirming the long period of time over which enforcement effects must be measured. Although the effect remains negative in the third year after an inspection, the coefficient is not significant in either equation. The effect of IPEN in the current year is insignificant for lost workdays and relatively small for incidents. As with general deterrence, this pattern is most consistent with case B in Figure 1. These results are quite robust to changes in the measures of general enforcement used, the lag lengths for the enforcement variables, or the inclusion of other controls. On the other hand, the magnitude of the specific deterrence effect is relatively small when compared to the general deterrence effect, as seen in the next section.[9]

D. Hypothesis 4: Asymmetrical Effects of Probability and Amount of Penalty

Classical deterrence theory assumes that the expected penalty for non-compliance (represented by the interaction term PPEN = PPROB*PAMT) should capture all of the relevant information in PPROB and PAMT. However, when we added PPEN to the regression in Table 4 (not reported here), the coefficients on PPROB and PAMT remain much the same, and PPEN is only significant in one lagged term. The superior performance of the separate variables over the combined version is consistent with the behavioral theory's suggestion that the probability and the amount of penalties are not the perfect substitutes suggested in classical deterrence theory.

To test the hypothesis that changes in the number of inspections with penalty and the average amount of penalty have different effects on accident rates, we have calculated the impact of a 10% change in each on accidents, as reported in Table 5. These calculations take account of the effect each enforcement change has on both the estimated probability of being penalized (PPROB) and amount of penalty (PAMT), as determined in the estimation equations for each of these variables reported in Table 3. In addition, a 10% increase in the number of total inspections is assumed to bring about a corresponding 10% increase in the number of inspections directly affecting the particular plants in our sample (IPEN), and therefore affects accidents through IPEN as well. Thus, for a given change in enforcement policy (dX), the effect on incidents (dPCHNUM) of the change in the probability of a penalty (working through INDPROB and IPEN) is given by:

$$\begin{aligned}(\mathrm{dPCHNUM}/\mathrm{dX}) = {} & (\mathrm{dPCHNUM}/\mathrm{dPPROB})*(\mathrm{dPPROB}/\mathrm{dINDPROB})*(\mathrm{dINDPROB}/\mathrm{dX}) \\ & + (\mathrm{dPCHNUM}/\mathrm{dPAMT})*(\mathrm{dPAMT}/\mathrm{dINDPROB})*(\mathrm{dINDPROB}/\mathrm{dX}) \\ & + (\mathrm{dPCHNUM}/\mathrm{dIPEN})*(\mathrm{dIPEN}/\mathrm{dX}).\end{aligned}$$

The first effect calculates the impact through PPROB, the second through PAMT, and the third through IPEN.[10] The results reported in Table 5 show that a 10% increase in inspections with penalty is estimated to decrease lost workday incidents by 1.61%, which is about 75% more than the decrease associated with the same change in average penalty. The difference in impact on lost workdays is similar (66%). These results reflect the general conclusion from most empirical research on deterrence (Lempert, 1982 and Nagin, 1978), and Viscusi's 1986 study of OSHA, which find that the probability of being punished is more likely to have significant effects on compliance than the amount of penalties.

These results are difficult to explain in terms of the classical deterrence theory with risk-neutral firms. The combined expected value term associated with that theory is less robust than the independent terms, and the independent terms have assymetrical effects on accidents. To explain these results as a simple departure from risk-neutrality would require risk-loving, not risk-averse firms.

E. The Impact of Enforcement on Accidents

Contrary to some studies (McCaffrey, 1983; Viscusi, 1979; Bartel and Thomas, 1985; see Viscusi, 1986b), our estimates suggest that OSHA enforcement has a significant impact on injury rates, whether measured by lost workday incidents or by the number of lost workdays. Furthermore, the magnitude of effects is somewhat greater than is suggested by those studies finding significant effects (Mendeloff, 1979; Smith, 1979; Viscusi, 1986a). For example, Viscusi estimates the total impact of OSHA on injuries to be 1.5-3.6%, while our results would indicate a 5-16% change in injuries from a 100% decrease in enforcement (although such an extrapolation is so far beyond the changes in enforcement on which the estimates are based that it is only given for comparison, and is not likely to capture what would happen if enforcement really dropped to zero). We believe that the availability of panel data on individual firms, and the more appropriate model we were able to use on this microdata, were better able to capture the relatively small true effects of enforcement than the previous studies based either on aggregate or cross-sectional data. On the other hand, we recognize that these estimates are most relevant for the relatively large, high-accident plants included in our sample -- the kind of plants that have remained among the primary concerns of OSHA. The impact on smaller, more transient firms in non-manufacturing industries may indeed be smaller than our estimates would indicate, since OSHA enforcement is likely to be more successful in reducing injuries among the firms on which it focuses most.

A numerical example may clarify the magnitude of impacts suggested by our estimates. If we consider a 10% increase in the enforcement effort directed towards our sample, there would be an increase of 68 in the annual number of inspections with penalties, which would reduce lost workday injuries by 2130 (31 injuries per inspection with penalty), and total lost workdays by 18243 (268 lost workdays per inspection with penalty). Alternatively, this would mean increasing the average penalty (per inspection with penalty) by $70 (total added penalties of $69,765) in order to reduce lost workday injuries by 1230 (18 per dollar of penalty) and total lost workdays by 10365 (1481 per dollar). The magnitude of these results may depend on the extensive inspection coverage and large size of the establishments in the sample, so applying these projections to the the entire manufacturing sector is less sound statistically.

V. CONCLUSION

We have found evidence in this study that OSHA enforcement has a significant impact on accidents. Accidents in plants do respond to changes in enforcement and to specific contacts with enforcement agencies, despite the fact that compliance is only indirectly related to accidents (Mendeloff, 1979), that expenditures on compliance may compete with more productive expenditures to improve safety (Bartel and Thomas, 1985), that OSHA resources do not permit extensive monitoring of most workplaces (Smith, 1976), and that OSHA penalties are relatively small compared to compliance costs. The fraction of accidents explained by enforcement is relatively small, as other studies have found. This is not surprising given the marginal role of regulation compared with the other forces affecting accident rates.

Perhaps more importantly, we have demonstrated that the classical deterrence model that dominates most enforcement analysis is relatively weak compared with the behavioral model when it comes to analyzing compliance effects on accident rates. The importance of self-correcting mechanisms, the relatively long lags between enforcement changes and effects, the difference in effects between the expected probability and the expected amount of a penalty, and the independent effect of inspections on accidents are all more consistent with the behavioral model than with the simplified forms of deterrence theory usually used in enforcement and compliance studies. Although the behavioral hypotheses we have developed and tested fall considerably short of a well-developed theory of compliance, they suggest that further research using the behavioral theory of the firm can contribute significantly to understanding compliance behavior and improving enforcement strategies.

A richer model of compliance may help improve the effectiveness and efficiency of enforcement strategies. Just as undue reliance on simple microeconomic models in other policy domains has led to myopic policy advice (Stern, 1985), reliance on deterrence theory alone limits the enforcement debate to a relatively narrow spectrum of the practical concerns facing enforcement officials. For example, our results suggest that, given OSHA's normal level of activities and the response of our sample firms, increasing the number of penalties has about a 50% greater effect in reducing accidents than a comparable percentage increase in the average amount of penalties. The two do not appear to be perfect substitutes, as would be suggested by simple deterrence theory. If these results prove to be correct, OSHA could increase its impact at the margin by shifting resources to doing more inspections, even if that meant having lower penalties for noncompliers.

The efficiency of safety standards in reducing accidents, while important, may be less important than the need to focus the firm's attention on safety problems. We cannot say whether the decreases in accidents found in this study are efficient (from society's point of view), but to the extent that the firm's safety expenditures were suboptimal because of inattention, the decreases in accidents may derive more from focusing the firm's efforts on an effective risk-reduction program than from safety improvements directly related to compliance with regulations. If the behavioral model proves to be as powerful as these preliminary results suggest, then the conventional wisdom on the role of regulation and enforcement in the economy will need to be reevaluated to include this attention-correction function (Scholz, 1984b).

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FOOTNOTES

1. Most analysts note, however, that mandated expenditures may not be the most efficient way for the firm to reduce accidents, and some contend that the mandated expenditures substitute for other investments that could potentially contribute more to decreasing risk (Bartel and Thomas, 1985).

2. Viscusi (1986b) argues that higher penalties for repeated violations may provide an alternative reason for firms to make investments in compliance (and safety) after an inspection that they would not have been willing to make before being inspected.

3. Those cases where it was not clear whether the records were properly matched were hand-checked. Hand-checking used to examine the matches on two state samples indicated that our error rates for false matches and missed matches were below one percent. To ensure that all plants in the final set contained no ambiguous matches, 198 plants were dropped from the original file.

4. Firms with 10 or fewer employees are not targeted for inspections, and are excluded from this comparison.

5. These predicted values are not instrumental variables in the usual sense, since we include the actual enforcement experience of the plant in our measures of specific deterrence. They are intended to measure the firm's expectation of the OSHA enforcement it will face (general enforcement).

6. Using regressions to explain the level (or logarithm) of accidents rather than the percentage change in accidents, frequently inspected plants were always found to have more accidents than less-inspected plants. We interpret this as caused by an omitted variable bias, where the omitted variable is the inherent hazardousness of the plant (associated with both more accidents and more inspections).

7. The results are not sensitive to varying from one to four years the lag lengths on either the autoregressive process or the enforcement variables.

8. When the same regressions are run omitting the DPEN variables, the coefficient on current DPROB goes to zero.

9. Both theories suggest that inspections without penalties are unlikely to have a strong effect on accidents. In earlier analyses we tested a variable measuring all inspections rather than only inspections with penalty, and found it to be much less strongly associated with the accident variables.

10. The first two effects have three components each: policy affects aggregate enforcement, which affects predicted (general) enforcement, which affects accidents. The third effect has two components: policy affects specific enforcement, which affects accidents.

Let us look in detail at the calculation of the first effect. Since the mean value of INDPROB is about .04, a 10% increase in enforcement would raise INDPROB by about .004 (dINDPROB/dX). This affects PPROB by the coefficient on INDPROB in Table 3, which is .79. Finally, this change affects PCHNUM over the next few years, given by summing the coefficients on DPPROB in Table 4 (both current and lagged values). yielding 1.2 - 1.4 - 0.6 = -.8. In total, the first effect is given by the product of these three terms, .004*.79*(-.8) = .0025, or .25% (the value of .22% in the table is based on using more precise values for the calculations).

The other effects in the table are based on the coefficients from Table 3 and Table 4, and a calculation of the effect of a 10% increase in enforcement on INDAMT (since INDAMT is based on log(penalties), we get dINDAMT/dX = .10) and on IPEN (the mean value is about .1, so dIPEN/dX=.01).

FIGURE 1

Alternative Patterns of Delay for the
Effects of Enforcement on Accidents

```
   Panel A                  Panel B                       Panel C

       |                       |                             |
       |                       |                             |
  *****      ***           *****                        *****
      |    **                  |**                           |*       ****
      |***                     |  **                         | **    *
      |                        |    ****                     |   ****

Immediate effect         Delayed permanent             Delayed effect
  with decay                  effect                     with decay
```

Note: Stars represent the number of accidents (measured vertically) over time (measured horizontally). The vertical line "|" indicates when the enforcement action occurred.

TABLE 1

COMPARISON OF SAMPLE WITH NATIONAL MANUFACTURING SECTOR

	Sample 1979	Sample (1979-85 average)	National Manufacturing Sector 1979
Number of establishments	6842	6842	209,851 [a]
Average number employees	523	479	87 [a]
Total number employees	3,575,394	3,277,318	18,291,000 [a]
Average lost workday injury rate	6.97	6.02	5.9 [b]
Average number lost workday injuries	25	19	5 [c]
Total number lost workday injuries	171,333	132,305	1,243,000 [b]
Average number lost workdays	363	303	79 [c]
Total number lost workdays	2,484,704	2,073,126	18,998,000 [b]
Probability of inspection	.27	.26	.023 [d]
Probability of inspection w/ penalty	.13	.10	
Average penalty per inspection	$498	$269	$275 [de]
Total penalties	$1,721,063	$697,654	$14,400,000 [d]

Sources:

a. National Census of Manufacturers, 1977. Includes only firms with more than 10 employees, since smaller firms have been excluded from targeted inspections since 1981.

b. Occupational Injuries and Illnesses in 1979: Summary (BLS, April, 1981)

c. Based on adjustments to estimate number of firms with over 10 employees in 1979, based on change in employment 1977-1979.

d. Centaur Associates (1985). Figures are for 1980, for all industries.

e. Calculation based on all inspections and on initial penalty imposed. Average penalty per inspection with penalty was $1,063 in 1978 (adjusted to 1983 prices), but dropped to $380 in 1983. Adjustments to penalties averaged 33% throughout the period.

TABLE 2

VARIABLES USED IN ANALYSIS

VARIABLE	MEAN	(STD. DEV)	DESCRIPTION
I. Accidents			
LWNUM	19.3	(40.4)	Number of Lost Workday Injuries
PCHNUM	-.051	(.80)	Proportional change in LWNUM: (LWNUM(t) - LWNUM(t-1))/[(LWNUM(t)+LWNUM(t-1))/2]
AVG2NUM	19.4	(37.8)	Average LWNUM in past two years: (LWNUM(t-1) + LWNUM(t-2))/2
LWDAYS	303	(731)	Number of Lost Workdays
PCHDAYS	-.036	(1.0)	Proportional change in LWDAYS (like PCHNUM)
II. Probability of Penalty			
IPEN	.099	(.29)	Inspection with penalty during year (dummy var)
PPROB	.106	(.06)	Predicted probability of inspection (based on Table 3 coefficients)
DPPROB	-.010	(.02)	Change in PPROB = (PPROB(t) - PPROB(t-1))
INDPROB	.038	(.03)	Industry probability of inspection w/ penalty (based on 2-digit SIC industry, national totals)
III. Amount of Penalty			
AMT	.600	(1.8)	Total log(penalties) assessed during year (note that mean AMT is based on all inspections, but PAMT and INDAMT are based only on inspections with penalty)
PAMT	5.962	(.31)	Predicted log(penalties) assessed if inspection w/ penalty occurred (based on Table 3 coefficients)
DPAMT	-.086	(.17)	Change in PAMT = (PAMT(t) - PAMT(t-1))
INDAMT	4.833	(.69)	Industry average log(penalty) assessed if inspection w/ penalty occurred (2-digit SIC national data)
IV. Size of Firm			
HOURS	926	(1912)	Hours worked during year (in thousands)
LOGHOURS	13.0	(1.1)	Log (HOURS)
PCHHOURS	-.019	(.25)	Percentage change in HOURS (like PCHNUM)
EMPS	479	(982)	Average employment during year
LOGEMPS	5.48	(1.1)	Log (EMPS)
PCHEMPS	-.019	(.22)	Percentage change in EMPS (like PCHNUM)

Means and standard deviations calculated for full sample of 48,794 plant-year observations.

TABLE 3

EQUATIONS PREDICTING PROBABILITY AND AMOUNT OF EXPECTED PENALTY, BASED ON ESTIMATION OF IPEN AND AMT
(Probit for IPEN, OLS on non-zero AMT)
(t-statistic in parentheses)

To predict:	Probability (PPROB)	Amount (PAMT)
Dep. Var.:	IPEN	AMT
Intercept	.041	3.803
	(6.2)	(4.9)
AVG2NUM	0.00050	.000001
	(18.4)	(.004)
PCHNUM (t-1)	0.0024	.048
	(1.3)	(1.7)
INDPROB	0.793	2.096
	(14.6)	(3.0)
INDAMT	0.0087	.058
	(2.7)	(1.2)
LOGHOURS	-0.039	.167
	(-16.6)	(1.7)
LOGEMPS	0.044	-.0035
	(15.2)	(-.03)
YEAR80	-0.011	-.173
	(-2.7)	(-3.6)
YEAR81	-0.010	-.385
	(-2.2)	(-6.2)
YEAR82	-0.007	-.535
	(-1.3)	(-6.9)
YEAR83	-0.016	-.436
	(-3.0)	(-6.0)
YEAR84	-0.010	-.509
	(-1.9)	(-7.2)
YEAR85	-0.008	-.424
	(-1.8)	(-7.0)
Num. obs.	47,894	4,735
Mean (dep var)	.0989	6.07
F-test	172.1	52.2
R-square		.115

TABLE 4
ESTIMATED IMPACT OF ENFORCEMENT ON FIRM ACCIDENTS
(Including Autoregressive Error Structure)
(Maximum Likelihood estimates, t-statistic in parenthesis)

	Incidents (PCHNUM)	Lost Workdays PCHDAYS
Intercept	-0.442	-.245
	(-11.3)	(-5.2)
DPPROB (t)	1.208	1.023
	(4.6)	(3.0)
DPPROB (t-1)	-1.357	-.840
	(-5.6)	(-2.6)
DPPROB (t-2)	-0.591	-.401
	(-2.3)	(-1.2)
DPAMT (t)	-0.897	-.446
	(-7.8)	(-3.6)
DPAMT (t-1)	-0.294	-.140
	(-3.8)	(-1.5)
DPAMT (t-2)	-0.381	-.280
	(-4.9)	(-2.9)
IPEN (t)	-0.036	-.001
	(-2.4)	(-.05)
IPEN (t-1)	-0.049	-.058
	(-3.1)	(-2.7)
IPEN (t-2)	-0.043	-.043
	(-2.8)	(-2.1)
IPEN (t-3)	-0.006	-.006
	(-0.4)	(-0.3)
PCHHOURS	0.672	.546
	(16.0)	(10.3)
PCHEMPS	0.516	.467
	(13.1)	(9.0)
Year Dummies:		
YEAR83	0.354	.160
	(9.3)	(3.6)
YEAR84	0.356	.198
	(9.7)	(4.4)
YEAR85	0.574	.331
	(11.2)	(5.4)
Autoregressive Errors:		
A(1)	-0.489	-.548
	(-49.8)	(-63.8)
A(2)	-0.316	-.329
	(-29.5)	(-31.1)
A(3)	-0.127	-.141
	(-9.8)	(-11.4)
Num. Obs	27,368	27,368
Mean (dep var)	-.046	-.026
Total R2	.289	.274
R2 (w/o autoreg)	.123	.063

NOTE: Industry dummy variables (2-digit SIC) were also included.

TABLE 5

IMPACT OF POLICY CHANGES ON ACCIDENT MEASURES

(Effect of a 10% change in enforcement variables on Lost Workday Incidents and on Lost Workdays)

LOST WORKDAY INCIDENTS

Affecting Incidents through:

10% Change in Enforcement:	Expected Probability (PPROB)	Expected Amount (PAMT)	Actual Inspection (IPEN)	TOTAL EFFECT
Inspections w. Penalty (INDPROB)	-.22%	-1.26%	-.13%	-1.61%
Average Penalty (INDAMT)	-.06%	-.87%		-.93%

LOST WORKDAYS

Affecting Lost Workdays through:

10% Change in Enforcement:	Expected Probability (PPROB)	Expected Amount (PAMT)	Actual Inspection (IPEN)	TOTAL EFFECT
Inspections w. Penalty (INDPROB)	-.07%	-.70%	-.11%	-.88%
Average Penalty (INDAMT)	-.02%	-.48%		-.50%