In the Path of the Storm: Does Distress Cause Non-Financial Firms To Risk-Shift?*

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Abstract

While risk-shifting is an important theoretical concept, there is so far no convincing evidence suggesting that financial distress causes non-financial firms to risk-shift. Reasons for this dearth of evidence are that risk-shifting by non-financial firms is hard to measure and likely endogenously related to financial distress. In our study, we view a non-financial firm as a portfolio of its operating segments. Accordingly, we apply modern portfolio theory to calculate firm risk, using segment data to derive the portfolio weights and equity data from single segment firm-industry portfolios to estimate the risk of the portfolio constituents. Our risk-shifting proxy is the change in firm risk induced only through changes in the segment data. To study whether financial distress causes risk-shifting, we use a triple-differences methodology. The methodology estimates the effect of hurricane-induced shocks to financial distress on the risk-shifting behavior of firms with different levels of pre-hurricane distress risk and located inside or outside of the affected regions. We find that moderately — but not highly — distressed firms located in the affected regions risk-shift. Risk-shifting leads to a pronounced increase in post-hurricane failure rates. In fact, the post-hurricane failure rates of risk-shifters surpass even those of non-risk-shifters that are initially more distressed than the risk-shifters. Analyzing why highly distressed firms do not risk-shift, we show that these firms are likely to have violated financial covenants in the past, and that it is the incidence of these violations that keeps them from risk-shifting.

Key words: Agency conflicts; Risk-shifting; Financial distress; Segment data; Hurricane strikes.

JEL classification: G32, G33.

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Abstract

While risk-shifting is an important theoretical concept, there is so far no convincing evidence suggesting that financial distress causes non-financial firms to risk-shift. Reasons for this dearth of evidence are that risk-shifting by non-financial firms is hard to measure and likely endogenously related to financial distress. In our study, we view a non-financial firm as a portfolio of its operating segments. Accordingly, we apply modern portfolio theory to calculate firm risk, using segment data to derive the portfolio weights and equity data from single segment firm-industry portfolios to estimate the risk of the portfolio constituents. Our risk-shifting proxy is the change in firm risk induced only through changes in the segment data. To study whether financial distress causes risk-shifting, we use a triple-differences methodology. The methodology estimates the effect of hurricane-induced shocks to financial distress on the risk-shifting behavior of firms with different levels of pre-hurricane distress risk and located inside or outside of the affected regions. We find that moderately — but not highly — distressed firms located in the affected regions risk-shift. Risk-shifting leads to a pronounced increase in post-hurricane failure rates. In fact, the post-hurricane failure rates of risk-shifters surpass even those of non-risk-shifters that are initially more distressed than the risk-shifters. Analyzing why highly distressed firms do not risk-shift, we show that these firms are likely to have violated financial covenants in the past, and that it is the incidence of these violations that keeps them from risk-shifting.

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1 Introduction

Risk-shifting is one of the key theoretical concepts in corporate finance. Intuitively speaking, risk-shifting implies that economic agents with convex payoff functions have incentives to increase the volatility of their payoff (Jensen and Meckling (1976)). For example, because shareholders hold the equivalent of a call option on the firm's assets, managers behaving in shareholders' best interests have incentives to increase firm risk. This incentive to risk-shift becomes stronger for firms with more debt outstanding (Black and Scholes (1973)). However, despite many theoretical studies analyzing the implications of risk-shifting for non-financial firms (e.g., see Green (1984), Campell and Kracaw (1990), Leland (1998), and Gârleanu and Zwiebel (2009)), there is so far no convincing evidence that such firms risk-shift (Almeida et al. (2011)).

Establishing that financial distress motivates non-financial firms to risk-shift is difficult for at least two reasons. First, coming up with an efficient risk-shifting proxy is not an easy task. To wit, while changes in equity volatilities or changes in structural model-implied asset volatilities might *look* like good candidates, they are partially determined by factors outside of managers' control, for example, the economic climate. Also, structural models lead changes in asset volatility to be mechanically related to changes in distress risk. Second, financial distress and risk-shifting are likely endogenously related. For example, it is sometimes reasoned that managerial appetite for risk decreases with tenure (e.g., Tufano (1996)). Thus, when a young manager takes over a firm, they will likely start accepting riskier investment projects than their predecessor, thereby boosting distress risk. However, the thereby generated positive relationship between risk-shifting and financial distress is not causal, but instead driven by an omitted variable. In the example, the omitted variable is managers' risk aversion (Opler and Titman (1994)).

In this article, we try to find solutions to the above two complications to establish the causal relationship between financial distress and risk-shifting. In doing so, our first contribution is to devise a risk-shifting proxy that (i) exclusively reflects managerial decisions to change firm risk and that (ii) is not mechanically linked to financial distress. Following the lead of Armstrong and Vashishta (2012), we view the firm as a portfolio of operating segments. Accordingly, we apply Markowitz' (1952) modern portfolio theory to determine firm risk. To be more specific, we approximate the portfolio weight of each segment by its book value of assets. To estimate the segments' variance-covariance matrix, we associate each segment with an equally-weighted stock portfolio containing only firms exclusively operating in the segment's industry (a "stand-alone firm industry portfolio"). Using the portfolio weights and the portfolio risk matrix as ingredients, we calculate firm risk from the formula for the return variance of a multi-asset portfolio. Our risk-shifting shifting proxy is current firm risk minus last year's firm risk, where both current and last year's firm risk are calculated using a variance-covariance matrix estimated from data spanning the period from the end of the last fiscal year to the end of the current.

Assuming that changes in asset values reflect investments,[1] our risk-shifting proxy is entirely driven by managerial choices, and not by factors outside of managers' control. Another advantage is that it is not mechanically related to popular distress risk proxies. Notwithstanding, our risk-shifting proxy is not a perfect measure for risk-shifting. To wit, we can only create the proxy for large multi-segment firms. Also, it does not capture more subtle forms of risk-shifting, as, for example, running machines over-time or shifting towards higher mark-up products.

Our second contribution is to use natural disasters as exogenous shocks to firms' distress risk. The natural disasters that we focus on are hurricane strikes. For hurricane strikes to be a *valid* instrument for distress risk, they must affect a majority of firms and they must be hard to predict. Reviewing studies on hurricane strikes, Dessaint and Matray (2014) conclude that around half of all U.S. firms are exposed to the risk of a hurricane strike and that hurricane strikes are difficult to predict. Also, Baker and Bloom (2013) find no evidence of an increase in newspaper mentions of hurricanes during the days shortly before the strike. For hurricanes to be a *powerful* instrument, they must have a meaningful effect on firms' distress risk. Our evidence shows that, while the default probability of hurricane-struck firms hovers around 5% over the year before the strike, it jumps to close to 10% over the two months following the strike, and it increases to a maximum of around 12% over the following six. From then on, it takes another twelve months before distress risk drops back to its pre-hurricane levels. Overall, we conclude that hurricane strikes constitute both a valid and powerful instrument for distress risk.

Using hurricane strikes as exogenous shocks to financial distress, we employ a triple-differences (DIDID) methodology to establish the causal effect of financial distress on risk-shifting. Intuitively speaking, our methodology compares the change in risk-shifting from the pre-hurricane period to the post-hurricane period between firms located inside the hurricane-struck areas and those

[1] This is a common assumption in the asset pricing literature (see Cooper et al. (2008), Hou et al. (2014), and Fama and French (2014)).

located outside of them. Within each type of area, we then also compare the change in risk-shifting between firms with different levels of pre-hurricane distress risk.

Not entirely consistent with the theoretical literature, we obtain an inverted U-shaped relationship between financial distress and risk-shifting. Specifically, firms with a low pre-hurricane distress risk never risk-shift, not even when they are headquartered in hurricane-struck areas. In contrast, firms with a moderate distress risk before the hurricane strike risk-shift — if they are located in the disaster areas. Finally, the most distressed firms again do not risk-shift, independent of whether they located inside or outside of the affected areas. We find economically meaningful risk-shifting effects in the data. For example, firms with a moderate level of pre-hurricane distress risk located in hurricane-struck areas increase their risk by 2.42% (6.64%) over the first year (the first two years) following the hurricane strike. In contrast, similarly distressed firms located outside of the affected areas increase their risk by only 0.48% (0.89%). Finally, the most and least distressed risk firms never increase their risk by more than two percent.

We investigate the real consequences of risk-shifting. Looking at firms in the disaster areas, the initially moderately distressed firms that risk-shift have higher failure rates than the initially weakly or highly distressed firms over several post-hurricane periods. They also have higher failure rates than the distressed firms in the non-disaster areas. The differences are economically important. For example, comparing firms with a moderate pre-hurricane distress risk over the ten years proceeding the hurricane, 63% of them fail in the disaster areas, whereas only 43% of them fail in the non-disaster areas. Of the initially most distressed firms, 36% of them fail in the disaster areas, but 53% of them fail in the non-disaster areas. Thus, the most distressed firms in the hurricane-affected areas actually seem to decrease — not increase — their risk.

What keeps the most distressed firms from risk-shifting? One possibility is that the prospects of the most distressed firms are so gloomy that risk-shifting no longer pays off for them.[2] However, this possibility is inconsistent with the finding that the most distressed firms have lower failure rates than the moderately distressed firms following the hurricane strike. Another possibility is that there are a relatively high number of financial covenant violators among the most distressed firms. If so, then most highly distressed firms would likely be tightly controlled by their financiers and thus unable to risk-shift (Chava and Roberts (2008) and Pryshchepa et al. (2013)). We offer

[2]Theory predicts that the incentive to risk-shift is strongest when the expected payoff is close to the kink in the payoff function (e.g., Murphy (1999)).

evidence consistent with the second possibility. In particular, highly distressed firms are 2-3 times more likely to have violated financial covenants than moderately distressed firms. Also, there is a strongly negative correlation between being a financial covenant violator and post-hurricane risk-shifting in the sample of moderately and highly distressed firms.

Our article contributes to a large literature studying whether financial distress causes economic agents to risk-shift. Focusing on firms from the financial industry, there is ample evidence to suggest that these risk-shift. For example, Saunders et al. (1990) and Laeven and Levine (2009) show that stockholder controlled-banks have higher volatilities and market betas, and lower z-scores (implying a higher distress risk), than other banks, especially in periods of relative deregulation. Using similar proxies, Esty (1997a) documents that savings and loan associations dramatically increased their asset risk in the 1980s and 1990s, leading most of them to eventually collapse. Esty (1997b) offers case study evidence corroborating these results. Relying on fund volatilities and tracking errors as risk-shifting proxies, Brown et al. (1996, 2001) and Basak et al. (2007) show that poor performance leads mutual funds and hedge funds to risk-shift if it helps fund managers to do so. Using an asset holding-based proxy similar to ours, Huang et al. (2011) offer evidence supporting risk-shifting behavior in the mutual funds industry.

In contrast, there is less research exploring whether non-financial firms risk-shift. In particular, Andrade and Kaplan (1998) investigate a small sample of distressed firms following leveraged re-capitalizations. They find no evidence that these risk-shift. Similarly, Graham and Harvey's (2001) and DeJong and VanDijk's (2007) surveys of U.S. and Dutch CFOs suggest that risk-shifting is of little relevance in practice — although it seems highly doubtful that CFOs would freely admit to risk-shifting even if they practiced it. Using the change in asset volatility implied from well-known structural models to proxy for risk-shifting, Fang and Zhong (2004) and Larsen (2006) find an inverted U-shaped relationship between financial distress and risk-shifting. Their inverted U-shaped relationship looks similar to ours. However, likely due to the above concerns with their empirical proxies for risk-shifting, their articles were never published.

To our best knowledge, Eisdorfer (2008) is the only one to offer convincing evidence that non-financial firms risk-shift. Using a real options model endogenizing when to invest, he shows that healthy firms maximize shareholder value by delaying investments in times of high uncertainty, whereas distressed firms do so by speeding up investments. Regressing firm investment proxies on aggregate volatility for both solvent and distressed firms, he obtains evidence supporting his hypotheses. Similarly, Esmer (2012) shows that the sign of the investment-volatility relationship switches from negative to positive once firms start to violate financial covenants. Notwithstanding, Eisdorfer (2008) and Esmer (2012) only study one very specific aspect of risk-shifting, particularly, the timing of investments. Whether distress risk causes firms to replace safer with riskier assets, the more standard definition of risk-shifting, is not clear from their work.

The rest of the paper is organized as follows. In Section 2, we discuss the construction of our risk-shifting proxy and the other variables. Section 3 describes the DIDID tests, while Section 4 outlines our data sources. Section 5 gives our empirical results. In Section 6, we report the results from several robustness and falsification tests. Section 7 summarizes and concludes.

2 Analysis Variables

2.1 The Risk-Shifting Proxy

2.1.1 Construction of the Risk-Shifting Proxy

Our risk shifting proxy interprets the firm as a portfolio of its operating segments. Accordingly, we apply Markowitz' (1952) modern portfolio theory to determine firm risk. To see how this works, denote by $A_{i_s,t}$ the value of segment s of firm i at the end of fiscal year t. Moreover, denote by $A_{i,t}$ the sum over the values of all segments belonging to firm i in fiscal year t and by $S_{i,t}$ the number of segments. Finally, let r_{s,t^*} be the period t^*-return of an equally-weighted portfolio containing only firms exclusively operating in segment s' industry (a "stand-alone firm industry portfolio"). The time index t^* is measured at a higher frequency than the t time index.

We define the return of a portfolio designed to mimick firm i's return, $r_{i,t^*}^{A_{i_s,t}}$, as:

$$r_{i,t^*}^{A_{i_s,t}} = \sum_{s=1}^{S_{i,t}} \frac{A_{i_s,t}}{A_{i,t}} r_{s,t^*}, \tag{1}$$

where the superscript in $r_{i,t^*}^{A_{i_s,t}}$ indicates the fiscal year at the end of which the segment values are measured. We calculate two time-series of mimicking portfolio returns for each firm i over all t^* periods in fiscal year t. The first time-series is calculated using segment values from the end of the current fiscal year, $r_{i,t^*}^{A_{i_s,t}}$, the second using segment values from the end of the previous fiscal year, $r_{i,t^*}^{A_{i_s,t-1}}$. We then construct our risk-shifting proxy, $RiskShifting_{i,t}^{(1)}$, as the ratio of the standard deviations of the two mimicking portfolio time-series minus one:

$$RiskShifting_{i,t}^{(1)} = \frac{\sigma(r_{i,t^*}^{A_{i_s,t}})}{\sigma(r_{i,t^*}^{A_{i_s,t-1}})} - 1, \tag{2}$$

where the superscript in $RiskShifting_{i,t}^{(1)}$ signals that the change in risk is calculated over the span of one year. To study risk-shifting over longer-time horizons, we compound the risk-shifting proxy. For example, if a firm increases its risk by 2% in the first year and by 4% in the second, the two year risk-shifting proxy, $RiskShifting_{i,t}^{(2)}$, is $(1.02 \times 1.04) - 1 = 0.061$ (6.1%). We use the same approach to calculate risk-shifting over even longer horizons.

Intuitively, we can interpret our risk-shifting proxy as the change in the volatility of firm i's operating segment (mimicking) portfolio from the end of fiscal year $t-1$ to the end of a later fiscal year. However, note that this change is exclusively driven by changes in the segments' values over this period — the numerator and the denominator of Equation (**??**) use the same returns for the stand-alone firm industry portfolios. We do so to isolate variations in firm risk attributable to managerial actions — the choice of which segments to invest in — from those that are not caused by managers, but instead by economy-wide changes in industry risk.

Our risk-shifting proxy can be seen as a continuous version of Acharya et al.'s (2011) risk-taking proxy. Acharya et al.'s (2011) risk-taking proxy is a dummy variable equal to one for firms engaging in focusing mergers and zero for those engaging in diversifying mergers. Thus, it focuses on how related a firm's core industry is with the industries that the firm expands into. Our proxy captures "relatedness" via correlation between the stand-alone firm portfolios mimicking the segments. However, our proxy also accounts for absolute industry risk by considering the volatilities of the stand-alone firm portfolios. Taking absolute risk levels into account is important, as otherwise a high-tech firm acquiring a utility firm would be considered as risk-taking. Our risk-shifting proxy follows the same logic as those of Huang et al. (2011) and Armstrong and Vashishta (2012). Huang et al. (2011) apply modern portfolio theory to the investment holdings of financial firms to determine risk. Armstrong and Vashishta (2012) apply modern portfolio theory to the operating segment portfolios held by non-financial firms. While close to us, the latter authors allow firm risk to vary with changes in the segment weights *and* the segments' risk — we only allow for variations due to changes in the segment weights.

2.1.2 Technical Details

We discuss how we implement the methodology outlined above in practice. We first describe how we calculate the segment weights used to set up a firm's mimicking portfolio ($A_{i_s,t}/A_{i,t}$) and then how we calculate the returns of the stand-alone firm industry portfolios (r_{s,t^*}).

Our main tests use a segments' book value of assets to proxy for its value and to calculate the segment weight. This choice is consistent with both Armstrong and Vashishta (2012), but also a large number of asset pricing studies interpreting changes in total assets as investments (e.g., Cooper et al. (2008), Hou et al. (2014), and Fama and French (2014)). A weakness of this choice is that accounting rules mandate that a segment's profits are added to its book value of assets at the end of the fiscal year. Thus, changes in a segment's book value do not exclusively reflect managerial actions to expand or contract a segment. To alleviate this problem, we later run robustness tests subtracting a segment's profitability from its book value of assets. Alternatively, we use the book value of assets from the previous fiscal year plus capital expenditures from the current fiscal year to proxy for a segment's value in the current fiscal year.

To calculate the stand-alone firm industry portfolios, we focus on the subsample of firms exclusively operating in one four-digit SIC code industry. At the beginning of each year, we sort this subsample of firms into four-digit SIC code industry portfolios, but only if the resulting portfolios always contain at least three firms. We collect the remaining firms — those in four-digit SIC code industry portfolios not fulfilling the above criterion — and sort them into three-digit SIC code industry portfolios, but again only if the resulting portfolios always contain at least three firms. We proceed in this way until we reach one-digit SIC Code industry portfolios. Overall, 77% of all stand-alone firm-year observations end up in four-digit SIC code portfolios.

For each stand-alone firm industry portfolio, we then calculate value-weighted weekly stock returns from the start of the year to its end. Consistent with other studies, we compute weekly returns by compounding daily returns from Wednesday of the previous week to Tuesday of the current. We use weekly returns as a compromise between efficiency and bias. To wit, although higher frequency returns should lead to more precise volatility estimates in Equation (**??**), they also produce a stronger upward-bias in them (Lo and MacKinlay (2004)). We use stock returns because, although asset returns are more reflective of operational risk than stock returns, they are unobservable for most firms. However, to rule out that our conclusions are driven by cross-sectional variations in industry leverage, we later de-lever stock returns using Merton's (1974) model and then use de-levered returns to calculate portfolio returns.[3]

2.2 Other Analysis Variables

We describe how we construct the other analysis variables. To proxy for financial distress, we extract an estimate of the one-year ahead default probability from Merton's (1974) model. To do so, we follow the iterative approach of Vassalou and Xing (2004), which works as follows. At the end of each month, we use the Black and Scholes (1974) call option formula to derive each firm's daily asset value, $A_{i,t}$, for each trading day over the prior twelve months:

$$E_{i,t} = A_{i,t} N\left[d_{1;i,t}\right] - K_{i,t} e^{-r} N\left[d_{2;i,t}\right], \tag{3}$$

where $E_{i,t}$ is the equity value (*Equity*), $K_{i,t}$ the face value of debt, and r is the annualized risk-free rate of return. The face value of debt is the sum of one-half of short-term debt and long-term debt. $N[.]$ is the cumulative standard normal density, $d_{1;i,t}$ is $\left(\ln\left(\frac{A_{i,t}}{K_{i,t}}\right) + (r + \frac{1}{2}\sigma_{i,t}^2)\right)/\sigma_{i,t}$, $d_{2;i,t}$ is $d_{1;i,t} - \sigma_{i,t}$, and $\sigma_{i,t}$ is the annualized asset volatility.[4] To estimate $\sigma_{i,t}$, we initially set it equal to a stock's annualized volatility calculated from daily data over the prior twelve months. However, using the resulting time-series of asset values, we update the asset volatility estimate and re-iterate over the previous steps until $\sigma_{i,t}$ converges (usually after 3-4 steps). Finally, we plug the latest implied asset value, the latest face value of debt, the implied annualized mean return, μ, and the implied asset volatility into Merton's (1974) formula for the twelve month-ahead default probability, $DistressRisk_{i,t}$:

$$DistressRisk_{i,t} = N\left[-\frac{\ln\left(\frac{A_{i,t}}{K_{i,t}}\right) + (\mu + \frac{1}{2}\sigma_{i,t}^2)}{\sigma_{i,t}}\right]. \tag{4}$$

[3] We have also tried forming the stand-alone firm industry portfolios using only all-equity firms. Unfortunately, there are too few all-equity firms (around 20%) for this strategy to be feasible.

[4] Note that $T = 1$ because the forecasting horizon is one year.

We use a standard set of controls. *Leverage* is the ratio of total liabilities to total assets. *Risk* is the annualized standard deviation of the mimicking portfolio representing the firm calculated using return and segment data over the prior twelve months, $\sigma(r_{i,t^*}^{A_{i_s},t})$. *Assets* is the natural log of total assets, in constant 2008 dollars. *BookToMarket* is the ratio of the book value of a firm's shares to their market value. *Capex* is capital expenditures scaled by total assets. Finally, *PP&E* is net property, plant, and equipment scaled by last fiscal year's total asset value.

3 The Causal-Based Inference Tests

3.1 Using Hurricane Strikes to Instrument Distress Risk

In our main tests, we use hurricane-induced increases in distress risk to establish whether distress risk causes risk-shifting. In this section, we offer more details about why hurricane strikes are a suitable instrument for distress risk and can thus be used as shock variable in DIDID tests.

Hurricanes are rapidly rotating storm systems ("tropical cyclones") with sustained winds of at least 34 metres per second or 74 miles per hour. They form in the North Atlantic Ocean or the Pacific Ocean. In most cases, they have an area of low atmospheric pressure at their center (an "eye"). For hurricanes to be a suitable instrument for distress risk, they need to fulfill three conditions. First, they need to affect a large number of areas. Unless they do, it is possible that a subset of firms — perhaps those that are better managed — relocate away from hurricane-affected areas to safer areas. This would cause a problem because the validity of our analysis relies on the assumption that firms are randomly assigned to treatment. Second, the origin and path of a hurricane need to be difficult to predict. Unless they are, managers may react to hurricanes long before they actually strike, contaminating the pre-event period. Third, to be a powerful instrument, hurricanes need to have a significant effect on firms' distress risk.

Satisfying the first of the above conditions, research on hurricanes suggests that most U.S. regions are exposed to the risk of a hurricane strike. For example, Blake et al. (2011) show that hurricanes do not only cause massive damage to coastal, but also to inland regions. Inland regions can be affected via storms or tornadoes spawned by hurricanes. Also, hurricanes can lead to flooding caused by heavy rainfalls accompanying them. Consistent with this evidence, Dessaint and Matray (2014) report that only around half of all U.S. counties have never been

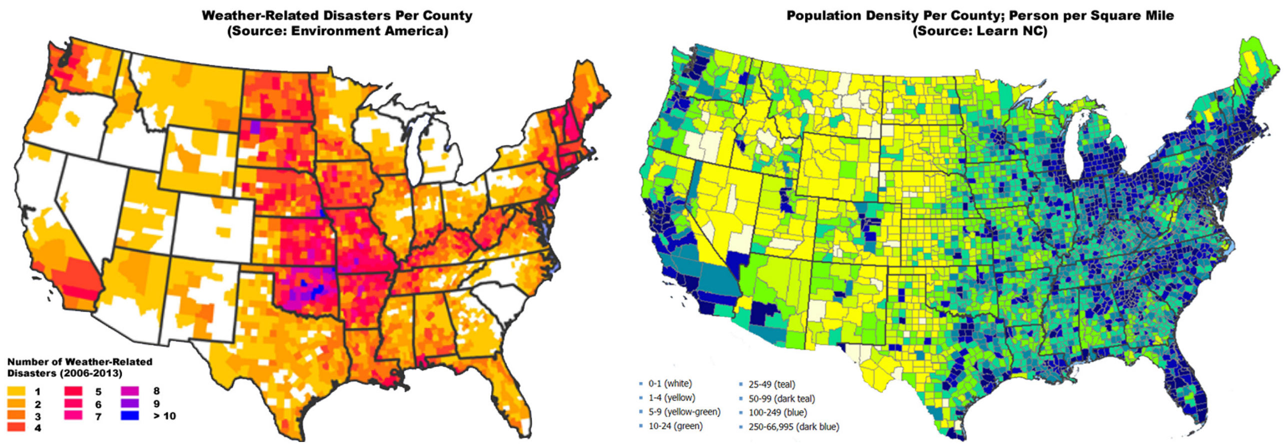

Figure 1. Weather-Related Disasters and Population Density By County This figure shows the number of weather-related disasters and population density by U.S. county. Weather-related disasters include flooding, tropical cyclones, fire, tornadoes, and other severe storms. The data on weather-related disasters cover the period from 2006 to 2013. These data are obtained from Environment America. The data on population density are measured at the end of 2000. We obtain these data from NC Learn.

affected by a hurricane over the 1851-2010 period. More importantly, Figure 1 shows that even fewer counties have never been affected by weather-related disasters, including, but not limited to hurricanes. Thus, there are few places to hide from extreme weather conditions. On top, these few places are predominately located in the Middle-West and sparsely populated. Thus, it is highly unlikely that many firms would find it beneficial to relocate to them.

The requirement that hurricanes must be difficult to predict is also fulfilled. For example, a report by the National Center for Atmospheric Research (NCAR) suggests that the incidence of a hurricane is challenging to predict because "small-scale features [such as minor variations in the atmosphere] can either nurture or crimp a potential hurricane." Further, the 2014 forecast verification report of the National Hurricane Center (NHC) suggests that around half of all five-day ahead forecasts of the location of an existing hurricane are off target by a whooping more than 200 miles.[5] Part of the difficulty stems from the fact that a hurricane's path is largely determined by local weather conditions, which are themselves hard to forecast.

Finally, there is ample evidence suggesting that hurricane strikes can cause massive economic damage. For example, Hurricane Katrina caused an estimated property damage of $113 billion (Pielke et al. (2008) and Blake et al. (2011)). Still, high damages do not directly imply a higher distress risk. Thus, to see whether the final condition is fulfilled, we will later plot the distress

[5] See <http://www.nhc.noaa.gov/>.

risk of firms located in hurricane-struck areas over the period surrounding the strike.

3.2 The Triple-Differences (DIDID) Methodology

We use a DIDID methodology to study the causal effect of distress risk on risk-shifting. Our tests are complicated by the fact that we have several, possibly overlapping shock periods and several groups of treated firms. To illustrate, both Hurricane Ophelia and Hurricane Rita hit the United States in 2005, producing an identical shock period. However, Hurricane Ophelia struck firms in several counties in North Carolina, while Hurricane Rita struck firms in several counties in Florida, Louisiana, and Texas. To address these complications, we use tests similar in spirit to those of Bertrand and Mullainathan (2003), however, adjusted for the fact that our sample firms are treated twice. Our first treatment is receiving an exogenous positive shock to distress risk. Our second treatment is having a high distress risk before the exogenous shock occurs. The second treatment is motivated by the theoretical prediction that high distress risk firms have greater incentives to risk-shift than safer firms (Murphy (1999)).

To be precise, we assign to the first treatment group ("experiencing an exogenous positive shock to distress risk") firm-year observations associated with firms headquartered in a county struck by a hurricane and within a specific period surrounding the strike. We call the period surrounding the strike the event period. For each hurricane, we then match the treated firm-year observations with those associated with firms headquartered in counties not affected by hurricanes over the same event period — the control firm-year observations. To rule out competition effects, we exclude from the matched observations those associated with firms headquartered in the five closest neighbors of the county in which the treated firm is headquartered. We construct a dummy variable, $Treated_{i,t}$, equal to one for firm-year observation receiving the first treatment and equal to zero for the matched control firm-year observations.

Our matching choices have two implications. First, most firms act simultaneously as treated and controls, but at different points in time. Second, a firm-year observation can act as control multiple times. For example, New York firms act as controls for both Hurricane Ophelia- and Hurricane Rita-affected firms in the year in which these hurricanes hit (2005).

Two remarks are in order here. First, independent of how long the event period is, we only ever study one observation before and one after the hurricane strike. For example, when the event period spans four years, the pre-hurricane observation captures risk-shifting over the two years preceding the hurricane, and the post-hurricane observation captures risk-shifting over the following two years. We do so because Bertrand et al. (2003) demonstrate that long pre- and post-shock periods generate upward-biased inference levels in shock-based tests. Second, firms hit by multiple hurricanes over short time periods create problems for our methodology. To see this, assume the same firm is hit by hurricanes in 2004 and 2005. If this happens, the post-event period of the first hurricane overlaps with the pre-event period of the second hurricane. To avoid such cases, we require a gap of least five years between the hurricane strikes observed by one firm. If the gap is shorter than five years, we exclude the whole time-series of data for this firm from the start of the pre-event period for the first hurricane to the end of the post-event period for the second hurricane. This should ensure that firms are not "permanently treated."

To assign firms to the second treatment group ("already being in poor health before the exogenous shock arrives"), we use firms' distress risk at the end of the previous fiscal year to sort them into portfolios. Because we never investigate more than one period after the hurricane, this strategy ensures that we only use pre-hurricane data to decide which firms are treated. Next, we assign all firms except those in the lowest distress risk portfolio to treatment, recognizing, however, that the firms in the higher distress risk portfolios receive a higher treatment dosage than those in the lower portfolios. We use the firms in the lowest distress risk portfolio as controls. We construct dummy variables to indicate membership in a portfolio. To wit, $DistressGroup_{i,t}^{(k)}$, is equal to one if a firm belongs to distress risk portfolio k and else zero.

We start with some simple univariate comparisons. To do so, we initially restrict our attention to firms hit by a hurricane ($Treated = 1$). Using this subsample of firms, we calculate the mean value of the risk-shifting proxy, $RiskShifting$, for each distress risk portfolio for the period before and the one after the strike. We delete firms which do not have data for both periods. For each distress risk portfolio, we then compute the change in mean risk-shifting from prior to the hurricane to after it. The change associated with the higher distress risk portfolio minus the change associated with the lowest one is the difference-in-difference (DID) estimate of the effect of distress risk on risk-shifting. Theory predicts this effect to be positive.

Next, we investigate the matched ("control") firms that are not simultaneously struck by a hurricane ($Treated = 0$). Using exactly the same steps as above, we calculate the change in mean $RiskShifting$ from the pre- to the post-hurricane period for each portfolio. Next, we subtract the mean $RiskShifting$ change observed by the lowest distress risk portfolio from that observed by the higher ones to get the DID estimate for the non-hurricane struck firms. Finally, subtracting the DID estimate for non-hurricane struck firms from that for hurricane struck firms, we obtain the DIDID estimate of the causal effect of distress risk on risk-shifting.

To control for the effects of other variables and to take account of firm- and year-fixed effects, we repeat the above tests within a regression framework. As before, we begin this analysis by looking at DID estimates, this time, however, conditioned on the level of pre-hurricane financial distress. For example, we now ask whether distressed firms hit by a hurricane behave differently from non-hit, but otherwise similarly distressed firms. To achieve this goal, we run the following regression separately for firms contained in the different distress risk portfolios:

$$RiskShifting_{i,t} = \alpha_i + \alpha_t + \beta Treated_{i,t} \times Shock_{i,t} + \gamma Treated_{i,t} + \delta Shock_{i,t} + \nu X_{i,t} + \varepsilon_{i,t}, \quad (5)$$

where $Shock_{i,t}$ is a dummy variable equal to one for both the firms treated by a specific hurricane and their matched controls in the years after the hurricane hit and else zero, and $X_{i,t}$ is a vector of firm-specific control variables. β, γ, δ, and ν are parameters, α_i and α_t are firm- and year-time invariant effects, and $\varepsilon_{i,t}$ is the residual. To be consistent with the univariate tests, we here also delete firms which do not have complete data for both the pre- and the post-event period. We stress that β can be interpreted as the DID estimate of the causal effect of distress risk on risk-shifting after accounting for the controls and for firm- and year-fixed effects.

Finally, we pool the firms in the distress risk portfolios and run a joint regression:

$$\begin{aligned} RiskShifting_{i,t} &= \alpha_i + \alpha_t + \sum_{k=2}^{K} \beta_k DistressGroup_{i,t}^{(k)} \times Treated_{i,t} \times Shock_{i,t} \\ &\quad + \gamma Treated_{i,t} \times Shock_{i,t} + \sum_{k=2}^{K} \delta_k DistressGroup_{i,t}^{(k)} \times Treated_{i,t} \\ &\quad + \sum_{k=2}^{K} \eta_k DistressGroup_{i,t}^{(k)} \times Shock_{i,t} + \theta Treated_{i,t} + \kappa Shock_{i,t} \\ &\quad + \sum_{k=2}^{K} \lambda_k DistressGroup_{i,t}^{(k)} + \nu X_{i,t} + \varepsilon_{i,t}, \end{aligned} \quad (6)$$

where K is the number of distress risk portfolios, and β_k, γ, δ_k, η_k, θ, κ, λ_k, and ν are the new parameters. Everything else is the same as in Equation (**??**). We note that, in this setting, β can be interpreted as the DIDID estimate of the causal effect of distress risk on risk-shifting after accounting for the controls and for firm- and year-fixed effects.

4 Data Sources

Market data are from CRSP, while accounting data are from COMPUSTAT. We collect segment data from the COMPUSTAT Historical Segment database. Three month T-Bill rates are from Kenneth French's website. Data on hurricane strikes, in particular, the names of the counties affected by hurricanes, the dates of the strike, and the amount of property damages caused, are obtained from the SHELDUS database (the "Spatial Hazard and Event Losses Database for the United States"). SHELDUS is administered by the University of South Carolina.

To construct our analysis sample, we begin with the cross-sections of firms covered by the COMPUSTAT Historical Segment database between 1990 and 2010. We restrict our analysis to the 1990-2010 period to alleviate concerns that changing segment accounting regulations distort the risk-shifting proxy. For example, *SFAS No. 131* significantly altered segment reporting regulations in 1997. Despite the fact that this law change occurred in 1997, we start our data in 1990 because the 1990-1997 segment data have been adjusted by Standard & Poor's (the database provider) to reflect the 1997 requirements. We drop all firms that only ever report information for the same single segment. Also, from the sample of firms switching between one single segment and another or between single segment- and multiple segment-status, we delete firm-year observations that occur before the first switch to the other single segment or to multiple segment-status — if and only if the firm starts out as a single segment firm.[6] We delete these observations because the risk-shifting proxy is zero for them by construction.

We then match the segment data with COMPUSTAT data. We drop from the merged data financial (SIC codes: 6000-6999) and utility firms (4900-4999). We also delete observations for which we are unable to calculate the distress risk proxy or the controls. To alleviate the effect of outliers, we winsorize all continuous variables at the 1st and 99th percentiles.

[6]Thus, there are a small number of singe segment firms in our data. Particularly, these are multi-segment firms that took the decision to become single segment firms.

Using the SHELDUS database, we consider those counties as hurricane-struck for which the total property damage caused by a hurricane is in excess of \$100,000. This choice has several advantages over the choice of, for example, focusing on all counties hit by the most important hurricanes. For example, one advantage is that this choice recognizes that even smaller hurricanes can cause massive damage shortly after their landfall, while even major hurricanes can only have negligible effects during their final days. Thus, it makes sense to distinguish between the severity with which counties are hit by hurricanes. Notwithstanding, we agree that \$100,000 is an ad-hoc threshold. We later show that our choice produces a significant upward jump in distress risk in the counties expected to be affected by hurricanes when the hurricanes strike.

5 Empirical Results

In this section, we offer our empirical results. In the first subsection, we take an initial glance at the risk-shifting proxy and its correlation with financial distress. In the next subsection, we use DIDID tests to determine the causal relationship between the two variables. In particular, we first show that the treated and control firms do not differ significantly across various pre-hurricane firm attribute values. Next, we show that hurricane strikes are a powerful instrument for distress risk. Finally, we offer DID and DIDID estimates to assess the causal relationship between financial distress and risk-shifting. In the next subsection, we look at the consequences of risk-shifting. The final subsection studies how covenant violations affect risk-shifting.

5.1 The Risk-Shifting Behavior of Non-Financial Firms

In Table 1, we offer descriptive statistics for our analysis variables. The descriptive statistics are based on all firm-year observations for which we can compute the risk-shifting proxy and the control variables. The table shows that a subset of firms actively manage their risk. For example, we observe economically meaningful changes in annual firm risk ($RiskShifting^{(1)}$) for around 50% of all sample observations. Around half of these changes are upward. However, the table also shows that changing a firm's risk profile takes time. To wit, the longer-ahead risk-shifting proxies have increasingly extreme outer-percentiles. For example, while the 90th percentile of the annual risk-shifting proxy is 4.1%, the same percentile for the three year-proxy

($RiskShifting^{(3)}$) is 13.1%, and the one for the six year-proxy ($RiskShifting^{(6)}$) is 20.5%. Thus, a small fraction of firms massively change their asset risk over longer time periods.

<u>TABLE 1 ABOUT HERE</u>

Looking at financial distress (*DistressRisk*), around 50% of all observations are associated with a negligible distress risk. The medians of total assets (*Assets*) and market capitalization (*Equity*) are $256 and $212 million, respectively. Hence, the sample firms are larger than the typical CRSP/COMPUSTAT firm. The reason is that larger firms are more likely to operate multiple segments. Our sample is also skewed towards more highly levered firms (*Leverage*) and value firms (*BookToMarket*). Finally, the median annual asset volatility is around 25% (*Risk*), and the median property, plant, and equipment is around 22% (*PP&E*).

In Figure 2, we offer a graphical representation of the one to ten year-risk-shifting behavior of firms with different levels of distress risk. Doing so allows us to assess how financial distress is correlated with risk-shifting. We construct the distress risk portfolios according to the values of *DistressRisk* in the previous fiscal year, using zero, the median, and the 90th percentile of this variable as breakpoints. We use zero as breakpoint because theory predicts that all-equity firms have no incentive to risk-shift. We use the median because below median-distress risk is negligible. We use the 90th percentile because distress risk values between the median and this percentile are often high, but rarely extreme. In contrast, distress risk values above the 90th percentile are easily extreme (in the region between 50% and 100%). Thus, following from our sorting scheme, the first portfolio contains only all-equity firms ($DistressGroup^{(1)} = 1$), the second low distress risk firms ($DistressGroup^{(1)} = 1$), the third moderate distress risk firms ($DistressGroup^{(2)} = 1$), and the fourth high distress risk firms ($DistressGroup^{(3)} = 1$).[7]

Supporting financial theory, the figure shows that equity-only firms do not risk-shift. More importantly, it also suggests a positive correlation between distress risk and risk-shifting. For example, while the low distress risk firms increase their risk by an average of only 0.6% over the

[7]Consistent with our labels, the first portfolio has a zero mean distress risk, whereas the second, third, and fourth have a mean distress risk of virtually zero, 6%, and 53%, respectively. Because both the first and second portfolio have a distress risk virtually equal to zero, we include both these portfolio in the $DistressGroup^{(1)} = 1$-control group. The distress risk portfolios produce the expected relationships with other variables. For example, distressed firms have low asset values (*Assets*), high leverage ratios (*Leverage*), and high book-to-market ratios (*BookToMarket*), and they can run large operating losses (Campbell et al. (2008)).

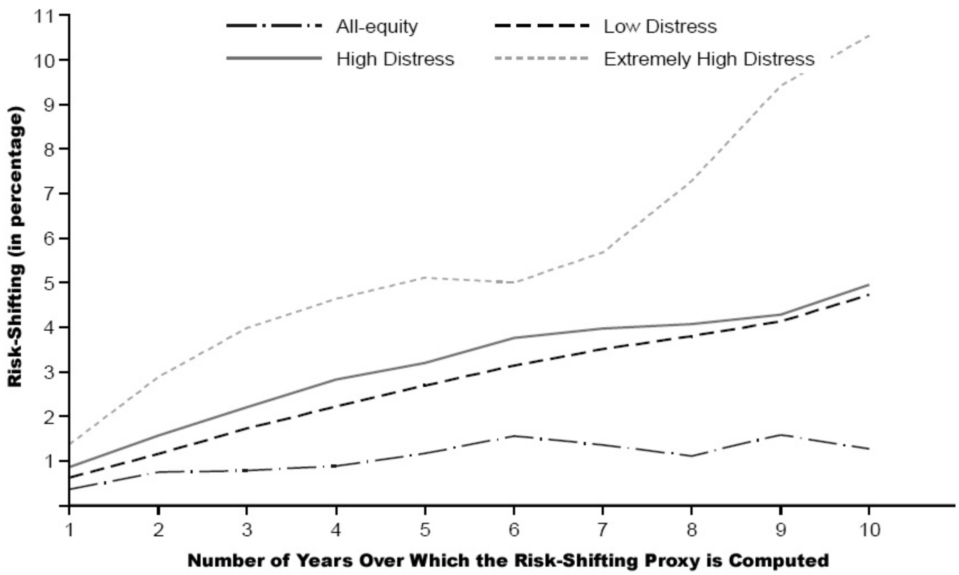

Figure 2. Risk-Shifting By Distress Risk and Horizon This figure shows the risk-shifting behavior of firm portfolios with different levels of distress risk over the one to ten years following the portfolio formation date. We use four portfolios in the figure, an only all-equity firms portfolio (All-Equity), a below median distress risk portfolio (Low Distress), an above median but below 90 percentile distress risk portfolio (High Distress), and an above 90th percentile distress risk portfolio (Extreme Distress). We use as portfolio formation date the end of each calendar year in our sample period. We first calculate the risk-shifting by portfolio and future horizon for each portfolio formation date, and then average over the portfolio formation dates.

first year, firms with a moderate distress risk increase it by an average of 0.9% and those with a high distress risk by an average of 1.4%. As before, we find that risk-shifting takes time. For example, while the high distress risk firms increase their risk by only 1.4% over the first year, they increase it by 5.1% over the first five and by 10.5% over the first ten years.

Figure 2 reveals why it is so difficult for standard ("OLS-based") methodologies to assign causality to the financial distress-risk-shifting relationship. Both distress risk and risk-shifting follow highly persistent processes, implying that it is impossible to tell if financial distress leads to risk-shifting or if risk-shifting leads to financial distress ("reverse causality"). Also, standard tests cannot rule out that a third variable drives the patterns in both distress risk and risk-shifting ("omitted variable bias"). We deal with causality issues in the next subsection.

5.2 Does Financial Distress Cause Risk-Shifting?

5.2.1 The Treated Firm Sample and the Matched Counterparts

We next turn our attention to whether there is a causal relationship between financial distress and risk-shifting. However, before doing so, we take a look at the sample of treated firms and compare this with the sample of matched controls. To achieve this goal, Table 2 reports descriptive statistics for pre-hurricane analysis variable values for both samples.

Table 2 About Here

The table shows that the treated firm sample contains only 178 firms. The reason for this relatively low number is that most hurricane strikes affecting a given county do not fulfill the condition that there are no other strikes in the same county during the period preceding and the period proceeding the current strike. The matched sample contains 7,331 (not necessarily unique) firms. Comparing the treated and control firm samples, we find no significant differences in their risk-shifting over the year before the strike ($RiskShifting^{(1)}$). More specifically, the median treated firm increases risk by 1.34% over this year, while the median control firm increases it by 0.57%. There are no statistically significant differences in the shape of the $RiskShifting^{(1)}$ distribution across the two samples of firms (KS-test p-value: 0.985).

We also find no significant differences across treated and control firms in terms of their size ($Assets$) and book-to-market ratios (*BookToMarket*). However, the treated firms are significantly more tangible asset-intensive (*PP&E*) than the controls, likely because they are often located in industrial coastal regions with access to a harbor. Probably due to their more pledgable assets, they also observe slightly higher leverage ratios (*Leverage*) than the controls (53% vs. 50%). The higher leverage ratios are likely responsible for their slightly higher default probabilities (*DistressRisk*; 6% vs. 4%). Notwithstanding these differences, our overall conclusion is that the treated firms are not strikingly different from the controls.

5.2.2 The Ability of Hurricane Strikes to Increase Financial Distress

We verify that hurricane strikes are a powerful instrument for distress risk. To achieve this objective, Figure 3 plots mean distress risk (*DistressRisk*) over the five year period surrounding

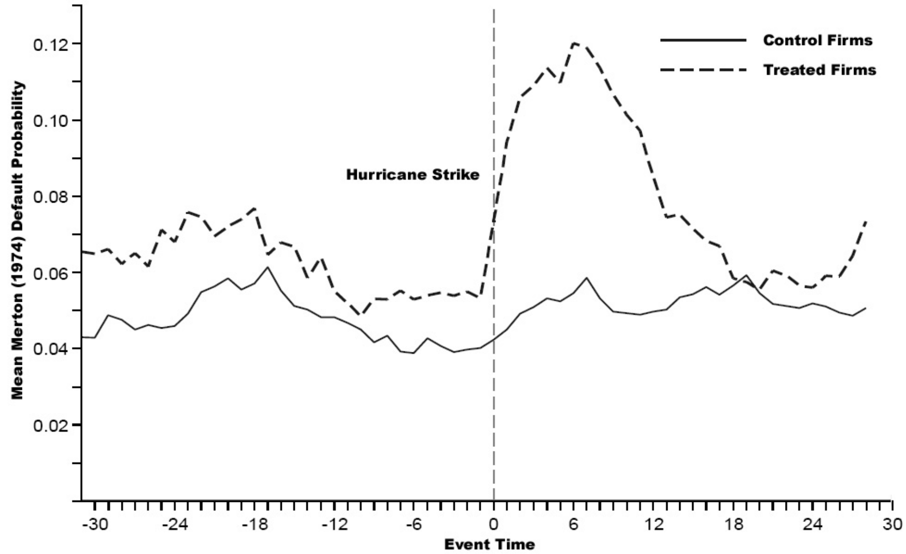

Figure 3. The Effect of Hurricane Strikes on Distress Risk This figure shows the effect of a hurricane strike on the mean distress risk of firms located in the affected counties and their matched counterparts. We measure distress risk using the Merton (1974) default probability. The hurricane strikes in event time zero.

the hurricane strike, distinguishing between treated and matched controls.

Figure 3 shows that, at the start of the event window, the treated firms observe a distress risk around 7-8%. However, as the hurricane strike approaches, their distress risk drops slightly, so that it hovers between 5% and 6% during the year preceding the strike. In comparison, the control firms have a slightly (1-2%) lower distress risk than the treated firms before the strike, which, however, follows the same trends. During the three months following the hurricane strike, the distress risk of the treated firms jumps from around 5% to close to 11%, and it continues to climb to around 12% six months after the strike. From then on, it takes another year before the distress risk of the treated firms drops back to its pre-hurricane level. In contrast, the distress risk of the control firms increases by less than 2% during the six months following the hurricane strike, and it stays fairly constant from then on. As a result, we can rule out that economy-wide shocks are behind the distress risk increases experienced by the treated firms.

We conclude that hurricanes have a significant effect on the financial health of firms affected by them. This effect can be as strong as, for example, the effect that economic recessions have on distress risk (see Figure 1 in Vassalou and Xing (2004)).

5.2.3 DIDID Estimates Calculated from Sample Means

Table 3 considers DIDID estimates for the causal effect of financial distress on risk-shifting. In the table, we calculate DIDID estimates from sample means, and we look at risk-shifting over the two years preceding and the two years proceeding the hurricane strike ($\textit{Risk-Shifting}^{(2)}$). We focus on risk-shifting over a two year-period because Figure **??** suggests that hurricanes inflate distress risk over a period of around 18-20 months. The rows of the table offer the mean of the risk-shifting proxy both for the pre-hurricane ($Shock = 0$) and the post-hurricane period ($Shock = 1$) and both for firms located in the struck counties ($Treated = 1$) and for their matched couterparts ($Treated = 0$). In contrast, the columns offer the means of the risk-shifting proxy for firms in three distress risk portfolios: low distress risk firms ($DistressGroup^{(1)} = 1$), moderate distress risk firms ($DistressGroup^{(2)} = 1$), and high distress risk firms ($DistressGroup^{(3)} = 1$).

Table 3 About Here

The table suggests that, during the two years before the hurricane hits, firms with different levels of distress risk and located in the affected counties do not differ in their risk-shifting behavior. In particular, during this time period, the firms in the low, moderate, and high distress risk portfolios risk-shift on average by 2.7%, 2.3%, and 3.4%, respectively. None of these numbers is statistically different from any of the others. Similarly, distress risk does also not condition risk-shifting among the firms in the unaffected counties. In contrast, during the two years following the hurricane strike, the moderately distressed firms located in the affected counties risk-shift by 6.6%. Not only is this estimate significantly larger than the corresponding estimates for the same county-firms contained in the lowest and highest distress risk portfolio (0.2% and -0.3%, respectively), it is also significantly larger than the estimates for firms with different levels of distress risk in the unaffected counties (between 0.9% and 1.8%).

The above numbers allow us to assess the causal effect of financial distress on the moderately distressed firms' risk-shifting behavior. In particular, the DID estimate for this effect is the change in the mean of $\textit{Risk-Shifting}^{(2)}$ experienced by the moderately distressed firms, 4.3%, minus the change experienced by the lowly distressed firms, -2.5%, a highly significant 6.8% (p-value: 0.005). To obtain the DIDID estimate for the same effect, we subtract from the latter number the DID estimate obtained from the matched sample. The moderately distressed firms in the non-struck counties observe an increase in the mean of *Risk-Shifting*$^{(2)}$ of -0.4%, while the lowly distressed firms observe one of -0.3%, yielding a DID estimate of -0.1%. Thus, the DIDID estimate is $6.8\% - (-0.1\%) = 6.9\%$, which is also highly significant.

Turning our attention to the most distressed firms, our conclusions change dramatically. The highly distressed firms experience an increase in the mean of *Risk-Shifting*$^{(2)}$ of -3.8%, and this change is not significantly different from that experienced by the weakly distressed firms. Thus, the DID estimate for the causal effect of financial distress on risk-shifting is insignificant for these firms. Next, subtracting the difference in the change experienced by the most and least distressed firms in the affected counties from the equivalent change in the non-affected counties also yields an insignificant number. Thus, the DIDID estimate for the effect is also insignificant. Overall, our evidence suggests that the most distressed firms do not risk-shift.

TABLE 4 ABOUT HERE

5.2.4 DIDID Estimates Calculated from Regressions

We next calculate DID and DIDID estimates using regression analysis. Using regressions has the advantage that it allows us to control for the effects of other covariates and for firm- and time-invariant effects. To be comparable with the univariate tests, we here also only consider one observation before the hurricane strike and one after. Also, we again exclude firms which do not have complete data for both the pre- and the post-hurricane period.

To start with, Table 5 reports the results of estimating Equation (**??**) separately for the firms in the three distress risk portfolios. Consistent with the sample mean comparisons, the regressions suggest that, while the most and least distressed firms do not risk-shift when they experience an exogenous hurricane-induced increase in distress risk, moderately distressed firms do so. For example, looking at risk-shifting over a one year horizon (*RiskShifting*$^{(1)}$), the coefficient on the interaction term between *Shock* and *Treated*, the regression-based DID estimate for the causal effect of financial distress on risk-shifting, is a highly significant 2.3% for the moderately distressed firms. In contrast, it is an insignificant -0.8% and -1.3% for the weakly and the highly distressed firms, respectively. Increasing the length of the time period over which risk-shifting is measured increases the magnitude of these effects. For example, looking at risk-shifting over a two year horizon (*RiskShifting*$^{(2)}$), the DID estimate for moderately distressed firms increases to a highly significant 4.4%. In contrast, the other DID estimates remain insignificant.

<u>Table 5 About Here</u>

Regarding the control variables, only lagged firm risk (*Risk*) significantly conditions risk-shifting. The negative sign on the coefficient of this variable is hardly surprising. Firms already operating a very risky asset mix find it difficult to increase firm risk further, implying that their only option is to leave firm risk unchanged — or to decrease it.

In Table 6, we show the results from estimating Equation (**??**) on the pooled sample featuring firms from all distress risk portfolios. As before, our results suggest that financial distress causes moderately, but not highly distressed firms to risk-shift. For instance, looking at risk-shifting over a one year horizon (*RiskShifting*$^{(1)}$), the coefficient on the triple interaction term between *Shock*, *Treated*, and *DistressGroup*$^{(2)}$, the DIDID estimate for the causal effect of financial distress on the risk-shifting of moderately distressed firms, is a significant 3.3%. In contrast, the coefficient on the triple interaction term containing *DistressGroup*$^{(3)}$ is both statistically and economically insignificant. Increasing the length of the period over which risk-shifting is measured (*RiskShifting*$^{(2)}$) again amplifies the above effects.

<u>Table 6 About Here</u>

In the DIDID regressions, not only the coefficient on asset risk (*Risk*), but also the one on tangible assets (*PP&E*) attracts significance. Tangible assets can be used to secure debt. As a result, the upward hump-shaped relationship between this variable and risk-shifting suggests that secured debt is able to prevent distressed firms from risk-shifting, with this effect, however, diminishing with the level of tangible assets (Rampini and Viswanathan (2013)).

5.3 The Real Consequences of Risk-Shifting

In Table **??**, we study the real consequences of risk-shifting. In particular, we examine whether firms that risk-shift experience higher failure rates than other firms over several post-hurricane periods. To achieve this goal, we calculate for each distress risk group located in either the disaster (Panel A) or the non-disaster zones (Panel B) the fraction of firms that fail over the two to five years after the strike- or the two to ten years after the strike-periods. We ignore the two year-period directly following the strike because failure rates are zero over this period, owing to the fact that our tests exclude firms with non-comprehensive data over the event period. We classify as failures bankruptcy filings and performance-related delistings.

Our evidence shows that the initially moderately-distressed firms that risk-shift fail significantly more often than other firms, including firms that were more distressed than they were before the disaster. For example, 63.3% of the moderately distressed firms in the affected regions fail over the two to ten years after the strike-period. In contrast, over the same period, only 35.7% of the highly distressed firms in the affected regions fail, whereas only 42.5% of the moderately distressed firms in the non-affected regions fail. Thus, risk-shifting leads to a pronounced increase in failure rates. Also interesting, initially highly distressed firms fail significantly more often in the non-disaster than in the disaster areas. This is consistent with the highly distressed firms in the affected regions decreasing — and not increasing — their firm risk (see Tables **??**-**??**). In the next subsection, we shed more light on the behavior of the highly distressed firms.

5.4 Covenant Violations and Risk-Shifting

As a final step, we analyze why the most distressed firms do not risk-shift. One possibility is that the prospects of the most distressed firms are so gloomy that risk-shifting no longer pays off for them.[8] However, assuming that the moderately distressed firms engage in an optimal amount of risk-shifting, this possibility is inconsistent with the fact that the most distressed firms have significantly lower failure rates than the moderately distressed firms after the hurricane strike. Another possibility is that the creditors of the most distressed firms prevent these firms from risk-shifting. For example, Chava and Roberts (2008) show that creditors actively intervene in the decisions of firms that have violated financial covenants in the past.

To test the second possibility, we obtain covenant violation data over the period from 1996 to 2010 from Michael Robert's website.[9] Using these data, we identify those firm-observations in our analysis sample that are associated with covenant violations over the prior one, two,

[8]For example, Murphy (1999) shows that the incentive to risk-shift is strongest when the expected value of a firm's assets is close to the kink in the equity payoff function.

[9]The URL address is: <http://finance.wharton.upenn.edu/ mrrobert/>.

three, four, or five years. In Panel A of Table **??**, we report the percentage of covenant violators in the low, moderate, or high distress risk groups in or outside of the hurricane-struck areas over the 2000-2010 period. Consistent with the second possibility, there is a significantly larger fraction of covenant violators among the most distressed than among the moderately distressed firms, especially in the disaster areas. For example, while 57% of the highly distressed firms located in the disaster areas have violated covenants over the previous five years, only 25% of the moderately distressed firms located in the same areas have done so, too.

Only considering the sample of moderately and highly distressed firms located in the disaster areas, we also calculate the correlations between post-hurricane risk-shifting and dummy variables indicating whether a firm has violated a covenant. Especially when looking at risk-shifting over the two years following the disaster ($RiskShifting^{(2)}$), we find a strong negative correlation of around -0.30. The negative correlation indicates that covenant violators are indeed far less likely to risk-shift than other firms after the hurricane strike, lending support to the hypothesis that creditors sometimes keep highly distressed firms from risk-shifting.

6 Robustness Tests

6.1 Parallel Trends

One of the main assumptions underlying DID (or DIDID) analyses is that firms are randomly assigned to the treated and control firm groups. To verify that this assumption holds, Table **??** reports changes in our main analysis variables over several pre-hurricane periods for both the treated and the control firms. The most important conclusion that we can draw from the table is that the treated and control firms do not differ in their risk-shifting behavior (*RiskShifting*) over the one to five year periods before the hurricane strike. More specifically, neither of the two groups ever changes its risk by more than 1%, and the differences in their changes are never significant. In a similar vein, neither of the two groups ever observes changes in distress risk (*DistressRisk*) exceeding 2%, and the differences in their changes are never significant. As a final step, we also look at some covariates, including tangible assets (*PP&E*), leverage (*Leverage*), and the book-to-market ratio (*BookToMarket*). We conclude from these tests that changes in the covariates also fail to significantly differ across the treated and control firms.

Table 7 About Here

7 Conclusion

While risk-shifting is one of the most important concepts in corporate finance, there is so far no convincing evidence that financial distress motivates non-financial firms to risk-shift. Reasons for this dearth of evidence could be that risk-shifting by non-financial firms is difficult to measure and that financial distress and risk-shifting are likely to be endogenously related. In our work, we try to find solutions to these two problems. Particularly, following the lead of Armstrong and Vashishta (2012), we view the firm as a portfolio of its operating segments and thus determine firm risk using Makowitz' (1952) modern portfolio theory. Our risk-shifting proxy then is the change in firm risk induced through changes in the weights of the operating segments. We try to tackle the endogeneity issue by using hurricane strikes to instrument distress risk. Specifically, we analyze the effect of hurricane-induced changes in distress risk on the risk-shifting behavior of firms with different levels of pre-hurricane distress risk and located either in hurricane-affected or hurricane-unaffected areas within a DIDID framework.

Our results show that financial distress causes moderately distressed firms to risk-shift. However, our results also suggest that financial distress does not cause the most highly distressed firms to risk-shift. Risk-shifting has important real consequences. For example, while the moderately distressed firms have lower distress risk values than the highly distressed firms before the hurricane strike, they experience significantly higher failure rates after the disaster. Analyzing why highly distressed firms refrain from risk-shifting, we show that these firms have often violated financial covenants in the past, and that it is the incidence of these violations which keeps them from risk-shifting. Thus, our evidence is consistent with the hypothesis that creditors actively intervene to keep the most distressed firms from engaging in risk-shifting.

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Table A.1
Variable Definitions

This table offers details about our analysis variables. In the first column, we show the names of the analysis variables. In the second column, we explain how the analysis variables are constructed. The second column also reports the CRSP or COMPUSTAT mnemonics (in parentheses) of the data items that we use to calculate the analysis variables.

Variable	Definition
$RiskShifting^{(1)}$	Net percentage change in firm risk calculated from the end of the prior fiscal year to the end of the current. We calculate firm risk at the end of the prior (current) fiscal year by applying Markowitz' (1952) variance formula to segment weights from the end of the prior (current) fiscal year and return data spanning the period from the end of the prior fiscal year to the end of the current.
$RiskShifting^{(x)}$; x > 1	Net percentage change in firm risk over the x-fiscal year period ending with the current fiscal year, calculated by compounding the $RiskShifting^{(1)}$ values over the x-fiscal year period.
DistressRisk	12-month ahead default probability extracted from Merton's (1974) model following the iterative methodology suggested by Crosbie and Bohn (1999) and Vassalou and Xing (2004).
Assets	Natural logarithm of total assets (at).
BookToMarket	Book value of common equity (ceq)-to-market value of common equity (prcc_f×csho).
Capex	Capital expenditures (capx)-to-total assets (at).
Equity	Market value of common equity (prcc_f × csho).
Leverage	Total liabilities (lt)-to-total assets (at).
PP&E	Net property, plant, and equipment (ppent)-to-beginning of year book value of assets (at).
Risk	Firm risk at the end of the prior fiscal year, calculated by applying Markowitz' (1952) variance formula to segment weights from the end of the prior fiscal year and return data spanning the period from the end of the prior fiscal year to the end of the current.

Table 1
Descriptive Statistics
This table reports descriptive statistics for the analysis variables. The analysis variables are described in Table A.1 in the Appendix. For ease of interpretation, we use the exponential of *Assets* and *Equity* in this table. The descriptive statistics are: the number of observations (Obs), the mean (Mean), the standard deviation (SD), the tenth (P10), 25h (P25), 50th (P50), 75th (P75), and 90th (P90) percentiles.

	Obs	Mean	SD	P10	P25	P50	P75	P90
$RiskShifting^{(1)}$	15,874	0.649	7.693	-2.707	-0.091	0.000	0.399	4.103
$RiskShifting^{(2)}$	13,259	1.222	10.485	-5.562	-0.311	0.000	1.344	8.856
$RiskShifting^{(3)}$	11,031	1.740	12.697	-7.754	-0.651	0.000	2.479	13.127
$RiskShifting^{(6)}$	6,199	2.883	16.578	-12.223	-1.911	0.000	5.721	20.533
$RiskShifting^{(10)}$	2,713	4.497	20.457	-16.480	-3.105	0.000	8.736	26.958
DistressRisk	15,874	0.063	0.179	0.000	0.000	0.000	0.005	0.206
Assets (non-logged)	15,064	1961.28	5462.06	20.64	58.39	255.56	1189.73	4271.70
Equity (non-logged)	15,064	2039.85	6315.62	15.97	50.34	211.80	1069.39	3989.86
Leverage	15,064	0.510	0.285	0.193	0.331	0.502	0.651	0.791
BookToMarket	15,064	0.652	0.640	0.141	0.300	0.521	0.861	1.369
Risk	15,064	0.291	0.137	0.155	0.196	0.258	0.347	0.467
PP&E	15,064	0.291	0.248	0.049	0.109	0.223	0.400	0.630

Table 2
Comparison of Treated and Control Firms
This table offers descriptive statistics for the treated and control firm-samples. The treated firm-sample includes all firm-year observations associated with firms headquartered in a county in which a hurricane causes more than $100,000 in property damages and within the five year-window surrounding the hurricane strike. We exclude from this sample those firms that are struck by more than one hurricane over the five year-window. We match the firm-year observations associated with firms treated by a hurricane with firm-year observations associated with firms not struck by a hurricane over the same five-year window (the control sample). We only use data from the two years prior to the hurricane strike to compute the descriptive statistics. The analysis variables are described in Table A.1. The descriptive statistics are: the number of observations (Obs), the mean (Mean), the 25th (P25), 50th (P50), and 75th (P75) percentiles, and the p-value from a Kolmogorov-Smirnov (KS) test of the equality of an analysis variable's distribution across the treated and control samples.

Variable		Obs	Mean	P25	Median	P75	KS p-value
RiskShifting$^{(1)}$	Treated	178	1.34	-0.29	0.00	0.71	0.985
	Controls	7,331	0.57	-0.18	0.00	0.56	
DistressRisk	Treated	178	0.06	0.00	0.00	0.00	0.031
	Controls	7,324	0.04	0.00	0.00	0.00	
Assets	Treated	178	5.88	4.46	5.93	7.25	0.240
	Controls	7,331	5.80	4.23	5.75	7.27	
BookToMarket	Treated	178	0.65	0.30	0.45	0.83	0.133
	Controls	7,331	0.63	0.30	0.51	0.83	
Capex	Treated	176	0.35	0.11	0.21	0.40	0.397
	Controls	7,219	0.32	0.12	0.21	0.35	
Leverage	Treated	178	0.53	0.40	0.53	0.65	0.010
	Controls	7,331	0.50	0.33	0.50	0.65	
PP&E	Treated	178	0.43	0.15	0.29	0.66	0.000
	Controls	7,294	0.28	0.11	0.22	0.39	
Risk	Treated	178	26.83	19.98	25.30	30.89	0.016
	Controls	7,331	29.20	19.49	25.80	34.85	

Table 3
Triple Differences Estimates: Mean Comparisons
This table offers DID estimates of the effect of financial distress on risk-shifting behavior using the samples of firms located in the hurricane-struck counties (Hurricane-struck: Yes) and the sample of firms not located in the hurricane-struck counties (Hurricane-struck: No). The sample of hurricane struck-firms includes all firm-year observations associated with firms headquartered in a county in which a hurricane causes more than $100,000 in property damages and within the five year-window surrounding the hurricane strike. We exclude from this sample those firms that are struck by more than one hurricane or that do not have complete data over the five year-window. We match the firm-year observations associated with firms struck by a hurricane with firm-year observations associated with firms not struck by a hurricane over the same five-year window to create the sample of non-hurricane struck firms. We use distress risk values from the fiscal year end before the hurricane strike to sort firms into distress risk groups: "Low" contains firms with a distress risk value below the median; "Mod(erate)" contains firms with a distress risk value above the median but below the ninth decile; and "High" contains the residual firms. "Before strike (-2 to -1)" reports the average value of $RiskShifting^{(2)}$ over the two fiscal years prior to the hurricane strike, "After strike (0 to +1)" reports the average value of $RiskShifting^{(2)}$ over the strike year and the following year. The table also shows the difference in risk-shifting across the distress risk groups (Difference) and the change from before to after (After–Before). "***", "**', and "*' indicate statistical significance at the 99, 95, and 90% confidence levels, respectively.

		Distress Groups			Difference	
	Hurricane struck?	Low (1)	Mod. (2)	High (3)	Mod.–Low (2)–(1)	High–Low (3)–(1)
Before Strike (-2 to -1)	Yes	2.74	2.32	3.44	-0.43	0.70
	No	1.19	1.29	1.63	0.09	0.44
After Strike (0 to +1)	Yes	0.23	6.64	-0.32	6.42***	-0.55
	No	0.93	0.89	1.78	-0.05	0.85*
After–Before	Yes	-2.52*	4.33	-3.77	6.84**	-1.25
	No	-0.26	-0.40	0.16	-0.14	0.42

Table 4
Difference-in-Difference Estimates: Regressions
This table shows the results from the following DID regression:

$$RiskShifting^{(x)}_{i,t} = \alpha_i + \alpha_t + \beta Treated_{i,t} \times Shock_t + \mathbf{X}_{i,t}\gamma + \varepsilon_{i,t},$$

where $RiskShifting^{(x)}_{i,t}$ is $RiskShifting^{(1)}_{i,t}$ or $RiskShifting^{(2)}_{i,t}$. We set $Treated$ equal to one for all firm-year observations associated with firms headquartered in a county in which a hurricane causes more than $100,000 in property damages and within the five year-window surrounding the hurricane strike. We exclude firm-year observations associated with firms that are struck by more than one hurricane or that do not have complete data over the five year-window. For each hurricane, we match the treated firm-year observations with firm-year observations associated with firms not struck by a hurricane over the same five-year window. We set $Treated$ equal to zero for these matched observations. We set *Shock* equal to one for treated and matched firm-year observations occurring after the hurricane strike. **X** is a vector of control variables, including the main effects (*Treated* and *Shock*) and the other control variables ($Assets$; *Risk*; *PP&E*; and *PP&E*^{2}). We describe the construction of the other controls in Table A.1. β and γ are parameters; α_i and α_t are firm- and year-fixed effects; $\varepsilon_{i,t}$ is the residual. We run the regressions separately for firms with different levels of pre-hurricane distress risk: "Low" contains firms with a distress risk value below the median; "Mod(erate)" contains firms with a distress risk value above the median but below the ninth decile; and "High" contains the residual firms. Distress risk is measured at the end of the fiscal year before the hurricane strike. For each treated and control firm, we only ever include one observation before the hurricane strike and one after. The table shows parameter estimates and standard errors (in parentheses). Standard errors are calculated from White's (1982) formula. "***", "**', and "*' indicate significance at the 99, 95, and 90% confidence levels, respectively.

	Endogenous Variable					
	Risk-shifting$^{(1)}$ Distress Groups			*Risk-shifting*$^{(2)}$ Distress Groups		
	Low	Mod.	High	Low	Mod.	High
Treated × *Shock*	-0.795	2.323**	-1.264	-1.608	4.432**	-2.661
	(0.641)	(0.951)	(1.789)	(1.264)	(2.129)	(3.604)
Treated	0.052	0.407	7.284	0.395	0.739	11.690
	(1.186)	(1.572)	(9.872)	(1.991)	(3.193)	(17.227)
Shock	-0.007	-0.099	0.761	-0.028	-0.213	1.565
	(0.111)	(0.161)	(0.498)	(0.224)	(0.323)	(0.991)
$Assets$	-0.239	-0.227	0.965	-0.479	-0.488	1.981
	(0.180)	(0.248)	(0.824)	(0.371)	(0.495)	(1.652)
Risk	-0.069***	-0.094***	-0.091***	-0.138***	-0.186***	-0.178***
	(0.007)	(0.010)	(0.034)	(0.015)	(0.019)	(0.067)
PP&E	2.361	2.298	8.161	4.044	4.462	16.304
	(1.522)	(1.926)	(5.986)	(2.990)	(3.905)	(11.955)
PP&E^2	-1.305	-1.055	-4.092	-2.452	-2.546	-8.200
	(1.008)	(1.199)	(3.201)	(1.995)	(2.515)	(6.409)
Observations	8,936	5,019	919	8,936	5,019	919
Adjusted R-squared	0.184	0.185	0.080	0.182	0.183	0.085

Table 5
Triple-Differences Estimates: Regressions
This table shows the results from the following DID regression:

$$\begin{aligned} RiskShifting^{(x)}_{i,t} &= \alpha_i + \alpha_t + \beta Treated_{i,t} \times Shock_t \times \textit{ModDistressRisk}_t + \\ &+ \gamma Treated_{i,t} \times Shock_t \times \textit{HighDistressRisk}_t + \mathbf{X}_{i,t}\delta + \varepsilon_{i,t}, \end{aligned}$$

where $RiskShifting^{(x)}_{i,t}$ is $RiskShifting^{(1)}_{i,t}$ or $RiskShifting^{(2)}_{i,t}$. We set $Treated$ equal to one for all firm-year observations associated with firms headquartered in a county in which a hurricane causes more than \$100,000 in property damages and within the five year-window surrounding the hurricane strike. We exclude firm-year observations associated with firms that are struck by more than one hurricane or that do not have complete data over the five year-window. For each hurricane, we match the treated firm-year observations with firm-year observations associated with firms not struck by a hurricane over the same five-year window. We set $Treated$ equal to zero for these matched observations. We set *Shock* equal to one for treated and matched firm-year observations occurring after the hurricane strike. *ModDistressRisk* is a dummy variable equal to one for firms with a distress risk value above the median but below the ninth decile, else zero. *HighDistressRisk* is a dummy variable equal to one for firms with a distress risk value above the ninth decile, else zero. Distress risk is measured at the end of the fiscal year before the hurricane strike. $\mathbf{X}_{i,t}$ is a vector of control variables, including the double interactions (*Treated* × *Shock*; *Treated* × *ModDistress*; *Treated* × *HighDistress*; *Shock* × *ModDistress*; and *Shock* × *HighDistress*), the main effects (*Shock*; *Treated*; *ModDistress*; and *HighDistress*), and the other controls (*Assets*; *Risk*; *PP&E*; and *PP&E*^{2}). We describe the construction of the other controls in Table A.1. β, γ, and δ are parameters; α_i and α_t are firm- and year-fixed effects; $\varepsilon_{i,t}$ is the residual. For each treated and control firm, we only ever include one observation before the hurricane strike and one after. The table shows parameter estimates and standard errors (in parentheses). For the sake of brevity, we do not report the coefficients on the main effects and the double interactions. Standard errors are calculated from White's (1982) formula. "***", "**', and "*' indicate significance at the 99, 95, and 90% confidence levels, respectively.

	Endogenous Variable	
	Risk-shifting$^{(1)}$	*Risk-shifting*$^{(2)}$
Treated × *Shock* × *ModDistress*	3.345***	6.439***
	(1.162)	(2.483)
Treated × *Shock* × *HighDistress*	-0.713	-1.477
	(1.742)	(3.472)
Assets	-0.142	-0.297
	(0.125)	(0.253)
Risk	-0.075***	-0.15***
	(0.005)	(0.011)
PP&E	2.400**	4.346**
	(1.081)	(2.150)
PP&E^2	-1.311**	-2.619*
	(0.669)	(1.359)
Main effects	YES	YES
Double interactions	YES	YES
Observations	14,874	14,874
Adjusted R-squared	0.184	0.184

Table 6
Future Failure Rates
This table reports the failure rates of firms with different levels of pre-hurricane distress risk and located inside (Panel A) or outside (Panel B) of the hurricane-affected regions over two post-event window periods. The firms located in the hurricane-affected regions (the treated firms) are those headquartered in a county in which a hurricane causes more than $100,000 in property damages. We exclude from this sample those firms that are struck by other hurricanes and those that do not have complete data over the five year-window surrounding the hurricane strike. We match the treated firms with firms not struck by a hurricane over the same five-year window (the control firms). We separately split the treated and control firm sample into low, moderate, and high distress risk firms. We do so according to their distress risk values at the end of the fiscal year before the hurricane strike. “Low” contains firms with a distress risk value below the median, “Mod(erate)” those with a distress risk value above the median but below the ninth decile, and “High” the residual firms. “Observations” shows the number of firms. “Failure Rates (2-to-5 Years)” (“Failure Rates (2-to-10 Years)”) gives the fraction of firms that fail during the three (seven) years following the event period (two years prior to the hurricane to two years after). Failures are bankruptcy filings and performance-related capital market delistings.

	Distress Risk		
	Low	Mod.	High
Panel A: Treated (Hurricane-Struck) Firms			
Observations	103	60	14
Failure Rates (2-to-5 Years After Hurricane Strike)	13.6	31.7	21.4
Failure Rates (2-to-10 Years After Hurricane Strike)	27.2	63.3	35.7
Panel B: Control (Non-Hurricane-Struck) Firms			
Observations	4,387	2,467	446
Failure Rates (2-to-5 Years After Hurricane Strike)	15.5	21.2	31.8
Failure Rates (2-to-10 Years After Hurricane Strike)	31.3	42.5	53.1

Table 7
Covenant Violations and Risk-Shifting
This table reports the fraction of financial covenant violators among samples of firms with different levels of pre-hurricane distress risk and located inside (Hurricane-struck: Yes) or outside (Hurricane-struck: No) of the hurricane-affected regions (Panel A). It also reports the correlation between dummy variables indicating whether or not a firm has violated financial covenants over the recent past and the risk-shifting proxies (Panel B). The firms located in the hurricane-affected regions (the treated firms) are those headquartered in a county in which a hurricane causes more than $100,000 in property damages. We exclude from this sample those firms that are struck by other hurricanes and those that do not have complete data over the five year-window surrounding the hurricane strike. We match the treated firms with firms not struck by a hurricane over the same five-year window (the control firms). We separately split the treated and control firm sample into low, moderate, and high distress risk firms. We do so according to their distress risk values at the end of the fiscal year before the hurricane strike. "Low" contains firms with a distress risk value below the median, "Mod(erate)" those with a distress risk value above the median but below the ninth decile, and "High" the residual firms. "Fraction Covenant Violators During the Last X Years" gives the fraction of firms that have violated at least one financial covenant over the last X fiscal years. "Dummy Covenant Violator During the Last X Years" is a dummy variable equal to one for firms that have violated at least one financial covenant over the last X fiscal years.

Panel A: Proportion of Covenant Violators

	Hurricane	Fraction Covenant Violators During the Last				
Distress Risk	Struck?	1 Year	2 Years	3 Years	4 Years	5 Years
Low	Yes	0.012	0.012	0.012	0.024	0.036
Mod.	Yes	0.107	0.143	0.214	0.250	0.250
High	Yes	0.000	0.429	0.429	0.571	0.571
Low	No	0.012	0.024	0.035	0.047	0.061
Mod.	No	0.048	0.104	0.141	0.173	0.203
High	No	0.069	0.196	0.261	0.302	0.326

Panel B: Correlation Between Risk-Shifting and Covenant Violator Dummy

	Dummy Covenant Violator During the Last				
	1 Year	2 Years	3 Years	4 Years	5 Years
$RiskShifting^{(1)}$	0.047	-0.038	-0.069	-0.094	-0.094
$RiskShifting^{(2)}$	-0.346	-0.209	-0.386	-0.351	-0.351

Table 8
Parallel Trends
This table shows the change in several analysis variables over various pre-hurricane event windows. The pre-hurricane event windows cover the one, two, three, four, or five fiscal years prior to the hurricane strike. The treated firm-sample includes all firms headquartered in a county in which a hurricane causes more than $100,000 in property damages. We exclude from this sample those firms that are struck by other hurricanes and those that do not have complete data over the five year-window surrounding the hurricane strike. We match the firms struck by a hurricane (the treated sample) with firms not struck by a hurricane over the same five-year window (the control sample). The analysis variables are described in Table A.1 in the Appendix. In addition to reporting the mean change for each analysis variable both for the treated- and control firm-samples, we also show the p-value of a t-test of the hypothesis that the mean change is different for the two samples.

		Length of Pre-Hurricane Period Over Which the Change is Measured (in Fiscal Years)				
Change in		One	Two	Three	Four	Five
RiskShifting	Treated	0.24	-0.43	0.96	-0.57	0.03
	Controls	-0.13	-0.08	-0.05	-0.08	-0.09
	P-value (Diff)	(0.745)	(0.683)	(0.304)	(0.684)	(0.946)
DistressRisk	Treated	-0.02	0.00	-0.01	-0.01	-0.02
	Controls	0.01	0.00	0.00	0.00	0.00
	P-value (Diff)	(0.152)	(0.672)	(0.523)	(0.749)	(0.291)
BookToMarket	Treated	-0.02	-0.03	-0.03	-0.05	-0.06
	Controls	0.02	-0.02	-0.02	-0.02	-0.02
	P-value (Diff)	(0.263)	(0.465)	(0.636)	(0.303)	(0.355)
Capex	Treated	-0.07	-0.03	-0.02	-0.07	-0.11
	Controls	-0.01	-0.01	-0.01	-0.01	-0.01
	P-value (Diff)	(0.161)	(0.682)	(0.921)	(0.291)	(0.198)
Leverage	Treated	-0.02	0.01	0.00	0.00	0.01
	Controls	0.00	0.00	0.00	0.00	0.00
	P-value (Diff)	(0.235)	(0.218)	(0.867)	(0.803)	(0.741)
PP&E	Treated	-0.01	-0.02	-0.02	-0.03	-0.05
	Controls	-0.01	-0.01	-0.01	-0.01	-0.01
	P-value (Diff)	(0.996)	(0.292)	(0.539)	(0.239)	(0.229)