Financing Constraints and Workplace Safety*

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ABSTRACT

We present evidence that financing frictions adversely impact investment in workplace safety, with implications for worker welfare and firm value. Using several identification strategies, we find that injury rates increase with leverage and negative cash flow shocks, and decrease with positive cash flow shocks. We show that firm value decreases substantially with injury rates. Our findings suggest that investment in worker safety is an economically important margin on which firms respond to financing constraints.

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Over 3.5 million workplace injuries and illnesses occur in the U.S. each year. The estimated annual cost of these injuries is $250 billion, more than the cost of all forms of cancer combined (Leigh (2001)). While workplace safety has been studied extensively in fields as diverse as industrial relations, operations management, and industrial-organizational psychology, its connections with finance remain largely unexplored. This paper studies how financing constraints impact workplace safety, which has implications for firm value and employee welfare.

Firms invest resources in improving workplace safety just as they invest in research and development, property, plant, and equipment, and organizational capital. As with other forms of investment, spending on safety must be financed out of either internal cash flow or externally-raised capital. In a world with financing frictions, a firm's investment may be sensitive to the financial resources available to finance that investment. Thus the safety of a firm's workplaces could depend on the financial resources at its disposal. Investment in safety may be especially vulnerable to cuts in the face of financing constraints, as its payoffs accrue slowly over time and are difficult to evaluate.

In this paper, we explore the impact of financing constraints on workplace safety by examining the sensitivity of workplace injury rates to the financial resources available to a firm using establishment-level injury data from the Bureau of Labor Statistics' (BLS') annual Survey of Occupational Injuries and Illnesses (SOII). As we lack exogenous variation in financial resources with which to completely isolate the effect of financing on injuries, we employ several empirical strategies. Each approach produces evidence pointing towards increased financial resource availability leading to fewer injuries, suggesting that financing constraints impair investment in safety. While any one piece of evidence is open to alternative interpretations, the evidence taken together is difficult to reconcile with any specific alternative.

We begin by examining the empirical relationship between injury rates and well-established drivers of a firm's capacity to finance investment, including cash flow, cash balances, and financial leverage. Cash balances and cash flow are sources of internal financing. Debt reduces cash flow through interest payments, and existing debt claims can make it difficult to raise additional external capital (Myers (1977)). Prior research shows that investment in general tends to increase with available cash (e.g., Fazzari, Hubbard, and Petersen (1988), Lamont (1997), Rauh (2006)) and decrease with leverage (e.g., Denis and Denis (1993), Lang, Ofek, and Stulz (1996)). If investment in safety is sensitive to a firm's financial resources, then injury rates should decrease with cash flow and cash balances and increase with leverage.

We find a robust positive relation between injury rates and leverage, controlling for establishment and firm characteristics as well as establishment, industry-year, and state-year fixed effects. A one-standard-deviation increase in a firm's debt-to-assets ratio is associated with a 5.6 percentage point increase in total workplace injuries the following year, relative to the sample mean injury rate. It is further associated with a 6.5 percentage point increase in injuries serious enough that the injured employee misses at least one full day of work, suggesting that it is not just minor injuries that are sensitive to leverage. These estimates are larger than existing estimates of the impact of penalty-imposing Occupational Safety and Health Administration (OSHA) inspections or plant unionization on workplace injury rates (Mendelhoff and Gray (2005)). Injury rates are sensitive to at least the first two annual lags of leverage but are unrelated to contemporaneous and future leverage, partially alleviating concerns about reverse causality.

Injury rates show strong negative relations with cash flow and cash balances in the cross-section. However, these relations disappear when we control for establishment fixed effects, suggesting that they may be driven by unobserved firm- or establishment-level heterogeneity. We also find some evidence that injury rates are negatively related to dividend payout and firm size. As these characteristics are often seen as inverse proxies for the severity of financing constraints, this evidence provides added support for the role of financing constraints in limiting investment in safety. Overall, the results from this analysis provide some support for the hypothesis that injury rates decrease with financial resources, though the evidence here is far from conclusive.

To better isolate the effect of financial resources on injury rates, we next study three quasi-natural experiments involving cash flow shocks. These include a repatriation tax holiday in 2004, the onset of the financial crisis in 2007 and 2008, and large oil price fluctuations during the 2000s. Variants of each have been used in prior papers to study the effect of cash flow on capital investment.[1] The cash flow shocks involved are large, plausibly exogenous with respect to injury rates, uncorrelated with each other, and affect some firms but not others. We exploit this last feature to conduct difference-in-differences analysis using matched samples, which mitigates though does not eliminate concerns about unobserved counterfactual changes in injury rates absent a shock. One useful feature of the set of experiments we study is that exposure is almost completely uncorrelated across the three, suggesting that they can be treated as three independent tests.

The results of these tests broadly support a negative (positive) response of injury rates to a positive (negative) cash flow shock, especially in firms with higher leverage. The estimates imply that, on average, a firm's injury rate would fall by 8.4 to 11.9 percentage points in response to a one-standard-deviation increase in its cash flow. We verify that the difference-in-differences estimates are unlikely to be driven by ex ante differences in treated and control establishments or by differential pre-existing trends in injury rates. While we cannot rule out the possibility that unobserved factors correlated with future trends in injury risk impact assignment, they would have to do so in ways that coincidentally produce consistent results across three separate experiments to explain the results. We also explicitly consider specific omitted characteristics, such as managerial skill and production technology, and conclude that it would be difficult for any single characteristic to explain all of the results in the paper. The weight of the evidence thus appears to support financing constraints as the most likely mechanism driving the results.

Stakeholder theory argues that a firm indirectly bears costs, in expectation, that its financial policies impose on nonfinancial stakeholders such as employees ex post (Titman (1984)). The natural channel in the case of costs due to higher workplace injury risk is a compensating wage differential that employees require to bear this risk. Firms may also bear costs directly in the form of decreased productivity resulting from increased downtime and poor employee morale.[2] In the last part of our analysis, we find a substantial negative relation between firm value and injury rates, with firm value decreasing by 6.1% for each one-standard-deviation increase in injury rate. While we cannot rule out the possibility of alternative explanations for this relation, the estimates imply plausible costs per injury in light of existing estimates of compensating wage differentials (Viscusi and Aldy (2003)). The results also imply that greater workplace injury risk may be a significant cost of policies that increase the likelihood that financing constraints bind in the future.

Our paper adds to a small set of papers examining the impact of financing on the risks faced by nonfinancial stakeholders in a firm. Rose (1990) and Dionne et al. (1997) find that the likelihood of serious accidents in the airline industry is negatively correlated with operating margins. Dionne et al. (1997) find some evidence that leverage impacts the likelihood of airline accidents, but only for carriers with negative equity. Phillips and Sertsios (2013) find that airlines mishandle more baggage and have fewer on-time arrivals when they are in financial distress. Beard (1992) finds that roadside inspection violations decrease with trucking company equity valuation. These studies are limited to a small number of firms in specific industries and do not focus on *employee* safety. In a current working paper, Kini, Shenoy, and Subramaniam (2014) present evidence that the likelihood of a product recall increases with a firm's leverage.

The closest papers to ours are Filer and Golbe (2003) and Nie and Zhao (2015). Filer and Golbe (2003) find that firms with more debt have *fewer* OSHA safety violations, a conclusion seemingly at odds with ours. However, their sample is small and they do not control for establishment or firm fixed effects. Moreover, they measure inspection violations rather than actual injuries, and constrained firms may cut spending on safety in ways that affect injury risk but do not trigger OSHA violations. In a recent working paper, Nie and Zhao (2015) show that workplace fatalities are positively related to firm leverage in China's coalmining industry.

Our paper also contributes to a small literature studying the effects of financing on employee welfare more generally. Gordon (1998) shows that higher firm debt levels are associated with reductions in employment that are not fully attributable to performance. Benmelech, Bergman, and Seru (2011) show that employment levels are sensitive to cash flow and that this sensitivity is greater for firms with higher leverage. Agrawal and Matsa (2013) present evidence that firms increase leverage in response to exogenous increases in unemployment benefits, suggesting that they internalize at least part of the cost of unemployment risk. Bronars and Deere (1991) and Matsa (2010) find that firms use financial leverage to gain bargaining power over their unions, suggesting that financing may impact employee wages. Ours is the first study we are aware of to provide evidence that financing impacts employee welfare through channels other than employment and compensation.[3]

The remainder of the paper is as follows. Section I discusses workplace safety and the potential impact of financing constraints on safety. In Section II, we describe our data and sample. Analysis of the determinants of injury rates is presented in Section III. We present results from the three quasi-natural experiments in Section IV. In Section V, we analyze the link between firm value and injury risk. Finally, Section VI concludes.

I. Financing Constraints and Workplace Safety

In this section, we discuss how firms invest in workplace safety and how financing constraints potentially impact this investment. This discussion is based largely on conversations with industrial safety practitioners and a case study on safety at Alcoa by Clark and Margolis (2000). We also discuss the ideal experiment for testing the impact of financing constraints on injury rates as well as the main challenge in approximating this ideal experiment with actual data.

A. Financial Resources and Investment in Workplace Safety

Workplace safety is instrumental to employee well-being. Poor safety conditions were a major driver behind the spread of unions in the U.S. in the early 1900s and ultimately led to major labor reforms (Brody (1960)). Yet many jobs remain inherently risky. Table I shows the percentage of injuries in the U.S. in 2012 by different causes (Panel A) and types (Panel B) as reported in the BLS' annual news release on employer-related workplace injuries and illnesses. The leading causes of workplace injuries are contact with objects, falls, and physical overexertion, while the most common injury types are sprains, strains or tears, soreness and pain, bruises and contusions, cuts and lacerations, and fractures.

— Insert Table I here —

Firms invest in a number of activities that reduce the risk of on-the-job injury. Some of these activities involve direct expenditures on the acquisition and upkeep of physical assets. These include maintaining existing equipment, replacing old and worn parts and machines, buying equipment with better safety features, and automating dangerous tasks. The physical assets involved can include both sophisticated machinery as well as simpler equipment. As an example of the latter, replacing steel cable used for hoisting objects with (more expensive) synthetic fiber cable can reduce injury risk by decreasing recoil and the incidence of sharp edges upon breakage.

Firms also expend considerable resources on less tangible activities that impact safety, including work flow organization, policies and procedures, training, and supervision. For example, lockout-tagout procedures prevent faulty machinery from being used until properly repaired.[4] Alcoa introduced a forklift speed limit of four miles per hour on a production floor to reduce collisions (Clark and Margolis (2000)). While such a policy may seem mundane, the leading source of workplace injuries in 2012 was floors, walkways, and ground surfaces.[5] Many plants establish safety committees to devise safety improvements. Perhaps the biggest innovation in safety management in the last few decades is the real-time, automated collection of data on a firm's production processes, which expedites the mitigation of potential hazards.

Like investment in physical assets, these organizational and policy activities consume financial resources. Allocating employee time to work on safety committees requires hiring more employees or paying overtime to maintain a given level of production. The same holds for training employees. Moreover, policies are only effective if they are actively enforced. Firms must therefore devote time to monitoring and auditing to ensure that employees follow prescribed practices. In addition, practices such as lockout-tagout for broken equipment may lengthen the time that productive equipment is out of operation.

Systematic estimates of the amount that companies spend on workplace safety are not available, as companies do not generally track such spending. However, anecdotes suggest that this spending can be substantial. Patterson-UTI Drilling Co., an oil and gas drilling company, estimates that it spent $150 million on training and safety improvements between 2001 and 2010, which amounts to 7% of its total income and 32% of its SG&A expense over the period.[6] Following three fatal accidents at its Mission Valley Plant between 1971 and 1988, Alcoa spent $4 million making safety improvements in 1988 at that plant alone (Clark and Margolis (2000)). While these examples may not be representative, safety experts with whom we spoke indicated that safety-related expenditures at their companies are substantial.

While safety-related activities are implemented at the establishment level, they are driven by firm-level decisions through budgetary and policy initiatives. An establishment may cut spending on safety in order to meet short-run budgeted cost targets. Safety practitioners with whom we spoke repeatedly mentioned that budget constraints were an important impediment to implementing workplace safety measures. Anecdotally, the Chemical Safety Board (CSB) blamed a catastrophic explosion at BP's Texas City Refinery in 2005 that killed 15 employees at least in part on an explicit decision not to replace a worn valve due to cost-cutting pressures.[7] Firm-level policy initiatives include hiring safety consultants to help improve safety practices, setting safety targets and holding managers accountable for achieving them, and implementing a safety culture.[8]

A lack of financial resources at the firm level can impact both tangible and intangible investments in safety at the establishment level. Improved safety generates returns to a firm over time in the form of reduced downtime, increased productivity, fewer lawsuits, and a lower compensating wage differential. However, a financially constrained firm may turn down even positive NPV projects in order to conserve resources in the short run. The long-run nature of returns to investment in safety may make it especially vulnerable to cuts in the face of financing constraints.[9] In this sense, a high level of workplace safety may be a luxury that a resource-constrained firm cannot afford. Moreover, serious workplace accidents are infrequent events, making the benefits of spending to improve safety difficult to quantify and hence justify to investors.[10]

B. Financing and Workplace Safety: the Identification Challenge

The ideal experiment for studying the effect of financing constraints on workplace safety would involve taking two identical firms, randomly shocking one with additional financial resources (e.g., cash or borrowing capacity), and then observing subsequent changes in injury rates in both. If financing constraints impede investment in safety, the injury rate should fall in the firm receiving the shock relative to the firm not receiving the shock. The main challenge in approximating this ideal experiment using actual data is that observed variation in financial resources is not exogenous, and firms with differing levels of resources are likely to also differ along other dimensions, some of which are unobserved. This raises the concern that any observed relation between measures of financial resources and injury rates could be driven by an omitted variable or reverse causality.

We consider four specific alternative mechanisms that could induce a negative relation between injury rates and measures of financial resources, similar to the effect that financing constraints should produce. First, employees working with heavy manufacturing equipment may face an especially high risk of injury. This equipment also tends to make good collateral because of its redeployability, and thus firms using such equipment may borrow heavily. A large existing debt load could at least create the appearance that a firm's additional financing capacity is limited. Second, poor operational management can increase injury risk while also depleting financial resources. Third, a fast-growing firm may experience temporarily high injury rates due to employee inexperience or excess workloads, and growth tends to consume financial resources in the short run. The fourth mechanism is reverse causality. Costs associated with actual injuries deplete financial resources.

Still other mechanisms could produce a *positive* relation between injury rates and financial resources. For example, the existential threat created by a persistent lack of financial resources may force a firm to operate with a high degree of efficiency, which could in turn lead to lower injury risk. This view is consistent with Jensen's (1986) argument that debt promotes operational efficiency by disciplining management. In addition, expenditures on safety consume resources in the short run, which could induce a quasi-mechanical positive relation between measures of financial resources and injury rates. These mechanisms would make the effects of financing constraints on injury rates more difficult to detect.

II. Data and Sample

In this section, we describe the data that we use in the paper. We also present sample summary statistics.

A. Data

Our data on workplace injuries come from the SOII of the BLS. Through a joint effort with OSHA, the BLS gathers data for hundreds of thousands of establishments each year in a stratified sampling process to produce aggregate statistics on the state of occupational risk in various industries in the U.S. Employers covered under the Occupational Safety and Health Act and employers selected to be part of the BLS survey are required to maintain a log recording any injuries "that result in death, loss of consciousness, days away from work, restricted work activity or job transfer, or medical treatment *beyond* first aid." These employers must make their injury logs available to OSHA inspectors and supply the data contained in the log to the BLS.

Each establishment in the data has a unique identifier. Each establishment-year record contains establishment name, location, SIC code, number of injuries ($Injuries$), number of injuries resulting in days away from work ($DAFWInjuries$), average number of employees ($Employees$), and total number of hours worked ($Hours$). We use these data to construct annual measures of the injury rate at each establishment. Our primary injury rate measure is $Injuries/Hour$, which is equal to $Injuries$ divided by $Hours$. We also construct $DAFWInjuries/Hour$, which is equal to $DAFWInjuries$ divided by $Hours$.[11] We multiply both of these injury rate measures by 1,000 to make the numbers easier to write.

The BLS data also include, for the period 2002 to 2009, the employer identification number (EIN) of the establishment's parent company. We use the EIN to match the establishment-level data to firm-level data in Compustat. Thus, our sample period is 2002 to 2009. Each firm in Compustat can have multiple establishments.

We calculate several firm-level financial variables using the Compustat data. The variable $Debt/Assets$ is book debt (the sum of Compustat items *dlc* and *dltt*) divided by total assets (*at*). The variable $Cash/Assets$ is total cash and equivalents (*ceq*) divided by total assets. The variable $CashFlow/Assets$ is the sum of income before extraordinary items (*ib*) and deprecation and amortization (*dp*), divided by lagged total assets. $Dividends/Assets$ is common dividends (*dvc*) divided by lagged total assets. The variable $Log(Assets)$ is the natural log of total book assets.[12] The variable $AssetTurnover$ is total sales (*sale*) divided by lagged total book assets. The variable $MarketToBook$ is the market value of assets divided by total book assets, where market value is the sum of the market value of common equity (the product of shares outstanding, *cshpri*, and the firm's stock price, $prcc\_f$), preferred stock (*pstkl*), and book debt, minus the book value of deferred taxes (*txdb*). We set the value of preferred stock or deferred taxes to zero if the relevant item is missing in Compustat. The variable $TangibleAssetRatio$ is net property, plant, and equipment (*ppent*) divided by total book assets. Finally, the variable $Capex/Assets$ is capital expenditures (*capx*) divided by lagged total book assets. We winsorize all of these variables at the 1st and 99th percentiles to reduce the possible influence of outliers, and lag the balance sheet variables by one year.

B. Sample

We exclude from our sample any observations for which any of the firm-level Compustat variables described above is missing. We also exclude all establishments belonging to financial firms (SIC code 6000-6999) or regulated utilities (4900-4999) from our sample. This leaves us with a primary sample consisting of 43,721 establishment-year observations for 25,380 unique establishments that belong to 2,251 unique firms. The median number of times an establishment appears in the sample is one, reflecting the fact that most establishments are sampled by the BLS only once during the sample period. However, 7,918 establishments appear in the sample multiple times and are together associated with 25,053 establishment-year observations (3.16 per establishment). For this subsample, we can account for establishment fixed effects in our regression analysis.

Table II presents summary statistics for the sample. Panel A reports the number of establishment-level observations in the sample by year. The number of observations is fairly stable over time.

— Insert Table II here —

Panel B presents establishment-level summary statistics calculated from the BLS data. Consistent with the BLS' confidentiality policy, we report only means and standard deviations and do not report statistics such as medians and individual percentiles that would present data for individual establishments. The average establishment in our sample has 353 employees, though this number varies widely across the sample. The average employee works 1,718 hours a year, or approximately 43 40-hour work-weeks. The average injury rate is 4.13%, in line with an average annual injury rate of 4.55% over our sample period as reported by the BLS in its aggregate statistics. Slightly less than one in three injuries results in days away from work. Panel C presents firm-level summary statistics for our sample. The mean values of the variables are in line with those for Compustat firms as a whole.

Panel D presents correlations among the variables. Overall, these correlations are modest. Only the correlation between $Capex/Assets$ and $TangibleAssetRatio$ exceeds 0.3 in absolute value. That this correlation is relatively high is unsurprising, as capital expenditures increment tangible assets by definition. The lack of strong correlations overall suggests that when we use these measures as explanatory variables in regressions, our estimates should not be excessively sensitive to small changes in the values of the variables.

An interesting and useful feature of the data is establishment-level rather than firm-level identification of industry. This identification allows us to assign each establishment a unique industry rather than pooling them over a coarse and potentially inapplicable firm-level industry classification. Table III reports the number of observations and injury rates (per hour worked and per average number of employees) for our sample across establishments in different industries. We define industries using the 48-industry classification of Fama and French (1997) and assign each establishment to one of these industries based on its SIC code as reported in the BLS data. Five industries with fewer than 25 observations in the sample each are omitted from Table III for the sake of brevity and because the relatively small number risks revealing the identity of individual establishments. Injury rates are highest in the Candy & Soda, Fabricated Products, and Transportation industries. Not surprisingly, they are lowest in primarily white collar industries such as Entertainment, Computers, and Trading.

— Insert Table III here —

Figure 1 plots the relative distribution of establishment size. As can be seen, establishment size varies widely from a minimum size of 10 full-time employees to well over 1,000 employees. Half of the establishments in the sample have fewer than 100 employees.

— Insert Figure 1 here —

Figure 2 plots the distributions of injury counts and rates for the sample. Almost 30% of establishment-years in the sample have zero injuries. This is not surprising given the relatively low overall injury rate of 4.13% and the small size of many establishments.

— Insert Figure 2 here —

To get a sense of the relative variation of injury occurrence in our sample, we calculate between- and within-group variances at the establishment-, firm-, and industry-level. Table IV reports these variances.

— Insert Table IV here —

One takeaway from this table is that injury rates are not simply an industry-specific effect. Rather, they actually vary more on a within-industry basis than across industries, though we note that our industry definitions are fairly coarse. As Table III shows, there is considerable variation across industries as well. Another takeaway is that, while there is more variation in injury rates across establishments than within establishments, there is considerable variation within an establishment over time. The standard deviation of injuries per employee within an establishment is 2.0% (for comparison, the mean number of injuries per employee over the full sample is 4.13%). This level of variation gives us power to identify the determinants of injuries even after controlling for establishment fixed effects.

III. Empirical Determinants of Injury Rates

In this section, we examine the determinants of workplace injury rates. We do so primarily by estimating fixed effects regressions of $Injuries/Hour$ on establishment- and firm-level characteristics, with a focus on those characteristics previously shown to relate to a firm's ability to finance investment. Our main regression specification is

$$Injuries/Hour_{it} = \alpha_i + \delta_{jt} + \gamma_{st} + \beta x_{it} + \epsilon_{it}, \tag{1}$$

where t indexes year, i establishment, j industry, and s the state in which the establishment is located. The vector x_{it} consists of establishment-year and firm-year variables, including those relating to financing. Establishment fixed effects α_i account for any time-invariant omitted establishment characteristics, industry-year fixed effects δ_{jt} for any time-variation in injury risk at the industry level (perhaps due to the evolution of production technology or industry growth), and state-year fixed effects γ_{st} for any state-level time-variation in injury risk (perhaps due to changes in the local labor environment or safety laws).

Table V presents results from estimating equation (1). Standard errors clustered at the firm level are shown in parentheses below each point estimate in this and all subsequent tables presenting regression results. Column (1) presents results without any fixed effects, column (2) results with only establishment fixed effects, and column (3) results with all of the fixed effects. Note that the number of observations decreases from 43,371 in column (1) to 25,053 in columns (2) and (3) because the inclusion of establishment fixed effects limits us to establishments appearing in the sample at least twice.

— Insert Table V here —

Column (1) shows that, without fixed effects, injury rates are positively related to $Debt/Assets$, significant at the 5% level, and negatively related to $Cash/Assets$ and $CashFlow/Assets$, significant at the 5% and 10% levels, respectively. These relations are all consistent with a greater ability to finance investment leading to greater workplace safety and hence a lower risk of injury. Columns (2) and (3) show that, once establishment fixed effects are included, injury rates cease to show a relation with $Cash/Assets$ and $CashFlow/Assets$. However, the coefficient on $Debt/Assets$ continues to be positive at the 5% level of statistical significance and increases in size.

The 0.0063 coefficient on $Debt/Assets$ in column (3) implies that a one-standard-deviation increase in leverage (0.219) is associated with a 0.0014 unit increase in $Injuries/Hour$. This represents a modest though nontrivial increase of 5.6% relative to the sample mean $Injuries/Hour$ of 0.0247. For perspective, this effect is larger than existing estimates of the impact of either penalty-imposing OSHA inspections or plant unionization on injury rates (Mendelhoff and Gray (2005)). While the coefficients on $Cash/Assets$ and $CashFlow/Assets$ are not statistically significant in columns (2) and (3), the coefficients in column (1) imply that a one-standard-deviation increase in these variables is associated with a 6.34% and 3.01% increase in $Injuries/Hour$ relative to the sample mean, respectively.

In all three columns, injury rates are negatively related to both $Dividends/Assets$ and $Log(Assets)$. Previous papers argue that a firm's propensity to pay dividends and its size are inverse proxies for the degree to which the firm is financially constrained. While more indirect, these relations also provide support for financing constraints adversely impacting investment in workplace safety.

Investment in safety should be more relevant in industries in which production involves physical assets than in more service-oriented industries. Column (4) reports estimates from the main specification when we restrict the sample to establishments in industries with above-median average $TangibleAssetRatio$. The coefficient on $Debt/Assets$ is 50% higher in column (4) than in column (3). As mean $Injuries/Hour$ is 0.0266 in these establishments, the coefficient of 0.0093 on $Debt/Assets$ implies that a one-standard-deviation increase in $Debt/Assets$ is associated with an increase in $Injuries/Hour$ of 7.66% relative to the mean.

Column (5) presents results using $DAFWInjuries/Hour$ — the rate of more serious injuries — is the dependent variable. The coefficient on $Debt/Assets$ in this regression is 0.023 and is statistically significant at the 5% level. A one-standard-deviation increase in leverage of 0.219 is associated with an increase in $DAFWInjuries/Hour$ of 0.0005, which represents a 6.5% increase relative to the sample mean $DAFWInjuries/Hour$ of 0.0077. Thus, more serious injuries appear to increase meaningfully with leverage.

In the next two regressions, we analyze the extensive and intensive margins of the injury process separately. Column (6) presents results from a linear probability model with establishment, year-industry, and year-state fixed effects. The dependent variable is one if the establishment reports a positive number of injuries in a given year and zero if it reports zero injuries. Not surprisingly, larger establishments (those with more employees) are more likely to experience at least one injury in a given year. Among the firm-level characteristics, the probability of an injury is only statistically significantly related to $Debt/Assets$, with which it has a positive relation.

Column (7) estimates equation (1) for with a positive number of injuries in the given year. As in the full sample (column (3)), the injury rate is positively related to $Debt/Assets$ and negatively related to $Dividends/Assets$ and $Log(Assets)$ for establishments on the intensive margin. These results help allay concerns that the large number of zeros in the injury data might somehow skew the regression results presented in the first three columns.

We further address concerns about the distribution of the injury data by estimating count models, which explicitly account for the nonnegative discrete nature of injuries. The dependent variables in these regressions is the number of injuries rather than the injury rate. We specify an exposure variable, $HoursWorked$, to account for the scale of baseline exposure. Column (8) reports estimates from an establishment fixed effects Poisson model.[13] We also include state-year dummies in the regression, though industry-year dummies are omitted because they cause the estimation routine to fail. The coefficient on $Debt/Assets$ is positive and statistically significant at the 1% level, consistent with the estimates in Table V.[14]

Column (9) presents estimates from a negative binomial model. Unlike the Poisson model, the negative binomial model does not admit fixed effects. However, it does relax the assumption of equal conditional mean and variance that the Poisson model imposes. We are able to include both industry-year and state-year dummies in this model. The coefficient on $Debt/Assets$ is again positive and statistically significant at the 1% level. The coefficient on $Cash/Assets$ is also negative and statistically significant at the 1% level in this regression.[15]

Overall, Table V presents evidence of a robust positive relation between an establishment's injury rate in a given year and parent firm financial leverage at the end of the prior year. We further explore this relation by regressing year t $Injuries/Hour$ on various leads and lags of $Debt/Assets$, controlling for the same variables as in Table V.[16] Table VI presents the results.

— Insert Table VI here —

Column (1) presents results when we include just the first lead and lag of leverage, along with contemporaneous leverage, as explanatory variables. The injury rate is positively related to year $t-1$ $Debt/Assets$. In contrast, it is unrelated to $t+1$ $Debt/Assets$, partially alleviating concerns that the results in Table V might be driven by reverse causality. It is also unrelated to contemporaneous $Debt/Assets$. In column (2), we include the first three leads and lags of $Debt/Assets$. The coefficients on all three lags of $Debt/Assets$ are positive, with those on the first two lags statistically significant at the 5% level. This suggests that any impact of leverage on injury risk persists for at least two years. The coefficients on all three leads of $Debt/Assets$ are small in magnitude and statistically insignificant. Columns (3) and (4) confirm these patterns in Poisson regressions.

Overall, the results in this section provide evidence that financing constraints adversely impact workplace safety, with the consistent relation between injury rates and financial leverage providing the strongest evidence. Controlling for firm-level variables such as $Capex/Assets$ and $TangibleAssetRatio$ helps account for differences in growth and production technology. The inclusion of establishment, industry-year, and state-year fixed effects requires that any omitted variable driving the relationship be time-varying within an establishment, and not purely through industry- or state-level time-variation. Ultimately, however, given the potentially endogenous nature of the explanatory variables with respect to injury rates, the results should not be interpreted as strong evidence of a causal link between leverage and injury risk.[17]

IV. Injury Rates and Cash Flow - Three Quasi-Natural Experiments

In this section, we further explore the effect of financing constraints on injury rates using three quasi-natural experiments involving cash flow shocks. The first experiment exploits a provision in the American Jobs Creation Act (AJCA) of 2004 allowing firms to pay a tax rate of 5.25% on repatriated foreign income on a one-time basis instead of the standard corporate tax rate of 35%. This shock represented a significant windfall for the domestic coffers of firms with profitable foreign subsidiaries. Firms collectively repatriated $312 billion in response to the AJCA according to IRS estimates. Dharmapala, Foley, and Forbes (2011) and Faulkender and Petersen (2011) study the effects of this shock on investment levels in general.

The second experiment exploits the maturity structure of firms' debt at the onset of the financial crisis in late 2007. Credit markets seized up in the U.S. starting in August 2007 and remained tight through 2008, making it difficult for firms to roll over maturing debt. A firm with a lot of debt maturing during this period effectively faced a negative cash flow shock. A firm's maturity structure as of the beginning of the crisis is plausibly exogenous with respect to factors that might affect injury risk, as it was unlikely that firms anticipated the crisis when setting maturity schedules in the preceding years. Almeida et al. (2012) study the effect of this shock on investment levels in 2008.

The third experiment exploits substantial fluctuations in oil prices during our sample period. Oil prices increased from around $25 per barrel in 2002 to over $130 per barrel in 2008, before falling to the $40s in 2009. Higher oil prices increase the cash flow generated by oil producers. Because firms can reallocate capital internally, this increases the cash flow available to any non-oil establishments owned by oil producers. Assuming that oil price movements do not impact these non-oil establishments for other reasons, these shocks can be treated as exogenous with respect to injury rates in these non-oil establishments. Following this logic, Lamont (1997) studies the effect of a 1985 drop in oil prices on investment.

Each of the cash flow shocks involved in the experiments impacted some firms more than others. The AJCA represented a cash flow shock only for firms with previously unrepatriated foreign profits. The financial crisis represented a larger shock for firms with high levels of debt maturing in 2008 than for firms with less debt maturing in 2008. Oil price movements represented a cash flow shock primarily for firms with oil-producing subsidiaries. For the AJCA and financial crisis experiments, we exploit the differential exposure to conduct difference-in-differences analysis. Specifically, we identify establishments exposed to the shock in question ("treated" establishments) and those not exposed ("untreated" establishments). We then match each treated establishment to an untreated establishment, which we refer to as a "control" establishment, to form a matched sample and estimate regressions of the following form using that sample:

$$Injuries/Hour_{it} = \beta Treatment_t * Exposure_i + \phi X_{it} + \alpha_i + \delta_{jt} + \gamma_{st} + \epsilon_{it}, \tag{2}$$

where $Treatment$ equals one for observations after the given shock and zero before, and $Exposure$ equals one for treated establishments and zero for control establishments. We include all of the explanatory variables in the previous section as control variables X_{it}, and also include establishment (α_i), industry-year (δ_{jt}), and state-year (γ_{st}) fixed effects.[18] We follow a similar approach for the oil price experiment, though here we set $Treatment$ to the log of the mean oil price in year t, which is continuous.

If more cash flow results in fewer injuries, β should be negative in the AJCA and oil experiments and positive in the financial crisis experiment. Motivated by the relation between injury rates and leverage documented in the previous section, we also run regressions where we include the interaction of $Debt/Assets$ with $Exposure * Treatment$ in the AJCA and oil experiments to see if the sensitivity of injury rates to cash flow shocks is stronger in firms with more debt.[19]

For the AJCA experiment, we restrict the sample to the two years before and after implementation of the AJCA (2002 to 2003 and 2005 to 2006) to focus on changes around the shock. We set $Treatment$ to one for observations in 2005 and 2006 and to zero for observations in 2002 and 2003. We set $Exposure$ to one if the sum of an establishment's parent-firm foreign profits (Compustat variable *pifo*) from 2001 through 2003 is positive and zero otherwise.[20]

For the financial crisis experiment, we restrict the sample to 2006 to 2008 to focus on the period around the onset of the crisis.[21] We set $Treatment$ to one for observations in 2008 and to zero for observations in 2006 and 2007. Following the approach of Almeida et al. (2012), we constrain our sample to firms with 2007 fiscal year-ends between September 2007 and January 2008, as firms with earlier 2007 fiscal year-ends could have altered their maturity structures before the crisis began.[22] For each firm, we define $DebtDueIn1Year/Assets$ as debt maturing within one year (Compustat $dd1$) as of fiscal year-end 2007 divided by total assets. We then set $Exposure$ to one if the parent firm's $DebtDueIn1Year/Assets$ is at or above the 75th percentile for the sample (0.0304) and zero otherwise. This cutoff is arbitrary, but the results are not sensitive to the choice of cutoff.

We use the full sample period for the oil price experiment, as oil prices moves continuously throughout this period. We set $Treatment$ equal to the natural log of the average oil price in a given year as reported by the U.S. Energy Information Agency. We set $Exposure$ to one if an establishment's parent firm is involved in oil and gas extraction and zero otherwise. We classify a firm as being in the oil and gas extraction business if it has an establishment in two-digit SIC code 13 (Oil and Gas Extraction) in the SOII at any time during the sample period or if it is classified by Capital IQ as being in the oil, gas, and consumable fuels business, excluding coal mining. We then remove establishments with a two-digit SIC code 13 from the sample.[23]

Panel A of Table VII summarizes the characteristics of treated and untreated establishments prior to treatment (since there is no pre-treatment period in the oil experiment, the characteristics for the first year an establishment appears in the sample are shown instead). Two features are worth noting. First, while the treated group is reasonably large in the

AJCA and financial crisis experiments, there are only 150 treated establishments (belonging to only 19 firms) in the oil price experiment. This raises concerns about the external validity of the oil price experiment. Second, in all three experiments, treated and untreated establishments differ substantially on observable dimensions.

— Insert Table VII here —

To minimize these differences, in each experiment we match each treated establishment to an untreated "control" establishment using propensity score matching. In the case of the AJCA and financial crisis experiments, we estimate a probit model where the dependent variable is *Exposure* and the explanatory variables are the regressors from Table V, using only the last observation for each establishment prior to the treatment year. We then fit the probit regression to estimate the propensity to be exposed, and match each treated establishment to the untreated establishment with the closest propensity.[24] We do the same in the case of the oil experiment, except that we use the first observation for each establishment in the sample period for matching because of the lack of a pre-treatment period.

Panel B of Table VII presents summary statistics for treated and matched control establishments in each experiment. The matching process is successful at eliminating differences in observable characteristics in the AJCA and oil experiments. This is important, as the validity of our difference-in-differences approach depends on assignment to treated and control groups being as if random, conditional on observables. While we cannot rule out the possibility that the two groups in these experiments differ on unobservable dimensions, we take comfort in the fact that they are similar on observable dimensions. Matching is less successful at eliminating differences in the financial crisis experiment, raising concerns about nonrandom assignment in this experiment. We return to this issue shortly.

Panel C presents the broad industry breakdown of establishments in the treated and control groups for the AJCA and financial crisis experiments.[25] Differences in industry composition are modest in the AJCA experiment but substantial in the financial crisis experiment. These differences raise further concerns about nonrandom assignment.[26]

Another assumption underlying difference-in-differences estimation is that there are no differences in pre-treatment trends in the outcome variable in treated and untreated establishments. For the AJCA and financial crisis experiments, we assess the validity of this "parallel trends" assumption by plotting the portion of injury rates not explained by other observable variables for treated and control establishments over time. To do so, we estimate equation (2), omitting the interaction of $Treatment$ and $Exposure$ as well as the fixed effects. Figures 3 and 4 plot the annual mean of these residuals from the regression for treated and control establishments in the AJCA and financial crisis experiments, respectively.

— Insert Figures 3 and 4 here —

While pre-treatment trends are almost identical in the AJCA experiment, they differ markedly in the financial crisis experiment, raising further concerns about the validity of this experiment — we return to this issue shortly as well. While there is no pre-treatment period in the oil experiment with which to assess validity of the parallel trends assumption (oil prices change every year), oil prices changed minimally from 2001, the year before the start of our sample period, to 2002 (from $21.84 to $22.51 per barrel). Over the same year, residual injury rates changed from -0.0048 in 2001 to -0.0047 in 2002 for treated establishments and from -0.0014 to -0.0016 for untreated establishments. The difference in change across the two groups is less than 1/10 of the mean absolute difference in annual change over the sample period, providing support for satisfaction of the parallel trends assumption in the oil experiment.[27]

Table VIII presents the difference-in-differences estimates for each of the three experiments (the first three columns), as well as the triple difference estimates involving leverage in the AJCA and oil experiments (the last two columns). Panels A and B present estimates for OLS and Poisson models, respectively. The results are consistent across both classes of models and across all three experiments. The coefficients on $Treatment * Exposure$ in the first three columns are all consistent with an establishment's injury rate falling (rising) after its parent firm receives a positive (negative) cash flow shock. The coefficients on the triple interaction terms in the last two columns are consistent with these responses being stronger in more indebted firms. These coefficients are all statistically significant at the 10% level or better.

— Insert Table VIII here —

As these experiments involve cash flow shocks, we interpret the economic magnitudes implied by the coefficients on $Treatment * Exposure$ in the first three columns of Panel A in terms of the implied effect of a one-standard-deviation shock to a firm's $CashFlow/Assets$. Because we do not observe the size of the actual cash flow shocks involved in these experiments, we must estimate them. Our estimates of economic magnitudes should therefore be interpreted cautiously.

From Table II, the sample standard deviation of $CashFlow/Assets$ is 0.146. Mean three-year cumulative foreign profits as a fraction of assets for treated establishments is 0.0406. Assuming that these profits are available for firms to invest domestically after the AJCA, they represent 27.8% of a one-standard-deviation cash flow shock. The coefficient of -0.0006 on $Treatment * Exposure$ in column (1) then implies that $Injuries/Hour$ decreases by 0.0022 following a one-standard-deviation increase in total cash flow, which represents an 8.7% decrease relative to the mean $Injuries/Hour$ of 0.0247.

Mean $DebtDueIn1Year/Assets$ is 0.0584 for treated establishments and 0.0138 for control establishments. Assuming that no debt could be rolled over in 2008, treated establishments in the financial crisis experiment suffered a cash flow shock scaled by assets of 0.0446 relative to nontreated establishments. This shock represents 30.5% of a one-standard-deviation change in $CashFlow/Assetes$. The coefficient of 0.0007 in column (3) then implies that $Injuries/Hour$ increases by 0.0030 following a one-standard-deviation decrease in total cash flow, which represents an 11.9% increase relative to mean $Injuries/Hour$.

To assess the economic magnitude implied by the coefficient on $Treatment * Exposure$ in the oil price experiment, we first regress $CashFlow/Assets$ on annual log oil price for sample firms in the oil business, controlling for firm fixed effects. The coefficient on log oil price in this regression is 0.056, which is statistically significant at the 1% level. This point estimate, which represents the expected increase in $CashFlow/Assets$ associated with a one-unit increase in log oil price, is 38.4% of the standard deviation of $CashFlow/Assets$. Thus, the coefficient of -0.0008 on $Treatment*Exposure$ in column (4) implies that a one-standard-deviation increase in $CashFlow/Assets$ is associated with a decrease in $Injuries/Hour$ of 0.0021, or 8.4% relative to mean $Injuries/Hour$.

Panels C and D repeat the regressions in Panels A and B, substituting $DAFWInjuries/Hour$ and $DAFWInjuries$, respectively, as the dependent variables. One note here is that the statistical power of these tests is likely to be low given the combination of the small sample sizes in the experiments and the relative infrequency of days-away-from-work injuries. The results are consistent with those in Panels A and B, though only three of the five coefficients of interest in Panel C and two of the five in Panel D are statistically significant. Nevertheless, the table provides at least some evidence that it is not only less serious injuries that decrease (increase) following positive (negative) cash flow shocks.

As noted previously, treated and control establishments differ markedly on observable characteristics in the financial crisis experiment, and the parallel trends assumption appears unlikely to be satisfied in this experiment. To further address these concerns, we next match treated and control establishments on observable characteristics one-at-a-time as well as on the pre-treatment injury rate trend separately and reestimate the Poisson model for each resulting matched sample. We match on pre-treatment trends by computing the pre-treatment annualized change in each establishment's unexplained injury rate as the annualized change in its unexplained injury rate the last two years it appears in the sample pre-2008. Table IX presents the results.

— Insert Table IX here —

The first three columns show that the matching exercise is successful at eliminating differences in each given characteristic. The final two columns show that the coefficient on the interaction of $Crisis$ and $HighDebtDue$ is positive in all of the regressions, and is statistically significant in all but one. This finding provides some comfort that the financial crisis results are not driven by differences in any single observable characteristic.

V. Firm Value and Injury Rates

In this section, we consider the implications of the results in Sections III and IV for optimal financial policy. If firms bear costs from an elevated workplace injury risk, then our results suggest a previously undocumented cost of financial policies, such as high leverage, that increase the likelihood that financing constraints bind in the future. Firms may bear such costs directly through higher health insurance costs, increased downtime, lower productivity, and compensable lawsuits, as well as indirectly through compensating wage differentials that employees demand for exposure to injury risk. To our knowledge, there are no existing estimates of the total costs to a firm of greater injury risk.

We attempt to estimate these costs by examining the relationship between firm value and injury rates at a firm's establishments. We do so by regressing a firm's Tobin's Q on its prior year's injury rate, controlling for a number of firm-level observable characteristics as well as firm and year fixed effects. One challenge is that we observe injuries at the establishment rather than the firm level. To compute a firm-level injury rate for a given year, we add up injuries at all establishments in the BLS data belonging to a firm during the year, and divide this figure by the sum of hours worked at those establishments during the year. We multiply this quotient by 1,000 to make the numbers easier to interpret. We refer to the resulting variable, measured in the prior year, as $LaggedInjuries/Hour$. This is a crude measure of the firm-level injury rate, as establishments participating in the BLS' survey change from year to year. We exclude any establishment appearing in the data only once during our sample period to reduce this noise.[28]

Table X presents the regression results. Columns (1) and (2) report estimates of the sensitivity of Q to lagged injury rate without and with firm-level control variables, respectively. The coefficients on $LaggedInjuries/Hour$ are negative and statistically significant at the 5% level. This is consistent with firms bearing costs from greater injury risk. We are cautious, however, about ascribing causality to this relationship. Despite the inclusion of firm fixed effects and controls in the regressions, it is difficult to rule out the possibility that some omitted variable drives the relation. For example, poorly managed companies are likely to have both high injury rates and low value. We attempt to address these concerns at least in part by examining the lead-lag structure of the relationship between Q and injury rate.

— Insert Table X here —

To do so, we compute $LeadInjuries/Hour$ as next year's firm-level injury rate, and include that in the regressions. The inclusion of $LaggedInjuries/Hour$ and $LeadInjuries/Hour$ in a regression requires that a firm be present three consecutive years in the data to be included in the sample, reducing the sample size from 4,469 to 2,843. Columns (3) and (4) present the results from the augmented regressions. Firm value continues to be negatively related to $LaggedInjuries/Hour$, and the magnitudes of these relations are similar to those shown in the first two columns. Firm value appears to be unrelated to future injury rates

($LeadInjuries/Hour$). While this certainly does not rule out alternative explanations for the relationship between value and injury rates, such explanations would have to also account for higher injury rates predicting lower future firm value but not vice versa.

The mean and standard deviation of the firm-level measure of $LaggedInjuries/Hour$ in our sample are 0.041 and 0.0225, respectively. The coefficient on $LaggedInjuries/Hour$ of -2.715 in column (2) therefore implies that a one-standard-deviation increase in injury rate is associated with a 6.1% decrease in firm value. To further assess the economic importance of the estimates, it is useful to estimate the expected decrease in firm value associated with one additional injury. Observe that $Tobin'sQ = \frac{Value}{Assets}$, where $Value$ is the market value of assets, and that, from the regression equation, $\frac{\partial E[Tobin'sQ]}{\partial Injuries} = \frac{\beta}{HoursWorked}$. Then

$$\frac{\partial E[Value]}{\partial Injuries} = Assets \times \frac{\partial E[Tobin'sQ]}{\partial Injuries} = \frac{\beta \times Assets}{HoursWorked}.$$

We set $Assets$ to \$3.580B, the mean of total assets for the firm-years used in the regressions. We set $HoursWorked$ to the product of the sample mean of $Hours/Employee$ and the sample mean of the number of employees reported by Compustat, or 1,718 $\times$ 16,739 = 28.758M hours. In so doing, we are implicitly assuming that the injury risk for employees of a firm as a whole is the same as the injury risk in the firm's establishments in our sample, which may not be a valid assumption in practice. Our estimate of the change in value per additional injury in the prior year is then $\frac{-2.715 \times \$3.580B}{28.758M} = -\$270,780$.

Workplace injuries are not an i.i.d. process, however, and since injury rates exhibit significant persistence, one additional injury represents a persistent change in future injuries as well. Consequently, this estimate is not attributable to a single injury, but rather to the cumulative discounted cost of this and all future injuries. Estimates from an AR(1) model of injury rates indicate persistence of 0.65 at an annual horizon. Assuming a constant cost per injury over time, the cost per additional injury can be estimated by solving

$$\$270,780 = \sum_{t=-1} \infty \rho^{t+1} \delta^t CostPerInjury = \frac{1 - \delta\rho}{\delta} CostPerInjury,$$

where $\rho = 0.65$ is the rate of persistence and δ is a discount factor. As injury risk is likely to be largely idiosyncratic, future costs should be discounted at approximately the risk-free rate. We therefore set δ to $1 - 0.0151$, where 0.0151 is the mean five-year inflation-indexed Treasury rate over the sample period. This produces an estimated $CostPerInjury$ of \$98,924.

While it is difficult to attribute this cost to specific sources, existing research, primarily focusing on the 1970s and 1980s, finds an average compensating differential of \$20,000 to \$70,000 per additional expected injury (Viscusi and Aldy (2003)). Adjusting for inflation, this translates into between \$50,000 and \$300,000 per injury in 2005 dollars, where 2005 is the midpoint of our sample period. Thus, compensating wage differentials appear to explain a significant portion of our estimates of the total cost to the firm per injury. The remainder is likely attributable to increased downtime, higher insurance premia, and reduced productivity. While individual estimates of these cost components are not available, Danna and Griffin (1999) argue that the cost associated with reduced productivity in particular is likely to be even larger than the compensating wage differential. Ultimately, given all of the assumptions that are required to estimate the cost per injury, the estimates provided here should be taken with a grain of salt. Nevertheless, the estimated costs appear large enough to support the argument that higher expected injury risk represents a significant cost of financial policies that increase the likelihood that financing constraints bind in the future.

VI. Conclusion

In summary, this paper provides new evidence that injury rates increase with leverage and decrease with operating cash flow. We contribute to the literature studying the effect of financing on employee welfare by uncovering evidence of a novel and important channel through which financing can affect the well-being of employees. Our results also have implications for optimal financial policy, as firm value appears to decline with injury rates. Further research in this area should yield additional insights into how firms internalize these costs and how the organizational form of the firm affects worker welfare.

Appendix A. Additional Tables

Table AI
Workplace Injuries and Cash Flow Shocks - Other Oil Price Shock Specifications

This table presents estimates of the effects of cash flow shocks arising from three quasi-natural experiments on injury rates. Panel A reports estimates from OLS models where the dependent variable is $Injuries/Hour$. Panel B reports estimates from Poisson models where the dependent variable is $Injuries$ and the exposure variable is $HoursWorked$. All models include establishment, industry-year, and state-year fixed effects. In the AJCA experiment, the sample is restricted to the years 2002, 2003, 2005, and 2006. $Treatment$ equals one post-2004 and zero pre-2004, and $Exposure$ equals one if the parent firm's cumulative reported foreign profits in 2001-2003 were positive and zero otherwise. In the financial crisis experiment, the sample is restricted to the 2006 to 2008 period. $Treatment$ equals one in 2008 and zero in 2006 and 2007, and $Exposure$ equals one if the parent firm's debt maturing within one year as a percentage of assets os of fiscal year-end 2007 exceeds 0.03064 (the 75th percentile for the sample). The oil price experiment uses all years in the sample (2002 to 2009). The sample consists of only non-oil producing establishments. $Treatment$ equals the natural log of the average oil price for the year, and $Exposure$ equals one if an establishment's parent firm is in the oil business (either has an oil-producing establishment in the BLS data in any year in the sample or is identified by Capital IQ as being in the oil, gas, and consumable fuels business, excluding coal mining) and zero otherwise. In each experiment, treated establishments ($Exposure = 1$) are matched with untreated establishments ($Exposure = 0$) using propensity score matching. See Table VII for information about the characteristics of treated and untreated establishments in each matched sample. Control variables $Debt/Assets$, $Cash/Assets$, $Log(Assets)$, $MarketToBook$, and $TangibleAssetRatio$, $CashFlow/Assets$, $Dividends/Assets$, $AssetTurnover$, $Capex/Assets$, $Log(Employees)$, and $Hours/Employee$ are included in the regressions but omitted from the table for brevity. Standard errors clustered at the firm level are reported in parentheses below each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: OLS Regressions, All Injuries				
	BLS matches only		Exclude fin crisis	
	(1)	(2)	(3)	(4)
Treatment * Exposure	-0.0005	0.0001	-0.0010*	-0.0003
	(0.0004)	(0.0007)	(0.0006)	(0.0009)
Debt/Assets * Treatment		0.0105		0.0148
		(0.0249)		(0.0333)
Debt/Assets * Exposure		0.1211		0.0678
		(0.1460)		(0.1404)
Debt/Assets * Treatment * Exposure		-0.0040**		-0.0024
		(0.0020)		(0.0022)
Observations	923	923	658	658
Adjusted R^2	0.3423	0.3475	0.3186	0.3190

Panel B: Poisson Regressions, All Injuries				
	BLS matches only		Exclude fin crisis	
	(1)	(2)	(3)	(4)
Treatment * Exposure	-0.7887***	0.6205***	-0.3813***	-0.1086
	(0.1370)	(0.2298)	(0.1006)	(0.2210)
Debt/Assets * Treatment		-0.7597*		-1.254**
		(0.4319)		(0.5375)
Debt/Assets * Exposure		-1.3257		6.0173*
		(4.8264)		(3.1114)
Debt/Assets * Treatment * Exposure		-0.7215		-2.3150***
		(1.2448)		(0.7949)
Observations	923	923	658	658
Log likelihood	-2,088	-2,022	-9,431	-9,238

Table AII
Workplace Injuries and Cash Flow Shocks - Matching Within Industry

This table presents estimates of the effects of cash flow shocks arising from three quasi-natural experiments on injury rates. Panel A reports estimates from OLS models where the dependent variable is $Injuries/Hour$. Panel B reports estimates from Poisson models where the dependent variable is $Injuries$ and the exposure variable is $HoursWorked$. All models include establishment, industry-year, and state-year fixed effects. In the AJCA experiment, the sample is restricted to the years 2002, 2003, 2005, and 2006. $Treatment$ equals one post-2004 and zero pre-2004, and *Exposure* equals one if the parent firm's cumulative reported foreign profits in 2001 to 2003 were positive and zero otherwise. In the financial crisis experiment, the sample is restricted to the 2006 to 2008 period. $Treatment$ equals one in 2008 and zero in 2006 and 2007, and *Exposure* equals one if the parent firm's debt maturing within one year as a percentage of assets as of fiscal year-end 2007 exceeds 0.03064 (the 75th percentile for the sample). The oil price experiment uses all years in the sample (2002 to 2009). The sample consists of only non-oil producing establishments. $Treatment$ equals the natural log of the average oil price for the year, and *Exposure* equals one if an establishment's parent firm is in the oil business (either has an oil-producing establishment in the BLS data in any year in the sample or is identified by Capital IQ as being in the oil, gas, and consumable fuels business, excluding coal mining) and zero otherwise. In each experiment, treated establishments ($Exposure = 1$) are matched with untreated establishments ($Exposure = 0$) using propensity score matching. See Table VII for information about the characteristics of treated and untreated establishments in each matched sample. Control variables *Debt/Assets*, *Cash/Assets*, *Log*(*Assets*), $MarketToBook$, and *TangibleAssetRatio*, *CashFlow/Assets*, *Dividends/Assets*, *AssetTurnover*, *Capex/Assets*, *Log*(*Employees*), and *Hours/Employee* are included in the regressions but omitted from the table for brevity. Standard errors clustered at the firm level are reported in parentheses below each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: OLS Regressions, All Injuries

Experiment	AJCA (1)	Fin Crisis (2)	Oil Price (3)	AJCA (4)	Oil Price (5)
Treatment * Exposure	-0.0006	0.0009**	-0.0006*	0.0007	-0.0000
	(0.0005)	(0.0004)	(0.0004)	(0.0005)	(0.0004)
Debt/Assets * Treatment				-0.0031	0.0098
				(0.0059)	(0.0250)
Debt/Assets * Exposure				-0.0040	0.0821
				(0.0172)	(0.1179)
Debt/Assets * Treatment * Exposure				-0.0059*	-0.0034**
				(0.0033)	(0.0017)
Observations	3,796	2,300	1,096	3,796	1,096
Adjusted R^2	0.0776	0.1187	0.3702	0.0785	0.3750

Panel B: Poisson Model Regressions, All Injuries

Experiment	AJCA (1)	Fin Crisis (2)	Oil Price (3)	AJCA (4)	Oil Price (5)
Treatment * Exposure	-0.0737**	0.1395***	-0.3230***	0.2750***	-0.1874
	(0.0338)	(0.0474)	(0.1088)	(0.0757)	(0.4221)
Debt/Assets * Treatment				0.8372***	-5.9375
				(0.1801)	(6.6059)
Debt/Assets * Exposure				-1.5359**	-2.4272*
				(0.4940)	(1.286)
Debt/Assets * Treatment * Exposure				-1.7257***	-0.8350
				(0.2999)	(1.6356)
Observations	3,796	2,300	1,096	3,796	1,096
Log likelihood	-5,790	-2,582	-1,235	-5,681	-1,222

Appendix B. Employment Changes Around the AJCA

To test whether employment grew in a firm's establishments in safer industries relative to its establishments in more dangerous industries after 2004 if it had positive foreign profits, we form a sample of all establishments that are in the data at least once in both the pre- and the post-AJCA periods. For each establishment, we compute the annualized percent change in employment from the pre- to post-AJCA period. For pre-AJCA employment, we use an establishment's 2003 employment if it is available and 2002 if is not. For the post-AJCA period, we use 2005 employment if it is available and 2006 if it is not. We then divide establishments into more or less dangerous establishments depending on whether an establishment's industry mean injury rate is above or below the median industry mean injury rate for all of the parent firm's establishments in the sample.[29] Table BI presents the mean percent change in employment around the AJCA in more and less dangerous establishments separately for firms with and without cumulative foreign profits over the 2001 to 2003 period.

— Insert Table BI here —

Firms with foreign profits appear to reduce employment in safer establishments more than in dangerous establishments after 2004, both in absolute terms and relative to firms without foreign profits, though the differences are not statistically significant. While this does not rule out the possibility of a differential shift in productive activities, such a shift would have to have occurred only within establishments and not (to a detectable degree) across establishments. The lack of an increase in employment overall is consistent with existing conclusions that, despite its intent, the AJCA failed to actually create jobs (e.g., Dharmapala, Foley, and Forbes (2011)).

Table I
Injury Risk Profile Changes Around the American Jobs Creation Act

The table shows the mean percent change in employment in more and less dangerous establishments from the 2002 to 2003 period (pre-AJCA) to the 2005 to 2006 period (post-AJCA) separately for firms with and without foreign profits at the time of the AJCA. We form a sample of all establishments that are in the data at least once in both the pre- and the post-AJCA periods. For each establishment, we compute the annualized percent change in employment from the pre- to post-AJCA period, using 2003 employment if it is available and 2002 if is not, and using 2005 employment if it is available and 2006 if it is not. We divide establishments into more or less dangerous establishments depending on whether an establishment's industry mean injury rate is above or below the median industry mean injury rate for all of the parent firm's establishments in the sample. Those with rates equal to the median of their parent firm are removed from the sample. Differences in percent changes for firms with and without foreign profits are shown to the right. Below them is the difference in these differential changes, with *t*-statistics shown in parentheses.

	Employment change %		
	More Dangerous Establishments	Less Dangerous Establishments	Difference
ForProf>0	-0.3%	-1.1%	0.8%
ForProf≤0	-2.4%	1.9%	-4.3%
Difference			5.1%
			(1.52)

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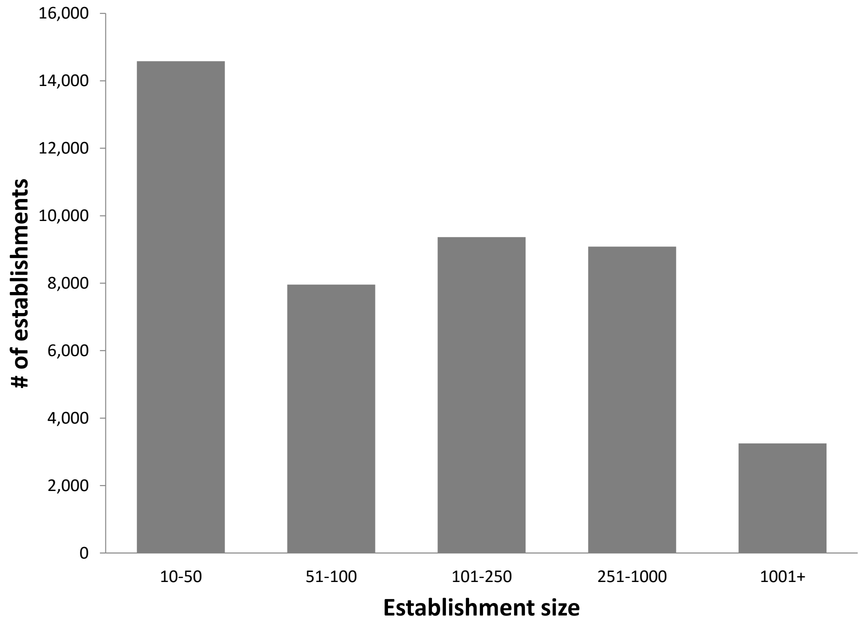

Figure 1. Distribution of establishment sizes. This figure presents the distribution of establishments by number of employees or full-time equivalents for each establishment observation.

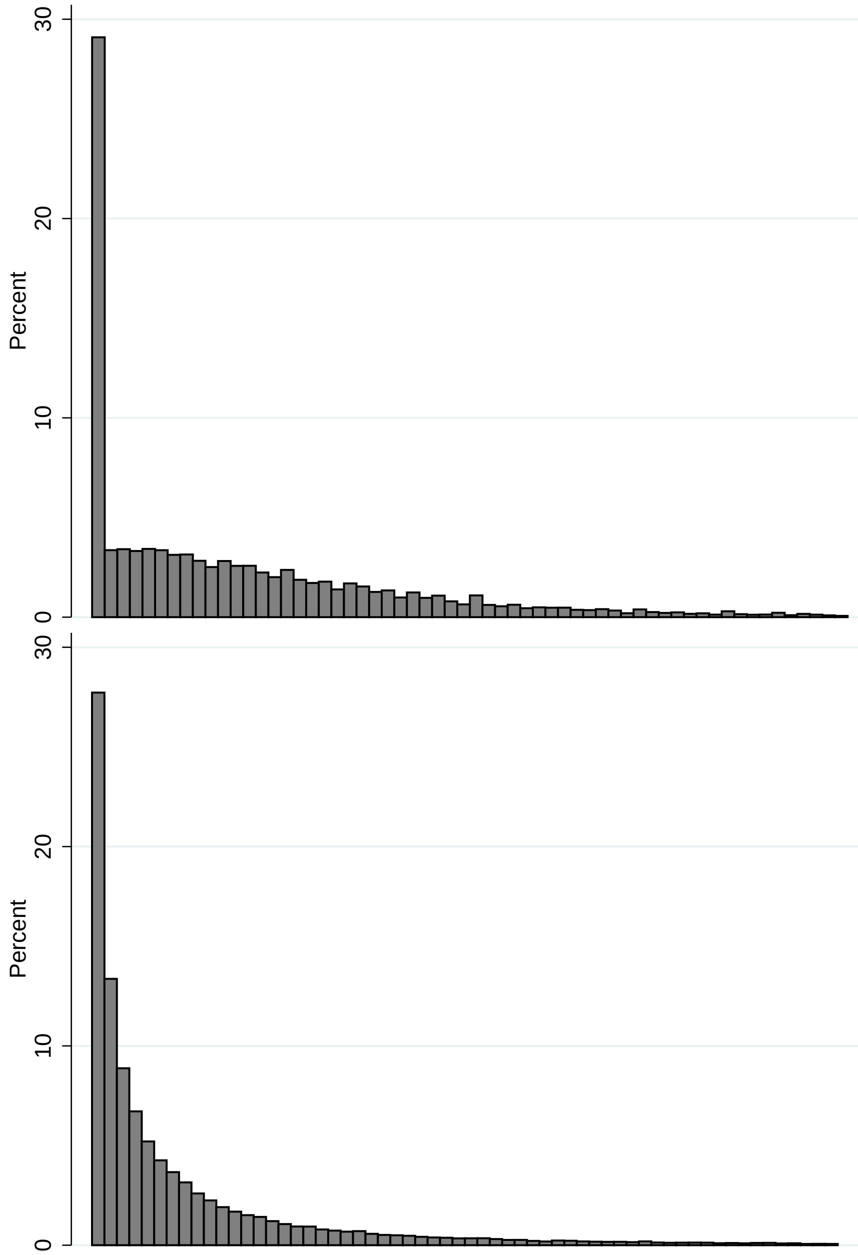

Figure 2. Distributions of injury rates and injury counts. This figure presents histograms showing the distribution of $Injuries/Employee$ (top portion of the figure) and number of injuries (bottom portion). To avoid revealing information about specific establishments, the x-axis is left intentionally unlabeled.

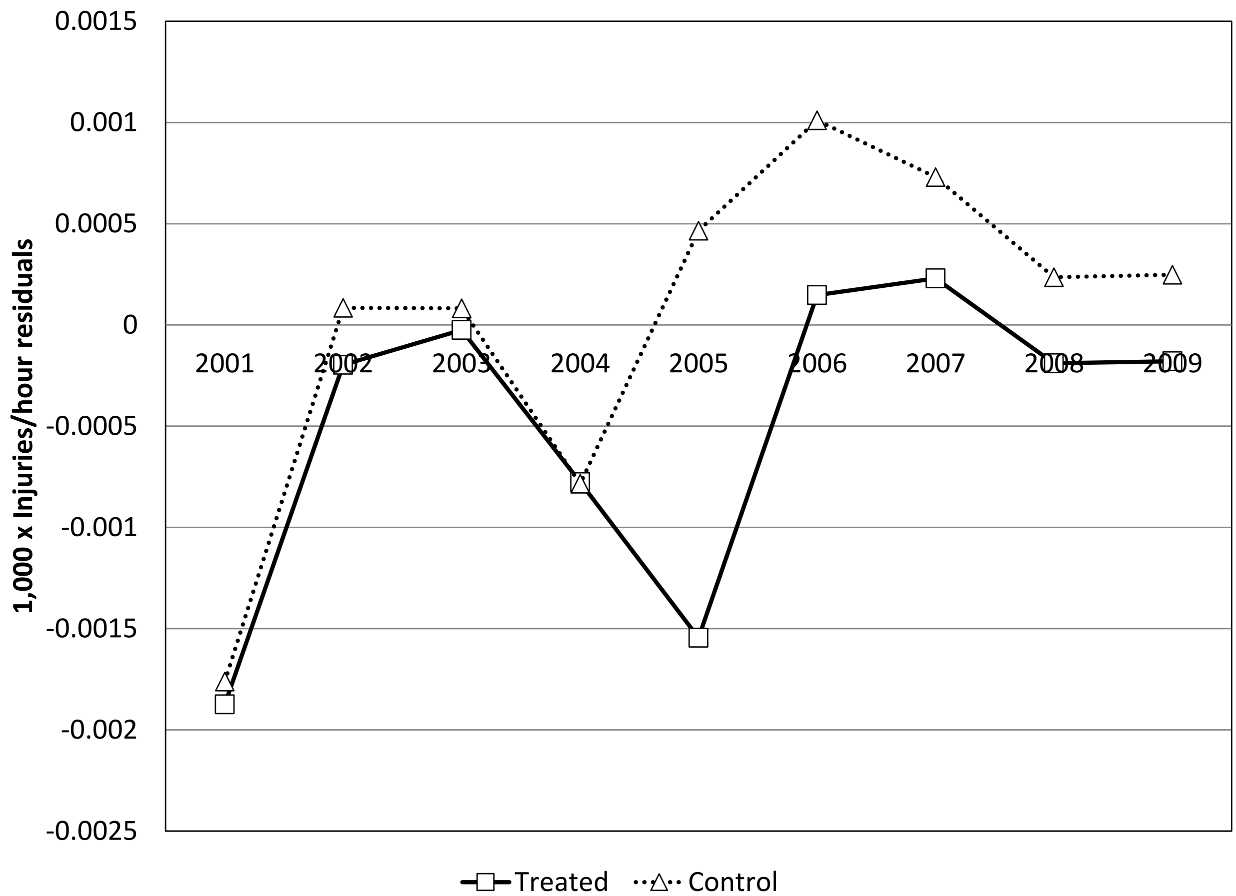

Figure 3. Injury rates over time by foreign profit status. This figure shows the portion of injury rates not explained by other observable firm- and establishment-specific variables over time for the propensity score-matched sample of firms with ("treated") and without ("control") foreign profits as of the AJCA in 2004. These unexplained injury rates are the residuals from an OLS regression of *Injuries/Hour* (times 1,000) on various firm and establishment characteristics. The solid line shows the mean unexplained injury rate for establishments belonging to firms reporting positive cumulative foreign profits over the 2001 to 2003 period. The dotted line shows the mean unexplained injury rate for establishments belonging to firms reporting zero or negative cumulative foreign profits over this period.

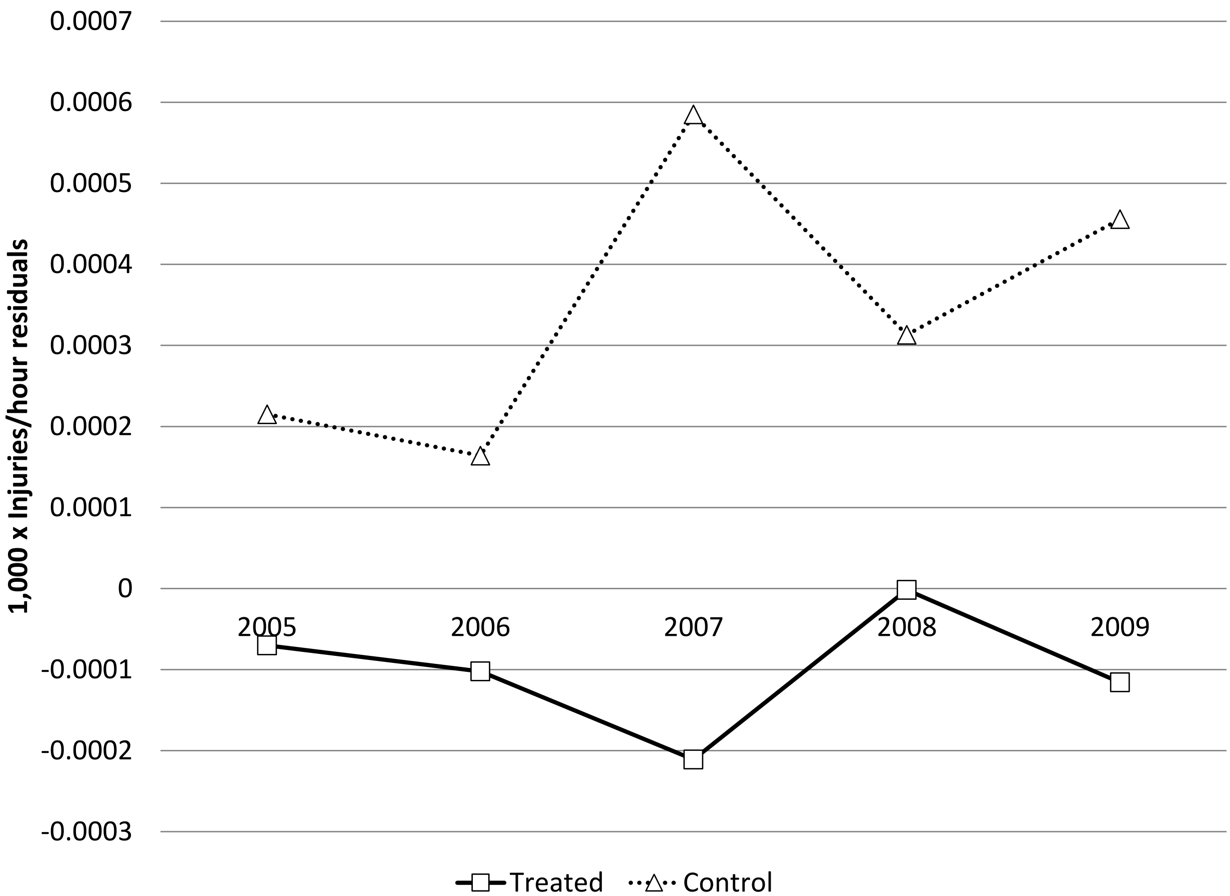

Figure 4. Injury rates over time by debt maturity status This figure shows the portion of injury rates not explained by other observable firm- and establishment-specific variables over time for the propensity score-matched sample of firms with ("treated") and without ("control") a large quantity of debt maturing within one year as of fiscal year-end 2007. These unexplained injury rates are the residuals from an OLS regression of $Injuries/Hour$ (times 1,000) on various firm and establishment characteristics. A firm is defined as having a large quantity of debt due within the next year if debt due within one year as of fiscal year-end 2007 as a percentage of assets exceeds the 75th percentile for the sample (3.064%). The solid line shows the mean unexplained injury rate for establishments belonging to firms with a large quantity of debt maturing within the next year. The dotted line shows the mean unexplained injury rate for establishments belonging to firms with little debt maturing within the next year.

Table I
Injuries by Cause and Type

This table reports the percentage of private sector U.S. on-the-job injuries in 2012 by nature (Panel A) and cause (Panel B), as reported by the BLS. These percentages are computed from incident rates available at http://www.bls.gov/news.release/pdf/osh2.pdf.

Panel A: Percent Injuries by Nature

Nature of injury	Percent
Sprains, strains, tears	38.16
Soreness, pain, including back	14.67
Bruises, contusions	8.33
Fractures	8.03
Cuts, lacerations	8.03
Multiple traumatic injuries and disorders	3.07
Heat (thermal) burns	1.49
Carpal tunnel syndrome	0.89
Amuptations	0.59
Chemical burns	0.40
Tendonitis (other or unspecified)	0.30
All other natures	16.06

Panel B: Percent Injuries by Cause

Cause of injury	Percent
Contact with objects	29.69
Fall on same level	19.56
Overexertion in lifting/lowering	14.44
Violence and other injuries by persons or animal	8.38
Transportation incidents	6.64
Fall to lower level	6.29
Exposure to harmful substances or environments	5.82
Slips or trips without fall	5.47
Repetitive motion	3.49
Fires and explosions	0.23

Table II
Summary Statistics

This table presents summary statistics for the data used in this study. Panel A reports the number of establishment-year observations by year, where an establishment refers to a single location of a company as identified by the BLS. Panel B reports summary statistics for the 43,731 establishment-year observations that we study. *HoursWorked* is the number of hours worked by employees of an establishment during a year. *AverageEmployment* is the average number of employees working at an establishment during a year. *Hours/Employee* is the ratio of the two. *Injuries* is the number of recorded injuries for an establishment in a year. *DAFWInjuries* is the number of days-away-from-work injuries recorded for an establishment in a year. Each of these injury counts is also reported per hour worked and per average number of employees. The per hour rates are multiplied by 1,000 to make them easier to read. Panel C reports summary statistics for the parent-level firm-year observations in our sample. *Debt/Assets* is book debt divided by book assets. *Cash/Assets* is cash and equivalents divided by assets. *CashFlow/Assets* is the sum of income before extraordinary items and depreciation, divided by lagged assets. *Dividends/Assets* is common dividends divided by lagged assets. *Assets* is total reported assets. *AssetTurnover* is sales divided by lagged assets. *MarketToBook* is the ratio of the market value of equity to the book value of equity. *TangibleAssetRatio* is net plant, property, and equipment divided by total assets. *Capex/Assets* is capital expenditures divided by lagged assets. Panel D reports correlations among the explanatory variables used in the paper.

Panel A: Observations by Year

Year	Observations	Percent
2002	5,383	12.31
2003	5,554	12.70
2004	5,191	11.87
2005	5,112	11.69
2006	6,064	13.87
2007	5,802	13.27
2008	5,698	13.03
2009	4,927	11.27

Panel B: Establishment Summary Statistics

	Mean	Std. Dev.
HoursWorked	651,985	2,429,613
AverageEmployment	353	1,260
Hours/Employee	1,718	418
1,000 × Injuries/Hour	0.0247	0.0322
Injuries/Employee	0.0413	0.0529
1,000 × DAFWInjuries/Hour	0.0077	0.0153
DAFWInjuries/Employee	0.0128	0.0249

Panel C: Firm Summary Statistics

	Mean	Std. Dev.	10th pctile	Median	90th pctile
Debt/Assets	0.230	0.219	0.001	0.206	0.445
Cash/Assets	0.121	0.152	0.013	0.069	0.300
CashFlow/Assets	0.102	0.146	-0.012	0.112	0.195
Dividends/Assets	0.014	0.025	0.000	0.007	0.035
Assets (in $millions)	12,457	23,804	260	4,252	27,723
Log(Assets)	8.140	1.864	5.562	8.355	10.230
AssetTurnover	1.690	0.948	0.663	1.512	3.163
MarketToBook	1.675	1.266	0.650	1.389	2.935
TangibleAssetRatio	0.353	0.200	0.110	0.330	0.614
Capex/Assets	0.055	0.068	0.010	0.035	0.113

Panel D: Correlation Matrix

	Debt/ Assets	Cash/ Assets	Cash Flow/ Assets	Divi- dends/ Assets	Log (Ass)	Asset Turn- over	Market ToBook	Tang Asset Ratio	Capex/ Assets	Log (Empl)	Hours/ Empl
Debt/Assets	1.000										
Cash/Assets	-0.261	1.000									
CashFlow/Assets	-0.155	-0.051	1.000								
Dividends/Assets	-0.095	-0.066	0.146	1.000							
Log(Ass)	0.031	-0.181	0.123	0.188	1.000						
AssetTurnover	-0.257	-0.054	0.180	-0.065	-0.077	1.000					
MarketToBook	-0.170	0.218	0.127	0.212	0.026	0.098	1.000				
TangAssetRatio	0.201	-0.251	0.107	0.058	0.184	0.092	0.006	1.000			
Capex/Assets	-0.095	-0.071	0.219	-0.018	-0.026	0.194	0.163	0.432	1.000		
Log(Empl)	0.063	0.014	-0.051	0.045	0.200	-0.218	-0.016	-0.061	-0.168	1.000	
Hours/Empl	0.081	-0.066	-0.086	0.085	0.066	-0.265	0.017	-0.150	-0.200	0.266	1.000

Table III
Injury Rates by Industry

This table shows various mean annual establishment-level injury rates across different industries from 2002 through 2009. An establishment refers to a single location of a company as identified by the BLS. Each industry listed represents one of the Fama-French 48 industries. Nine industry categories (Tobacco Products; Non-Metallic, Industrial Metal Mining; Shipbuilding; Railroad Equipment; Banking; Insurance; Real Estate; and Almost Nothing) are omitted because the small number of establishments in these industries risks revealing the identity of an individual establishment or firm. Industries are sorted from highest *Injuries/Employee* to lowest. See Table II for definitions of the injury rate variables.

Industry	Obs	Injuries/Employee	Injuries/Hour × 1,000	DAFWInjuries/ Employee	DAFWInjuries/Hour × 1,000
Candy & Soda	216	0.0829	0.0418	0.0219	0.0111
Fabricated Products	139	0.0822	0.0405	0.0214	0.0106
Transportation	3,207	0.0771	0.0454	0.0456	0.0271
Automobiles and Trucks	382	0.0685	0.0353	0.0154	0.0081
Steel Works Etc	551	0.0656	0.0313	0.0153	0.0074
Food Products	805	0.0613	0.0298	0.0137	0.0065
Construction Materials	1,364	0.0567	0.0280	0.0125	0.0062
Rubber and Plastic Products	504	0.0535	0.0267	0.0133	0.0066
Electrical Equipment	386	0.0515	0.0260	0.0097	0.0048
Machinery	911	0.0506	0.0253	0.0105	0.0053
Apparel	139	0.0501	0.0305	0.0110	0.0072
Consumer Goods	540	0.0494	0.0255	0.0094	0.0048
Agriculture	216	0.0491	0.0251	0.0120	0.0064
Recreation	88	0.0465	0.0248	0.0085	0.0043
Beer & Liquor	54	0.0447	0.0248	0.0118	0.0064
Restaraunts, Hotels, Motels	1,312	0.0431	0.0313	0.0099	0.0074
Business Supplies	679	0.0415	0.0205	0.0118	0.0059
Personal Services	917	0.0408	0.0252	0.0133	0.0081
Healthcare	502	0.0406	0.0251	0.0108	0.0065
Retail	8,355	0.0403	0.0286	0.0113	0.0081
Shipping Containers	322	0.0380	0.0184	0.0068	0.0033
Wholesale	11,614	0.0374	0.0235	0.0103	0.0064
Construction	183	0.0352	0.0173	0.0112	0.0056
Textiles	163	0.0336	0.0172	0.0054	0.0026
Business Services	2,532	0.0329	0.0179	0.0098	0.0054
Medical Equipment	162	0.0323	0.0166	0.0085	0.0043
Printing and Publishing	359	0.0316	0.0183	0.0090	0.0052
Utilities	47	0.0309	0.0152	0.0090	0.0042
Communication	1,110	0.0308	0.0162	0.0143	0.0076
Pharmaceutical Products	238	0.0301	0.0150	0.0072	0.0036
Aircraft	573	0.0242	0.0120	0.0048	0.0024
Petroleum and Natural Gas	338	0.0239	0.0118	0.0077	0.0038
Measuring and Control Equipment	455	0.0219	0.0112	0.0053	0.0027
Defense	214	0.0213	0.0106	0.0050	0.0025
Chemicals	904	0.0201	0.0097	0.0047	0.0022
Electronic Equipment	1,120	0.0183	0.0093	0.0043	0.0022
Entertainment	999	0.0140	0.0120	0.0031	0.0025
Computers	809	0.0119	0.0060	0.0031	0.0016
Trading	67	0.0110	0.0055	0.0027	0.0014

Table IV
Panel Variance Statistics

This table presents a summary of the relative variation between and within establishment, firm, and industry groups. The first two rows report the mean and standard deviation of the variable for the full sample. The second set of rows reports the standard deviation across different establishments controlling for the time-series mean and within each establishment controlling for the establishment mean. The third set of rows reports the standard deviation between and within firms. The fourth set of rows reports the standard deviation between and within each of the 48 Fama-French industry categories. See Table II for definitions of the injury rate variables.

	Injuries/Hour x 1,000	Injuries/Employee
Overall Mean	0.024	0.041
Overall Std. Dev.	0.032	0.053
Between Establishment	0.033	0.053
Within Establishment	0.013	0.020
Between Firm	0.021	0.037
Within Firm	0.027	0.044
Between Industry	0.010	0.019
Within Industry	0.031	0.050

Table V
Determinants of Injury Rates

This table presents estimates of the relation between establishment-level injury rates and firm and establishment characteristics from a series of regressions. Columns (1) through (7) present OLS estimates. The dependent variable in columns (1) through (4) and (7) is $Injuries/Hour$. The dependent variable in column (5) is $DAFWInjuries/Hour$. The dependent variable in column (6) is an indicator variable equal to one if $Injuries > 0$ and zero if $Injuries = 0$. Columns (8) through (9) present estimates from Poisson and negative binomial models, respectively, where the dependent variable is $Injuries$ and the exposure variable is $HoursWorked$. Only establishments in industries with mean $TangibleAssetsRatio$ above the sample median of 0.276 are used in column (4). The explanatory variables are all measured at the establishment's parent firm level except for *Log*(*Employees*) and *Hours*/*Employee*, which are measured at the establishment level. *Debt*/*Assets*, *Cash*/*Assets*, *Log*(*Assets*), *MarketToBook*, and *TangibleAssetRatio* are lagged one year, while *CashFlow*/*Assets*, *Dividends*/*Assets*, *AssetTurnover*, *Capex*/*Assets*, *Log*(*Employees*), and *Hours*/*Employee* are measured contemporaneously. See Table II for variable definitions. Standard errors clustered at the firm level are reported in parentheses below each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Model	OLS	OLS	OLS	OLS	OLS
Dep variable	Inj/Hour	Inj/Hour	Inj/Hour	Inj/Hour	DAFW/Hour
Sample	All	All	All	High TAR	All
	(1)	(2)	(3)	(4)	(5)
Debt/Assets	0.0047**	0.0075**	0.0063**	0.0093***	0.0023**
	(0.0024)	(0.0036)	(0.0029)	(0.0030)	(0.0011)
Cash/Assets	-0.0103**	-0.0171**	-0.0016	-0.0009	0.0002
	(0.0050)	(0.0068)	(0.0031)	(0.0041)	(0.0011)
CashFlow/Assets	-0.0051*	-0.0033	0.0023	0.0028	0.0003
	(0.0029)	(0.0038)	(0.0033)	(0.0033)	(0.0007)
Dividends/Assets	-0.0139*	-0.0098	-0.0218**	-0.0197**	-0.0004
	(0.0079)	(0.0121)	(0.0088)	(0.0084)	(0.0004)
Log(Assets)	-0.0020***	-0.0010**	-0.0040***	-0.0032***	-0.0010**
	(0.0006)	(0.0005)	(0.0012)	(0.0010)	(0.0005)
AssetTurnover	0.0033***	0.0052***	0.0005	0.0003	0.0002
	(0.0012)	(0.0015)	(0.0010)	(0.0008)	(0.0004)
MarketToBook	-0.0003	-0.0004	0.0002	0.0005	-0.0002
	(0.0006)	(0.0007)	(0.0004)	(0.0003)	(0.0002)
TangibleAssetRatio	0.0267***	0.0376***	0.0106*	0.0098**	0.0105***
	(0.0072)	(0.0090)	(0.0062)	(0.0046)	(0.0026)
Capex/Assets	-0.0397**	-0.0576**	0.0052	0.0071	0.0021
	(0.0160)	(0.0234)	(0.0080)	(0.0065)	(0.0032)
Log(Employees)	0.0007	0.0053***	-0.0006	-0.0013	-0.0002
	(0.0004)	(0.0017)	(0.0009)	(0.0008)	(0.0003)
Hours/Employee	-0.0000***	-0.0000	-0.0000***	-0.0000***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Establishment fixed effects	No	Yes	Yes	Yes	Yes
Year x Industry dummies	No	No	Yes	Yes	Yes
Year x State dummies	No	No	Yes	Yes	Yes
Observations	43,371	43,371	25,053	18,427	25,053
Adjusted R^2	0.0273	0.0490	0.0618	0.0621	0.0231

Table V
Determinants of Injury Rates (Continued)

Model	OLS	OLS	Poisson	Neg Bin
Dep variable	Injuries>0	Inj/Hour	Injuries	Injuries
Sample	All	Inj>0	All	All
	(6)	(7)	(8)	(9)
Debt/Assets	0.0385*	0.0049**	0.3522***	0.4213***
	(0.0208)	(0.0023)	(0.1284)	(0.1374)
Cash/Assets	-0.0406	0.0036	-0.1476	-0.5993***
	(0.0290)	(0.0036)	(0.1625)	(0.1513)
CashFlow/Assets	0.0253	0.0028	0.0736	-0.0610
	(0.0211)	(0.0026)	(0.0998)	(0.0643)
Dividends/Assets	-0.1200	-0.0166**	0.1196	0.8369
	(0.0889)	(0.0084)	(1.1256)	(0.3268)
Log(Assets)	-0.0150	-0.0046***	0.0319	-0.0680***
	(0.0105)	(0.0009)	(0.0217)	(0.0464)
AssetTurnover	0.0069	0.0006	0.0917**	0.1771***
	(0.0098)	(0.0009)	(0.0409)	(0.0513)
MarketToBook	-0.0023	0.0006*	-0.0303**	-0.0187
	(0.0032)	(0.0003)	(0.0136)	(0.0210)
TangibleAssetRatio	0.0063	0.0132***	-0.1409	0.7803***
	(0.0456)	(0.0043)	(0.2256)	(0.2154)
Capex/Assets	-0.0194	0.0004	-0.5553**	-0.6433
	(0.0733)	(0.0107)	(0.2729)	(0.4466)
Log(Employees)	0.1368***	-0.0050***	-0.1198**	-0.0229
	(0.0084)	(0.0008)	(0.0491)	(0.0168)
Hours/Employee	0.0000***	-0.0000***	-0.0004***	-0.0002***
	(0.0000)	(0.0000)	(0.0001)	(0.0010)
Establishment fixed effects	Yes	Yes	Yes	No
Year x Industry dummies	Yes	Yes	No	Yes
Year x State dummies	Yes	Yes	Yes	Yes
Observations	43,371	32,076	25,053	43,731
Adjusted R^2	0.0262	0.0387		
Log likelihood			-54,253	-112,745

Table VI
Injury Rates and Lagged and Future Leverage

This table presents estimates of the relation between establishment-level injury rates and lead and lagged $Debt/Assets$. Columns (1) and (2) present estimates from OLS models where the dependent variable is $Injuries/Hour$. Columns (3) and (4) present estimates from Poisson models, where the dependent variable is $Injuries$ and the exposure variable is $HoursWorked$. The regressions in columns (1) and (3) include only the first lead and lag of $Debt/Assets$, while the regressions in columns (2) and (4) include the first three leads and lags of $Debt/Assets$. Control variables, omitted from the table for brevity, include $Cash/Assets$, $Log(Assets)$, $MarketToBook$, and $TangibleAssetRatio$, which are lagged one year, and $CashFlow/Assets$, $Dividends/Assets$, $AssetTurnover$, $Capex/Assets$, $Log(Employees)$, and $Hours/Employee$, which are measured contemporaneously. See Table II for variable definitions. Standard errors clustered at the firm level are reported in parentheses below each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed z-test.

	OLS		Poisson	
	(1)	(2)	(3)	(4)
Debt/Assets ($t-3$)		0.0033		0.1004
		(0.0057)		(0.1458)
Debt/Assets ($t-2$)		0.0074**		0.2451**
		(0.0038)		(0.1256)
Debt/Assets ($t-1$)	0.0074***	0.0098***	0.3581***	0.2840**
	(0.0025)	(0.0035)	(0.1194)	(0.1375)
Debt/Assets (t)	0.0023	0.0034	0.1954*	0.1245
	(0.0020)	(0.0037)	(0.1203)	(0.1209)
Debt/Assets ($t+1$)	-0.0024	-0.0004	-0.0318	-0.0514
	(0.0021)	(0.0030)	(0.1049)	(0.1205)
Debt/Assets ($t+2$)		0.0044		-0.0123
		(0.0040)		(0.1317)
Debt/Assets ($t+3$)		-0.0010		0.0488
		(0.0047)		(0.1502)
Controls	Yes	Yes	Yes	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes
Industry-year dummies	Yes	Yes	No	No
State-year dummies	Yes	Yes	Yes	Yes
Observations	19,887	14,380	19,887	14,380
Adjusted R^2	0.0684	0.0750		
Log likelihood			-43,201	-43,117

Table VII
Comparison of Treated and Untreated Establishments

This table details various firm and establishment characteristics of treated and untreated establishments in each of the three quasi-natural experiments. An establishment is in the treated group in the AJCA experiment if it reported positive cumulative foreign profits over the three years prior to 2004 (the year of the AJCA), and in the untreated group otherwise. An establishment is in the treated group in the financial crisis experiment if its parent firm had debt due in the next year as of fiscal year end 2007 in the top quartile in the sample, and zero otherwise An establishment is in the treated group in the oil experiment if its parent firm is in the oil production business and zero otherwise. Oil-producing establishments themselves are omitted from the sample. Panel A reports means for all treated and untreated establishments. Panel B reports means for treated establishments and matched control establishments. See Table II for variable definitions. ***, **, and * indicate that a characteristic differs between treated and untreated establishments at the 1%, 5%, and 10% level, respectively, based on a *t*-test. Panel C reports the breakdown of treated and control establishments by broad industry category. The industry breakdown for the oil price experiment is excluded because of disclosure concerns.

Panel A: Characteristics, All observations

	AJCA			Financial Crisis			Oil Price		
	Treated	Untreated		Treated	Untreated		Treated	Untreated	
Establishments	949	1,339		575	1,500		150	7,725	
Debt/Assets	0.252	0.241		0.317	0.207	***	0.207	0.236	*
Cash/Assets	0.089	0.122	***	0.076	0.123	***	0.090	0.110	
CashFlow/Assets	0.100	0.099		0.118	0.121		0.112	0.106	
Dividends/Assets	0.014	0.010	***	0.013	0.021	***	0.022	0.012	***
Log(Assets)	8.537	7.721	***	9.187	8.333	***	8.988	8.214	***
AssetTurnover	1.354	1.706	***	1.636	1.496	***	0.986	1.706	***
MarketToBook	1.617	1.646		1.508	1.810	***	1.279	1.765	***
TangibleAssetRatio	0.317	0.387	***	0.443	0.306	***	0.384	0.367	
Capex/Assets	0.048	0.060	***	0.076	0.062	***	0.067	0.065	
Log(Employees)	5.765	5.200	***	5.350	5.656	***	5.308	5.044	***
Hours/Employee	1,919	1,716	***	1,757	1,906	***	2,084	1,756	***

Panel B: Characteristics, Matched Sample

	AJCA			Financial Crisis			Oil Price		
	Treated	Control		Treated	Control		Treated	Control	
Establishments	949	949		575	575		150	150	
Debt/Assets	0.252	0.263		0.317	0.413	***	0.207	0.186	
Cash/Assets	0.089	0.083		0.076	0.122	***	0.090	0.113	
CashFlow/Assets	0.100	0.093		0.118	0.091	***	0.112	0.114	
Dividends/Assets	0.014	0.012	*	0.013	0.010	***	0.022	0.018	
Log(Assets)	8.537	8.493		9.187	8.590	***	8.988	9.192	
AssetTurnover	1.354	1.330		1.636	1.306	***	0.986	1.002	
MarketToBook	1.617	1.493	*	1.508	1.965	***	1.279	1.327	
TangibleAssetRatio	0.317	0.319		0.443	0.454		0.384	0.342	
Capex/Assets	0.048	0.049		0.076	0.079		0.067	0.064	
Log(Employees)	5.765	5.971		5.350	5.554	*	5.308	5.257	
Hours/Employee	1,919	1,924		1,757	1,741		2,084	2,083	

Panel C: Industry Composition, Matched Sample

	AJCA		Financial Crisis	
	Treated	Control	Treated	Control
Industry category				
Consumer	35.09%	37.83%	59.30%	21.39%
Manufacturing	36.35%	23.92%	10.61%	20.52%
Other	28.56%	38.25%	30.09%	58.08%

Table VIII
Workplace injuries and Cash Flow Shocks

This table presents estimates of the effects of cash flow shocks arising from three quasi-natural experiments on injury rates. Panel A reports estimates from OLS models where the dependent variable is $Injuries/Hour$. Panel B reports estimates from Poisson models where the dependent variable is $Injuries$ and the exposure variable is $HoursWorked$. Panels C and D report estimates from analogous regressions, where $DAFWInjuries/Hour$ and $DAFWInjuries$ are the dependent variables. All models include establishment, industry-year, and state-year fixed effects. In the AJCA experiment, the sample is restricted to the years 2002, 2003, 2005, and 2006. $Treatment$ is one post-2004 and zero pre-2004, and *Exposure* is one if the parent firm's cumulative reported foreign profits in 2001 to 2003 were positive and zero otherwise. In the financial crisis experiment, the sample is restricted to the 2006 to 2008 period. $Treatment$ equals one in 2008 and zero in 2006 and 2007, and *Exposure* equals one if the parent firm's debt maturing within one year as a percentage of assets of fiscal year-end 2007 exceeds 0.03064 (the 75th percentile for the sample). The oil price experiment uses all years in the sample (2002 to 2009). The sample consists of only non-oil producing establishments. $Treatment$ is equal to the natural log of the average oil price for the year, and *Exposure* equals one if an establishment's parent firm is in the oil business (either has an oil-producing establishment in the BLS data in any year in the sample or is identified by Capital IQ as being in the oil, gas, and consumable fuels business, excluding coal mining) and zero otherwise. In each experiment, treated establishments ($Exposure = 1$) are matched with untreated establishments ($Exposure = 0$) using propensity score matching. See Table VII for information about the characteristics of treated and untreated establishments in each matched sample. Control variables *Debt/Assets*, *Cash/Assets*, *Log*(*Assets*), $MarketToBook$, and *TangibleAssetRatio*, $CashFlow/Assets$, $Dividends/Assets$, $AssetTurnover$, $Capex/Assets$, *Log*(*Employees*), and *Hours/Employee* are included in the regressions but omitted from the table for brevity. See Table II for variable definitions. Standard errors clustered at the firm level are reported in parentheses below each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: OLS Regressions, All Injuries

Experiment	AJCA (1)	Fin Crisis (2)	Oil Price (3)	AJCA (4)	Oil Price (5)
Treatment * Exposure	-0.0006*	0.0007**	-0.0008**	0.0006*	-0.0002
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0005)
Debt/Assets * Treatment				-0.0021	0.0125
				(0.0052)	(0.0232)
Debt/Assets * Exposure				-0.0055	0.0820
				(0.0143)	(0.1266)
Debt/Assets * Treatment * Exposure				-0.0063**	-0.0027*
				(0.0030)	(0.0016)
Observations	3,796	2,300	1,096	3,796	1,096
Adjusted R^2	0.0823	0.1254	0.3794	0.1155	0.3803

Panel B: Poisson Model Regressions, All Injuries

Experiment	AJCA (1)	Fin Crisis (2)	Oil Price (3)	AJCA (4)	Oil Price (5)
Treatment * Exposure	-0.0719**	0.0685**	-0.4107***	0.2253***	-0.0120***
	(0.0338)	(0.0347)	(0.0773)	(0.0774)	(0.0035)
Debt/Assets * Treatment				0.2035*	-2.8345***
				(0.1147)	(0.9177)
Debt/Assets * Exposure				0.9826**	-0.0276***
				(0.4436)	(0.0094)
Debt/Assets * Treatment * Exposure				-0.8259***	-1.6321*
				(0.2513)	(0.8989)
Observations	3,796	2,300	1,096	3,796	1,096
Log likelihood	-6,328	-2,896	-2,355	-6,198	-2,304

Table VIII
Workplace injuries and Cash Flow Shocks (Continued)

Panel C: OLS Regressions, DAFW Injuries

Experiment	AJCA (1)	Fin Crisis (2)	Oil Price (3)	AJCA (4)	Oil Price (5)
Treatment * Exposure	-0.0034	0.0007	-0.0030*	0.0010	-0.0001
	(0.0038)	(0.0035)	(0.0019)	(0.0035)	(0.0024)
Debt/Assets * Treatment				-0.0022	0.0058
				(0.0021)	(0.0082)
Debt/Assets * Exposure				-0.0037	0.0535
				(0.0059)	(0.0480)
Debt/Assets * Treatment * Exposure				-0.0201*	-0.0158*
				(0.0120)	(0.0089)
Observations	3,796	2,300	1,096	3,796	1,096
Adjusted R^2	0.0324	0.0420	0.1097	0.0338	0.1100

Panel D: Poisson Model Regressions, DAFW Injuries

Experiment	AJCA (1)	Fin Crisis (2)	Oil Price (3)	AJCA (4)	Oil Price (5)
Treatment * Exposure	-0.1157**	0.0006	-0.5055***	0.1998*	-0.3259
	(0.0534)	(0.0047)	(0.1420)	(0.1091)	(0.3663)
Debt/Assets * Treatment				-0.3004	-0.8264
				(0.1855)	(0.6793)
Debt/Assets * Exposure				2.3180***	3.4785
				(0.5619)	(9.9183)
Debt/Assets * Treatment * Exposure				-0.1902	-1.2850
				(0.3232)	(2.5876)
Observations	3,796	2,300	1,096	3,324	1,096
Log likelihood	-3,250	-1,730	-1,136	-3,216	-1,131

Table IX
Workplace Injuries and Debt Maturity During the Financial Crisis: Characteristic-by-Characteristic Matching

This table presents estimates of the effect of having a large quantity of debt maturing at the onset of the financial crisis on injury rates using a series of samples matched on individual firm or establishment characteristics. The first column identifies the matching characteristic. The second shows the mean value of the characteristic for firms in the treated group. The third shows the mean value of the characteristic for firms in the untreated group. The third column shows the t-statistic for the difference in means. The fourth and fifth columns show the coefficient on $Treatment * Expsoure$ and its standard error clustered at the firm level from the OLS fixed effects regression in Table VIII, Panel A, column (2). Injury rate trend is the annualized pre-2008 change in the portion of an establishment's injury rate not explained by other firm and establishment characteristics. See Table II for definitions of the other variables. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed z-test.

Matched	Characteristic mean			Crisis * HighDebtDue	
characteristic	Treated	Control	t_{diff}	Coeff	Std Err
Debt/Assets	0.317	0.317	[0.041]	0.0012***	(0.0004)
Cash/Assets	0.076	0.077	[0.066]	0.0012***	(0.0003)
CashFlow/Assets	0.118	0.116	[0.407]	0.0009**	(0.0004)
Dividends/Assets	0.013	0.013	[0.031]	0.0015***	(0.0005)
Log(Assets)	9.187	9.185	[0.019]	0.0013***	(0.0003)
AssetTurnover	1.635	1.634	[0.040]	0.0010**	(0.0005)
MarketToBook	1.508	1.506	[0.043]	0.0007*	(0.0004)
TangibleAssetRatio	0.443	0.442	[0.038]	0.0015**	(0.0006)
Capex/Assets	0.075	0.076	[0.072]	0.0003	(0.0004)
Log(Employees)	5.350	5.349	[0.008]	0.0012***	(0.0003)
Hours/Employee	1,757	1,757	[0.002]	0.0011***	(0.0004)
Injury Rate Trend	0.0013	0.0013	[0.048]	0.0007**	(0.0003)

Table X
Firm Value and Workplace Injuries

This table presents results from OLS regressions of firm value on injury rates. The dependent variable in each model is a firm's Tobin's Q for the give year, where Tobin's Q is calculated as the sum of the market value of equity and book value of debt less deferred taxes, divided by total assets. *LaggedInjuries/Hour* and *LaggedInjuries/Hour* are injuries per hour worked the year before and the year after, respectively, across all of a firm's establishments in the BLS data, multiplied by 1,000. See Table II for definitions of all of the other explanatory variables. The control variables include *Debt/Assets*, *Cash/Assets*, *Log(Assets)*, *MarketToBook*, and *TangibleAssetRatio*, which are lagged one year, and *CashFlow/Assets*, *Dividends/Assets*, *AssetTurnover*, *Capex/Assets*, *Log(Employees)*, and *Hours/Employee*, which are measured contemporaneously. All regressions include firm and year fixed effects. Standard errors clustered at the firm level are reported in parentheses below each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed *t*-test.

	(1)	(2)	(3)	(4)
LaggedInjuries/Hour	-3.190***	-2.715**	-3.524**	-3.079**
	(1.164)	(1.129)	(1.452)	(1.369)
LeadInjuries/Hour			-0.414	-0.036
			(0.950)	(0.871)
Debt/Assets		-0.047		0.448*
		(0.438)		(0.251)
Cash/Assets		0.511***		0.362
		(0.192)		(0.292)
CashFlow/Assets		0.636		1.281***
		(0.501)		(0.319)
Dividends/Assets		0.353		0.008
		(1.198)		(1.464)
Log(Assets)		-0.476***		-0.383***
		(0.132)		(0.107)
AssetTurnover		0.106		0.025
		(0.102)		(0.098)
TangibleAssetRatio		-0.891*		-2.032***
		(0.470)		(0.687)
Capex/Assets		0.709**		1.312***
		(0.352)		(0.489)
Establishment fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	4,469	4,469	2,843	2,843
Adjusted R^2	0.1000	0.1571	0.1308	0.2021

Notes

[1]Dharmapala, Foley, and Forbes (2011) and Faulkender and Petersen (2012) examine the effect of the repatriation tax holiday, Almeida et al. (2012) study the effect of the onset of the financial crisis, and Lamont (1997) studies the effect of an oil price shock in 1985.

[2]Danna and Griffin (1999) argue that these costs are likely to be greater than those due to compensating wage differentials.

[3]In other related papers, Bae, Kang, and Wang (2011) find that firms with more debt score lower on a third party rating of employee friendliness, and Brown and Matsa (2013) show that firms in financial distress have fewer and lower quality job applicants.

[4]Lockout procedures involve isolating and disabling power sources in dangerous machinery in a systematic, step-by-step way. Tagout procedures ensure that only specific employees can unlock and untag a machine, ensuring that malfunctioning equipment is not accidentally brought back online before it is repaired.

[5]Source: http://stats.bls.gov/news.release/archives/osh2_11262013.pdf.

[6]Source: http://www.mysanantonio.com/news/energy/article/Eagle-Ford-pay-is-high-but-work-can-be-fatal-4285405.php.

[7]Source: http://www.csb.gov/assets/1/19/CSBFinalReportBP.pdf.

[8]See DuPont case study on Norfolk Southern: http://www2.dupont.com/Sustainable_Solutions/en_US/assets/downloads/case_studies/NorfolkSouthern_CaseStudy.pdf.

[9]While regulatory safety inspections and penalties could force firms to bear more of the cost of workplace hazards in the short run, OSHA and its state affiliates inspected less than 1.2% of worksites in the U.S. in 2012, according to OSHA's website. Firms with high safety standards may also cut spending on safety in ways that do not trigger formal violations of safety rules when financially constrained.

[10]See http://blogs.hbr.org/2010/06/the-safety-calculus-after-bp/ for a discussion of this last issue.

[11]We obtain similar results throughout if we compute injury rates per employee rather than per hour worked, but overall exposure to injury risk is ultimately a function of the number of hours that employees spend working.

[12]We obtain similar results throughout if we measure firm size using total sales or total employees.

[13]As in an OLS model, the fixed effects allow each unit of observation to have a different baseline-level injury rate.

[14]The economic magnitude of a coefficient from a Poisson model can be assessed by examining the incident rate ratio associated with the coefficient, $e^{\beta} - 1$. This represents the expected percentage point change in

injury count per unit change in an explanatory variable, and is 0.422 for the $Debt/Assets$ coefficient. A one-standard-deviation increase in $Debt/Assets$ (0.219) then is associated with an 9.2 percentage point increase in expected injuries in the following year, somewhat larger than the association implied by the OLS coefficients in Table V.

[15]The negative binomial model produces an α parameter estimate of 0.729, which is statistically different than zero at the 1% level, suggesting that the Poisson model's assumption of equal mean and variance is likely violated. Violation of this assumption does not bias model Poisson estimates, but does reduce their efficiency (Wooldridge (2002), ch. 19).

[16]Note that the additional data requirement lowers the number of usable observations.

[17]One additional cautionary note in interpreting the results of the regressions in this section is that correlation among the explanatory variables combined with possible measurement error could produce biases. In particular, $Log(Employees)$ and $Hours/Employee$ are subject to this concern, as average employment and hours worked are self-reported by the establishments in the BLS survey and, unlike the firm-level variables, are unaudited. As Table II shows, these variables have little correlation with most of the firm-level variables, especially those relating most directly to a firm's financial resources.

[18]Note that the main effects of $Exposure$ and $Treatment$ are fully absorbed by the establishment and interacted year fixed effects, respectively.

[19]We do not run this test for the financial crisis experiment, as exposure to treatment itself is based on a firm's capital structure.

[20]We choose a three-year window because it is long enough to reliably measure recent foreign profitability while avoiding foreign profits from the distant past that may no longer reside in a foreign subsidiary. Our results are robust to alternative windows for cumulating foreign profits. Establishments for which $PosFP = 0$ include establishments of firms with foreign losses over the 2001 to 2003 period and those with no foreign subsidiaries, with approximately 95% being comprised of the latter. We obtain similar results if we exclude firms with foreign losses over the 2001 to 2003 period from our sample.

[21]We obtain similar results if we extend the sample period back to 2005.

[22]Approximately 80% of firms have 2007 fiscal year-ends between September 2007 and January 2008.

[23]The conclusions of our analysis are unchanged if we use only the BLS data to classify firms in the oil business or if we exclude the financial crisis period (see Appendix Table AI).

[24]If multiple potential matches have the same propensity score, we randomly choose one.

[25]We are not able to show the breakdown using more narrowly defined industries because of disclosure

concerns. We also cannot show the industry breakdown for the oil price experiment because the small number of firms and establishments in each category raises disclosure concerns.

[26]In our main analysis, we do not require matched establishments to be in the same industry as doing so would greatly limit the number of possible matches for many establishments, making it difficult to match precisely on other observables. However, in Appendix Table AII, we show that the results are similar to those presented here if we do require within-industry matching.

[27]One concern with the AJCA experiment is that the multinationals exposed to the AJCA shock might have invested repatriated cash disproportionately in safer activities, with more dangerous activities outsourced overseas. We would ideally like to measure the effect of a cash windfall on an employee's injury risk, holding fixed the activities in which the employee is engaged. While we do not observe the mix of activities within an establishment, in Appendix B, we show that firms with foreign profits do not shift employment towards establishments in safer industries after 2004. This finding provides some comfort that shifts in activities do not drive the results that follow.

[28]This filter shrinks the usable sample of firms from 5,471 to 4,469. The results are similar if we do not exclude these establishments.

[29]If a firm has an odd number of establishments in the data both before and after the AJCA, we discard the establishment with the median industry median injury rate.