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DEBT, OPERATING MARGIN, AND INVESTMENT IN WORKPLACE SAFETY*

Randall K. Filer†
Devra L. Golbe‡

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 U.S. industries for the period 1972-87. Our results suggest that, at the lowest levels of operating margins, firms with higher operating margins have safer workplaces. Firms with more debt also have safer workplaces, but only when operating margins are relatively low. These results are consistent with a number of theoretical models in which financial factors influence operating decisions.

I. INTRODUCTION

In this paper we investigate the relationship between firms' financial condition and their decisions regarding workplace safety. Understanding this particular link between financial and operating decisions will contribute to the design of efficient regulatory regimes for workplace safety as well as more efficient design of both public and private insurance schemes. We find that, in a broad range of industries, the level of safety in a firm's workplaces

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Both authors are Professors of Economics at Hunter College and the Graduate Center of The City University of New York. Filer is also senior scholar at CERGE-EI, a joint workplace of Charles University and the Academy of Sciences of the Czech Republic and a Fellow of the William Davidson Institute. The order in which the authors are listed was determined by the flip of a fair coin.

Authors' affiliations: †Randall K. Filer, Department of Economics, Hunter College, 695 Park Avenue, New York, N.Y., 10021, U.S.A.
email: rfiler@hunter.cuny.edu

‡Devra L. Golbe, Department of Economics, Hunter College, 695 Park Avenue, New York, N.Y., 10021, U.S.A.
email: dgolbe@hunter.cuny.edu

is related to that firm's operating margin and indebtedness, particularly at low levels of operating margin.

Although workplace safety is of great importance in its own right,[1] on a more general level our results contribute to an increasing body of literature showing that a firm's financial structure substantially affects its real operating decisions. 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 several avenues through which financing decisions can affect the return stream (or the markets' valuation of the stream). In addition to the divergent interests of managers, shareholders and bondholders, these include taxes, the effect of information asymmetries, and the possibility of costly financial distress.[2] 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.[3] In this paper, we focus on the impact of financial structure and operating performance on one particular input choice, the firm's investment in workplace safety. We find that, at low levels of operating margin where bankruptcy and its attendant distortions are presumably more real possibilities, more profitable firms and more highly leveraged firms are safer places in which to work. For firms with the highest levels of operating margin, workplace safety appears to be independent of the financial factors we investigated.

II. PREVIOUS WORK

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.[4] 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 workers' compensation premia) and lower wages (assuming workers

[1] The Bureau of Labor Statistics reports that in 1998 more than 3 out of every 100 workers suffered an injury serious enough to result in one or more lost workdays, while over 6,000 Americans were killed on the job.

[2] See Harris and Raviv [1991] and Ravid [1988] for surveys of the literature.

[3] 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]. Brooks [2002] also points out that liability and bankruptcy concerns may affect the firm's choice to contract out risky activity to judgement-proof entities.

[4] See, e.g., Viscusi [1979].

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 involved in worker training. The effect of safety on productivity is theoretically ambiguous. Increasing safety may involve reducing 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, various 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.[5] 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 these investments 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. Several models[6] suggest that firms in financial distress may have an incentive to incur excess levels of risk. Managers, acting as agents for equity owners, have as their goal the maximization of the value of equity. They may, therefore, take into account the fact that 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, who become the claimants of the firm. This suggests that there will be underinvestment in safety since equity holders assume all of the costs but receive only a portion of the return to accident avoidance. Higher leverage makes bankruptcy more likely and thus reduces the expected share of the returns to safety investment that will accrue to equity holders, implying that firms with more debt might be expected to assume greater risks and hence invest less in safety.

[5] 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].

[6] See, for example, Shavell [1984], Brander and Lewis [1986], Golbe [1988], and Gollier, Koehl and Rochet [1997].

More recent models suggest that higher leverage (and therefore a greater probability of bankruptcy) may instead create incentives for increased investment in safety. Beard [1990] presents a model where the possibility of bankruptcy creates a subsidy to spending on safety (or anything else) because if the firm is eventually bankrupt, what it spends on safety will not affect the owners' terminal wealth. At the same time, the possibility of bankruptcy reduces the expected payoff (to the firm) from safety expenditures. Consequently, the net effect on safety investment is ambiguous. Larson [1996] generalizes this result to a world where future profitability itself also depends on decisions regarding safety levels and derives similar results. Dionne *et al*. [1997] present a similar model in which the ambiguity of the relationship between financial factors and safety performance is generated by assuming that accident avoidance requires costly effort on the part of owner-managers. Financial factors affect both the efficiency of such effort expenditures (because effort and physical capital are complementary, and physical capital is linked to debt), and the benefits from accident avoidance, which depend on how close the firm is to a bankruptcy threshold.

Thus, the theoretical literature contains a number of alternative models suggesting that investment in workplace safety is likely to be related to a firm's leverage ratio, but that the direction of this relationship is indeterminate. These models generally define bankruptcy in asset terms (i.e., the firm is bankrupt if the market value of its assets is less than the market value of its liabilities). Once we consider a cash-flow definition of bankruptcy (i.e., liquid assets are insufficient to discharge current obligations), it is clear that the probability a given firm will come close to or cross the bankruptcy threshold depends not only on leverage but also on the firm's cash flow from ongoing operations. More successful firms will be more likely to be able to cover the costs imposed by an accident with cash flow from ongoing operations. Thus, whatever the sign of the impact of increased leverage on safety, this impact should be greater for firms with lower operating margins. Mahul [2000] presents a model in this vein, where costs of liquidating assets should a producer not generate sufficient cash to cover fixed debt obligations causes risk-neutral firms to behave in a risk averse manner when their cash reserves are close to the debt service obligation, suggesting that, in our world, safety investment (tolerance of accident risk) should be a non-linear function of both operating margins (representing available cash) and debt levels.

Financial factors may affect safety decisions through other, less direct, means as well. 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. 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 these 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. Several authors have shown that increased leverage is advantageous to the firm in bilateral bargaining with workers, because it reduces the surplus available for sharing with input suppliers.[7] This factor would suggest that payoffs to workers would be negatively related to leverage. However, payoffs to workers also depend on the share allocated to workers, which in turn depends on their bargaining power. Dasgupta and Sengupta [1993] show that debt is negatively correlated with the (*ex ante*) bargaining power of the firm. The sign of the relationship between worker payoff and bargaining power, and hence between debt and worker payoff, is ambiguous. Increased firm bargaining power simultaneously decreases debt and the share of total surplus claimed by workers. On the other hand, 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 generally 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, even an all-equity firm may not provide the social-welfare maximizing level of workplace safety. Since the direction of the impact of leverage on safety is indeterminate, whether increased leverage will increase or offset any inefficiencies is also theoretically ambiguous. In either case, empirical analysis is clearly necessary to ascertain the size and direction of the effects of leverage on safety.

[7] See Dasgupta and Sengupta [1993] and Bronars and Deere [1991].

Given the theoretical ambiguities, it is not surprising that prior empirical work examining the effects of firm financial condition on safety has produced a mixed pattern of results. Most of the literature has been motivated by concerns arising from transportation deregulation and focused 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], Talley and Bossert [1990], and Dionne *et al.* [1997] 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. Dionne *et al.* found that debt/equity ratios affected accidents for a sample of Canadian airlines. Interestingly, this relationship was nonlinear, with higher debt associated with more safety for profitable airlines but a greater frequency of accidents for unprofitable ones. Feinstein [1989], in a study of nuclear power plants, used a 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 positive relationship between safety and financial strength.

The results presented below extend the previous empirical work in two ways. First, they are not limited to a single industry, but instead examine a broad range of different types of productive activities. Second, they focus on workplace safety rather than consumer safety.

III. DATA AND ESTIMATION

III(i). *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.

In effect, we think of safety as an unobserved latent variable. Previous work has used accidents as an imperfect signal of this unobserved variable. Although accidents are an intuitively appealing measure of safety, their rarity and the large importance of random factors in their occurrence make them an imperfect proxy for the firm's safety decisions. We use a different, and presumably less stochastic, signal consisting of plant level data on violations of Occupational Safety and Health Administration (OSHA) standards.[8] Our measure of safety is serious violations of OSHA safety standards in thirteen large U.S. industries for the period 1972–1987.[9]

We are not interested in OSHA violations as such. Instead, we argue that violations uncovered during OSHA inspections represent a 'snapshot' of safety conditions in the plant at a given moment and that this provides a more accurate representation of the firm's underlying safety decisions than accidents, which represent safety decisions only with a very large stochastic element. The results of previous research investigating the link between OSHA and workplace health and safety suggests that OSHA violations may be an excellent proxy for safety conditions.[10] If, as most researchers have argued, the penalties imposed on violators of OSHA standards are too small to provide effective incentives for hazard abatement [Gray, 1990], then safety conditions in the plant (as measured by observed violations) should reflect firm decisions based on factors other than avoiding fines imposed by OSHA: exactly the influences we seek to measure. To the extent that some common types of workplace accidents are unlikely to be affected by compliance with workplace standards [Mendeloff, 1979], OSHA violations will measure only a fraction of workplace hazards, and the estimated coefficients predicting violations will be smaller in absolute value than would be coefficients predicting all hazards, but their signs and significance will be unaffected.

A potential problem in using OSHA violations as a proxy for safety is that inspection targets might not be randomly selected. To the extent that the variables correlated with selection for inspection by OSHA are correlated with the variables of interest in explaining violations, failure to account for the selection model could bias our estimates of the impact of financial conditions on safety. However, selection is unlikely to be a serious problem

[8] 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.

[9] OSHA conducts health as well as safety inspections; 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 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.

[10] Several studies have suggested that OSHA had little, if any, impact on accident rates [Viscusi, 1979; Mendeloff, 1979; Smith, 1979; McCaffrey, 1983; Bartel and Thomas, 1985; and Ruser 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. Gray and Scholz [1993] find that when penalties are imposed inspections lead to a 22% decline in injuries over several subsequent years.

in the estimations we report below. OSHA targets general or 'programmed' inspections at plants in high-risk industries, where it bases its measure of risk on industry-level data collected by the BLS. While any plant may be subjected to such an inspection, they are targeted toward larger workplaces in more hazardous industries.[11] OSHA identifies the riskiest industries in each state based on state or national injury rates and inspects plants in those industries starting with those predicted to be the most dangerous [Gray, 1990]. Our estimating equations account for both industry and size. In addition, we control for inspection probability based on the ratio of plants in a given industry and state that were inspected to the total in that industry/state cell for each year. We are aware of no evidence that OSHA targets firms for inspections based on the financial condition of the firm.

Finally, problems may arise if inspections are only partially successful in identifying violations. Feinstein [1990] presents a binary probit model of violations that incorporates a model of the process by which violations are detected and shows that the possibility that some violations remain undetected biases coefficients towards zero. More serious biases may result if explanatory variables are correlated with both detection and the commission of violations. Detection controlled estimators increase the estimated effects of unionization, which he argues indicates that OSHA assigns lower quality inspectors to unionized firms. With respect to our main variables of interest, the firm's financial condition, we know of no evidence that OSHA assigns inspectors according to firm financial condition. We have, therefore, retained consistency with the vast majority of the literature and assumed that detected violations are a reasonable proxy for actual violations. In addition, we include a measure of the scope of inspections in our estimates to help control for imperfect detection.

We confine our attention here to 'serious' violations. OSHA categorizes violations as being either 'serious' or 'not serious' according to the degree of threat posed to worker health and safety. Serious violations pose a 'substantial probability that death or serious physical harm could result.'[12] Violations are assigned to a category according 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 at issue. 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.

We limit our analysis to 'general' inspections. Other inspections are aimed at plants where accidents have occurred or complaints have been lodged, or

[11] Unionized plants also have a higher probability of being inspected and are scrutinized more intensively [Weil, 1991]. This is one reason for controlling for the extent of unionization.

[12] See *OSHA Regulations* (*Standards-29 CFR*) Definitions-1960.2

are 'followup' inspections.[13] We omit these inspections from our analysis because the selection method for such observations clearly differs from that for general inspections. In addition, we omit observations in the twenty-one states that have chosen to substitute state-level inspection programs for federal OSHA inspections, because inspections in these states may differ from federal inspections in their selection process or inspection methodology.[14]

Our initial data set containing records of all OSHA inspections from 1972 through 1987 was restricted to a subset of thirteen industries in order to reduce the data set to a tractable size.[15] These industries were selected to exhibit the following characteristics: (1) a large number of publicly traded firms; (2) a wide variation in financial performance among those firms; and (3) a likelihood of measurable safety issues as indicated by Bureau of Labor Statistics reports of workplace injuries. After eliminating observations with missing data, the final sample contained 1065 inspections in 606 plants (255 firms). Table I shows the number of firms and plants as well as the mean number of inspections per plant and violations found per inspection by industry. Many inspections turned up no cited violations. As can be seen in Table II, which shows the frequency distribution of violations, over 70% discovered no serious violations.

III(ii). *Independent Variables*

Previous studies of the impact of the firm's financial condition on safety have used a variety of measures of financial condition.[16] As measures of the profitability and leverage variables the theories discussed above suggest are important, we use the firm's operating margin (operating income divided by

[13] 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. It should be expected that few violations would 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.

[14] 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.

[15] 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.

[16] Golbe [1983] and [1986] considered net income and rate of return measures of profitability. Rose [1990] used operating margin (defined as 1 − (operating expenses)/(operating revenues)) 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. Dionne, *et al.* [1997] used measures of leverage, working capital and operating margin.

TABLE I
SAMPLE CHARACTERISTICS

Industry	Number of Firms	Number of Plants	Mean Number of Inspections/Plant	Mean Number of Serious Violations/ Inspection
Pharmaceuticals	11	22	1.64	0.86
Soaps and Cleaners	19	37	2.64	0.65
Paints and Varnishes	9	33	1.72	0.62
Grinding	33	82	2.25	1.44
Metal Forging	33	57	1.99	0.98
Farm Machinery	21	48	2.79	0.83
Construction Machinery	35	80	2.50	1.30
Automobiles	30	108	2.15	2.48
Aircraft	24	42	1.94	1.70
Shipbuilding	14	35	5.15	0.77
Petroleum	10	20	2.84	1.22
Paper	12	38	3.42	3.72
Steel (inc. Minimills)	4	4	2.14	11.71

TABLE II
DISTRIBUTION OF NUMBER OF SAFETY VIOLATIONS

Number of Serious Violations	Frequency (%)	Cumulative Frequency (%)
0	70.42	70.42
1	8.64	79.06
2	6.76	85.82
3	3.57	89.39
4	2.44	91.83
5	1.88	93.71
6	0.85	94.55
7	1.03	95.59
8	0.09	95.68
9	0.38	96.06
10	0.47	96.53
11	0.28	96.81
12	0.19	97.00
13	0.28	97.28
14	0.19	97.46
15 or more	2.54	100.00

sales)[17] 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).[18]

We allow for non-linearity in the relationship between operating margin and safety by fitting a linear spline with knots set arbitrarily at the

[17] 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.

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

thirty-third and sixth-seventh percentiles of the distribution.[19] In addition, because theoretical considerations suggest that the effect of indebtedness on safety investment may vary with a firm's available cash, we interact the firm's debt ratio with dummy variables that divide the sample according to the level of operating margins. The divisions are arbitrarily chosen to divide the sample into three groups: those with negative operating margins, those with operating margin between zero and 10%, and those with operating margin above 10%.[20]

Our source for financial data is the R&D Master file assembled from COMPUSTAT and other sources by the NBER [Cummins, *et al*. 1988].[21] This data source presents a potential problem in that only large, publicly traded firms are covered. *A priori*, firms in COMPUSTAT data are more likely to be financially stable than are small, privately-held firms. Analysis of the data, however, 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 one 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 12% with a standard deviation of 5% while the mean debt ratio was 32% with a standard deviation of 17%.

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. The OSHA data set reports whether or not at least one union contract was present in each included plant. Unfortunately, however, the probability that workers will elect to be represented by a union is likely to be a function of the level of safety in their workplace, making plant-level union coverage endogenous. Thus, we instrument union coverage at the plant level. Whether a plant is unionized is estimated as a function of industry, unionization at the firm level (using data provided by Bronars, Deere and Tracy [1994]), plant employment, and whether the plant is located in a Southern state using a linear probability model [Angrist, 2001]. The linear probability model

[19] Denote the spline variables by S_1, S_2, and S_3, the knots as k_1 and k_2, and the original variable by X. Then $S_1 = \min(X, k_1)$, and $S_i = \max(\min(X, k_i), k_{i-1}) - k_{i-1}$ $i = 2, 3$.

[20] Operating margin at the sample median was 11.1%.

[21] 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.

predicts all but four values in the acceptable range from zero to one. (Those four observations lie in the interval from 1 to 1.12.)

We also control for industry. Industries may vary by their 'natural compliance' with OSHA regulations [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.[22] 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. In addition to the industry dummies, we control for inspection probability defined as the ratio of general inspections for plants in each industry in a given state in a given year to total plants in that industry as reported in Country Business Patterns for the state and year.

It is likely that the number of violations reported will be related to how long it has been since the previous inspection. We include two variables to capture this effect, the time since the last inspection of the plant (in days) and a dummy for whether or not the inspection in question is the first for the plant.

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. Finally, as we noted above, the likelihood of inspection may be related to plant size. Thus, we include plant size, measured by number of employees, in our estimating equations.[23]

We also include dummies to account for the scope and intensity of inspections. Dummies are used to identify inspections reported as 'partial' or 'records-only.' The latter refers to a policy during the 1980s whereby the inspector would first examine injury logs, and proceed to a full inspection only if the injury rate exceeded some threshold. (About 20% of our sample consists of partial inspections; another 20% are 'records-only' inspections.) In addition, 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

[22] Industry dummies also help to account for non-random selection of inspection targets by OSHA, and potential differences across industries in the rate at which inspections of the same intensity generate reported violations.

[23] Larson [1996] derives theoretical results relating firm size to safety investment. He finds that the sign of the relationship is generally ambiguous. 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.

violations decreased. Under Reagan, the level of penalties assessed fell dramatically, and targeted inspections and serious violations increased [Gray, 1990].

One obvious potential control is missing from our regressions. The workers' compensation system is likely to affect the costs and benefits to both workers and firms of investing in workplace safety, and consequently the incentives of firm's to invest in safety.[24] Consequently, we would like to control for the exogenous characteristics of the workers' compensation system to which the firm is subject. Workers' compensation programs vary over time and from state to state along a number of dimensions which have been hypothesized to affect incentives: for example, whether the insurance is provided by the state or private carriers or whether firms self-insure, and the extent to which regulation and other factors generate deviations from perfect experience rating [Burton and Chelius, 1997; Thomason *et al.*, 2001]. While we have been unable to secure this data,[25] it does not appear that its omission presents a serious problem for our results. Any state-specific characteristics that do no change over time will be subsumed in our plant effects. In addition, our presidential administration dummies should control at least partially for changes in workers' compensation schemes over time. Finally, our industry dummies will reflect in part variations in workers' compensation premia arising from experience rating at the industry level.

III(iii). *Estimation Methods*

Because our dependant variable takes the form of (possibly over-dispersed) count data, we focus on negative binomial models. In addition, a large fraction of our observations are repeated observations on the same plants.[26] Errors within panels may be correlated because of both omitted plant-specific variables and because it is unlikely that the temporal distribution of violations is random. Thus, we present several alternative estimating techniques including Liang and Zeger's generalized estimating equation (GEE) approach as well as negative binomial specifications allowing for random plant effects and correlated errors. The structure of the models estimated, including their panel nature and an endogenous dummy variable, means that analytic standard errors are not available. We therefore present

[24] Under assumptions of perfect markets, including perfect information, perfect experience rating and actuarially fair premia, the existence of workers' compensation should not affect safety incentives [Burton and Chelius, 1997]. Empirically, of course, such assumptions must be tested.

[25] Available data sets (e.g. that in Thomason, *et al.* [2001]) do not cover the full period of our sample.

[26] 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.

bootstrapped standard errors (clustered on plant) and bias corrected confidence intervals based on 1000 possible repetitions.[27]

IV. RESULTS

Table III shows estimates of negative binomial models which take a variety of approaches to dealing with the possibility of correlation among errors across observations, as well as different specifications for the relationship between leverage and safety. Across this range of models, results consistently indicate that both operating margin and leverage have a significant impact on safety violations. Before discussing the impact of financial variables more fully, however, we make some preliminary remarks about appropriate specification.

Our simplest specification (Column 1) is a negative binomial model with linear terms in operating margin and leverage. In that model, both operating margin and leverage are negatively related to violations, though neither is statistically significant. To investigate further, we allow for more complicated relationships between financial factors and safety. Thus, we fit a linear spline to operating margin to allow for non-linearity, and interact the firm's debt ratio with a set of three dummies to divide the sample by operating margin. These results are reported in Column 2 of Table III.

First, we note that estimates of α, the overdispersion parameter, are significantly different from zero, indicating that negative binomial rather than Poisson models are the appropriate characterization of the data. Second, many of the plants in our sample were inspected more than once. We can exploit the repeated inspections to estimate plant effects. Plants may differ for many reasons that may or may not be correlated with included explanatory variables. Consequently, accounting for such plant effects can help provide unbiased estimates of the variables of interest. Thus we estimated a negative binomial model with random plant effects and report these results in column 3.

We also use Liang and Zegler's generalized estimating equation (GEE) approach to allow for more complicated correlation structures within panels. In particular, we estimate both a simple exchangeable correlation structure, which constrains within-panel correlations to be constant, as well as an unstructured correlation model which requires only that the diagonal elements of the within-panel correlation matrix equal one. These results are in columns 4 and 5 of Table III. We note also that the GEE model is a population-averaged estimator (i.e., only a marginal distribution is specified), while the random-effects model is a subject-specific estimator, so that the coefficient vectors represent slightly different concepts.

[27] Actual repetitions are less than 1000 for the random effects and GEE models because some repetitions failed to converge.

TABLE III
DETERMINANTS OF SERIOUS OSHA VIOLATIONS. BOOTSTRAPPED STANDARD ERRORS & BIAS-CORRECTED 95% CONFIDENCE INTERVALS

	(1) Neg. Binomial	(2) Neg. Binomial	(3) Random Effects Neg. Binomial	(4) GEE Neg. Bin. (exchangeable)	(5) GEE Neg. Bin. (unstructured)
Operating Margin	– 2.09				
	(2.20)				
	(– 6.92 1.93)				
Operating Margin—spline 1		– 11.57*	– 11.74	– 11.09*	– 10.98*
		(5.78)	(6.01)	(5.14)	(5.29)
		(– 22.52 – 0.85)	(– 20.56 1.76)	(– 23.25 – 2.44)	(– 20.36 – 8.24)
Operating Margin—spline 2		– 10.68	0.42	– 11.56*	– 12.02†
		(11.39)	(9.95)	(10.23)	(10.45)
		(– 39.92 6.59)	(– 17.27 7.02)	(– 28.51 – 1.68)	(– 28.7 1.86)
Operating Margin—spline 3		1.89	0.51	3.98	4.35
		(4.37)	(3.47)	(4.74)	(4.41)
		(– 6.63 10.26)	(– 2.68 6.56)	(– 3.76 9.70)	(– 5.11 11.6)
Debt Ratio	– 0.89				
	(0.59)				
	(– 2.08 0.25)				
Debt Ratio (Op Margin ≤ 0)		– 5.31*	– 5.03*	– 5.06*	– 5.2†
		(45.63)	(9.20)	(3.51)	(4.75)
		(– 102.39 – 0.80)	(– 7.62 – 1.74)	(– 15.47 – 2.82)	(– 22.25 1.56)
Debt Ratio (0 < Op Margin ≤ 0.1)		– 1.54*	– 0.52	– 1.14*	– 1.12*
		(0.75)	(0.62)	(0.55)	(0.57)
		(– 3.12 – 0.20)	(– 1.72 0.65)	(– 2.03 – 0.25)	(– 1.96 – 0.26)
Debt Ratio (Op Margin > 0.1)		– 0.58	– 0.44	– 0.19	– 0.18
		(0.84)	(0.73)	(0.67)	(0.91)
		(– 2.27 1.03)	(– 0.97 0.80)	(– 1.62 0.95)	(– 2.49 1.14)
Unionization (Predicted)	3.29*	3.29*	1.28†	3.61*	3.44*
	(1.51)	(1.51)	(1.30)	(1.54)	(1.49)
	(0.75 6.68)	(0.51 6.60)	(– 0.28 6.46)	(1.66 8.48)	(1.68 8.22)
Plant Employment (000s)	0.16*	0.16*	0.10	0.09	0.09
	(0.09)	(0.10)	(0.05)	(0.08)	(0.09)
	(0.00 0.39)	(0.01 0.41)	(– 0.02 0.16)	(– 0.05 0.21)	(– 0.13 0.23)
Log Inspection Probability	– 0.05	– 0.04	– 0.19†	0.02	0.01
	(0.13)	(0.13)	(0.09)	(0.11)	(0.13)
	(– 0.27 0.21)	(– 0.26 0.24)	(– 0.32 0.00)	(– 0.14 0.15)	(– 0.25 0.18)

TABLE III. (Contd.)

	(1) Neg. Binomial	(2) Neg. Binomial	(3) Random Effects Neg. Binomial	(4) GEE Neg. Bin. (exchangeable)	(5) GEE Neg. Bin. (unstructured)
First inspection	0.10	0.12	0.29	0.10	0.08
	(0.28)	(0.28)	(0.24)	(0.25)	(0.27)
	(– 0.43 0.62)	(– 0.43 0.64)	(– 0.23 0.53)	(–0.30 0.67)	(– 0.36 0.67)
Time since prior inspection	– 1.3E – 4	– 1.3E – 4	7.4E – 5	– 1.1E – 4	– 1.3E – 4
	(1.4E – 4)	(1.5E – 4)	(5.9E – 5)	(1.2E – 4)	(1.3E – 4)
	(– 4.8E – 4 1.0 E – 4)	(– 4.9E – 4 1.1E – 4)	(– 2.3E – 4 2.3E – 4)	(– 3.3E – 4 1.8E – 4)	(– 5.3E – 4 8.4E – 5)
Carter	2.52*	2.51*	2.22*	2.43*	2.42*
	(0.24)	(0.24)	(0.25)	(0.26)	(0.27)
	(2.00 2.91)	(2.0 2.92)	(1.74 2.42)	(1.94 2.97)	(1.89 2.89)
Reagan	2.29*	2.23*	2.03*	2.11*	2.09*
	(0.26)	(0.27)	(0.27)	(0.26)	(0.29)
	(1.74 2.72)	(1.67 2.69)	(1.35 2.33)	(1.64 2.51)	(1.50 2.56)
Partial Inspection	– 1.09*	– 1.09*	– 1.40*	– 1.10*	– 1.09*
	(0.35)	(0.35)	(0.26)	(0.33)	(0.38)
	(– 1.73 – 0.49)	(– 1.73 – 0.50)	(– 1.71 – 0.72)	(– 1.62 – 0.73)	(– 1.95 – 0.61)
Records-only Inspection	– 5.19*	– 5.08*	– 5.52*	– 5.02*	– 5.00*
	(7.67)	(14.58)	(7.92)	(1.59)	(1.23)
	(– 23.47 – 3.90)	(– 22.78 – 3.84)	(– 20.76 – 4.42)	(– 18.3 – 4.78)	(– 12.6 – 4.72)
alpha (overdispersion parameter)	3.15*	3.08*			
	(0.46)	(0.45)			
	(2.58 4.09)	(2.57 3.99)			
r			2.51*		
			(0.22)		
			(2.95 3.19)		
s			5.49		
			(0.18)		
Repetitions	1000	1000	950	166	227

NOTES:

*95% confidence interval excludes zero.

†90% confidence interval excludes zero.

Coefficients for constant term and industry dummies omitted.

Bootstrap samples are clustered on plant.

The relationship between violations and operating margin is quite consistent across estimation methods and models. For low levels of operating margin, the relationship is negative: firms with higher operating margin have fewer violations. At higher levels of operating margin, safety violations are unrelated to operating margin. That is, for firms with the lowest levels of operating margin (where bankruptcy may be a real possibility if cash reserves are not sufficiently large) safety and economic success are positively related, while for firms at the upper tail of the operating margin distribution safety is independent of operating margins.

The relationship between leverage and safety violations is also complex but consistent with theoretical expectations. Allowing the effect of leverage to differ according to the level operating margin clearly establishes a significant negative relationship between leverage and violations for plants with negative operating margins. For firms with positive but below average operating margins, the relationship is still negative, although smaller in magnitude. For firms with the highest operating margins (e.g., those likely to be far from bankruptcy) there is no relationship between leverage and safety violations.

Our results with respect to leverage measures indicate that, at least when bankruptcy is a real possibility, more highly leveraged firms invest more in safety. These results are thus consistent with the more recent theoretical models, which suggest that leverage may induce either more or less safety investment, but contradict the earlier models which predict a positive relationship between leverage and risk-taking. They are also consistent with the argument that leveraged firms are likely to bias their safety technology towards easily monitored standards, and that workplace safety may represent a form of rent-sharing with labor.

The negative relationship between operating margin and violations which holds for firms with the lowest operating margins is clearly consistent with 'pecking order' models of investment. In addition, of course, other factors may cause a spurious relationship between operating margin and safety violations. Firms that are better managed may have both fewer violations and higher operating margins. Over time, firms may have adopted new technologies which simultaneously reduced violations and increased profits. Our random plant effects specification, however, provides evidence against the managerial quality hypothesis. To the extent that it is fixed over time, managerial competence should be picked up in the plant effects. The inclusion of these random effects does not weaken the measured effect of operating margin. Thus we conclude that the negative relationship between operating margin and safety violations is not primarily an artifact of managerial competence.

Theory suggests that our model should contain industry dummies. Nevertheless, given our concern that industry dummies may absorb some of the effects of financial variables, we re-estimated the models of Table III without such dummies. Since the coefficient estimates were essentially unchanged, those results are not reported here.

The other control variables generally behave in sensible ways. Plant size is positively related to serious violations since there are simply more opportunities to violate standards in larger plants. In models that include plant effects, where variation in plant size represents only changes in employment within a given plant, the coefficient on plant size is smaller in magnitude (although still positively related to serious violations) and no longer statistically significant. Coefficients of the first inspection dummy, elapsed-time since the last inspection, and inspection probability are not significantly different from zero. Unionization increases safety violations, even after accounting for the endogeneity of union status. We doubt that this apparent impact is causal in nature. Rather, unions may influence the propensity of OSHA inspectors to find violations, perhaps by bringing matters to their attention. The negative coefficients on the partial-inspection and records-only dummies indicate, not surprisingly, that the less thorough the inspection, the fewer violations found.[28] 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 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.

V. CONCLUSIONS

The results presented above provide evidence that financial performance significantly affects firms' workplace safety decisions, at least in the industries we have studied, and to the extent that these decisions are reflected in compliance with OSHA standards. More highly leveraged firms violate fewer OSHA standards but this effect is reduced at higher levels of operating margin. Across our total sample, for firms in the lower tail of the distribution of operating margins, safety violations are negatively related to income. These findings that financial factors influence the real decisions of firms provide support for a number of theoretical models of the interaction

[28] There is evidence that inspection scope may be endogenous with respect to nonfinancial variables such as unionization although not with respect to financial variables. Unfortunately, endogeneity of one variable may lead to biased coefficients on other variables [Windmeijer and Santos Silva, 1997; Terza, 1998; and Windmeijer, 2000]. We therefore re-estimated the equations excluding the scope variables. The results, including, in particular, the coefficients on the financial variables, are essentially unchanged, suggesting that endogeneity is not an important problem for the issues we investigate here.

DATA APPENDIX

Variable Descriptions and Sources

Variable		Definition	Mean (Std. Dev.)	Min	Max
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.	1.63 (5.85)	0	77
Operating Margin		(Operating Income before Depreciation)/Sales	0.12 (0.05)	– 0.10	0.31
Operating Margin—Spline 1		min(Operating margin, k_1) where k_1, k_2 are knots in spline	0.08 (0.02)	– 0.10	0.09
Operating Margin—Spline 2		max[min(Operating margin, k_2), k_1] $-$ k_1	0.02 (0.02)	0	0.04
Operating Margin—Spline 3		max(Operating margin, k_2) $-$ k_2	0.01 (0.03)	0	0.18
Debt Ratio		Long Term Debt/(long-term debt + preferred stock + common stock) (all valued at market prices)	0.32 (0.17)	0.02	0.92
Debt Ratio (Op Margin ≤ 0)		Debt Ratio if Op Margin ≤ 0; 0 otherwise	0.007 (0.066)	0	0.72
Debt Ratio ($0 <$ Op Margin $\leq .1$)		Debt Ratio if $0 <$ Op Margin $\leq .1$; 0 otherwise	0.0148 (0.213)	0	0.92
Debt Ratio (Op Margin $> .1$)		Debt Ratio if $0 <$ Op Margin $\leq .1$; 0 otherwise	0.162 (0.177)	0	0.78
Scope Dummies (full omitted)	Partial	1 if partial inspection; 0 otherwise	0.20 (0.40)	0	1
	Records only	1 if records-only; 0 otherwise	0.20 (0.40)	0	1
Presidential Dummies (pre-Carter omitted)	Carter	1 for years 1977–80; 0 otherwise	0.21 (0.41)	0	1

DATA APPENDIX (Contd.)

Variable	Definition	Mean (Std. Dev.)	Min	Max
Reagan	1 for years 1981–87; 0 otherwise	0.42 (0.49)	0	1
Total Employees	Total employees in inspected plant, in 1000s, from OSHA records.	1.10 (2.40)	0.001	23.5
First Inspection	1 if first general inspection in plant; 0 otherwise	0.56 (0.50)	0	1
Time since first inspection	number of days since prior inspection	517.8 (885.9)	0	4252
Probability of Inspection	log(100* number of inspections by state and industry/number of establishments by state and industry)	1.25 (1.05)	– 1.44	4.61
Union (fitted)	Linear prediction of plant union status predicted by firm-level unionization, South, total employees, and industry dummies	0.56 (0.16)	0.07	1.12
Firm-level 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. (1979–82 figures were used for data points from 1979–87.)	31.21 (19.52)	1.28	197.21
South	1 if State = Alabama, Arkansas, Delaware, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia; 0 otherwise	0.27 (0.44)	0	1

Sources:

Safety measures and plant-level unionization: OSHA inspection records.

Financial data: NBER R&D Master File, which is in turn derived from COMPUSTAT.

Firm-level union coverage rates: Bronars, Deere, and Tracy (1994).

Establishments by state and industry: U.S. Bureau of the Census, *County Business Patterns*, 1972–1987, Table 1B.

of financing and investment decisions, and suggest 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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