Debt, Profitability, and Investment in Workplace Safety

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Abstract:

We investigate how a firm's financial performance affects workplace safety. We provide empirical estimates of the relationship between a firm's financial condition and its investment in workplace safety using plant-level proxies for safety performance from OSHA records for thirteen large industries for the period 1972-87. Our results suggest that firms with higher operating margins are safer work places as, in general, are firms with more debt in their capital structure. These results are consistent with a number of theoretical models in which financial factors influence operating decisions.

Abstrakt:

V našem článku zkoumáme jak ovlivňuje finanční situace firmy bezpečnost pracovního prostředí. Poskytujeme empirické odhady vztahu mezi finančními ukazateli firmy a jejími investicemi do bezpečnosti pracovního prostředí. Naše odhady jsou založeny na datech na firemní úrovni ze záznamů OSHA, které popisující přibližný vývoj bezpečnosti pro třináct velkých průmyslových odvětví v letech 1972-87. Dosažené výsledky naznačují, že firmy s většími operačními náklady mají bezpečnější pracovní prostředí, stejně jako to lze obecně říci o firmách, které mají výraznější podíl dluhu ve svých kapitálových strukturách. Tyto výsledky odpovídají mnoha teoretickým modelům, ve kterých finanční faktory ovlivňují operační rozhodování.

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I. Introduction

In this paper we investigate the relationship between firms' financial condition and their decisions regarding workplace safety. Workplace injuries and illnesses extract a high cost from American industry: in 1994, there were 3.8 incidents per 100 workers resulting in lost workdays. Regulatory and compliance costs are also significant. Fry and Lee (1989) provide estimates that suggest that firm values decline significantly on announcement of Occupational Safety and Health Administration (OSHA) enforcement actions. Variation across firms in accident rates and compliance is considerable, even within a given industry. It is, therefore, important to understand the factors that affect the firm's decisions regarding investment in workplace safety. In particular, we find that firms' decisions are related to levels of profitability and indebtedness. Understanding this relationship can contribute to the design of efficient regulatory regimes for workplace safety. It may also lead to more efficient use of regulatory resources as well as more efficient design of both public and private insurance schemes. Finally, our results provide additional evidence in support of the literature linking financial factors to real operating decisions.

Recent theoretical literature suggests that operating and financial decisions are not independent. Classical microeconomic models of the firm focus on the maximization of operating income independent of the source of investment funds or the subsequent division of earnings between equity holders and debt holders. Modigliani and Miller's (1958, 1963) proof that the value of the firm is independent of capital structure rests on the notion that the total stream of returns to the firm is independent of capital structure in frictionless markets. In subsequent work, various authors have cited a number of avenues through which financing decisions can affect the return stream (or the market's estimate of the stream). These include taxes, the divergent interests of managers, shareholders, and bondholders, the effect of information asymmetries, and the possibility of costly financial distress.[1] These factors may affect the amount of risk the firm chooses to bear, the level of investment, the level of output, and the firm's input choices.[2] In this paper, we focus on the impact of financial structure and profitability on one particular input choice, the firm's

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[1]See Harris and Raviv (1991) and Ravid (1988) for surveys of the literature.

[2]On risk, see , e.g., Jensen and Meckling (1976), and Golbe (1988). On the level of investment see , e.g., Myers (1977) and Myers and Majluf (1984). On output market effects, see Brander and Lewis (1986). On input choices see Bronars and Deere (1991), Dasgupta and Sengupta (1993), and Kim and Maksimovic (1990a and 1990b).

investment in workplace safety. The empirical results in this study provide evidence that more profitable firms and more highly leveraged firms are safer places in which to work, although the impact of leverage on safety is smaller for more profitable firms.

Prior empirical work examining the effects of firm financial condition on safety has produced mixed results. The most closely related work was motivated by concerns arising from transportation deregulation and focuses on consumer safety, rather than workplace safety. Golbe (1983), using data from the railroad industry in the 1960s, presented evidence suggesting that firms in financial difficulty have weaker safety records. Golbe (1986), Rose (1990), and Talley and Bossert (1990) studied the relationship between firm profits and airline safety. Golbe found little statistical relationship between profits and safety while Rose found accident rates to be negatively related to firm profits (particularly for small airlines). Talley and Bossert found that profitability is related to maintenance expenditures, but that maintenance expenditures are not significantly related to accidents. Feinstein (1989), in a study of nuclear power plants, used a different measure of financial strength based on bond ratings and found no evidence that economic incentives affect safety. Beard (1992) used cash flow measures in a study of motor carrier safety and found a significant relationship between safety and financial status.

The remainder of the paper is organized as follows. Section II discusses the relevant theory; Section III, our data and estimation strategy, and Section IV, our empirical results. Our conclusions comprise Section V.

II. Theoretical Background

In the basic model of workplace safety, the firm chooses its level of safety investment by balancing the marginal costs and benefits of such investments.[3] Accidents are random events that depend on the level of safety investment, worker behavior, and perhaps other factors. The benefits of increased safety include reduced accident costs (including direct costs and worker compensation premiums) and lower wages (assuming workers demand a compensating differential for hazardous workplaces). If safety is regulated, the benefits of increased safety investment also include a decrease in the level of penalties imposed for violating government safety standards. The costs of increased safety include direct costs of equipment replacement or modification as well as costs

[3]See, e.g., Viscusi (1979).

involved in worker training. The effect of safety on productivity is theoretically ambiguous. Increasing safety may involve reduction of the pace of work, lowering productivity and thereby imposing a cost. On the other hand, accidents themselves can cause interruption of production and lost output. Thus whether increased safety raises or lowers productivity depends on the specific conditions in individual workplaces. This simple model omits any potential influences from financial structure. There are, however, a number of avenues through which financial factors can influence operating decisions such as the level and type of safety investment.

Several models suggest that cash flow may affect operating decisions. One plausible route is through capital market imperfections. Myers and Majluf (1984) argue that information asymmetries between managers and outsiders may generate a "pecking order" for capital in which internally generated funds are cheaper than external funds.[4] Some investments may be profitable at the cost of internal funds, but not at the cost of external funds. For firms without financial slack, increasing cash flow may thus increase investments, including safety investments. Safety investments may involve considerable information asymmetries, so that such investment may be particularly sensitive to the availability of internal funds.

Leverage and its attendant risk of bankruptcy may also generate links between financial structure and investment. Firms in financial distress may have an incentive to incur more risk (e.g., Brander and Lewis (1986), Golbe (1988)). Managers whose goal is to maximize the value of equity may not make efficient investments in safety, because the initial costs of safety investments are borne by the equity holders, while if the firm goes bankrupt accident costs will be borne by the bondholders. Higher leverage makes bankruptcy more likely, implying that firms with more debt might be expected to assume greater risks and hence invest less in safety.

On the other hand, higher leverage may create incentives for greater investment in safety. First, debt may serve as a monitoring device. Jensen (1986) argues that the increased threat of bankruptcy which accompanies increased debt forces managers to make more efficient use of the firm's resources. Second, while increased debt increases the likelihood of financial distress, financial distress may not eliminate equity holders' interest in the firm. Only 10% of firms in

[4] Fazzari, Hubbard, and Petersen (1988) provide evidence that such effects are empirically important. See also Calomiris and Hubbard (1990), Hoshi, Kashyap, and Scharfstein (1990), and Cohen (1990).

financial distress proceed to liquidation; in addition, equity holders frequently retain some residual interest in a firm following a formal bankruptcy in the United States (see Wruck (1990)). Since the costs of bankruptcy or reorganization can be significant,[5] managers will have an incentive to avoid crossing the threshold that triggers these events. This suggests that firms with greater leverage (and, therefore, a higher possibility of bankruptcy) will have an incentive to be extra cautious and spend larger amounts on accident avoidance. Thus, the net effect of leverage on safety investment is ambiguous. It is clear, however, that the effects of debt should be stronger for firms near financial distress. We would, therefore, expect that the empirical influence of debt on safety investments will vary with profitability.

The literature also suggests that capital structure can influence the firm's input mix. Kim and Maksimovic (1990a,b) argue that in the presence of conflicts of interest between equity holders and debt holders, the firm's choice of inputs will be affected by the existence of debt. In particular, leveraged firms are likely to show a preference for inputs which can be more easily monitored and collateralized. To the extent that adherence to OSHA safety standards is capital based and, therefore, more easily monitored than other kinds of safety techniques, we would expect the firm to bias its safety technology towards meeting the standards. Note that, in the presence of such an effect, a decrease in violation of standards may represent not an increase in the firm's level of safety, but rather a change (possibly inefficient) in the technology it uses to achieve the desired level of safety (which itself may be affected by agency costs). The signs of these effects are theoretically ambiguous and dependent on the firm's production function.

Similarly, the literature has demonstrated that capital structure may affect the firm's bargaining position with its employees.[6] Several authors have shown that increased leverage is advantageous in bilateral bargaining with workers, because it reduces the surplus available for sharing with input suppliers. Dasgupta and Sengupta (1993) show that debt is negatively correlated with the (*ex ante*) bargaining power of the firm, where bargaining power is defined as the share allocated to the firm in an asymmetric Nash bargaining solution. However, the sign of the relationship between worker payoff and bargaining power, and hence between debt and worker

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[5]See Weiss (1990) and Wruck (1990) for citations to some other recent papers.

[6]See Dasgupta and Sengupta (1993) and Bronars and Deere (1991).

payoff, is ambiguous. Increased firm bargaining power simultaneously decreases debt and the share of total surplus claimed by workers. However, lower leverage may decrease agency costs associated with debt and thus increase the divisible surplus. Consequently, debt and worker payoff may be positively or negatively correlated. In the context of our model, if workplace safety is one form of rent-sharing with labor, workplace safety may increase or decrease with leverage.

In sum, the literature suggests a variety of mechanisms through which financial factors can affect a firm's investment in workplace safety. We have not addressed the efficiency aspects of these effects. If workers are imperfectly informed about the likelihood and costs of workplace risks, and if regulation corrects imperfectly for any such market failures, an all-equity firm will not in general provide the social-welfare maximizing level of workplace safety. Whether the levered firm will provide more or less workplace safety than the unleveraged firm, and hence increase or decrease any inefficiencies, is theoretically ambiguous. Empirical analysis is necessary to ascertain the size and direction of the effects of leverage and profitability on safety.

III. Data and Estimation

A. Sample and Safety Measures

Measurement of safety investment is a particularly difficult problem, since the firm's safety expenditures will often not be so classified. Many, if not most, such costs will be embedded in equipment or training costs or in a slower speed of the assembly line. Thus, it is difficult to measure safety investment directly. As proxies, we use the record of plants' violations of Occupational Safety and Health Administration (OSHA) standards and the penalties assessed for those violations.[7]

Although we began with a data set containing records of all OSHA inspections from 1972 through 1987, the sample analyzed was limited in a number of ways. In order to reduce the data set

[7]Although the use of accidents or the resulting lost workdays as a measure of safety investment is intuitively appealing, there are a number of potential problems. OSHA does report the fraction of workdays lost to occupational accidents. However, in approximately 26 percent of the inspections, data on lost workdays are not reported. In an additional 62 percent of the observations, lost workdays are reported as zero. While some of these are truly zero, others are undoubtedly missing observations.

to a tractable size,[8] we focused on a subset of thirteen industries. These industries were selected according to three criteria: (1) a large number of publicly traded firms; (2) a wide variation in financial performance among those firms; and (3) a high probability of safety problems as indicated by Bureau of Labor Statistics reports of workplace injuries.

We further limited our analysis to safety inspections. OSHA inspections are directed towards either violations of safety standards or violations of health standards. The two types of inspections are independent and conducted by different inspectors. There are a number of reasons to believe that safety violations will differ from health violations in their relationship to the independent variables. First, the standards themselves differ. OSHA safety standards tend to be technological in nature, providing detailed specifications for machinery and procedures, while the health standards are more outcome-oriented, specifying exposure levels for hazardous substances rather than the use of a particular technology. Second, the harms to workers, who the standards are intended to deter, are much more immediate and obvious in the case of safety standards than of health standards. Thus, firms' incentives to meet the standards will differ between the two types.

Finally, because we will use panel data estimators (see discussion below), we limited the analysis sample to plants with more than one inspection.[9] After eliminating observations with missing data, the final sample contained 5,492 inspections in 793 plants. Table 1 shows the number of firms and plants as well as the mean number of inspections per plant and violations found per inspection by industry.

OSHA categorizes violations as being either "serious" or "not serious." Serious violations differ from other violations in the degree of threat to worker health and safety posed, with serious violations posing a significant and immediate threat. Violations are assigned to a category according

[8]In order to match the OSHA data to the financial data discussed below it was necessary to track the ownership chains of plants by hand.

[9]Although our financial information is at the firm level, there may be inherent differences across plants within a given firm due to age, technology in use, union/management relations or other factors.

to the standard violated and inspectors are supposed to have no discretion over the category into which a violation falls. In addition, a small number of "willful and repeated" violations are categorized as serious no matter what standard is being violated. Serious violations are more likely to be related to true safety hazards. The reported number of serious violations also seems less likely to be related to inspectors' discretion. Thus, we focus on models of serious violations, although we also examine the penalties imposed for violations. Many inspections turned up no cited violations. As can be seen in Table 2, which shows the frequency distribution of each type of violation, almost 44% of inspections in the sample we are analyzing found no violations at all, while over 70% discovered no serious violations. Serious violations and all violations are highly related, with a correlation coefficient of .65 across all inspections in our sample.

Violations of OSHA standards indicate hazards that can be reduced by capital investments in equipment or changing production speed and technology, actions which we have argued above are likely to be affected by a firm's financial condition. Although previous research investigating the link between OSHA and workplace health and safety has produced mixed results,[10] it is important to realize that criticism of OSHA's effectiveness does not necessarily imply that the data are inappropriate for our research. Explanations for OSHA's small impact on injury and illness rates fall into two categories. The first is that "the low probability of inspection and small penalties imposed on violators [provide] little incentive for hazard abatement" (Gray, 1990). Thus, the assertion is that OSHA citations do not provide sufficient incentives to alter behavior, not that compliance with OSHA standards would not reduce accidents. The second is that many common types of workplace accidents are unlikely to be affected by compliance with OSHA standards (Mendeloff (1979)). Nevertheless, investments that would bring a firm into compliance with OSHA safety standards should create a reduction in the expected cost of some types of accidents and, therefore, in the total expected costs due to accidents.

[10]Several studies have suggested that OSHA had little, if any, impact on accident rates (see Viscusi (1979), Mendeloff (1979), Smith (1979), McCaffrey, (1983), Bartel and Thomas (1985), and Rusher and Smith (1991)). Other work (Viscusi (1986), Scholz and Gray (1990) and Gray and Jones (1991a,b)) has found a small but significant effect from OSHA inspections in increasing workplace safety.

B. Independent Variables

Previous studies of the impact of the firm's financial condition on safety have used a variety of measures of financial condition. Golbe (1983) and (1986) considered net income and rate of return measures of profitability. Rose (1990) used operating margin[11] as a measure of profitability as well as the interest coverage ratio (as a measure of leverage), and working capital and current ratios to measure liquidity. Feinstein (1989) constructed a variable based on Moody's bond ratings as a measure of financial condition. Talley and Bossert (1990) used operating ratio (operating cost as a fraction of operating revenue) as a measure of profitability. Beard (1992) used a measure of capitalized cash flow as a measure of financial condition.

As proxies for the financial and profitability variables specified in the theories described above we use the firm's operating margin (operating income divided by sales)[12] and debt ratio (defined as long-term debt divided by the sum of long-term debt, preferred stock and common stock, all valued at market prices).[13] We interact the firm's debt ratio with a set of three dummy variables, arbitrarily chosen to divide the sample into three groups: those with negative operating margins, those with operating margin below 10%, and those with operating margin above 10%.[14] This division was designed to test the hypothesis discussed above that the effect of leverage is greater for firms near financial distress. Our source for financial data is the R&D Master file assembled from COMPUSTAT and other sources by the NBER (Cummins, et al 1988).[15] This data source presents a potential problem in that only large, publicly traded firms are covered, thus limiting our sample through missing data as discussed above. A priori, firms in COMPUSTAT data are more likely to

[11] Defined as 1 - (operating expenses)/(operating revenues). This is essentially a measure of operating income scaled by revenues.

[12] While cash flow may be a better match for the theoretical constructs discussed above, many of the firms in our sample did not report depreciation, making it impossible for us to calculate this variable.

[13] These as well as other variable definitions are summarized in the Data Appendix.

[14] Operating margin at the sample median was 10.3%. Replication of results using other divisions produces the same general patterns discussed below.

[15] The name of the company that owns each plant found in the OSHA data was matched by hand to the COMPUSTAT data. If the company name was not found in the COMPUSTAT data, directories of corporate ownership were used to determine if the owner had a listed parent company.

be financially stable than are small, privately-held firms. However, analysis of the data suggests that there is sufficient variation in financial condition among the firms in this sample to enable estimation of the impact of financial conditions on safety investment even though results should be extrapolated to smaller firms only with considerable caution. Over three percent of inspections in our sample were for plants whose parent company had negative operating margins. For the full sample, the mean operating margin was 10% with a standard deviation of 5% while the mean debt ratio was 39% with a standard deviation of 22%.

In addition to the financial variables of interest, we control for a number of other factors that may influence safety investments. Weil (1991) discusses a number of avenues through which unionization may affect safety investment and compliance with OSHA regulations. Unions often maintain their own health and safety programs. They may also facilitate the exercise of employee rights to participate in OSHA enforcement. Unfortunately, we do not have a measure of the extent of union coverage at the plant level where inspections take place. Instead, we are forced to rely on overall unionization coverage for the parent company across all its operations. We use union coverage rates supplied by Bronars, Deere, and Tracy (1994). Unionization rates are calculated from averaged values of union employment (estimated by rescaling Bureau of Labor Statistics data on major collective bargaining agreements) and total employment (from COMPUSTAT), where data are averaged over the periods 1971-74, 1975-78, and 1979-82.[16] These data are then merged with our panel, so that for each firm, the union variable will have the same value for 1972-74, another constant value for 1975-78, and a third for 1979-87. To the extent that union coverage measured at the firm level and averaged across years measures plant-level coverage with error, the result will be classical measurement error and our coefficients should be biased towards zero.

We also control for why OSHA undertook each inspection in our sample. The basic inspection is known as a "general inspection." While any plant may be subjected to such an inspection, they are targeted toward larger, more hazardous plants.[17] OSHA identifies the riskiest industries in each state based on state or national injury rates and inspects plants in those industries

[16]See Bronars, Deere and Tracy (1994) for details.

[17]In addition, inspections are apparently targeted toward larger firms. Unionized plants have a higher probability of being inspected and are scrutinized more intensively (see Weil (1991)). This suggests the possibility of selection bias in our sample.

starting with the most dangerous (see Gray, 1990). In addition to general inspections, a plant may be inspected in response to an accident or a complaint (typically from a worker).[18] Among the inspections we analyzed, 26.4% were general programmed inspections, 59.4% were in response to complaints, and 14.2% followed accidents. General inspections typically discover significantly more total violations than more limited types of inspections. The mean number of violations for general, accident and complaint inspections are 7.6, 3.9 and 2.0, respectively. In contrast, all types of inspections turn up an average of between one and one-and-a-half serious violations.

It is likely that the number of violations reported (and penalties assessed) will be related to how long it has been since the previous inspection. We cannot use the actual time elapsed since the previous inspection, since that variable is undefined for the first inspection. We define a variable which takes on values from zero to one to proxy the time elapsed since the previous inspection. The larger this variable, the more time has elapsed since a previous inspection and the more a given inspection is like a first inspection. For the first general inspection of a plant this variable is equal to one by definition since our data begins with the inception of OSHA inspections. For accident and complaint inspections prior to a general inspection it is also set to one. For all other inspections, we define the time-since-last-inspection variable as:

$$t = \frac{2e^{t'}}{1 + e^{t'}} - 1$$

where t' is the time (in years) since the most recent general inspection. Thus, $0 \leq t \leq 1$, where t increases as t' increases.

Differences in plant size may also affect safety violations and (per employee) accident arrival rates. Larger plants should have more opportunities to violate standards. Plant size may also be related to a plant's technological design and, therefore, its underlying safety independent of any investment decisions by the firm. Thus, we include plant size, measured by number of employees,

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[18]If significant violations are found during an inspection OSHA will revisit the plant at a later date to be sure that these have been corrected. We have excluded these "follow-up" inspections from our analysis since they focus only on areas where previous violations have been found and it should be expected that few violations will be found during the reinspection, given that firms know the nature of previous violations and that a reinspection will occur. Thus, they are a poor indicator of how a firm's decisions are influenced by its financial performance.

in our estimating equations. We allow for non-linearity in this relationship by also including the square of employment.[19]

Twenty-one states have taken advantage of provisions under OSHA legislation to substitute state-level inspection programs for federal OSHA inspections. Since inspections in these states may differ in their rigor from federal inspections we include a dummy variable for the state-program states.[20] At the federal level, frequencies and intensities of OSHA regulation enforcement may have varied across Presidential administrations. In the years immediately following adoption of the act, inspections were typically brief and trivial violations were often cited. During the Carter administration, penalties for violations increased, while citations for less important violations decreased. Under Reagan, the level of penalties assessed fell dramatically, and targeted inspections and serious violations increased (Gray (1990)).

The issue of whether to control for industry is complicated. Industries may vary by their "natural compliance" with OSHA regulations (see Bartel and Thomas (1985)). Firms in "naturally compliant" industries will, all other things the same, show fewer violations. Industry dummies may also pick up the effects of other omitted variables. On the other hand, if financial performance is correlated across firms in the same industry, industry dummies could incorporate effects of financial condition on OSHA violations as well as differences in the responsiveness of different industries to variations in these conditions. Thus, results containing and omitting industry dummies are reported below for comparison purposes.

IV. Results

The distribution of number of violations reported in Table 2 suggests that OLS regression is not the most appropriate method for estimating our model. Only integer values are possible and the data are dominated by zeros and small values, so that an appeal to approximate continuity is

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[19]Of course, as Brown and Medoff (1990) show, firm size is also likely to incorporate a number of other effects and its meaning in empirical work is not well understood.

[20]These states are Vermont, Indiana, Michigan, Minnesota, Iowa, Maryland, Virginia, North Carolina, South Carolina, Kentucky, Tennessee, Arkansas, New Mexico, Arizona, Nevada, Utah, Wyoming, Washington, California, Oregon and Hawaii.

inappropriate. When confronted with data of this type, a natural alternative is to use a Poisson regression model, where the probability of each possible outcome (y_t) is given by:

$$pr(y_t) = \frac{\lambda_t^{y_t} \, e^{-\lambda_t}}{y_t \, !}$$

with $\lambda_t = \exp(x_t\beta)$. Such a model can only take integer values, while the exponentiation ensures that these values will be positive.[21]

The simple version of the Poisson model suffers from a serious drawback by assuming equidispersion in the data, implying that the conditional mean and variances in the model are equal (i.e., $E(y_t \mid X_t) = \text{var}\ (y_t \mid X_t) = \exp\ (X_t\beta)$). While the estimated coefficients from this model will be consistent, if the data are actually overdispersed estimated standard errors will be biased downwards and significance levels will be overstated. Similarly, underdispersed data will lead to upwardly biased standard errors.

The most common alternative in such a situation is the Negative Binomial model, which extends the Poisson regression by assuming that y_t, conditional on λ_t, follows a Poisson distribution while λ_t itself follows a gamma distribution. Commonly, the form of this gamma distribution is chosen so that the conditional mean of y_t remains the same as the Poisson model ($\exp\ (X_t\beta)$) but the variance contains the additional parameter α such that $\text{Var}[y_t] = E[y_t]\{1 + \alpha E[y_t]\}$ to provide for overdispersed data (see Cameron and Trivedi (1986)).

We take advantage of the fact that most plants were inspected several times over the period observed to estimate a Negative Binomial model with random plant effects.[22] Such a model extends the simple Poisson model by adding an individual (here plant) effect such that $\lambda_{it} = \exp(x_{it}\beta + u_i)$. If u_i is assumed to follow a gamma distribution with parameters (θ_i, θ_i), the result is a Negative Binomial model with a parameter that varies across plants (indexed by *i*). $\theta_i/(1+\theta_i)$ is assumed

[21] See Gourieroux, Monfort and Trognon (1984), Hausman, Hall and Griliches (1984), and Cameron and Trivedi (1986). See Gray and Jones (1991a) for an application to OSHA violations.

[22] See Hausman, Hall and Griliches (1984). Wooldridge (1990) discusses the conditions under which this quasi-conditional maximum likelihood estimator is consistent and asymptotically normal.

distributed as a beta (a,b) random variable.[23] Note that there is randomness across firms and across time, because λ_{it} is a realization from a probability distribution. Note also that this procedure estimates the parameters of the beta distribution but that θ is not identified, so we cannot compute expected values for these models.

. Table 3 contains estimates of both a conventional cross-section negative binomial and a random effects panel-data model. For each model, estimates without and with industry dummies are reported. The data indicate considerable overdispersion; estimates of α, the overdispersion parameters in the cross-section estimates, are significantly different from zero and imply an overdispersion rate of about 7.8. The random effects panel data models imply a mean overdispersion rate of about 3.7 (without industry effects) or 4.5 (with industry effects).[24] Standard likelihood ratio tests indicate that the industry dummies add significant explanatory power for both models.[25] It is clear that financial results, especially with respect to operating margins, are correlated across firms in an industry. Inclusion of industry dummies reduces the magnitude of the coefficients on operating margin by about 50%. Thus, models including industry dummies may understate the effect of financial variables on safety violations. On the other hand, results omitting them may erroneously attribute industry differences in intrinsic safety (or intensity of OSHA inspection) to financial effects. In any case, only the quantitative magnitude and not the general pattern of results depends on whether we control for industry.

The imposition of random plant effects has a number of significant effects on the estimated coefficients. Most important, from our point of view, is that the sign of the leverage coefficient is sensitive to the inclusion or exclusion of these effects. Without plant effects, the debt ratio has a positive effect on violations which increases in magnitude and significance as operating margin increases. When we control for random plant effects, leverage has a negative effect on violations

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[23]A beta r.v. has the density function $f(x) = [B(a,b)]^{-1}x^{a-1}(1-x)^{b-1}$ for $0< x< 1$, where the beta function $B(a,b) = \Gamma(a)\Gamma(b)/\Gamma(a+b)$. Its expected value is $a/(a+b)$.

[24]$a/(a+b)= E[1/(1+\alpha_i)]$, so $\text{plim}(b/a) = E(\alpha)$, and the model without industry effects implies a variance to mean ratio of 3.7 and the model with industry effects implies a ratio of 4.5.

[25]For the cross section estimates, -2Δ log likelihood = $-2(-6448--6415.6) = 64.86$. For the panel data estimates, -2Δ log likelihood = $-2(-6374.1--6308.9) = 130.3$ Both statistics are distributed as chi-square with 12 degrees of freedom; the critical value at the 1% level (2-tailed test) is 28.3.

that becomes smaller (in absolute value) as operating margin increases. In addition, controlling for random plant effects switches the coefficient on accident inspections from negative to positive and eliminates the apparent effect of plant size. We test the alternative specifications using a likelihood ratio test. The negative binomial model without random effects is nested within the random effects model. Thus, 2Δ log likelihood is distributed as chi-square with one degree of freedom. Whether or not industry dummies are included, the test suggests that the models omitting the random plant effects can be rejected.[26] Thus, in interpreting our results, we focus on the models with plant effects.

It is clear that firms with higher operating margins are cited for fewer serious violations. This may be because these safety investments are profit maximizing (possibly as a result of relaxing the liquidity constraint in a "pecking order" model) or because firms with higher operating margins are generating greater economic rents that must be shared with workers who value safety. The latter interpretation is consistent with our results for unionization. We find that firms where a greater fraction of the workers are unionized also have significantly fewer serious OSHA violations.[27] This finding is especially strong since we have not been able to control for such features as the average age of the capital stock; we would expect to find that unionized workers are concentrated in older plants that should *ceteris paribus* have more safety violations due to the use of older technologies and machinery.

In the cross-section estimates, greater leverage is associated with more citations for firms with positive operating margin. Once we take into account plant-specific effects with panel data estimators, there is a strongly significant negative relationship between debt ratios and serious violations for firms with low operating margins. This relationship becomes much less pronounced (and may even turn positive) at high levels of operating margin. This suggests that the unmeasured characteristics of plants which make them prone to more violations are positively correlated with the leverage ratio of the owning firm. The variation in the effect of debt ratios across different levels of profitability is attenuated if industry dummies are included in the estimates. Thus, there are

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[26]For the models omitting dummies, -2Δ log likelihood = $-2(-6448 - -6374.1) = 147.8$. When industry dummies are included, -2Δ log likelihood = $-2(-6415.6 - -6308.9) = 213.4$. Both statistics are distributed as chi-square with one degree of freedom; the critical value at the 1% level (2-tailed test) is 7.88.

[27]Weil (1991) estimates a simple OLS model and finds that unionization is positively correlated with both violations and penalty per violation.

apparently strong intra-industry commonalities in financial performance.[28]

This negative relationship between leverage and OSHA violations provides evidence relevant to several of the models described in Section 1. It suggests that if increasing leverage increases the incentive for managers to take more risks, the effect of that incentive change is outweighed by other factors such as the increased monitoring by debt holders or the desire to avoid bankruptcy or reorganization costs that become more likely at higher leverage ratios. The increase in the importance of debt ratios as operating margin falls adds weight to this conclusion. Second, the results are consistent with Kim and Maksimovic's argument that increased leverage biases firms toward the use of inputs which are more easily monitored. As we noted above, the increase in compliance with OSHA standards may represent not an increase in workplace safety, but a change in the technology chosen to achieve a desired level. Finally, we note the hypothesis that leverage may affect the firm's bargaining power vis-à-vis its employees, thus generating a correlation between debt and worker payoffs.

Control variables generally behave in sensible ways. Plant size is positively related to serious violations when we ignore plant effects. In models with random plant effects, however, plant size is unrelated to serious violations. This suggests that, at least in part, the effects of plant size (which may be truly size-related or may be due to other factors correlated with size such as technological vintage) may be subsumed in the random plant effects. The coefficient of the elapsed-time variable in the equation for serious violations is significantly different from zero only when plant effects are included, but industry effects are excluded; in that case the coefficient is negative, so that fewer violations are found the longer the elapsed time since the previous inspection. This set of results could arise if inspections are targeted toward more hazardous industries. The time between inspections will vary negatively with hazardousness and could be expected, therefore, to be negatively correlated with accidents. When industry dummies are included, they apparently account for this effect. There is a clear pattern over time (or across administrations) in the number of violations cited. During the Carter Administration there were more serious violations than at the

[28]Even when industry dummies are included, if operating margin is stratified more finely there are insignificant coefficients on debt ratios at the upper end of the distribution of operating margins. Since theory does not enable us to determine exactly where this relationship between leverage and safety should disappear, we have elected to report results for a limited number of categories.

beginning of our sample period. During the Reagan years, the number of serious violations cited was lower than during the previous administration but was still greater than in the pre-Carter years. We cannot tell from the data available to us whether this pattern resulted from increasing compliance or from a deliberate change in the criteria used to determine whether to issue a citation.

The cross-section estimates suggest that states that conduct their own inspections find fewer violations , but this effect disappears when plant-level effects are included, suggesting that it is an artifact of the types of plants found in particular states. We further investigate the effect of the state opt-out option by analyzing violations separately for state and federal inspection states. Table 4 reports the results of the panel estimates from this analysis.[29] Overall, the results are similar between the two inspection regimes. Operating margin appears to have a greater impact on the number of violations found and unionization a smaller one when the states control the inspection process. This last result may occur, in part, because these states are a minority of the sample and are less unionized than the country as a whole, suggesting both that unions may have less power and that the measurement error created by attributing national firm-level unionization rates to individual plants in such states may be greater. While the magnitude of the debt ratio coefficients is not much different in state-inspection states, the significance is lower, due to smaller sample sizes.

Accident inspections obviously occur only after an accident and focus on specific areas of a plant where there has been an accident. Not surprisingly, they find more serious violations once plant-specific effects are taken into account. Inspections in response to complaints find fewer violations than other types of inspections across the board. This last finding at first appears somewhat puzzling since one might expect that a worker complaint would be indicative of a violation. To investigate this result we repeated our estimates excluding inspections generated by complaints. Results are shown in Table 5; they parallel those for the full set of inspections with two important differences. In every specification, the negative effect of unionization is substantially diminished (usually to less than half its previous size), becoming generally insignificant. This suggests that unions may use OSHA complaints as a way of harassing employers independent of the actual presence of violations. We also see a difference with respect to the debt ratio variables.

[29] In order to obtain convergence in the state-inspection state results for serious violations, it was necessary to combine the Pharmaceuticals, Soaps and Cleaners, Paints and Varnishes, Aircraft, and Oil industries with the reference group (Steel).

Operating margin is still significantly negatively related to violations whether industry and plant effects are included or excluded. However, while debt ratio is significantly related to violations only when we allow for random plant effects, the relationship has the same pattern across all specifications: a negative relationship between leverage and violations which is attenuated as operating margin increases.

Although we do not report the coefficients for the industry dummies, the regression results show that (after correcting for other variables) citations were highest in the Steel industry, followed by Construction Machinery, Grinding, Paper, and Automobiles. The fewest violations were found in Pharmaceuticals, Paints and Varnishes, Petroleum, and Soaps and Cleaners. Since we have controlled for financial condition in these estimates, these results suggest that not all of the industry effects are financial in nature and that a case can be made for including the industry dummies. Financial effects are found even when we limit our analysis to a single industry at a time. Coefficients on the financial variables are reported in Table 6.[30] In every industry in our sample for which estimates could be obtained, higher operating margins are associated with fewer violations, suggesting that the result is not an artifact of inter-industry differences. Results with respect to debt are less clear. Because of smaller sample sizes, we were not able to get estimates with debt ratio classified by operating margin for all industries. Coefficients on debt are generally negative, but significant only in Construction Machinery and Autos; only the Auto industry shows an attenuation in the effect of debt as operating margin rises.

Table 7 contains an analysis of the determinants of total penalties assessed as a result of inspections.[31] Given that total penalties are bounded from below at zero, both cross-section and random effects panel data Tobit models are presented.[32] Results generally parallel those for violations. Higher operating margins are associated with lower penalties. Debt ratios are positively

[30] Estimates for some industries did not converge.

[31] This variable is reported in units of $10,000.

[32] Convergence was difficult to obtain. In order to do so, the top 2% of observations (with penalties greater than $10,000) were recoded as $10,000 and a two-limit Tobit model (with the upper bound at 1 = $10,000) was estimated. As might be expected, the magnitude of these coefficients is more sensitive to changes in equation specification than those in the previous table although we have seen no modifications (such as omitting a variable) that alters the main thrust of the results.

related to violations when plant effects are ignored, but once plant-specific effects are taken into account, the results are difficult to interpret, since the signs and significance vary depending on the inclusion of industry dummies. Plants operated by more unionized firms have lower penalties as do plants inspected as a result of a worker's complaint. Once plant-specific effects are taken into account, when a given plant has more employees it tends to have lower levels of penalties as do plants that have been inspected longer ago. Again, if OSHA targets inspections toward more hazardous plants, then time since last inspection will be negatively correlated with hazardousness and therefore with penalties. The plant size effects are more puzzling since one might expect that penalties would be higher where more employees are at risk. The plant size results may be, at least in part, a reflection of vintage effects or financial condition.

The Tobit model makes the implicit assumption that the coefficients in an equation estimating the amount of penalties conditional on the fact that a firm received any penalty are identical to those determining the probability of receiving any penalty at all (see Cragg (1971) and Fin and Schmidt (1984)). This assumption may be questioned given that almost sixty percent of the inspections in our sample resulted in no penalty. The appropriate alternative in this case is to estimate a probit model for the probability of any penalty and a truncated regression model for those observations that received penalties. Table 8 reports the results of probit estimates of the probability of an inspection resulting in any penalties. The truncated regression for amount of penalties, conditional on there being positive penalties, did not converge and Ordinary Least Squares are inconsistent in such a situation, so no such results are presented. The determinants of the probability of being penalized appear to differ from those for the number of violations and the amount of penalties in several significant ways. Once industry is taken into account, there is no relationship between the probability that a penalty is imposed and a firm's operating margin even though more profitable firms have fewer violations and lower levels of penalties. Firms with higher debt ratios are *more* likely to be penalized even though they have fewer serious violations. Although this effect is smaller at higher levels of profitability it is always statistically significant. This result raises issues of whether OSHA's propensity to impose penalties reflects actual safety hazards or a perception by the agency that certain types of firms (i.e., more highly leveraged) should be penalized independent of their safety performance.

V. Conclusions

The results provide strong evidence that financial performance affects firms' workplace safety decisions, at least to the extent that these decisions are reflected in compliance with OSHA standards. Operating margin is consistently negatively related to violations and penalties: firms with higher operating margins violate fewer OSHA standards and pay lower penalties. While the statistical relationship with respect to leverage is somewhat less strong, the pattern appears clear: more highly leveraged firms violate fewer OSHA standards, though the effect is attenuated at higher levels of operating margin. These findings, that financial factors influence the real decisions of firms suggests that financial performance, should be considered in setting regulatory policy and allocating enforcement resources, as well as in determining insurance premiums.

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DATA APPENDIX

Variable Descriptions and Sources

Safety measures are taken from OSHA inspection records. Financial data are derived from the NBER R&D Master File, which is in turn derived from COMPUSTAT. Firm-level union coverage rates are from Bronars, Deere, and Tracy (1994).

Serious Violations		Violations of particular standards which pose an immediate health or safety threat. Also included in this category are a small number of "willful and repeated" violations which would not otherwise be classified as serious.
Penalties		Total penalties assessed in $10,000s, deflated with the GNP deflator.
Operating Margin		(Operating Income before Depreciation)/Sales
Debt Ratio		Long Term Debt/ (long-term debt + preferred stock + common stock) (all valued at market prices)
Inspection Type (General omitted)	Complaint	1 if complaint inspection; 0 otherwise
	Follow-Up	1 if follow-up inspection; 0 otherwise
	Accident	1 if accident inspection; otherwise
State Inspection		1 for state inspection states; 0 otherwise
Presidential dummies (pre-Carter omitted)	Carter	1 for years 1977-80; 0 otherwise
	Reagan	1 for years 1981-87; 0 otherwise
Total Employees		total employees in inspected plant, in 1000s, from OSHA records.

Time since first inspection	1 for first inspections, otherwise $= \frac{2e^{t'}}{1 + e^{t'}} - 1$ where t′ = time (in years) since previous general inspection
Unionization	Bureau of Labor Statistics (BLS) data on major collective bargaining agreements are used to estimate unionized employment by year for each firm. Total employment for each firm by year is obtained from COMPUSTAT. Unionization rates were calculated from averaged values of union and total employment, where data were averaged over the periods 1971-74, 1975-78, and 1979-82. See Bronars, *et al* for details.

TABLE 1

SAMPLE CHARACTERISTICS

Industry	# of Firms	# of Plants	Mean # of Inspections per Plant	Mean # of Total Violations / Inspection	Mean # of Serious Violations / Inspection
Pharmaceuticals	9	15	3.4	1.98	0.59
Soaps and Cleaners	15	32	3.3	4.52	0.73
Paints and Varnishes	9	26	3.2	2.82	0.73
Grinding	25	65	3.5	5.3	1.17
Metal Forging	38	81	4.4	3.55	0.72
Farm Machinery	17	53	4.7	6.54	0.67
Construction Machinery	33	115	4.9	5.56	1.26
Automobiles	34	171	5.7	4.81	1.06
Aircraft	26	51	6.3	4.84	0.86
Shipbuilding	18	53	12.2	3.69	0.92
Petroleum	15	34	7.3	3.01	0.79
Paper	13	59	6.9	7.8	1.98
Steel (inc. Minimills)	17	38	33.3	3.6	1.47

TABLE 2

DISTRIBUTION OF NUMBER OF VIOLATIONS

	Total Violations	Serious Violations
0	44%	70%
1	16%	14%
2	8%	5%
3	5%	3%
4	4%	2%
5	3%	1%
6	3%	1%
7	2%	0.5%
8	2%	0.4%
9	1%	0.4%
10	1%	0.3%
11 or more	11%	2

TABLE 3

DETERMINANTS OF SERIOUS OSHA VIOLATIONS

Negative Binomial Models

	No Plant Effects		Random Plant Effects		Variable Mean (Std. Dev.)
Operating Margin	-2.91 (5.01)	-1.62 (1.71)	-7.36 (15.27)	-3.61 (5.06)	0.102 (0.055)
Debt Ratio (Operating Margin ≤ 0)	0.16 (0.37)	0.33 (0.75)	-1.26 (5.05)	-0.67 (2.39)	0.618[1] (0.142)
Debt Ratio ($0<$Operating Margin≤ 1)	0.35 (2.50)	0.33 (2.03)	-0.57 (4.64)	-0.50 (3.98)	0.498[1] (0.218)
Debt Ratio (Operating Margin $>.1$)	0.86 (4.49)	0.54 (2.68)	0.03 (0.18)	-0.30 (1.49)	0.287[1] (0.156)
Unionization	-0.57 (3.76)	-0.65 (3.73)	-0.86 (6.35)	-1.11 (8.13)	0.432 (0.214)
Plant Employment (10,000's)	3.52 (2.84)	3.01 (2.12)	-0.44 (0.47)	-1.09 (1.14)	0.034 (0.056)
Plant Employment Squared	-4.45 (0.98)	-3.64 (0.69)	0.73 (0.23)	1.72 (0.54)	0.005 (0.023)
Time Since Last General Inspection	-0.05 (0.70)	0.13 (1.42)	-0.35 (4.72)	-0.13 (1.56)	0.806 (0.306)
Accident Inspection	-0.51 (3.90)	-0.46 (3.61)	0.20 (2.59)	0.16 (2.00)	0.142
Complaint Inspection	-0.62 (10.37)	-0.64 (8.95)	-0.50 (8.50)	-0.48 (7.71)	0.594
State Inspection State	-0.32 (4.08)	-0.28 (3.19)	-0.10 (1.45)	-0.02 (0.33)	0.234
Carter Administration	1.46 (22.18)	1.49 (19.47)	0.94 (17.95)	1.03 (19.08)	0.325
Reagan Administration	0.69 (10.22)	0.69 (8.46)	0.23 (3.67)	0.32 (5.00)	0.313
Industry Dummies	No	Yes	No	Yes	
α	5.11 (31.8)	4.94 (30.9)			
a			2.2 (8.9)	2.57 (7.6)	
b			5.2 (6.7)	7.89 (5.8)	
Log Likelihood	-6448.0	-6415.6	-6374.1	-6308.9	
N	5492	5492	5492	5492	

[1]Conditional on Operating Margin falling in specified range.

TABLE 4

DETERMINANTS OF SERIOUS OSHA VIOLATIONS

by State Inspection Type

Panel Data Estimates

	Federal Inspection States		State Inspection States	
Operating Margin	-6.89 (12.93)	-2.94 (3.60)	-9.70 (6.31)	-8.15 (4.90)
Debt Ratio (Operating Margin ≤0)	-1.36 (4.96)	-0.79 (2.42)	-1.38 (1.77)	-0.93 (1.15)
Debt Ratio (0<Operating Margin≤.1)	-0.62 (4.65)	-0.62 (4.14)	-0.39 (0.89)	-0.09 (0.19)
Debt Ratio (Operating Margin >.1)	-0.07 (0.33)	-0.52 (2.14)	0.44 (0.86)	0.72 (1.27)
Unionization	-0.84 (5.65)	-1.17 (7.66)	-0.78 (1.90)	-0.71 (1.57)
Plant Employment (10,000's)	-0.58 (0.43)	-0.97 (0.74)	-0.08 (0.03)	-0.79 (0.19)
Plant Employment Squared	-0.90 (0.14)	1.11 (0.24)	0.28 (0.03)	1.21 (0.08)
Time Since Last General Inspection	-0.43 (4.79)	-0.14 (1.42)	-0.12 (0.68)	-0.14 (0.72)
Accident Inspection	0.17 (2.08)	0.13 (1.49)	0.27 (1.28)	0.25 (1.13)
Complaint Inspection	-0.54 (8.09)	-0.51 (7.11)	-0.36 (2.34)	-0.38 (2.47)
Carter Administration	0.96 (15.58)	1.04 (16.49)	0.91 (7.55)	0.98 (7.60)

Reagan Administration	0.26 (3.58)	0.38 (4.92)	0.15 (1.19)	0.16 (1.23)
Industry Dummies	No	Yes	No	Yes [2]
a	2.15 (7.9)	2.58 (2.4)	2.54 (3.5)	2.64 (3.1)
b	5.6 (5.9)	9.0 (4.9)	4.16 (2.7)	4.51 (2.3)
Log Likelihood	-5031	-4971	-1331.9	-1326.3
N	4208	4208	1284	1284

[1]Conditional on Operating Margin falling in specified range.

[2]Industries with less than two percent (each) of state inspections (Pharmaceuticals, Soaps and cleaners, Paint and varnishes, Aircraft, and Oil) combined with reference group (Steel and minimills), because estimates did not converge with small groups treated separately. These industries together comprise seven percent of the observations.

TABLE 5

DETERMINANTS OF SERIOUS OSHA VIOLATIONS

Complaint Inspections Omitted

	No Plant Effects		Random Plant Effects	
Operating Margin	-3.90 (4.7)	-3.62 (2.5)	-7.38 (8.4)	-3.85 (3.4)
Debt Ratio (Operating Margin ≤0)	-1.02 (1.3)	-0.91 (1.1)	-1.51 (3.7)	-0.89 (2.0)
Debt Ratio (0<Operating Margin≤1)	-0.29 (1.3)	-0.28 (1.0)	-0.74 (3.2)	-0.59 (2.3)
Debt Ratio (Operating Margin >.1)	0.29 (1.0)	0.02 (0.0)	-0.30 (1.0)	-0.59 (1.9)
Unionization	-0.20 (0.9)	-0.45 (1.7)	-0.38 (1.4)	-0.53 (1.8)
Plant Employment (10,000's)	11.23 (3.6)	11.08 (3.5)	-0.62 (0.3)	-2.25 (0.9)
Plant Employment Squared	-23.26 (2.4)	-21.4 (2.0)	10.06 (1.3)	17.11 (1.9)
Time Since Last General Inspection	0.08 (0.9)	0.04 (0.3)	-0.06 (0.5)	0.08 (0.6)
Accident Inspection	-0.59 (5.2)	-0.49 (4.1)	0.29 (1.0)	0.20 (2.3)
State Inspection State	-0.41 (4.0)	-0.36 (2.8)	-0.12 (1.1)	-0.02 (0.1)
Carter Administration	1.30 (11.1)	1.4 (9.0)	0.85 (9.4)	0.95 (10.3)
Reagan Administration	0.70 (8.3)	0.72 (6.0)	0.05 (0.6)	0.12 (1.3)
Industry Dummies	No	Yes	No	Yes
α	3.65 (20.2)	3.4 (18.7)		
a			2.2 (7.3)	2.3 (6.6)
b			3.5 (4.7)	4.2 (4.3)
Log likelihood	-2730.8	-2704.3	-2673.8	-2647.3
N	2062	2062	2062	2062

TABLE 6

EFFECT OF OPERATING MARGIN BY INDUSTRY

PANEL DATA MODEL - SERIOUS VIOLATIONS

Industry	Operating Margin	Debt Ratio (Operating Margin ≤ 0)	Debt Ratio ($0 <$ Operating Margin $\leq .1$)	Debt Ratio (Operating Margin $> .1$)	Debt Ratio (not by Operating Margin)
Soaps and Cleaners	-28.62 (0.92)				-3.72 (0.38)
Grinding	-16.77 (4.03)	0.51 (0.00)	-1.84 (1.15)	-0.44 (0.35)	
Metal Forging	-1.70 (0.53)	-1.43 (0.24)	-1.06 (1.23)	-1.20 (1.06)	
Construction Machinery	-4.09 (1.93)	-1.38 (0.87)	-1.90 (2.57)	-2.02 (2.51)	
Automobiles	-10.98 (4.87)	-2.01 (2.72)	-0.78 (2.06)	0.96 (1.62)	
Aircraft	-6.41 (2.10)				-0.19 (0.26)
Shipbuilding	-10.36 (3.50)	-1.71 (0.69)	-0.92 (1.23)	-0.33 (0.31)	
Petroleum	-3.89 (1.33)				0.17 (0.10)
Steel (inc. Minimills)	-4.73 (1.45)	-0.58 (0.63)	-0.41 (0.81)	-0.18 (0.31)	

TABLE 7

TOBIT ESTIMATES OF DETERMINANTS OF PENALTIES[1]

	No Plant Effects		With Plant Effects		Variable Mean (Std. Dev.)
Operating Margin	-0.61 (6.29)	-0.14 (1.18)	-0.11 (10.47)	-0.11 (8.14)	0.107 (0.054)
Debt Ratio (Operating Margin ≤0)	-0.02 (0.28)	0.10 (1.72)	0.02 (2.08)	0.004 (0.31)	0.602[2] (0.162)
Debt Ratio (0<Operating Margin≤.1)	0.11 (5.10)	0.10 (4.48)	-0.01 (1.34)	-0.15 (2.66)	0.480[2] (0.219)
Debt Ratio (Operating Margin >.1)	0.21 (6.34)	0.11 (3.41)	-0.05 (0.90)	-0.03 (0.35)	0.282[2] (0.153)
Unionization	-0.08 (3.64)	-0.09 (3.67)	0.02 (1.33)	-0.09 (3.87)	0.410 (0.211)
Plant Employment (10,000's)	0.21 (1.67)	0.15 (1.13)	-0.01 (0.53)	-0.07 (2.34)	0.035 (0.055)
Plant Employment Squared	-0.04 (0.17)	0.06 (0.23)	-0.65 (7.01)	-0.11 (3.75)	0.004 (0.024)
Time Since Last General Inspection	0.01 (0.88)	0.07 (4.60)	-0.10 (3.40)	-0.12 (5.94)	0.815 (0.301)
Accident Inspection	0.04 (2.52)	0.02 (1.57)	0.07 (2.68)	-0.12 (1.32)	0.135

[1]Sample restricted to first 30 observations on any plant due to limitations in Limdep.

[2]Conditional on Operating Margin falling in specified range.

Complaint Inspection	-0.08 (7.40)	-0.07 (6.96)	0.17 (5.21)	-0.19 (3.77)	0.58
State Inspection State	-0.02 (2.04)	-0.01 (0.71)	0.01 (0.43)	-0.05 (1.46)	0.231
Carter Administration	0.01 (1.43)	0.02 (2.44)	-0.08 (0.26)	-0.16 (4.78)	0.307
Reagan Administration	-0.12 (10.55)	-0.10 (9.20)	-0.70 (0.61)	-0.08 (3.86)	0.296
Industry Dummies	No	Yes	No	Yes	
Log Likelihood	-1656.7	-1571.3	-1622.3	-1574.8	
N	4946	4946	4946	4946	

TABLE 8

DETERMINANTS OF PROBABILITY OF BEING PENALIZED[1]

	No Plant Effects		With Plant Effects		Variable Mean (Std. Dev.)
Operating Margin	-0.89 (2.21)	0.38 (0.73)	-0.70 (1.56)	0.37 (0.61)	0.107 (0.054)
Debt Ratio (Operating Margin ≤0)	0.79 (3.39)	1.07 (4.36)	0.75 (2.98)	1.00 (3.79)	0.602[2] (0.162)
Debt Ratio (0<Operating Margin≤1)	0.60 (6.49)	0.67 (6.85)	0.58 (5.09)	0.67 (5.77)	0.480[2] (0.219)
Debt Ratio (Operating Margin >.1)	0.73 (5.36)	0.59 (4.05)	0.71 (4.65)	0.63 (4.04)	0.282[2] (0.153)
Unionization	-0.32 (3.49)	-0.42 (4.16)	-0.27 (2.32)	-0.38 (3.13)	0.410 (0.211)
Plant Employment (10,000's)	-1.26 (1.91)	-1.31 (1.81)	-1.35 (1.21)	-1.14 (0.99)	0.035 (0.055)
Plant Employment Squared	3.40 (1.62)	3.64 (1.56)	3.20 (0.81)	3.05 (0.76)	0.004 (0.024)
Time Since Last General Inspection	0.27 (4.97)	0.31 (5.00)	0.27 (4.57)	0.29 (4.42)	0.815 (0.301)
Accident Inspection	0.15 (2.43)	0.15 (2.35)	0.14 (2.26)	0.14 (2.20)	0.135
Complaint Inspection	-0.35 (7.76)	-0.36 (7.82)	-0.38 (8.00)	-0.39 (7.90)	0.58

[1]Sample restricted to first 30 observations on any plant due to limitations in Limdep.

[2]Conditional on Operating Margin falling in specified range.

State Inspection State	-0.08 (1.71)	-0.05 (1.00)	-0.09 (1.55)	-0.08 (1.29)	0.231
Carter Administration	-0.20 (4.50)	-0.18 (4.00)	-0.20 (4.57)	-0.19 (4.24)	0.307
Reagan Administration	-0.64 (14.07)	-0.64 (13.63)	-0.70 (14.74)	-0.70 (14.26)	0.296
Industry Dummies	No	Yes	No	Yes	
Log Likelihood	-3175.7	-3145.9	-3150.4	-3127.0	
N	4946	4946	4946	4946	