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FIRM-LEVEL FINANCIAL RESOURCES AND ENVIRONMENTAL SPILLS

Jonathan Cohn
Tatyana Deryugina

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ABSTRACT

Using novel US environmental spill data, we document a robust negative relationship between the number of spills a firm experiences in a given year and its contemporaneous and lagged (but not future) cash flow. In addition, studying two natural experiments, we find an increase (decrease) in spills following negative (positive) shocks to a firm's financial resources, both in absolute terms and relative to control firms. Overall, our results suggest that firms' financial resources play an important role in their ability to mitigate environmental risk.

Jonathan Cohn
University of Texas at Austin
McCombs School of Business
Austin, TX 78712
jonathan.cohn@mccombs.utexas.edu

Tatyana Deryugina
Department of Finance
University of Illinois at Urbana-Champaign
515 East Gregory Drive, MC-520
Champaign, IL 61820
and NBER
deryugin@illinois.edu

There are approximately 30,000 reported environmental spills in the US and its coastal waters each year. These range from small leaks to catastrophic discharges of hazardous materials such as oil that cost billions of dollars to clean up, inflict billions more in economic damage on affected areas, and sometimes cause injury or death (Cohen, 2010). Companies handling oil, gas, and other hazardous materials in the course of their operations spend billions of dollars per year in an effort to minimize the risk of such spills (Lux Research, 2013). However, in capital-constrained companies, these activities compete with investment in marketing, expanding capacity, and developing new products. In order to preserve financing capacity for mission-critical investments, constrained companies may cut corners on activities that mitigate spill risk, creating a potential dependence of the incidence of spills on a company's available financial resources. This paper explores that dependence empirically by combining corporate financial information with novel incident-level environmental spill data from the US.

Our empirical analysis takes two forms. First, we use multivariate regression analysis to examine the empirical determinants of environmental spill incidence. Controlling for a host of firm characteristics as well as firm and industry-year fixed effects, we find a consistent negative relationship between a firm's current and lagged cash flow and the occurrence of spills for which that firm is responsible. Our estimates imply that a one-standard deviation increase in cash flow is associated with a 2.8% decrease in the number of contemporaneous spills and a 2.3% decrease in the number of spills the following year, relative to the sample mean. We also find some evidence that this relationship is stronger for firms for which cash constraints are likely tighter - firms with high levels of debt and, especially, smaller firms.

We further explore the relationship between environmental spill risk and cash flow by exploiting detailed information about each spill in our sample. We find that the relationship between spill risk and prior-year cash flow holds not only for spills in general, but also for spills resulting in evacuations or injuries specifically - i.e., for high-impact spills. In addition, we find that spills attributed to operator error are more closely related to contemporaneous cash flow, while those attributed to equipment failure are more closely related to lagged cash flow. While speculative, it seems plausible that reduced investment in activities that directly affect operator error risk such as training, supervision, and proper staffing would manifest itself in higher spill risk quickly, while reduced investment in equipment maintenance would take more time to do so.

Although we focus primarily on the impact of financial resource availability on spill risk, we also find a positive relationship between spill rates and both contemporaneous and lagged sales growth. This finding coheres with the idea that investments in activities that mitigate spill risk compete for scarce resources with other productive activities, as competition for resources is likely to be most intense in fast-growing firms. In addition, we find a positive relationship between spill risk and financial leverage (net debt-to-assets ratio), a factor that previous papers have linked to difficulties in financing investment (Denis and Denis, 1993; Lang et al., 1996). However, this relationship is not statistically significant at conventional levels.

While these results are consistent with greater financial resources enabling firms to spend more to prevent spills, they are also open to alternative interpretations. For example, poor operational management may result in both low levels of cash flow and high levels of spill risk. There may also be quasi-mechanical relationships between spills and cash flow, as the incidence of spills and spending to mitigate spill risk can both reduce cash flow in the short run. Our second approach addresses the identification challenge by isolating variation in cash flow that should be related to the incidence of spills only through its impact on the resources available to spend on spill prevention. Specifically, we examine (1) negative cash flow shocks due to the difficulty firms faced in rolling over maturing debt during the financial crisis starting in late 2007 and (2) positive cash flow shocks to the US operations of multinational firms due to a one-time tax holiday allowing accumulated overseas profits to be repatriated at an exceptionally low tax rate.[1]

We exploit firms' differential exposure to these shocks and conduct a difference-in-differences analysis, comparing the change in spill incidence around the time of these shocks for firms more exposed to them to that of observationally-similar control firms that were either less exposed or completely unexposed. The results provide further support for a causal link between financial resources and spill occurrence: Firms exposed to a negative (positive) cash flow shock experience an increase (decrease) in spill incidence in the second and third years after the shock. The short lag in response seems reasonable if investments in averting spills take time to affect spill risk. The difference-in-differences estimates imply a considerably larger response of spills to financial shocks than our baseline multivariate regression analysis does, though the estimates are also less precise. While each piece of evidence we present could have alternative interpretations, the evidence taken together points to a dependence of a firm's environmental spill risk on its internal financial resources.

Our results contribute to our understanding of how firms manage risks associated with handling hazardous materials. They suggest that firms devote fewer resources to managing these risks when they have less capacity to finance investment in these resources, potentially imposing externalities on the communities in which they operate. This implication in turn suggests that minimizing capital-raising frictions that create a dependence on internally-generated cash flow to finance investment can generate previously-unrecognized social benefits. Our results also have implications for regulators, as they suggest a factor that can be used to identify firms more likely to experience spills in the near future.

Ours is not the first paper to examine the impact of changes in a firm's ability to finance investment on the well-being of stakeholders beyond its investors. Rose (1990) and Dionne et al. (1997) find that airlines are more likely to experience accidents when their operating

[1] Almeida et al. (2012) study the effect of the onset of the financial crisis on capital investment of firms with high levels of debt maturing, while Dharmapala et al. (2011) and Faulkender and Petersen (2011) examine the effect of the repatriation tax holiday on capital investment.

margins are lower, while Phillips and Sertsios (2013) find that airlines' mishandled baggage and late arrival rates are higher when they are financially distressed. Cohn and Wardlaw (2016) present evidence that workplace injury rates are higher in firms with more debt and increase (decrease) in response to negative (positive) cash flow shocks. Relatedly, Nie and Zhao (2015) find that Chinese coal mining companies have higher workplace fatality rates when they have more debt. Finally, Kini et al. (2017) find that firms with higher debt loads are more likely to issue product recalls.

These studies all focus on how financing impacts important but narrowly-defined sets of stakeholders - employees and customers. To our knowledge, ours is the first study to explicitly consider the impact of a firm's financial resources on the broader community in which it operates. While employees and customers can mitigate costs they face due to a firm's lack of financing by severing their relationship with the firm, communities cannot generally sever these ties, making it difficult for them to mitigate the threat of environmental damage.

To our knowledge, ours is also the first paper to examine firm-level determinants of environmental risk. By contrast, other papers considering the relationship between environmental performance and firm financial health focus exclusively on trying to identify the effect of environmental performance on firm profitability (see Horváthová (2010) for a review). Another related body of literature considers firm-level determinants of participation in voluntary environmental programs and of corporate social responsibility initiatives (see Reinhardt and Stavins (2010) for a review). Other work studies the effects of regulation and market structure on environmental safety, abstracting from firm-level attributes. For example, Galiani et al. (2005) find that water privatization in Argentina decreased child mortality, Hausman (2014) finds improvements in nuclear plants' safety following electricity market restructuring and divestiture, and Boomhower (2016) finds that insurance requirements reduced oil and gas firms' production but also improved their environmental performance. Our paper is distinct in that we focus on market frictions rather than government regulation. Finally, our finding that firms pursuing aggressive growth experience higher spill rates lines up with evidence of a more general trade-off between economic activity and safety at the country level (Jones, 2016) and with concerns about the harmful environmental impacts of the recent boom in hydraulic fracturing (Hill, 2013; Adgate et al., 2014; Jackson et al., 2014).

The remainder of the paper proceeds as follows. Section I discusses several US laws relevant to environmental spills and outlines a conceptual framework for understanding how financing frictions can affect spill risk. We present our empirical methodology in Section II. In Section III, we describe the data and sample. Sections IV and V present the paper's empirical results. Finally, Section VI concludes.

I. Environmental Spills, Risk Management, and Financing

A. Laws governing hazardous material discharge in the United States

Numerous US laws and regulations govern how firms must handle hazardous materials. Some of these regulations specify the precautions firms must take to prevent accidental discharge of such materials as well as the consequences for failing to do so. Here, we briefly discuss major federal legislation relevant to the spills in our sample: Section 311 of the Clean Water Act (CWA), which covers discharge of hazardous substances into water, the Oil Pollution Act of 1990 (OPA), which cover oil spills, and the Resource Conservation and Recovery Act (RCRA), which covers a broad range of hazardous materials as well as solid waste.[2] Individual states can and do implement laws that are at least as strict as federal ones, but a review of state legislation is beyond the scope of this paper.

[2]A related regulation, the Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA), also known as "Superfund", covers abandoned hazardous waste sites and is less likely to be relevant to our setting.

Section 311 of the CWA, titled “Oil and hazardous substance liability,” states that the owner or operator of a facility or vessel from which a hazardous substance was unlawfully discharged is liable for the full amount of cleanup costs, including the cost of restoring natural resources and mitigating damages. In cases where the responsible partly is found to be negligent, other penalties may be levied. The responsible party is also required to notify the US government whenever it becomes aware of a prohibited discharge. With the exception of oil, the CWA leaves it up to the Environmental Protection Agency (EPA) to determine which substances are hazardous when discharged into water.

The OPA, passed in 1990, amended the CWA in several important ways. First, it introduced new requirements focused on oil spill prevention, including for vessel construction and crew qualifications. It also broadened the definition of damages and increased the liability limits and civil penalties for violating Section 311 of the CWA. Finally, the OPA established a fund to finance quick oil removal and to pay compensation for damages in cases where the responsible party was not found to be negligent.

The RCRA, which was enacted in 1976, covers all hazardous materials not regulated by other acts and a wide range of non-hazardous solid waste. Importantly, the RCRA is not limited to water discharges and gives the EPA broad powers to regulate hazardous waste, including its generation, transportation, storage, and disposal. In cases where hazardous substances are released, the EPA or a state authority must compel the responsible party to take corrective action.

A key theme across these and other regulations is that those responsible for discharging hazardous materials are fully liable for cleanup costs and, in some cases, for any other damages caused by the spill. Fines may also be levied on a firm acting negligently. It is thus important to recognize that spills are not costless to firms, which we proceed to do in the conceptual framework below.

B. Conceptual Framework

Corporations invest billions of dollars per year to reduce the risk of environmental spills. As an example of the consequences of failing to make such investments, a methyl isocyanate (MIC) gas leak at Union Carbide's pesticide plant outside Bhopal, Madhya Pradesh, India, killed an estimated 2,259 people immediately and is blamed for thousands more deaths over time. Many experts attribute the "Bhopal disaster" directly to budget cuts that limited investment in activities that might have prevented a major spill. For example, a history of the disaster concludes that "cuts ... meant less stringent quality control and thus looser safety rules. A pipe leaked? Don't replace it, employees said they were told ... MIC workers needed more training? They could do with less. Promotions were halted, seriously affecting employee morale and driving some of the most skilled ... elsewhere" (Kurzman, 1987).

As with all forms of investment, a firm must finance any investment in reducing spill risk either out of internal cash flow or by raising external capital. We now develop a simple conceptual framework for illustrating how shocks to financial resources affect the risk of environmental spills in financially-constrained firms. Consider a firm choosing how much to invest in two technologies - a production technology that generates cash flow and a technology that mitigates the risk of environmental spills, which reduce cash flow when they occur. An investment of K in the production technology yields a cash flow of $F(K, z)$, where z is a productivity parameter reflecting the marginal return to investment in production. Production has declining returns to scale - that is, $F_1 > 0$ and $F_{11} < 0$. A higher value of z implies higher productivity - that is, $F_{12} > 0$. An investment of M in the spill-risk mitigation technology results in a spill probability equal to $P(M)$, with $0 \leq P(M) < 1$, $P_1 < 0$, and $P_{11} > 0$. The firm's total investment is denoted by $I = K + M$.

The firm must finance its total investment of I out of internal and external funds. The firm has internal funds of W, which carry a unit opportunity cost of 1. The firm bears a deadweight capital-raising cost of $C(E)$ when it raises external funds of E, with $C_1(0) = 0$,

$C_1 > 0$ for $E > 0$, and $C_{11} > 0$. To make the solution non-trivial, we assume that the firm lacks the internal resources to finance all of its preferred level of investment - i.e., the level at which it would invest if it had unlimited financial resources.[3] This assumption, combined with the assumption that $C_1(0) = 0$, ensures that the firm will raise a positive of amount of external capital - that is, $E^* > 0$.

If a spill occurs, the firm bears a cost of S, reflecting, among other consequences, the lost value of spilled material, cleanup costs, litigation, any damages paid to spill victims, and lost production due to disruptions in operations. A spill may also result in costs that are external to the firm. For example, the community in which a spill occurs may suffer economic damage that is difficult to verify and that it therefore may not be able to recover from the firm. Similarly, people exposed to hazardous materials may suffer health problems that are difficult to link to the spill and may therefore not receive full compensation for their losses. These externalized costs do not factor into the firm's payoff, and we do not take a stand on what fraction of the total costs of a spill is borne by the firm versus the rest of society.

Given this setup, the firm chooses its investments in production and spill risk mitigation technologies to maximize its value. Formally, it solves the problem:

$$\max_{K \geq 0, M \geq 0} F(K, z) - P(M)S - C(E) - I, \tag{1}$$

subject to $I = W + E$. The first order conditions to this problem are:

$$F_1(K, z) = 1 + C_1(I - W) \tag{2}$$

[3] In a more general setting, even a firm possessing sufficient financial resources to finance its equilibrium level of investment in the current period may act as though it is constrained in its ability to do so if there is a risk that it will lack the resources to finance investment at some point in the future.

and

$$-P_1(M)S = 1 + C_1(I - W). \tag{3}$$

Intuitively, the firm sets the marginal return to investment in its production technology ($F_1(K, z)$ in Equation 2) and the marginal return to investment in spill prevention ($-P_1(M)S$ in Equation 3) equal to the marginal cost of an additional dollar of external funds, $1+C_1(I-W)$.[4]

We now illustrate how changes in a firm's internal resources W affect its investment decisions. Implicitly differentiating the first order conditions with respect to W and solving the resulting system of equations yields the following comparative statics:

$$\frac{dK}{dW} = \frac{C_{11}(I-W)P_{11}(M)S}{C_{11}(I-W)P_{11}(M)S - (C_{11}(I-W) + P_{11}(M)S)F_{11}(K,z)}$$

and

$$\frac{dM}{dW} = \frac{-C_{11}(I-W)F_{11}(K,z)}{(C_{11}(I-W) - F_{11}(K,z))P_{11}(M)S - C_{11}(I-W)F_{11}(K,z)}.$$

Since $C_{11} > 0$, $P_{11} > 0$, $F_{11} < 0$, and $W < I$, both of these expressions are unambiguously positive. Not surprisingly (given continuous marginal returns), a financially-constrained firm invests more in all available technologies when it has more internal financial resources available to invest. By contrast, if a firm is not financially constrained, both C_1 and C_{11} are equal to zero, implying that $\frac{dK}{dW} = \frac{dM}{dW} = 0$. The chief implication of $\frac{dM}{dW} > 0$ for our empirical analysis is that $\frac{dP(M)}{dW} = P_1(M)\frac{dM}{dW} < 0$. That is, the likelihood of spills declines with a firm's internal financial resources.

While we focus on the effects of financial resources on spills in our empirical analysis, the model also generates clear predictions about the effect of z on spills. Implicitly differentiating

[4] The second order conditions for maximization are trivially satisfied given the assumptions on F, P, and C.

Equations 2 and 3 with respect to z and solving the resulting system of equations yields:

$$\frac{dK}{dz} = -\frac{(C_{11}(I-W) + P_{11}(M)S)F_{12}(K,z)}{C_{11}(I-W)P_{11}(M)S - (C_{11}(I-W) + P_{11}(M)z)F_{11}(K,z)},$$

which is unambiguously positive, and

$$\frac{dM}{dz} = -\frac{C_{11}(I-W)F_{12}(K,z)}{C_{11}(I-W)P_{11}(M)S - (C_{11}(I-W) + P_{11}(M)z)F_{11}(K,z)},$$

which is unambiguously negative. Noting that $\frac{dP(M)}{dz} = P_1(M)\frac{dM}{dz} > 0$, the likelihood of spills increases with the quality of the firm's productive investment opportunities, which substitute for investment in mitigating spill risk when capital is scarce. In our empirical analysis, we also consider how spills vary with a firm's sales growth - an indirect test of this secondary implication.

II. Empirical Methodology

This section describes the methodology we use to test the response of environmental spills to a firm's financial resources. We use two approaches. First, we conduct multivariate regression analysis, focusing on the relationship between the number of spills a firm experiences in a given year and its cash flow. We then use a difference-in-differences approach to analyze two natural experiments involving shocks to financial resources that affected some firms much more than others.

A. *Multivariate Regression Analysis*

If external financing is costly, a firm's investment may depend on its internally generated cash flow. Following this argument, the financing constraints literature examines the sensitivity of various forms of investment to cash flow. Adhering to the spirit of that literature, we estimate the empirical relationship between environmental spills and cash flow, controlling for potentially confounding factors. Our regression specification is:

$$Spills_{it} = \beta_1 CashFlow_{it} + \beta_2 CashFlow_{i,t-1} + \mathbf{X_{it}}'\theta + \alpha_i + \alpha_{st} + \epsilon_{it}, \tag{4}$$

where $Spills_{it}$ is the total number of spills attributable to firm i in year t, and α_i and α_{st} represent firm and 2-digit-SIC-code-by-year fixed effects, respectively. We cluster all standard errors in our regressions at the firm level to account for possible time-series correlation in the error term within firm. The two variables labeled $CashFlow$ correspond to the firm's contemporaneous and lagged cash flow as a fraction of assets.

The financing constraints literature focuses primarily on the sensitivity of investment to contemporaneous cash flow. We consider the sensitivity of spills to both contemporaneous and lagged cash flow for two reasons. First, the dependent variable in our regressions, spills, is not actually a measure of investment. Rather, it is an outcome of investment in activities that mitigate spill risk. Some activities, such as the supervision of employees and monitoring of production variances, may reduce spill risk quickly. However, other activities, including the maintenance of production equipment and storage facilities, are likely to reduce spill risk over time rather than immediately and may thus be detectable only with a lag.

Second, there are strong *a priori* reasons to anticipate a negative relationship between spills and contemporaneous cash flow due to reverse causality. As already noted, firms are generally responsible for the costs of spill remediation. Thus, a spill in a given year is likely to entail a set of costs that reduce cash flow in the year of the spill and possibly in future years as well. This effect of spills on cash flow may induce a negative relationship between spill incidence and contemporaneous cash flow, muddying interpretation of the coefficient on contemporaneous cash flow. However, the cost of remediating spills in one year should *not* affect cash flows in the prior year. Therefore, any relationship between spill incidence and lagged cash flow is unlikely to be contaminated by the effects of reverse causality.

Another factor worth considering in interpreting estimates of Equation 4 is that any expenditures that a firm makes to mitigate the risk of environmental spills will tend to directly reduce cash flow. This factor could attenuate negative estimates of the sensitivity of spills to both current and past cash flows, since firms spending more to mitigate spill risk should have both less cash flow in the current period and fewer spills in both the current and future periods. If this effect is important, then one might reasonably interpret any negative relationship between spills and lagged cash flow as a lower bound on the estimated effect of financial resources on next-year spill risk.

We also include a vector of other time-varying firm characteristics, $\mathbf{X_{it}}$, in estimating Equation 4. The variables in this vector includes the log of total assets, PP&E as a percentage of assets, debt (net of cash) to assets, sales growth and, in some specifications, capital expenditures and sales per dollar of assets. We discuss the construction of these variables in detail in Section III. While the primary purpose of including these variables is to control for potentially confounding factors, some of these variables have a natural interpretation in the context of financing constraints. For example, sales growth plausibly proxies for the presence of investment opportunities that compete with investment in mitigating spill risk for scarce corporate resources. The theoretical framework presented in the previous section suggests that this competition for resources can increase spill risk. Theory also suggests that firms with higher debt loads may have more difficulty raising additional capital to finance investment, creating a potential dependence of spill risk on financial leverage. We explore these possibilities in our regression analysis as well.

The coefficients β_1 and β_2 capture the causal effect of changes in firm cash flows on environmental spills under the condition that there are no omitted determinants of spills that are correlated with cash flow, i.e. $Cov(CashFlow_{it}, \epsilon_{it}) = Cov(CashFlow_{i,t-1}, \epsilon_{it}) = 0$. There are several possible reasons why this condition might not hold. For example, unobservable work intensity might increase both cash flows and spill probability. Poor managerial quality could decrease cash flows and increase spill risk. Similarly, better production technology could lead to both higher cash flows and fewer spills. Finally, as mentioned above, spills themselves could affect a firm's cash flow by increasing its costs, inducing reverse causality.

While such identification assumptions are fundamentally untestable, we try to assess the probability that they are violated through several indirect means. First, we examine how sensitive our estimates are to varying the included controls $\mathbf{X_{it}}$. Second, we examine whether the timing of the sensitivities to cash flows of spills with different underlying causes lines up with the time horizons over which a lack of financial resources should impact them. Third, we assess whether spill risk is more sensitive to cash flows in firms more likely to be constrained *a priori* as a further test of the theoretical mechanism. Finally, to identify the effects of financial resource availability on environmental outcomes more cleanly, we also study two quasi-natural experiments involving shocks to financial resources. We discuss these experiments next.

B. Natural Experiment 1: American Jobs Creation Act

Our first natural experiment exploits a provision in the American Jobs Creation Act (AJCA) of 2004. US multinationals were historically required to pay US corporate taxes on foreign-sourced income upon repatration of that income, with a credit for foreign corporate taxes paid. This encouraged companies to defer repatriation of income earned in low-tax jurisdictions. The AJCA allowed US multinationals to repatriate foreign income on a one-time basis at a low 5.25% tax rate. This shock represented a significant windfall for the domestic coffers of firms with profitable foreign subsidiaries. According to IRS estimates, US firms collectively repatriated $312 billion in response to the AJCA.[5] However, the AJCA

[5]Dharmapala et al. (2011) and Faulkender and Petersen (2011) study the effects of this shock on investment levels in general.

represented a cash flow shock only for firms with previously unrepatriated foreign profits.

We exploit differential exposure to this shock based on the presence of foreign profits to conduct difference-in-differences analysis around the AJCA. Specifically, we compare the change in spill rate from before to after the AJCA For firms likely to have unrepatriated foreign profits prior to the AJCA to those without. In doing so, we implicitly treat whether or not a firm has unrepatriated foreign profits at the time that AJCA was passed as exogenous with respect to future changes in its spill risk.

To implement this difference-in-differences analysis, we define $PosFrgnProf$ as a firm-specific indicator variable equal 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.[6] Firms for which $PosFrgnProf = 1$ represent the treatment group in the experiment. To form a control group, we match each firm in the treatment group to one firm for which $PosFrgnProf = 0$, using propensity score matching without replacement. The dependent variable is $PosFrgnProf$, and the explanatory variables are $CashFlow/Assets$, $Log(Assets)$, $SalesGrowth$, $PPE/Assets$, and $Debt/Assets$, all measured in the year prior to the shock, i.e., as of fiscal year-end 2003. We provide the exact definitions of these variables in Section III.

Our final sample consists of all treated and control firm-year observations in the periods 2001-2003 and 2005-2007. We define an indicator variable $Post2004$ to be equal to one for observations in years 2005-2007 and zero for observations in years 2001-2003. We then estimate the following regression equation:

$$Spills_{it} = \beta^{ajca} Post2004_t * PosFrgnProf_i + \alpha_i^{ajca} + \delta_t^{ajca} + \epsilon_{it}, \quad (5)$$

[6]Establishments for which $PosFrgnProf = 0$ 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.

where α_i^{ajca} represents a vector of firm fixed effects and δ_t^{ajca} represents a vector of year fixed effects. The difference-in-differences coefficient β^{ajca} represents the change from before to after the AJCA in the expected annual number of spills for firms with accumulated foreign profits at the time of the Act relative to control firms.

C. Natural Experiment 2: Financial Crisis of 2007

Our second natural experiment exploits variation in the maturity structure of firms' debt at the onset of the financial crisis in late 2007. US credit markets seized up starting in August 2007 and remained tight through 2008, making it difficult for firms to roll over maturing debt.[7] While this tightening may have affected access to capital more generally, it was an especially large negative shock to financial resources for firms with a lot of debt maturing during this period. These firms would have needed to either repay their maturing debt or, if possible, roll it over at high interest rates. The maturity structure of a firm's debt at the beginning of the crisis is plausibly exogenous with respect to factors that might affect spill risk, as it is unlikely that firms anticipated the crisis when setting maturity schedules in the preceding years.

We exploit differential exposure to the shock based on debt maturity to conduct a difference-in-differences analysis. Specifically, we compare the change in spill rate from before to after the onset of the crisis For firms with high levels of debt maturing shortly after the onset of the crisis to those without. In doing so, we implicitly treat a firm's debt maturity structure at the time of the onset of the financial crisis as exogenous with respect to future changes in its spill risk.

To implement this difference-in-differences analysis, we define $DebtDueIn1Year/Assets$ as debt maturing within one year (Compustat $dd1$) as of fiscal year-end 2007 divided by

[7]Almeida et al. (2012) study the effect of this shock on capital expenditure levels in 2008, and other papers have studied its effect on other forms of investments and in other contexts (e.g., Benmelech et al., 2017).

total assets for each firm the sample.[8] We then define $HighMat$ as a firm-specific indicator variable equal to one if $DebtDueIn1Year/Assets \geq HighMaturityThreshold$ for a given level of $HighMaturityThreshold$ and zero otherwise. As we do not have strong priors on what the "right" threshold $HighMaturityThreshold$ is, we use three different threshold values in our analysis: 0.07, 0.05, and 0.03.

Firms for which $HighMat = 1$ represent the treatment group in the experiment. To form a control group, we again use propensity score matching without replacement to match each firm in the treatment group to one firm for which $HighMat = 0$. For each treated firm, we choose the untreated firm with the closest propensity score as its match. We calculate the propensity to be treated as the fitted value from a probit regression, where the dependent variable is $HighMat$, and the explanatory variables are $CashFlow/Assets$, $Log(Assets)$, $SalesGrowth$, $PPE/Assets$, and $Debt/Assets$, all measured as of fiscal year-end 2006, the year prior to the shock.

Our final sample consists of all firm-year observations in 2005-2010 belonging to treated and control firms. We define a variable $Post2007$, which equals one for observations in years 2008-2010 and zero for observations in years 2005-2007. We then estimate the following equation:

$$Spills_{it} = \beta^{crisis} Post2007_t * HighMat_i + \alpha_i^{crisis} + \delta_t^{crisis} + \epsilon_{it}, \tag{6}$$

where α_i^{crisis} represents a vector of firm fixed effects and δ_t^{crisis} represents a vector of year fixed effects. The difference-in-differences coefficient β^{crisis} represents the change from before to after the onset of the financial crisis in the expected annual number of spills for firms with a high level of debt maturing within one year as of the onset of the crisis, relative to control firms.

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

III. Data and Sample

Our data on spills come from the U.S. Coast Guard's National Response Center (NRC).[9] This database includes all air, water, and land releases of various substances, as well as maritime security incidents, reported by either the responsible party or a third party, which includes individuals calling the NRC hotline.[10] The unit of observation in the data is a *reported* incident; thus, some *ex post* innocuous spills may be included, although this is less likely in the case of larger events. Our data, which span 1990-2012, include over 710,000 such spills and incidents.

Whenever such information is available, each entry in the NRC database reports what was spilled and how much. Importantly, the name and address of the party responsible for the spill are also reported, as well as whether the responsible party is a private enterprise, a governmental entity, a private citizen, a public utility, or other. In some cases, the substance, the amount, and/or the responsible party are not known: we conjecture that these are likely to be fairly minor incidents.

Table I summarizes several aspects of the spill data. About 37% of the incidents occur in a "fixed facility." Slightly under 14% occur on a vessel, followed in frequency by mobile sources (9.5%), railroads (8.2%), platforms (5.4%), and storage tanks (4.8%). 0.66% of incidents involve aircraft, and slightly under 15% do not have a known source. In the vast majority of cases where the responsible party is known, it is a private enterprise, and slightly more than half of the reports are made by the responsible party as opposed to a third party.

Most of the spills involve releases into water (55.9%), followed by land (22.0%) and air (18.7%). About 3.2% of the incidents result in injuries, 2.8% result in fatalities, and 1.6% result in evacuations. Finally, the database contains such detailed information as reported

[9] Available from `http://nrc.uscg.mil/`.

[10] According to the data, about half of the incidents are reported by phone. In 44 percent of cases the communication mode is unavailable. The rest of the reports arrive through the web, the fax, the news, and "other" sources.

"sheen color," underscoring its comprehensiveness.

— Table I here —

Our data on firm financials for 1990-2012 come from Compustat's North American Annual Fundamentals database, which is populated from annual 10-K filings. We define $CashFlow/Assets$ as the sum of income before extraordinary items (Compustat item *ib*) and depreciation and amortization (*dp*), divided by lagged total assets (*at*); $Log(Assets)$ as the natural log of total assets; *SalesGrowth* as the percent change in sales (*sale*) from the prior year; $PPE/Assets$ as net property, plant and equipment (*ppent*) divided by total book assets; *Debt/Assets* as book debt (the sum of Compustat items *dlc* and *dltt*) minus cash (*ceq*), divided by total assets; $Capex/Assets$ as capital expenditures (*capx*) divided by lagged total assets; and $Sales/Assets$ as sales divided by lagged total assets. We winsorize all variables at the 1st and 99th percentiles to address concerns about possible outliers.

To match responsible parties in the NRC database to firms in the Compustat database in each year, we primarily use firms' names and addresses.[11] Because of some clear mis-spellings in the NRC database (e.g., "ExonMobil") as well as variations in how responsible party names are reported (e.g., "Exxon", "ExxonMobil", "Exxon Mobil"), we do not always require a perfect match between firm names to attribute a spill to a particular Compustat firm. Instead, we set match thresholds based on the normalized number of differences between the two company names. Specifically, for every pair of names in NRC/Compustat, we calculate a "similarity ratio" equal to $2\frac{M}{T}$, where M is the number of matching elements, and T is the total number of elements in the two names. Note that M cannot be larger than the length of the shorter name. When matches are not perfect (i.e., $2\frac{M}{T}$ is less than 1), we also compare the responsible party location reported in the NRC database to the Compustat firm's headquarters location; if these match, we allow for a lower level of similarity between

[11]For more details on the matching procedure, see the Data Appendix.

company names to label them a match. In addition, we account for the presence of 200 common words in company names. If both companies' names contain a common word and the common words do not match, we label that pair a non-match. If the common words match perfectly or if one or both of the companies do not have a common word in their name, we compare the non-common components of the firms' names. For the most ambiguous matches near the cutoffs, we use Mechanical Turk workers, who are asked to judge the likelihood that the responsible party is a particular Compustat company based on the companies' names and the Compustat industry sector and business description. In Appendix Table B1, we demonstrate that our results are robust to using only perfect matches.

Figure 1 shows the geographic distribution of spills in our final sample. Panel (a) shows the number of spills, and panel (b) shows amounts spilled for cases in which we are able to calculate them.[12] During our sample period, each state experiences at least 20 spills attributable to publicly traded companies. Many states experience several hundred spills. The largest number of spills in our final sample occur in Texas, Louisiana, and California. When considering quantity spilled, these three states remain near or at the top of the distribution. The states of Wyoming, Nevada, Oklahoma, Indiana, Ohio, and Florida are also near the top, each with over 4,800 pounds of materials spilled by publicly traded companies during our sample period.

— Figure 1 here —

Finally, Table II provides summary statistics for firm-year observations in our sample, which includes all firm-years in Compustat between 1990 and 2012. The mean number

[12]Spill amounts are reported either in units of weight (e.g., pound, gram) or units of volume (e.g., gallon, teaspoon). Because weight units are more common, we convert as many quantities as possible to pounds. Oil and oil-related products are the most commonly reported substances in volume-based quantities. Thus, for quantities reported in volume units, we assume that the material spilled is petroleum oil, which has a weight of 7.35 pounds per gallon *unless* the material name contains the term "natural gas," another commonly spilled substance with volume-based reporting. In that case, we assume the material is compressed natural gas and assign it a weight of 5.66 pounds per gallon.

of annual spills per firm is 0.11, and about 5% of firms have a spill in any given year. Evacuations, injuries, and fatalities are much rarer: evacuations or injuries occur in only 0.30-0.32% of observations, and fatalities are limited to 0.06% of observations.

— Table II here —

Panel B of Table II reports summary statistics for firm financial variables. The average cash flow is -12% of assets, but the median is a positive 6%. Both the average and median sales growth are positive, and average sales are about 125% of a firm's assets. The standard deviation of cash flow divided by assets, which we later use to assess the economic magnitude of our estimates, is 0.62.

IV. Results: Empirical Determinants of Environmental Spill Risk

We first present results from analysis of the firm-level determinants of environmental spill risk, starting with variants of Equation 4. Table III presents the baseline results. Recall that the dependent variable in these regressions is the number of spills, and the main explanatory variables are contemporaneous and lagged $CashFlow/Assets$. The regressions include firm and industry-year fixed effects. Standard errors, clustered at the firm level, are reported in parentheses below each point estimate.

— Table III here —

We control for prior-year ending $Log(Assets)$ in each specification to account for differences in firm size. All else equal, a firm with larger operations should have a greater exposure to the risk of hazardous spills. Column (1) presents estimates from a regression where we do not include any additional control variables. Unsurprisingly, the coefficient on $Log(Assets)$ is positive and highly significant. The coefficients on contemporaneous and lagged $CashFlow/Assets$ are both negative and statistically significant at the one-percent level.

As discussed in Section II, the relationship between spills and contemporaneous cash flow is difficult to interpret. While greater cash flow may reduce spill risk by allowing additional investment in mitigating this risk, spills are also likely to affect cash flows as firms must expend resources on remediating the effects of spills. However, the negative relationship also holds for lagged cash flows and is consistent with greater cash resources prompting a firm to invest more in mitigating spill risk, though we discuss caveats regarding this interpretation shortly. The point estimates imply that a one-standard deviation increase in $CashFlow/Assets$ is associated with a 2.0% decrease in spills in the same year relative to the sample mean and a 3.3% decrease in the following year.

In column (2), we add $SalesGrowth$ to the regression. The relationships between spills and both lagged and contemporaneous cash flows remain essentially unchanged. Spills are positively associated with sales growth (statistically significant at the one percent level). The coefficient implies that a one-standard deviation increase in sales growth is associated with a 1.6% increase in the expected number of spills relative to the sample mean. As the conceptual framework we present in Section I demonstrates, in cash-constrained firms, investment in growth competes with investment in factors that mitigate environmental spill risk. To the extent that annual sales growth reflects investment in growth, the positive coefficient on $SalesGrowth$ is consistent with investment in growth crowding out investment in mitigating spill risk.

In column (3), we add two additional control variables: $PPE/Assets$ and $Debt/Assets$. In principle, firms with more physical assets may face a greater risk of an environmental spill because of the nature of their production technology. However, the inclusion of firm fixed effects in the regressions requires that any such effect be driven by time-series variation in the nature of that technology, which is unlikely to vary much over time. Financial leverage may also be related to spill risk because more indebted firms have incentives to take on risk and may find it difficult to raise capital. The coefficient on *Debt/Assets* is positive. However, the coefficients on the two additional variables are both statistically insignificant at the ten percent level.

In column (4), we add *Capex/Assets* as an additional control variable. Capital expenditures could be either negatively related to spills if expenditures reflect technological upgrades that reduce spill risk or positively related if expenditures reflect the addition of assets that require integration into the company's overall production process. The coefficient on *Capex/Assets* is statistically insignificant at the ten percent level.

Finally, column (5) adds *Sales/Assets* as an additional control variable. Sales per dollar of assets could be positively related to spills if additional sales put a strain on a company's production process. The coefficient on *Sales/Assets* is statistically insignificant at the ten percent level. The coefficients on the cash flow variables remain unchanged in columns (2)-(5) as these additional control variables are added, demonstrating the stability of our estimates.

Exposure to spill risk varies substantially across different industries. This risk is likely to be negligible in some industries, especially those in the service sector. We refrain from removing firms in industries where spill risk is likely to be low in constructing our sample for two reasons. First, any criteria for removing such firms is necessarily arbitrary. Second, firms may operate across multiple industries, and exclusion based on a firm-level industry code will remove some firms that do, in fact, face non-trivial spill risk. However, in Appendix Table B2, we demonstrate that our results are, if anything, slightly stronger if we exclude firms in 3-digit SIC industries that average fewer than 0.01 spills per firm-year in our sample.[13]

We next estimate a series of regressions where we include both the cash flow and control variables at different time horizons. We include the one-year lagged as well as contem-

[13]This filter removes approximately 25% of the observations in the sample.

poraneous $CashFlow/Assets$ in regression Equation 4 to account for the possibility that investments in mitigating spill risk may pay off in fewer spills with some delay. However, our specification of the exact timing of the relationship between cash flow and spill risk is arbitrary, and it is possible that investments affect spill risk over an even longer time horizon. To that end, we include additional lags in our estimation, both to explore the dynamics of the relationships between spills and these variables and to ensure that our results are not sensitive to including controls measured over different horizons. Table IV presents the results from these regressions.

— Table IV here —

In column (1), we add the two-year lagged value of $CashFlow/Assets$ as an additional explanatory variable. The coefficients on contemporaneous and lagged $CashFlow/Assets$ remain negative and statistically significant at the one-percent level when we add two-year lagged $CashFlow/Assets$. However, two-year lagged $CashFlow/Assets$ itself enters with a positive sign, is small in magnitude, and is statistically insignificant.

In column (2), we add next year's cash flow to the estimating equation. While the focus of our analysis is on how cash flow affects spills, remediating spills can be costly, so spills are likely to affect cash flow as well. The exact timing of this effect is unclear. Some of the costs of remediating a spill may occur after the year of the spill, though our cash flow measures are based on accrual accounting variables, and companies may take accounting charges in the year of a spill to account for future costs. The coefficients on contemporaneous and lagged $CashFlow/Assets$ remain negative and statistically significant at the one-percent level when we add next year's cash flow. The coefficient on future cash flows is negative, consistent with spills reducing cash flow in future periods. However, the coefficient is statistically insignificant at the ten percent level. This accounting approach would shift the measured costs of a spill from future years to the year in which the spill occurs.

In column (3), we include $CashFlow/Assets$ in each year from two years prior to the year of the observation to the year after. The coefficients on the contemporaneous and lagged cash flow variables remain negative and statistically significant at the one-percent level. In addition, the coefficient on next year's $CashFlow/Assets$, which remains negative, now becomes statistically significant at the ten percent level, consistent with some of the costs of spills accruing in the year after they occur.

Finally, in columns (4) and (5), we demonstrate the robustness of our estimates to additional lagged control variables. The coefficients on both contemporaneous and lagged cash flows remain negative and highly statistically significant. Cash flows two years ago continue to lack explanatory power over this year's spills. Next year's cash flow continues to be negatively related to spills this year at a ten-percent level of statistical significance.

We seek to further assess the economic importance of the association between spills and cash flows by narrowing our focus to a set of relatively costly spills - those involving injuries, fatalities, or evacuations. Specifically, we estimate variations of regression Equation 4 where we replace total spills as the dependent variable with indicators for spills of each of these three types. We use indicators for the occurrence of these types of spills rather than counts because there are very few cases where a firm has multiple spills of one of these types in a given year. Table V presents the results. All of the coefficients and standard errors in Table V are multiplied by 100 to make the table easier to read.

— Table V here —

The probabilities of having spills involving injuries and evacuations (but not fatalities) in a given year are negatively associated with prior-year cash flows. These associations are statistically significant at the ten percent level, with p-values of 0.052 and 0.099 respectively. Given the 0.62 standard deviation of $CashFlow/Assets$, and noting that the coefficients are multiplied by 100 in the table, the coefficients imply 0.032 and 0.021 percentage point decreases in the probability of spills resulting in evacuations and injuries, respectively, in response to a one-standard deviation increase in prior-year cash flow. These decreases are a substantial 10.7% and 6.6% of the 0.30% and 0.032% unconditional probabilities of having at least one of these types of spills in a given year, respectively. However, we recommend caution in interpreting the magnitudes implied by these estimates, as the relative infrequency of the outcomes involved means that the estimates are relatively imprecise.

We next exploit information about the underlying cause of each spill in our data to gain further insights into the mechanisms by which financial resources might affect spill risk. As we describe in Section I, investments in activities that mitigate spill risk can take a variety of forms. Some forms involve investments in hard assets, including expenditures to maintain and upgrade equipment. Others involve investments in employees who handle dangerous materials. These investments include expenditures on training, supervision, and retention of more-skilled employees.

These different forms of investment plausibly affect environmental spill risk over different time horizons. For example, cuts to employee training or the loss of more-skilled employees are likely to increase spill risk quickly. Deferred equipment maintenance, on the other hand, may take time to manifest itself in higher spill risk.

Motivated by these differences, we examine the sensitivity of spills caused by operator error and equipment failure separately to cash flows at different horizons. Specifically, we estimate variants of regression Equation 4, substituting measures of spills caused by equipment failure and operator error separately for total spills as the dependent variable. Given the modest numbers of spills attributed to each of these two underlying causes (especially those attributed to operator error), we use spill indicators as the dependent variables in these regressions rather than counts. Table VI presents the estimates.

— Table VI here —

Columns (1) and (2) demonstrate that spills due to equipment failure are sensitive primarily to lagged rather than contemporaneous cash flow. In contrast, columns (3) and (4) demonstrate that spills due to operator error are more sensitive to contemporaneous rather than lagged cash flow. While not conclusive, the differences between the time horizons over which spills caused by equipment failure and operator error are sensitive to cash flow are consistent with how different types of investment are likely to affect spill risk.

We next examine how the relationship between spill incidence and cash flow varies with firm characteristics likely to reflect how cash-constrained a firm is. However, we note that there is some debate about whether more-constrained firms should actually exhibit a higher sensitivity of investment to internal resources (Kaplan and Zingales, 1997).[14] We measure the likelihood that a firm is cash-constrained using three characteristics that are theoretically-motivated and that the literature has used to measure cash constraints previously: $Log(Assets)$, $Debt/Assets$, and $Dividends/Assets$. Firms are generally thought to be more cash constrained if they are smaller or have higher debt loads, which make raising additional external capital difficult. A firm paying few or no dividends has one fewer margin on which to adjust its uses of cash in order to free up cash flow for investment. We estimate regressions where we interact each of these three characteristics with current-year and prior-year $CashFlow/Assets$, measuring each characteristic in the year prior to the cash flow variable with which it is interacted. Table VII presents the results.

— Table VII here —

Spill incidence is more sensitive to both contemporaneous and lagged cash flow for firms with higher debt-to-asset ratios, though only the differential sensitivity to contemporaneous

[14] Investment in general may appear to be more sensitive to cash flow for firms seen as likely to be cash-constrained even absent such constraints because cash flow may proxy for unobserved investment opportunities for these types of firms (Altı, 2003). This concern is unlikely to be relevant in our setting. If anything, the conceptual framework we present in Section I suggests that firms with better investment opportunities may invest *less* in mitigating spill risk because of crowding out from other forms of investment.

cash flow is statistically significant (column (1)). The p-value of the coefficient on the interaction of lagged cash flow and debt-to-assets is 0.12. In addition, spill incidence is more sensitive to lagged cash flows for smaller firms (column (2)). These results are consistent with an increase in financial resources allowing more investment in mitigating spill risk in firms more likely to be constrained in their ability to invest *a priori*. However, we find no statistical relationship between a firm's dividend policy and the sensitivity of its spill risk to cash flow (column (3)).

Overall, the results in this section consistently and robustly suggest a dependence of spill risk on a firm's ability to finance investment out of internal cash flow. However, none of the evidence presented here is based on truly exogenous variation in internal financial resources. As noted in the discussion of our empirical methodology in Section I, interpreting the relationship between spill incidence and contemporaneous cash flows is especially challenging. To more closely approximate the ideal experiment, which would entail randomly shocking firms' financial resources and measuring subsequent changes in spills, we next analyze the response of spill incidence to two quasi-natural experiments that produced such shocks.

V. Results: Spill Risk and Cash Flow Shocks

This section presents results from analyzing the two quasi-natural experiments described in Sections II.B and II.C. We first present comparisons of treatment and control samples from each experiment. As described in Section II.B, the treatment sample in the AJCA experiment consists of firms with foreign profits over the three years prior to the AJCA, while firms in the control sample lack such profits over that period. As described in Section II.C, the treatment sample in the financial crisis experiment consists of firms with high levels of debt maturing within one year as of the onset of the crisis, while firms in the control sample have low levels of debt maturing within one year. We use three separate cutoff values of

$DebtDueIn1Year/Assets$ to assign firms to high-debt maturity (treatment) and low-debt maturity (control) groups: 0.07, 0.05, and 0.03.

Table VIII compares the mean values of several variables in the matched treatment and control sample pairs for each of the two experiments. Panel A presents comparisons for the AJCA experiment, while Panel B presents comparisons for the financial crisis experiment. For the comparison in the AJCA experiment, we measure variables in fiscal year 2003, except for $Spills/Year$, which is the mean number of spills per year over the period 2001-2003. For the comparison in the financial crisis experiment, we measure variables in fiscal year 2007, expect for $Spills/Year$, which is the mean number of spills per year over the period 2005-2007.

— Table VIII here —

For the AJCA experiment, differences in the means of all of the variables presented except for $Spills/Year$ are small and statistically insignificant. Despite the fact that we include pre-treatment spills per year in the set of match variables in propensity score matching when we construct the control sample, firms with foreign profits pre-AJCA have much higher pre-AJCA spill rates than control firms do. As a robustness check, in the Appendix, we repeat the analysis in this section for the AJCA experiment matching only on $Spills/Year$ instead of on multiple predictive variables. This approach does not homogenize the treatment and control samples on other dimensions, but it does ensure that treatment and control samples are similar in terms of pre-treatment spill rates. The results are slightly weaker than those presented in this section, in large part because the standard errors of the estimates are slightly higher.

For the financial crisis experiment, differences in the means of all of the variables presented are small and statistically insignificant, with the exception of $SalesGrowth$ when the $DebtDueIn1Year$ threshold is 0.03. Of particular importance is the fact that $Spills/Year$

differs little between treatment and control establishments for all three thresholds.

Next, we plot the mean number of spills for the treatment and control groups around the two events. Figure 2 presents these plots. Figure 2 (a) presents a plot for the AJCA experiment, while Figures 2 (b), (c), and (d) presents plots for each of the three variants of the financial crisis experiment.

— Figure 2 here —

For the financial crisis experiment, the patterns are roughly consistent with mean spills increasing for the treatment groups relative to the control groups in 2009 - the second year after the onset of the crisis - and remaining elevated in 2010. While there is noise in spills from year to year in both the treatment and control groups (but especially in the treatment groups), we see no obvious patterns in the time series prior to 2009 that would indicate a violation of the parallel trends assumption required for a valid difference-in-difference estimation. The timing of the response is consistent with investments in mitigating spill risk taking time to implement and/or manifest themselves in reduced risk.

As already noted, the pattern is more difficult to interpret in the AJCA case. Firms in the treatment group have considerably higher spill rates pre-treatment than control firms do in this experiment. However, treatment- and control-firm spill rates appear to begin converging only after the AJCA and nearly reach parity in 2007.

Next, we implement formal difference-in-differences estimation based on regression Equations (5) and (6). Table IX presents estimates based on the AJCA experiment, first without controlling for additional time-varying firm characteristics (columns (1)-(3)) and then with $CashFlow/Assets$, $Log(Assets)$, $SalesGrowth$, $PPE/Assets$, and $Debt/Assets$ added to the regressions as controls (columns (4)-(6)).

— Table IX here —

The negative coefficient on $Post2004 * PosFrgnProf$ in columns (1) and (4) indicates a decrease in spills for firms with accumulated foreign profits, relative to control firms, after the AJCA allowed the repatriation of foreign profits at a low tax cost. In columns (2) and (5), we present separate estimates of the differential change in spills from before the AJCA to each post-AJCA year. We do so by including interactions of indicators for each post-AJCA year ($Year2005$, $Year2006$, and $Year2007$) with the $PosFrgnProf$ indicator rather than lumping post-AJCA years together into a single $Post2004$ indicator. The estimates show that declines in all three post-AJCA years relative to the pre-AJCA period. However, the drop is clearly largest and is only statistically significant in 2007.

For consistency with the financial crisis difference-in-differences results that we describe next, we also define an indicator variable $Years2006 - 07$, which takes a value of one in either of those two years and zero otherwise. We then estimate a regression where we interact this variable with $PosFrgnProf$ and also include the interaction of $Year2005$ and $PosFrgnProf$. Columns (3) and (6) present these estimates. The coefficient on $Year2006 - 07 * PosFrgnProf$ is negative and statistically significant at the ten and five percent levels, respectively.

Table X presents results for the crisis experiment. Panels A, B, and C present results for matched treatment-control samples where the high-maturity cutoff is set at 0.07, 0.05, and 0.03, respectively. The regression results follow the same pattern as those in Table IX.

— Table X here —

Column (1) in each panel presents the base difference-in-differences estimate of the change in annual number of spills from before to after the onset of the crisis for firms with a high level of maturing debt relative to controls. The coefficient on $Post2007 * HighMat$, which represents the difference-in-differences estimate, is positive in all three panels, though it is only statistically significant (at the ten percent level) when the $DebtDuein1Year/Assets$

threshold for high debt maturity is 0.05. However, as Figure 2 suggests, pooling together all post-crisis onset years potentially masks important patterns in the differential evolution of spill incidence after the crisis. As already noted, a decrease in investment in mitigating spill risk due to the constraining effects of debt maturing during the crisis is likely to take time to manifest itself in higher spill rates.

In column (2) of each of the three panels, we present separate estimates of the differential change in spills from before to each year after the onset of the crisis. We do so by including interactions of indicators for each post-crisis year ($Year2008$, $Year2009$, and $Year2010$) with the $HighMat$ indicator rather than lumping post-onset years together in a single $Post2007$ indicator. The estimates in this column show no response of spill incidence in 2008, the first year after the onset of the crisis, except in the case where the high-debt maturity threshold is 0.03, where the estimate is negative. However, the estimates do show a considerable relative increase in spills in both 2009 and 2010 compared to the pre-treatment period. These relative increases are of similar magnitudes in all three samples. The relative increase for 2009 compared to pre-treatment years is statistically significant when the high-debt maturity threshold is 0.03 and for 2010 compared to pre-treatment years when the threshold is 0.07 or 0.05.

To more formally test the significance of the relative increase in spills in 2009 and 2010, we define an indicator variable $Years2009-10$, which takes a value of one in either of those two years and zero otherwise. We then estimate a regression where we interact this variable with $HighMat$ and also include the interaction of $Year2008$ and $HighMat$. Column (3) presents these estimates. The coefficient on $Year2009 - 10 * HighMat$ is positive and statistically significant at the five, ten, and ten percent levels in the three samples, respectively.

We do not include any firm-level control variables in the first three columns because of concerns about including endogenous controls in difference-in-differences estimation. However, for completeness, columns (4)-(6) in each panel repeat the regressions from columns

(1)-(3) with the five additional control variables mentioned above. The addition of the control variables generally increases the estimates of relative change in spills compared to the pre-treatment period, resulting in higher levels of statistical significance in some cases. For example, column (6) in each of the three panels shows that the coefficients on the interactions of $Years2009-10$ and $HighMat$ are all statistically significant at the five percent level when we include control variables.

Given the relatively small number of potential control firms, we do not require each control firm to fall within the same industry as the treated firm with which it is matched. As a robustness check, we impose the requirement that matches fall within the same 2-digit SIC industry and re-estimate the regressions in Tables IX and X. Tables B5 and B6 in the Online Appendix present these results. Generally, the results for the crisis experiment are similar to those presented in Table X. The signs of the coefficients of interest in the AJCA experiment are the same as those in Table IX, though the magnitudes are smaller, and none of the coefficients of interest are statistically significant at the ten percent level. However, if we adopt a specification that is less taxing on statistical power by replacing firm and year fixed effects with 2-digit-SIC-industry-by-year and 3-digit SIC industry fixed effects, both the crisis and AJCA experiment results hold even when we match within 2-digit SIC industry. Tables B7 and B8 present these final results.

The relative changes in spills following financial resource shocks that Tables IX and X depict are considerable in magnitude. For example, the estimates in columns (3) and (6) of the three panels in Table X indicate an increase in annual spills of 0.025 to 0.036 for high-debt maturity firms in the second and third years after the onset of the crisis. The magnitudes of the estimated response are similar in the AJCA experiment. Relative to the mean firm in our sample, which experiences 0.11 spills per year, these estimates imply a 23%-33% change in the number of environmental spills following these considerable shocks to financial resources.

VI. Conclusion

In summary, this paper presents evidence that the number of hazardous environmental spills for which a firm is responsible decreases with the firm's capacity to finance investment. This evidence takes two forms. In fixed effects regressions, spill incidence decreases with both contemporaneous and prior-year cash flow. It also increases with sales growth. The relationship with prior-year cash flow holds for spills in general as well as specifically for those relatively high-impact spills that result in evacuations or injuries. The relationship is stronger for smaller and more highly-indebted firms that likely have limited access to external financing. Second, difference-in-differences analysis indicates that firms subject to positive (negative) cash flow shocks in two quasi-natural experiments display a subsequent decrease (increase) in spills relative to control firms.

Overall, our results suggest that firms cut spending on activities that reduce spill risk when they lack the financial resources to fund them. Our findings add to the growing literature examining the impact of a firm's ability to finance on the firm's non-financial stakeholders. One novel feature of our evidence is that it supports an impact on the community at large in which a firm operates rather than a well-defined set of stakeholders such as employees or customers. Our findings also have implications for environmental regulators, as they point to observable factors that can be used to identify firms more likely to experience spills, including high-impact spills, in the near future.

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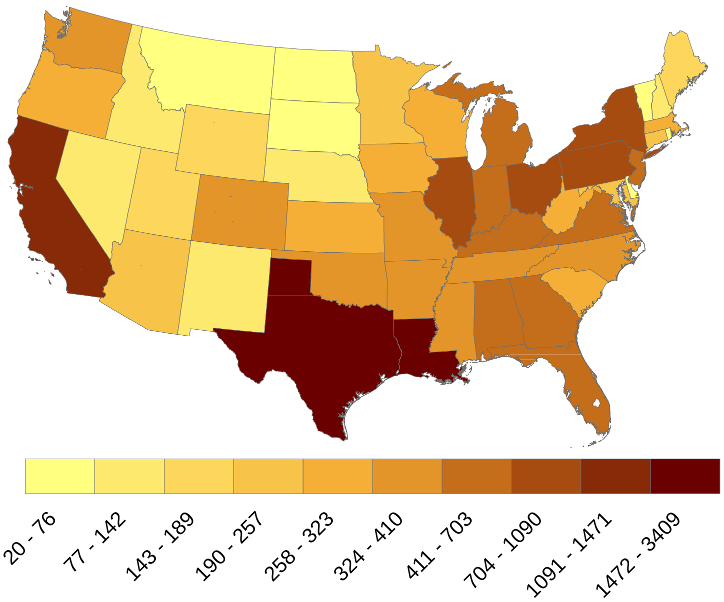
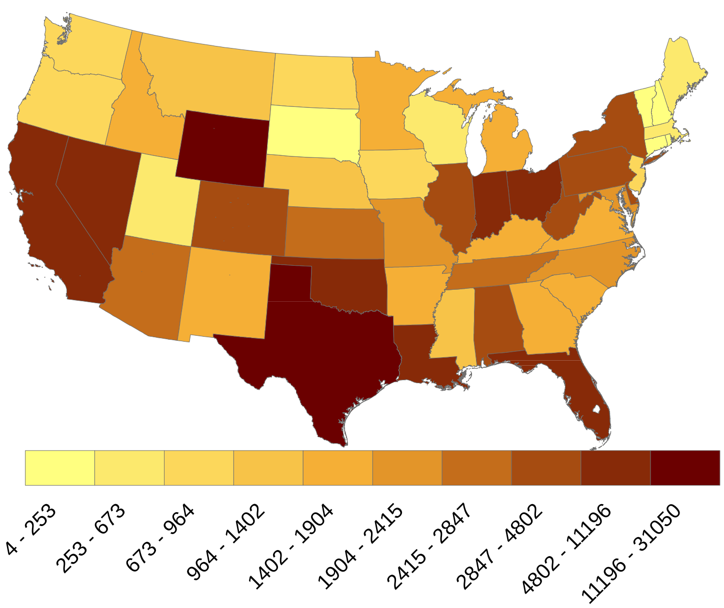
Figures

Figure 1: Spatial distribution of in-sample spills

(a) Number of spills



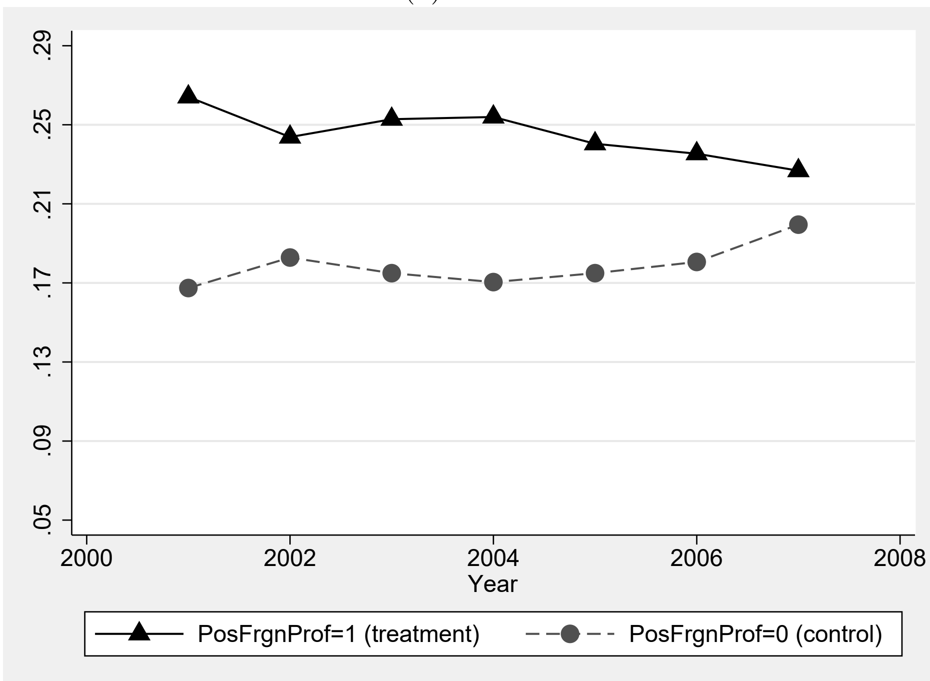
(b) Quantity spilled



Quantities are in thousand of pounds. Not all spills have quantifiable amounts reported.

Figure 2: Mean Spills by Year for Treatment and Control Firms

(a) AJCA



(b) Financial crisis - high maturity threshold: 0.07

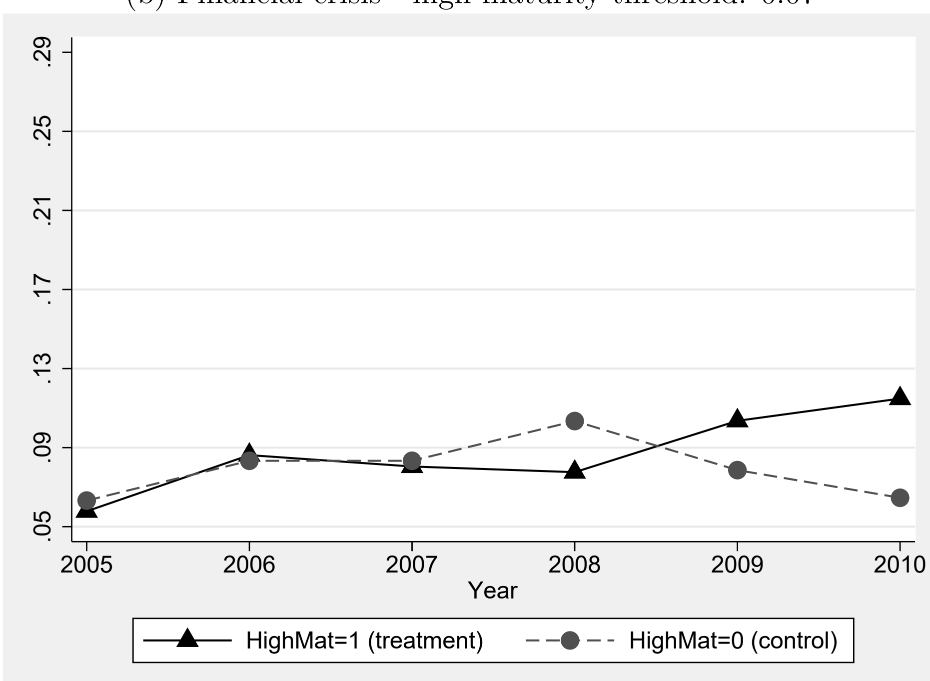
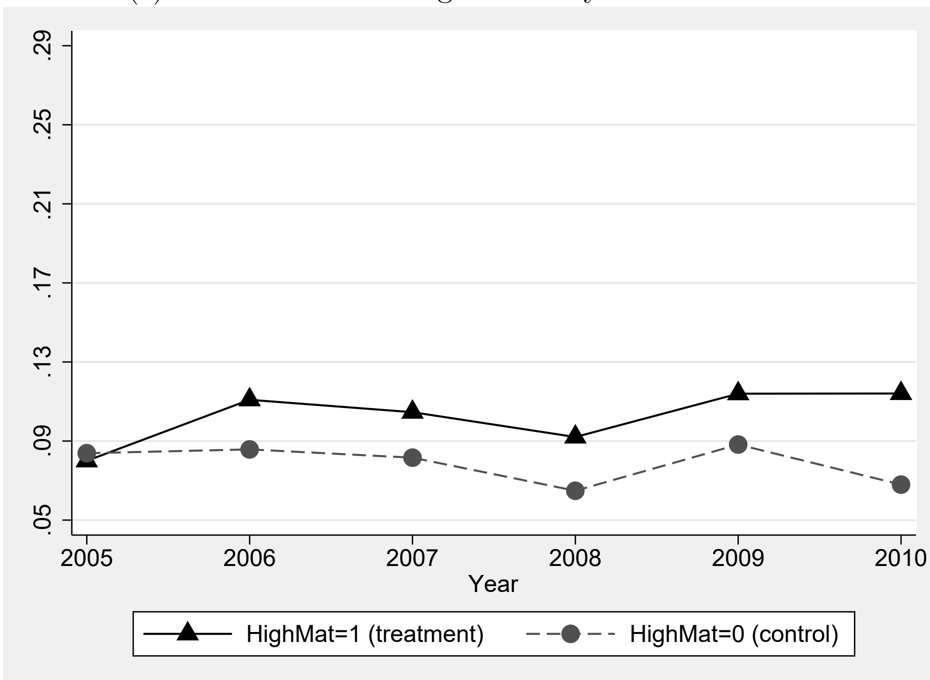
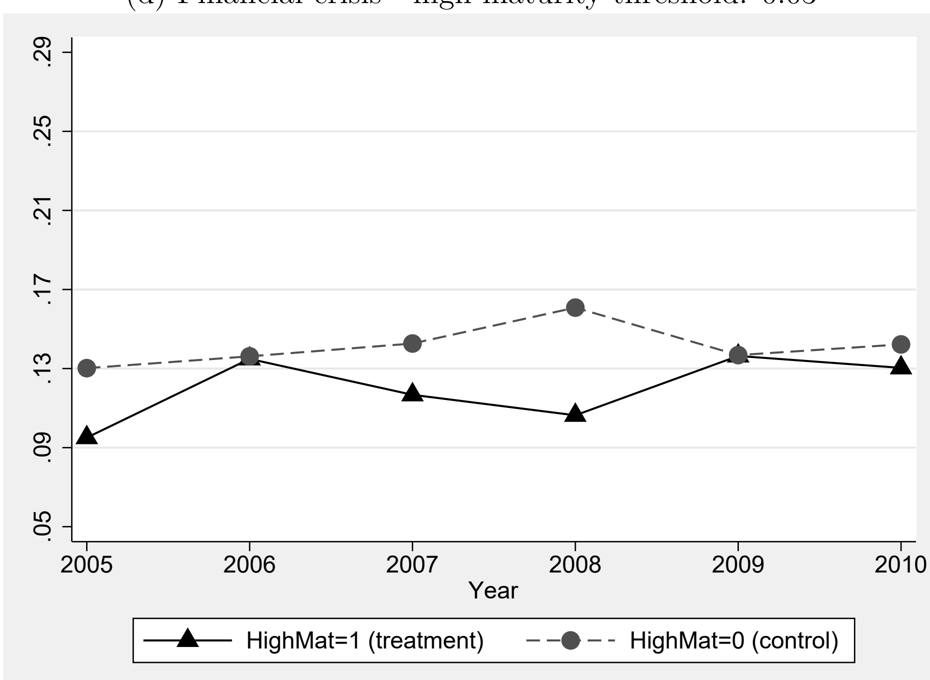


Figure 2: Mean Spills by Year for Treatment and Control Firms (continued)

(c) Financial crisis - high maturity threshold: 0.05



(d) Financial crisis - high maturity threshold: 0.03



Tables

Table I: Summary of environmental spill data

This table reports the percentage of spills in the U.S. Coast Guard's National Response Center (NRC) database with various characteristics.

Incident type	
Fixed Facility	37.38%
Vessel	13.65%
Mobile	9.46%
Railroad	8.20%
Pipeline	5.55%
Platform	5.43%
Storage Tank	4.79%
Aircraft	0.66%
Unknown/other	14.90%

Medium	
Water	55.93%
Land	21.98%
Air	18.69%
Soil	1.58%
Ballast	1.31%
Subsurface	0.51%

Consequence	
Fatalities	2.80%
Injuries	3.24%
Evacuation	1.64%
Road closure	1.87%
Rail track closure	1.97%

Responsible party	
Private enterprise	60.02%
Governmental entity	4.45%
Private citizen	4.14%
Public utility	3.07%
Unknown/other	28.32%

Reporting party	
Responsible party	51.68%
Third party	48.32%

Sheen color	
Rainbow	45.68%
Silvery	11.67%
Dark Black	5.03%
Redish	3.19%
Blueish	2.27%
Brown	2.22%
Whitish	1.84%
Grayish	1.74%
Dark Brown	1.52%
Yellowish Brown	0.71%
Light Brown	0.65%
Light Black	0.24%
Unknown/other	23.24%

Table II: Summary statistics

This table presents summary statistics for firm-year observations in our sample. Total spills (spill indicator) are the number of spills (indicator for the number of spills) reported in the U.S. Coast Guard's National Response Center (NRC) database for which a firm is responsible. CashFlow/Assets is the sum of income before extraordinary items and depreciation, divided by lagged assets. Assets are total reported assets. SalesGrowth is fraction change in sales divided by assets from the prior year. PPE/Assets is net plant, property, and equipment divided by total assets. Debt/Assets is book debt minus cash, divided by book assets. Capex/Assets is capital expenditures divided by lagged assets. Sales/Assets is sales divided by lagged assets.

	(1) Observations	(2) Mean	(3) Std Dev	(4) 10th	(5) Median	(6) 90th
Panel A: Spill statistics						
Total spills	185,671	0.11	0.76	0	0	0
Spill indicator	185,671	0.05	0.22	0	0	0
Evacuation indicator	185,671	0.0030	0.0547	0	0	0
Injuries indicator	185,671	0.0032	0.0564	0	0	0
Fatalities indicator	185,671	0.0006	0.0252	0	0	0
Panel B: Compustat variables						
CashFlow/Assets	157,809	-0.12	0.62	-0.56	0.06	0.22
Assets (millions)	173,378	1,523	5,141	3	92	2,759
Log(Assets)	159,257	4.54	2.62	1.28	4.53	7.94
SalesGrowth	147,984	0.31	1.19	-0.25	0.09	0.75
PPE/Assets	159,044	0.30	0.26	0.03	0.22	0.72
NetDebt/Assets	158,722	0.06	0.42	-0.52	0.09	0.56
Capex/Assets	156,430	0.09	0.17	0	0.04	0.21
Sales/Assets	158,465	1.25	1.16	0.09	0.99	2.58

Table III: Firm-level determinants of the total number of spills

This table presents esimates from OLS regressions of spill incidence on firm-level characteristics. The unit of observation is a firm-year. The dependent variables is the number of spills. See Table II for definitions of the explanatory variables. All regressions include firm and industry-year fixed effects, where industry is measured as the firm's primary 2-digit SIC code. 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)	(5)
CashFlow/Assets, time t	-0.003***	-0.004***	-0.004***	-0.004***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CashFlow/Assets, time t-1	-0.006***	-0.005***	-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Assets), t-1	0.011***	0.011***	0.011***	0.011***	0.012***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
SalesGrowth, t		0.001***	0.002***	0.001***	0.001**
		(0.000)	(0.000)	(0.000)	(0.000)
PPE/Assets, t-1			0.017	0.018	0.018
			(0.011)	(0.011)	(0.011)
Debt/Assets, t-1			0.002	0.002	0.002
			(0.003)	(0.003)	(0.003)
Capex/Assets, t				0.011	0.009
				(0.009)	(0.010)
Sales/Assets, t					0.001
					(0.001)
Observations	126,933	126,933	126,933	126,933	126,933
Adjusted R2	0.723	0.723	0.723	0.723	0.723

Table IV: Environmental spills and cash flows over different horizons

This table presents estimates from OLS regressions of spill incidence on firm-level characteristics, including cash flow measured at different horizons. The unit of observation is a firm-year. The dependent variables is the number of spills. See Table II for definitions of the explanatory variables. All regressions include firm and industry-year fixed effects, where industry is measured as the firm's primary 2-digit SIC code. 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)	(5)
CashFlow/Assets, time t+1		-0.002	-0.003*		-0.003*
		(0.001)	(0.002)		(0.002)
CashFlow/Assets, time t	-0.005***	-0.004***	-0.005***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
CashFlow/Assets, time t-1	-0.006***	-0.004***	-0.005***	-0.004**	-0.004**
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
CashFlow/Assets, time t-2	0.001		0.001		0.002
	(0.001)		(0.001)		(0.002)
Log(Assets), t-1	0.014***	0.012***	0.014***	0.013***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
SalesGrowth, t	0.001***	0.002***	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
PPE/Assets, t-1	0.020	0.011	0.014	0.016	0.012
	(0.013)	(0.012)	(0.014)	(0.012)	(0.013)
Debt/Assets, t-1	0.002	0.003	0.002	0.002	0.002
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Log(Assets), t-2				0.003**	0.002
				(0.001)	(0.002)
SalesGrowth, t-1				0.002***	0.002***
				(0.001)	(0.001)
PPE/Assets, t-2				0.006	0.004
				(0.010)	(0.011)
Debt/Assets, t-2				0.001	0.002
				(0.003)	(0.003)
Observations	111,831	111,608	98,017	110,714	96,966
Adjusted R2	0.728	0.727	0.732	0.728	0.733

Table V: Firm-level determinants of evacuations, injuries, and fatalities

This table presents esimates from OLS regressions of spill characteristics on firm-level characteristics. The unit of observation is a firm-year. The dependent variables is indicated in each column. See Table II for definitions of the explanatory variables. All regressions include firm and industry-year fixed effects, where industry is measured as the firm's primary 2-digit SIC code. 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) Evacuation (x 100)	(2) Any injuries (x 100)	(3) Any fatalities (x 100)
CashFlow/Assets, time t	0.002	0.004	0.003
	(0.015)	(0.024)	(0.009)
CashFlow/Assets, time t-1	-0.052*	-0.034*	0.005
	(0.027)	(0.021)	(0.007)
Log(Assets), t-1	0.035**	0.045***	-0.003
	(0.017)	(0.017)	(0.008)
SalesGrowth, t	-0.001	0.014*	-0.001
	(0.006)	(0.008)	(0.002)
PPE/Assets, t-1	0.135	0.306*	0.131*
	(0.138)	(0.162)	(0.068)
Debt/Assets, t-1	0.012	-0.062	-0.021
	(0.052)	(0.044)	(0.018)
Observations	126,933	126,933	126,933
Adjusted R-squared	0.180	0.195	0.184

Table VI: Effect of cash flow on spills caused by operator error versus equipment malfunction

This table presents esimates from OLS regressions of spill incidence on firm-level characteristics. The unit of observation is a firm-year. The dependent variables is an indicator for a spill due to the cause listed above each column. All coefficients and standard errors have been scaled by 100. See Table II for definitions of the explanatory variables. AAll regressions include firm and industry-year fixed effects, where industry is measured as the firm's primary 2-digit SIC code. 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)
	Spill due to equipment failure (x 100)		Spill due to operator error (x 100)	
CashFlow/Assets, time t	-0.10*	-0.10	-0.09**	-0.09*
	(0.06)	(0.07)	(0.04)	(0.05)
CashFlow/Assets, time t-1	-0.17***	-0.19**	-0.05	-0.07
	(0.06)	(0.08)	(0.04)	(0.05)
CashFlow/Assets, time t-2		-0.02		0.01
		(0.06)		(0.04)
Log(Assets), t-1	0.39***	0.45***	0.19***	0.21***
	(0.06)	(0.07)	(0.04)	(0.05)
SalesGrowth, t	0.09***	0.10***	-0.02*	-0.04**
	(0.02)	(0.03)	(0.01)	(0.02)
PPE/Assets, t-1	0.28	0.47	-0.02	-0.12
	(0.49)	(0.57)	(0.29)	(0.34)
Debt/Assets, t-1	0.12	0.01	0.06	0.09
	(0.15)	(0.17)	(0.09)	(0.11)
Observations	126,933	111,831	126,933	111,831
Adjusted R-squared	0.51	0.51	0.29	0.29

Table VII: Environmental spills and cash flow in the cross-section

This table presents esimates from OLS regressions of spill incidence on firm-level characteristics, including interactions with proxies for the degree to which a firm is likely to be cash-constrained. The unit of observation is a firm-year. The dependent variables is the number of spills. See Table II for definitions of the explanatory variables. All regressions include firm and industry-year fixed effects, where industry is measured as the firm's primary 2-digit SIC code. 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)
CashFlow/Assets, time t	-0.004***	-0.005***	-0.004***
	(0.001)	(0.002)	(0.001)
CashFlow/Assets, t-1	-0.005***	-0.010***	-0.006***
	(0.002)	(0.002)	(0.002)
(CashFlow/Assets, t) × (Debt/Assets, t-1)	-0.004**		
	(0.002)		
(CashFlow/Assets, t-1) × (Debt/Assets, t-2)	-0.003		
	(0.002)		
(CashFlow/Assets, t) × log(Assets, t-1)		0.001	
		(0.001)	
(CashFlow/Assets, t-1) × log(Assets, t-2)		0.004***	
		(0.001)	
(CashFlow/Assets, t) × (Dividends/Assets, t-1)			-0.267
			(0.212)
(CashFlow/Assets, t-1) × (Dividends/Assets, t-2)			0.038
			(0.134)
Log(Assets), t-1	0.014***	0.012***	0.014***
	(0.002)	(0.002)	(0.002)
SalesGrowth, t	0.001***	0.001***	0.002***
	(0.001)	(0.001)	(0.001)
PPE/Assets, t-1	0.020	0.018	0.019
	(0.013)	(0.013)	(0.013)
Debt/Assets, t-1	0.000	0.001	0.002
	(0.004)	(0.004)	(0.004)
Debt/Assets, t-2	-0.001		
	(0.003)		
Log(Assets), t-2		0.003**	
		(0.001)	
Dividends/Assets, t-1			-0.013
			(0.063)
Dividends/Assets, t-2			0.084
			(0.052)
Observations	111,922	112,072	111,436
Adjusted R-squared	0.728	0.728	0.727

Table VIII: Comparision of characteristics of treated and control firms in AJCA and crisis experiments

This table presents comparisons of the mean pre-treatment values of observable characteristics for treatment and control firms in the two quasi-natural experiments that we analyze. Panel A presents the comparisons for the AJCA experiment. A firm is in the treatment group in the AJCA experiment if it reported positive cumulative foreign profits over the three years prior to 2004 (the year of the AJCA). Panel B presents the comparisons for the financial crisis experiment. A firm is in the treatment group in the financial crisis experiment if its debt due in the next year divided by assets as of fiscal year end 2007 is greater than a specified threshold. We use three separate thresholds: 0.07, 0.05, and 0.03. The control group for each treatment group is constructed via propensity score matching without replacement.

Panel A: AJCA

	PosFrgnProf	Control
CashFlow/Assets	0.084	0.084
Log(Assets)	6.829	6.867
SalesGrowth	0.111	0.100
PPE/Assets	0.249	0.250
Debt/Assets	0.069	0.058
Spills/year	0.254**	0.175
Firms	886	886

Panel B: Financial crisis

	0.07 cutoff		0.05 cutoff		0.03 cutoff	
	HighMat	Control	HighMat	Control	HighMat	Control
CashFlow/Assets	-0.214	-0.181	-0.151	-0.136	-0.096	-0.095
Log(Assets)	5.271	5.381	5.544	5.739	5.753	5.860
SalesGrowth	0.418	0.374	0.390	0.457	0.319**	0.216
PPE/Assets	0.306	0.306	0.310	0.311	0.316	0.330
Debt/Assets	0.265	0.270	0.239	0.230	0.213	0.212
Spills/year	0.747	0.767	0.099	0.084	0.116	0.136
Firms	348	348	478	478	771	771

Table IX: Difference-in-differences around AJCA

This table presents difference-in-differences analysis of the change in number of spills after the American Jobs Creation Act of 2004 for firms with and without recent foreign profits prior to the Act. The sample consists of firm-year observations in the periods 2001-2003 and 2005-2007 for matched treatment and control groups. The dependent variable is the number of spills. The indicator $PosFrgnProf$ takes a value of one if the firm's cumulative reported foreign profits in 2001-2003 were positive and zero otherwise. The indicator $Post2004$ takes a value of one for observations in the period 2005-2007 and zero for observations in the period 2001-2003. The indicators $YearT$ take a value of one for observations in year T, $T = 2005, 2006, 2007$, and zero otherwise. The indicator $Year2006-07$ takes a value of one for observations in the years 2006 and 2007 and zero otherwise. 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)	(5)	(6)
Post2004 * PosFrgnProf	-0.024*			-0.025*		
	(0.013)			(0.014)		
Year2005 * PosFrgnProf		-0.014	-0.014		-0.015	-0.015
		(0.016)	(0.016)		(0.016)	(0.016)
Year2006 * PosFrgnProf		-0.017			-0.019	
		(0.018)			(0.018)	
Year2007 * PosFrgnProf		-0.043**			-0.044**	
		(0.018)			(0.018)	
Year2006-07 * PosFrgnProf			-0.030*			-0.031**
			(0.016)			(0.016)
Controls	No	No	No	Yes	Yes	Yes
Observations	12,063	12,063	12,063	11,890	11,890	11,890
Adjusted R2	0.815	0.815	0.815	0.816	0.816	0.816

Table X: Difference-in-differences around financial crisis

This table presents difference-in-differences analysis of the change in number of spills after the onset of the financial crisis for firms with and without high levels of debt maturing within one year as of the onset. The sample consists of firm-year observations between 2005 and 2010 for matched treatment and control groups. The dependent variable is the number of spills. The indicator $HighMat$ takes a value of one if a firm has debt due within one year as of the end of fiscal year 2007 divided by total assets exceeding a specified threshold. The thresholds in Panels A, B, and C are 0.07, 0.05, and 0.03, respectively. The indicator $Post2007$ takes a value of one for observations in the period 2008-2010 and zero for observations in the period 2005-2007. The indicators $YearT$ take a value of one for observations in year T, $T = 2008, 2009, 2010$, and zero otherwise. The indicator $Year2009-10$ takes a value of one for observations in the years 2009 and 2010 and zero otherwise. 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.

Panel A: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.07

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.013			0.011		
	(0.012)			(0.013)		
Year2008 * HighMat		-0.024	-0.024		-0.026	-0.026
		(0.017)	(0.017)		(0.018)	(0.018)
Year2009 * HighMat		0.026			0.025	
		(0.019)			(0.020)	
Year2010 * HighMat		0.047***			0.048**	
		(0.017)			(0.019)	
Year2009-10 * HighMat			0.036**			0.036**
			(0.015)			(0.016)
Controls	No	No	No	Yes	Yes	Yes
Observations	3,973	3,973	3,973	3,779	3,779	3,779
Adjusted R2	0.794	0.794	0.794	0.795	0.796	0.796

Table X: Financial Crisis (continued from previous page)

Panel B: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.05

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.024*			0.028*		
	(0.014)			(0.015)		
Year2008 * HighMat		0.012	0.012		0.016	0.016
		(0.015)	(0.015)		(0.016)	(0.016)
Year2009 * HighMat		0.018			0.023	
		(0.019)			(0.020)	
Year2010 * HighMat		0.044**			0.050**	
		(0.019)			(0.020)	
Year2009-10 * HighMat			0.030*			0.036**
			(0.017)			(0.018)
Controls	No	No	No	Yes	Yes	Yes
Observations	5,481	5,481	5,481	5,208	5,208	5,208
Adjusted R2	0.739	0.739	0.739	0.741	0.741	0.741

Panel C: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.03

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.003			0.007		
	(0.012)			(0.012)		
Year2008 * HighMat		-0.034**	-0.034**		-0.031**	-0.031**
		(0.014)	(0.014)		(0.015)	(0.015)
Year2009 * HighMat		0.030*			0.034**	
		(0.017)			(0.017)	
Year2010 * HighMat		0.020			0.024	
		(0.016)			(0.017)	
Year2009-10 * HighMat			0.025*			0.030**
			(0.014)			(0.014)
Controls	No	No	No	Yes	Yes	Yes
Observations	8,872	8,872	8,872	8,486	8,486	8,486
Adjusted R2	0.790	0.791	0.791	0.794	0.794	0.794

Appendix A. Data Appendix

In this section, we describe in detail the procedure by which we match firms in the National Response Center (NRC) database to firms in the Compustat database. Matches are year-specific. A spill can only match to one company-year in Compustat, but a company-year can be matched to multiple spills. We adopt a heirarchial matching procedure, where we first identify the highest-quality matches and then consider the possibility of lower quality matches for the remaining spills.

Basic processing First, we standardize the naming conventions in both the NRC and Compustat databases by replacing words like "company" and "corporation" with their abbreviations (i.e., "co", "corp", etc). We also replace the word "and" with the symbol "&", remove all punctuation, and convert all text to lower case.

Compustat reports both a firm's "legal name" and its "common name", while the NRC database has only one entry with a company's name ("responsible company"). We first identify spills where there is perfect agreement between either a firm's legal name and the responsible company name or a firm's common name and the responsible company name. This yields 4,400 "perfect" matches. We then identify 3-digit SIC codes in the Compustat database that never appear among the perfect matches and drop all Compustat firm-years in these sectors from further consideration (7,955 company-year observations out of 271,930). This step is done to minimize false positives. Next, we remove the following common suffixes from company names in both datasets: "corp", "co", "inc", "llc", "ltd", "plc", "comp", "coop", and "lp". We repeat the string matching again, and add the resulting new matches to our set of perfect matches. At this point, the matched database will include all firms whose names match exactly or only differ by a suffix.

Non-perfect matches To aid us in identifying less straightforward matches, we next calculate the frequency of each word in the responsible company name in the entire NRC database and identify the 200 top common words and their variations that are also common English words. For example, we exclude the word "Chevron" from this list of common words because even though it appears frequently in the NRC database, it is not a common English word. However, we do include proper nouns and adjectives such as "American", provided they are sufficiently general. The final word list is available upon request. As explained below, this step is also done to minimize false matches.

Next, we calculate the "similarity ratio" between the responsible party name for each of the unmatched spills and the firm legal and common names in the Compustat database for that year. The similarity ratio for a pair of strings is defined as $2\frac{M}{T}$. M is the number of matching elements and cannot be larger than the length of the shorter string; T is the total number of elements in the two strings. Thus, the similarity ratio is bounded below by zero (two strings with no common letters) and bounded above by 1 (two identical strings). To implement this comparison, we use SequenceMatcher from the difflib library in Python. We differentiate the results by whether (a) the common words in company names, if any, match and (b) whether the firms' reported locations match. Intuitively, we should discount pairs with high similarity ratios if the names contain common words and have higher confidence in matches if the reported firm locations are the same.

Specifically, if firm names in the two datasets each contain a common word and the common words do not match (after allowing for word varitions), we classify the pair as a non-match. If the common words do match (again, allowing for word variations), if one company name contains a common word but the other does not, *or* if neither company name contains a common word, we compare the non-common parts of the responsible company name to the Compustat company names. If the common words match up to simple word variations *and* the non-common parts match perfectly, we record the pair as a match. This step adds 8,653 matched spills where slight differences in common words between company names exist.

The remaining pairs are further split into four categories, ranked by the expected likelihood that the pairs are a true match: (1) common words and locations both match; (2) common words match but locations do not; (3) at most one name contains common words and locations match; and (4) at most one name contains common words and locations do not match.

It is not feasible to manually check all the imperfectly matched pairs. Instead, for each of these categories, we visually inspected the data to determine similarity ratio cutoffs above which roughly 90 percent of the pairs looked like true matches. Pairs with similarity ratios above these cutoffs were recorded as matches. Similarly, we determined similarity ratio cutoffs below which only 25 percent or so of pairs looked like true matches. Pairs with similarity ratios below these cutoffs did not qualify as matches. The lower and upper bounds as well as the number of matches (i.e., pairs whose similarity ratio exceeds the upper bound) for each category are shown in Table B3.

In a few cases, the procedure for determining non-perfect matches results in the same responsible company matching to two different Compustat companies (i.e., because both pairs will exceed the relevant threshold in the same category or in different categories). Visual inspection of such cases revealed that it would be difficult to systematically determine the match status in such cases, and they were dropped from the set of possible matches.

Mechanical Turk Pairs with similarity ratios between the lower and upper bounds in their respective category in Table B3 were manually reviewed by Mechanical Turk workers to determine if they are a likely match. The total number of potential matches in this category was 13,957. However, because these pairs are year-specific, only 6,067 pairs spanning all unique name combinations were necessary to classify the uncertain matches.

Each worker was asked to "evaluate whether two sets of information identify the same company." The overall instructions given to the worker were as follows.

> You will be given the name of a company responsible for a chemical spill. You will also be given information about a "suspect company", including its name, industry, and a desription of what it does. Your task will be to decide if the two match.

Each entry was laid out as follows, with italicized text indicating which pieces of information the workers received.

> Give your opinion about the likelihood that the two companies about which information is provided below are the same company.
>
> **Company responsible for spilling material such as oil or hazardous waste**
>
> - Company Name: *Name of responsible company*
>
> **Suspect Company**
>
> - Company Name: *Legal name of Compustat company*
> - Industry Sector: *Description of the company's 3-digit SIC code*
> - Business Description: *Business description of Compustat company*

The workers were then asked to answer:

> How likely is it that the responsible company and the suspect company are the same company?

- ○ Very likely
- ○ Somewhat likely
- ○ No opinion
- ○ Somewhat unlikely
- ○ Very unlikely

Each pair of potential matches was reviewed by three Mechanical Turk workers. Workers could review multiple pairs but could not review the same pair twice. A pair was deemed a match if at least two workers said that it was "very" or "somewhat" likely that the responsible company and the suspect company were the same company. As a result, 8,057 matched pairs were added to our database and 5,900 non-matches were discarded.

Appendix B. Appendix Tables

Table B1: Robustness to ignoring imperfect matches

This table presents esimates from OLS regressions of spill incidence on firm-level characteristics. The unit of observation is a firm-year. The dependent variables is the number of spills. See Table II for definitions of the explanatory variables. All regressions include firm and industry-year fixed effects, where industry is measured as the firm's primary 2-digit SIC code. 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)	(5)
CashFlow/Assets, time t	-0.002**	-0.002**	-0.002***	-0.002**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CashFlow/Assets, time t-1	-0.003***	-0.003**	-0.002**	-0.002**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Assets), t-1	0.008***	0.008***	0.008***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
SalesGrowth, t		0.001**	0.001**	0.001**	0.001**
		(0.000)	(0.000)	(0.000)	(0.000)
PPE/Assets, t-1			0.011	0.012	0.012
			(0.010)	(0.010)	(0.010)
Debt/Assets, t-1			0.002	0.002	0.002
			(0.003)	(0.003)	(0.003)
Capex/Assets, t				0.008	0.009
				(0.008)	(0.008)
Sales/Assets, t					-0.001
					(0.001)
Observations	126,933	126,933	126,933	126,933	126,933
Adjusted R-squared	0.700	0.700	0.700	0.700	0.700
Fixed effects	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year

Table B2: Robustness to excluding industries with few spills

This table presents esimates from OLS regressions of spill incidence on firm-level characteristics, excluding 3-digit SIC industries that average fewer than 0.01 spills per firm-year. The unit of observation is a firm-year. The dependent variables is the number of spills. See Table II for definitions of the explanatory variables. Standard errors clustered at the firm level are reported in parentheses below each point estimate. All regressions include firm and industry-year fixed effects, where industry is measured as the firm's primary 2-digit SIC code. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

	(1)	(2)	(3)	(4)	(5)
CashFlow/Assets, time t	-0.004**	-0.004**	-0.005***	-0.004**	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
CashFlow/Assets, time t-1	-0.008***	-0.007***	-0.006***	-0.006***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Log(Assets), t-1	0.016***	0.016***	0.016***	0.017***	0.017***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
SalesGrowth, t		0.002***	0.002***	0.002***	0.002***
		(0.001)	(0.001)	(0.001)	(0.001)
PPE/Assets, t-1			0.018	0.019	0.019
			(0.015)	(0.015)	(0.015)
Debt/Assets, t-1			0.002	0.003	0.002
			(0.005)	(0.005)	(0.005)
Capex/Assets, t				0.018	0.016
				(0.012)	(0.013)
Sales/Assets, t					0.001
					(0.002)
Observations	93,740	93,740	93,740	93,740	93,740
Adjusted R-squared	0.723	0.723	0.723	0.723	0.723
Fixed effects	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year	gvkey, SIC-2-by-year

Table B3: Similarity ratio cutoffs for imperfect matches, by category

Category	Lower bound	Upper bound	Number of matches
Common words and locations both match	0.50	0.61	73
Common words match but locations do not	0.66	0.84	1,109
At most one name contains common words; locations match	0.62	0.90	1,316
At most one name contains common words; locations do not match	0.85	0.95	4,693

Table B4: Difference-in-differences around AJCA - matching on pre-AJCA spill rate only

This table presents difference-in-differences analysis of the change in number of spills after the American Jobs Creation Act of 2004 for firms with and without recent foreign profits prior to the Act. The sample consists of firm-year observations in the periods 2001-2003 and 2005-2007 for matched treatment and control groups. Controls here are matched only on similarity of spills per year over the period 2001-2003. The dependent variable is the number of spills. The indicator $PosFrgnProf$ takes a value of one if the firm's cumulative reported foreign profits in 2001-2003 were positive and zero otherwise. The indicator $Post2004$ takes a value of one for observations in the period 2005-2007 and zero for observations in the period 2001-2003. The indicators $YearT$ take a value of one for observations in year T, $T = 2005, 2006, 2007$, and zero otherwise. The indicator $Year2006-07$ takes a value of one for observations in the years 2006 and 2007 and zero otherwise. 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)	(5)	(6)
Post2004 * PosFrgnProf	-0.015			-0.019		
	(0.014)			(0.014)		
Year2005 * PosFrgnProf		0.003	0.003		-0.000	-0.000
		(0.018)	(0.018)		(0.018)	(0.018)
Year2006 * PosFrgnProf		-0.020			-0.025	
		(0.020)			(0.020)	
Year2007 * PosFrgnProf		-0.032			-0.036*	
		(0.020)			(0.020)	
Year2006-07 * PosFrgnProf			-0.026			-0.030*
			(0.017)			(0.017)
Controls	No	No	No	Yes	Yes	Yes
Observations	12,064	12,064	12,064	11,899	11,899	11,899
Adjusted R2	0.808	0.808	0.808	0.809	0.809	0.809

Table B5: Difference-in-differences around AJCA, matching within industry

This table presents difference-in-differences analysis of the change in number of spills after the American Jobs Creation Act of 2004 for firms with and without recent foreign profits prior to the Act. The sample consists of firm-year observations in the periods 2001-2003 and 2005-2007 for matched treatment and control groups. Treatment and control firms are matched within 2-digit SIC industry. The dependent variable is the number of spills. The indicator $PosFrgnProf$ takes a value of one if the firm's cumulative reported foreign profits in 2001-2003 were positive and zero otherwise. The indicator $Post2004$ takes a value of one for observations in the period 2005-2007 and zero for observations in the period 2001-2003. The indicators $YearT$ take a value of one for observations in year T, $T = 2005, 2006, 2007$, and zero otherwise. The indicator $Year2006 - 07$ takes a value of one for observations in the years 2006 and 2007 and zero otherwise. 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)	(5)	(6)
Post2004 * PosFrgnProf	-0.006			-0.007		
	(0.014)			(0.014)		
Year2005 * PosFrgnProf		0.003	0.003		0.002	0.002
		(0.016)	(0.016)		(0.017)	(0.017)
Year2006 * PosFrgnProf		-0.011			-0.011	
		(0.020)			(0.020)	
Year2007 * PosFrgnProf		-0.012			-0.013	
		(0.019)			(0.019)	
Year2006-07 * PosFrgnProf			-0.011			-0.012
			(0.017)			(0.017)
Controls	No	No	No	Yes	Yes	Yes
Observations	10,543	10,543	10,543	10,387	10,387	10,387
Adjusted R2	0.797	0.797	0.797	0.797	0.797	0.797

Table B6: Difference-in-differences around financial crisis, matching within industry

This table presents difference-in-differences analysis of the change in number of spills after the onset of the financial crisis for firms with and without high levels of debt maturing within one year as of the onset. The sample consists of firm-year observations between 2005 and 2010 for matched treatment and control groups. Treatment and control firms are matched within 2-digit SIC industry. The dependent variable is the number of spills. The indicator $HighMat$ takes a value of one if a firm has debt due within one year as of the end of fiscal year 2007 divided by total assets exceeding a specified threshold. The thresholds in Panels A, B, and C are 0.07, 0.05, and 0.03, respectively. The indicator $Post2007$ takes a value of one for observations in the period 2008-2010 and zero for observations in the period 2005-2007. The indicators $YearT$ take a value of one for observations in year T, $T = 2008, 2009, 2010$, and zero otherwise. The indicator $Year2009 - 10$ takes a value of one for observations in the years 2009 and 2010 and zero otherwise. 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.

Panel A: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.07

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.019			0.020		
	(0.012)			(0.013)		
Year2008 * HighMat		0.001	0.001		0.001	0.000
		(0.015)	(0.015)		(0.016)	(0.016)
Year2009 * HighMat		0.007			0.010	
		(0.020)			(0.021)	
Year2010 * HighMat		0.056***			0.060***	
		(0.020)			(0.021)	
Post2009-10 * HighMat			0.030*			0.033*
			(0.015)			(0.017)
Controls	No	No	No	Yes	Yes	Yes
Observations	3,853	3,853	3,853	3,646	3,646	3,646
Adjusted R2	0.809	0.810	0.809	0.811	0.812	0.811

Table B6: Financial Crisis (continued from previous page)

Panel B: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.05

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.005			0.006		
	(0.013)			(0.014)		
Year2008 * HighMat		-0.021	-0.021		-0.018	-0.018
		(0.017)	(0.017)		(0.018)	(0.018)
Year2009 * HighMat		0.015			0.017	
		(0.017)			(0.018)	
Year2010 * HighMat		0.026			0.028	
		(0.019)			(0.020)	
Post2009-10 * HighMat			0.020			0.022
			(0.014)			(0.015)
Controls	No	No	No	Yes	Yes	Yes
Observations	5,345	5,345	5,345	5,113	5,113	5,113
Adjusted R2	0.758	0.759	0.759	0.763	0.764	0.764

Panel C: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.03

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.015			0.017		
	(0.011)			(0.011)		
Year2008 * HighMat		-0.008	-0.008		-0.006	-0.006
		(0.013)	(0.013)		(0.013)	(0.013)
Year2009 * HighMat		0.024			0.026	
		(0.016)			(0.016)	
Year2010 * HighMat		0.035**			0.037**	
		(0.015)			(0.016)	
Post2009-10 * HighMat			0.029**			0.031**
			(0.013)			(0.014)
Controls	No	No	No	Yes	Yes	Yes
Observations	8,537	8,537	8,537	8,186	8,186	8,186
Adjusted R2	0.782	0.782	0.782	0.786	0.786	0.786

Table B7: Difference-in-differences around AJCA, matching within industry, alternative fixed effects

This table presents difference-in-differences analysis of the change in number of spills after the American Jobs Creation Act of 2004 for firms with and without recent foreign profits prior to the Act. The sample consists of firm-year observations in the periods 2001-2003 and 2005-2007 for matched treatment and control groups. Treatment and control firms are matched within 2-digit SIC industry. The dependent variable is the number of spills. The indicator $PosFrgnProf$ takes a value of one if the firm's cumulative reported foreign profits in 2001-2003 were positive and zero otherwise. The indicator $Post2004$ takes a value of one for observations in the period 2005-2007 and zero for observations in the period 2001-2003. The indicators $YearT$ take a value of one for observations in year T, $T = 2005, 2006, 2007$, and zero otherwise. The indicator $Year2006 - 07$ takes a value of one for observations in the years 2006 and 2007 and zero otherwise. All regressions include 2-digit SIC industry by year and 3-digit SIC industry 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)	(5)	(6)
Post2004 * PosFrgnProf	-0.036			-0.046**		
	(0.023)			(0.023)		
Year2005 * PosFrgnProf		-0.017	-0.017		-0.033	-0.033
		(0.025)	(0.025)		(0.025)	(0.025)
Year2006 * PosFrgnProf		-0.043			-0.052*	
		(0.027)			(0.027)	
Year2007 * PosFrgnProf		-0.051*			-0.055**	
		(0.027)			(0.027)	
Year2006-07 * PosFrgnProf			-0.047*			-0.053**
			(0.025)			(0.025)
Controls	No	No	No	Yes	Yes	Yes
Observations	10,532	10,532	10,532	10,369	10,369	10,369
Adjusted R2	0.345	0.345	0.345	0.381	0.381	0.381

Table B8: Difference-in-differences around financial crisis, matching within industry, alternative fixed effects

This table presents difference-in-differences analysis of the change in number of spills after the onset of the financial crisis for firms with and without high levels of debt maturing within one year as of the onset. The sample consists of firm-year observations between 2005 and 2010 for matched treatment and control groups. Treatment and control firms are matched within 2-digit SIC industry. The dependent variable is the number of spills. The indicator $HighMat$ takes a value of one if a firm has debt due within one year as of the end of fiscal year 2007 divided by total assets exceeding a specified threshold. The thresholds in Panels A, B, and C are 0.07, 0.05, and 0.03, respectively. The indicator $Post2007$ takes a value of one for observations in the period 2008-2010 and zero for observations in the period 2005-2007. The indicators $YearT$ take a value of one for observations in year T, $T = 2008, 2009, 2010$, and zero otherwise. The indicator $Year2009-10$ takes a value of one for observations in the years 2009 and 2010 and zero otherwise. All regressions include 2-digit SIC industry by year and 3-digit SIC industry 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.

Panel A: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.07

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.034			0.032		
	(0.026)			(0.025)		
Year2008 * HighMat		0.011	0.011		0.008	0.008
		(0.027)	(0.027)		(0.026)	(0.026)
Year2009 * HighMat		0.020			0.020	
		(0.031)			(0.031)	
Year2010 * HighMat		0.079**			0.079**	
		(0.032)			(0.033)	
Year2009-10 * HighMat			0.048*			0.047
			(0.029)			(0.029)
Controls	No	No	No	Yes	Yes	Yes
Observations	3,843	3,843	3,843	3,634	3,634	3,634
Adjusted R2	0.378	0.379	0.378	0.402	0.403	0.402

Table B8: Financial Crisis (continued from previous page)

Panel B: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.05

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.036			0.032		
	(0.022)			(0.021)		
Year2008 * HighMat		0.010	0.010		0.005	0.005
		(0.023)	(0.023)		(0.022)	(0.022)
Year2009 * HighMat		0.048*			0.045*	
		(0.028)			(0.027)	
Year2010 * HighMat		0.055**			0.051*	
		(0.027)			(0.027)	
Year2009-10 * HighMat			0.051**			0.048**
			(0.024)			(0.024)
Controls	No	No	No	Yes	Yes	Yes
Observations	5,335	5,335	5,335	5,101	5,101	5,101
Adjusted R2	0.273	0.273	0.273	0.317	0.318	0.318

Panel C: HighMat = 1 if DebtDueIn1Year/Assets $\geq$ 0.03

	(1)	(2)	(3)	(4)	(5)	(6)
Post2007 * HighMat	0.044**			0.047**		
	(0.019)			(0.019)		
Year2008 * HighMat		0.022	0.022		0.027	0.027
		(0.020)	(0.020)		(0.020)	(0.020)
Year2009 * HighMat		0.054**			0.058**	
		(0.023)			(0.023)	
Year2010 * HighMat		0.059**			0.060***	
		(0.023)			(0.023)	
Year2009-10 * HighMat			0.056***			0.059***
			(0.021)			(0.021)
Controls	No	No	No	Yes	Yes	Yes
Observations	8,532	8,532	8,532	8,181	8,181	8,181
Adjusted R2	0.236	0.236	0.236	0.267	0.267	0.267